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Working Papers in Linguistics No. 52

## HISTORICAL LINGUISTICS

*Edited by*

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Summer 1999

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Working Papers in Linguistics No. 22

# HISTORICAL LINGUISTICS

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Summer 1999

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Historical Linguistics

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## Introduction

As a discipline, Historical Linguistics takes in a number of related but distinct pursuits. Historical linguists compare different stages of the one language to determine what changes have taken place, they examine present-day variation for evidence of language change in progress, they reconstruct earlier pre-historic stages of languages and language families, they determine relationships among languages, they investigate the effects of contact between and among speakers of different languages, they analyze earlier synchronic stages of a language, and so on. In short, they cover most aspects of both the study of language history and the study of language change.

The papers in this volume present research carried out primarily by graduate students in linguistics, but in two instances by graduate students in the Department of Slavic and East European Languages and Literatures (Weller and Whaley), in one instance by a undergraduate minor in linguistics (Karnitis), and in one instance by a visiting scholar (Arvaniti) engaged in a research project with a Linguistics Department faculty member. Overall, these papers represent the field of historical linguistics well, inasmuch as there are papers on aspects of the history of individual languages, on variation as it pertains to change in language, on reconstruction, on language relatedness and the methods for testing such relations, and on patterns of change in phonology, morphology, syntax, and semantics.

Brian D. Joseph

Columbus, June 1999



## Chinese and Austronesian: what's up?

Martin Jansche

### 0 Introduction

In a series of recent papers, Laurent Sagart (Sagart 1993a,b, 1994, 1995a,b, as well as earlier) has revived the claim that the Chinese languages are genetically related to the Austronesian family (see e.g. Sagart 1993b, n. 1) for references to earlier proponents of this view). The evidence he presents consists mainly of lexical similarities between Proto-Austronesian (PAN, 3000 BC according to Sagart 1994, 4000 BC according to Blust 1995) and Old Chinese (OC, 800–500 BC according to Sagart 1993b, at most 1100 BC according to Sagart 1994), on the basis of which he constructs reportedly regular sound correspondences (Sagart 1993b) that might be taken to explain the origin of Middle Chinese tones (Sagart 1993a). More recently Sagart also claims to have found morphological correspondences as well (Sagart 1994; Baxter & Sagart 1998).

Sagart's early views have been challenged early on by Matisoff (1992), whose cautionary remarks about long-distance comparisons (Matisoff 1990) in general certainly apply to Sagart's claims as well. Matisoff (1990, §2.1) points out that similarities between languages can be due to regional diffusion, calquing, loans, etc., whereas true genetic relationships can be obscured by contamination, blending, folk etymology, etc. His complaints that "Sagart's criteria for phonological correspondence are lax, so that it is easy to find lookalikes in the huge [Austronesian] and Chinese lexica" (Matisoff 1992, p. 159) and that "[h]is criteria for semantic correspondence are also extremely tolerant, and often a prioristic" (*op. cit.*, p. 160) have been noticed by Sagart (1994, in the abstract).

Sagart (1994, abstract) addresses Matisoff's objections and promises "a highly constrained methodology" (see p. 5 below) and a re-evaluation of his own earlier proposals. Since I am not aware of a more recent version of Sagart's proposal, I will mainly review

Sagart 1994.<sup>1</sup> Sagart (1994, p. 275) presents "two kinds of linguistic evidence: morphological congruence [...] and sound correspondences in the basic vocabulary". I will address each kind of evidence below and conclude that neither is conclusive.

Li (1995) objects to Sagart's revised (1994) proposal, coming to much the same conclusions as Matisoff (1992). Although Li (1995) backs up his claims with specific references to the work he is criticizing, many of the specific points he finds unsatisfactory are taken from Sagart 1993b and have been revised in Sagart 1994. The issue of loose semantic cognates (Li 1995, p. 94) has been explicitly addressed by Sagart (1994), as has the level of reconstruction of Austronesian morphology (Li 1995, p. 95). (Sagart (1995a, p. 361ff.) makes the same point in his reply to Li 1995.) Unfortunately Li (1995) does not provide a detailed discussion of why Sagart's claims should not be accepted, but instead concentrates on a lengthy comparison of the Austronesian and Sino-Tibetan core vocabularies, showing that there are few (if any) plausible cognates to be found.

Other critical voices include Pulleyblank (1995) and much more so Blust (1995), sharply contradicted by Starosta (1995). While I am still surprised about the number of people engaged in this debate given the rather weak evidence for Sagart's original claims, I will refer to the valuable points contributed by these researchers in the discussion below. The remainder of this paper is organized similar to Sagart 1994: after a discussion of the morphological data in Section 1, I turn to his lexical comparisons in Section 2, which leads directly to a review of the claimed phonetic correspondences in Section 3. Section 4 tries to caution anyone from jumping to conclusions.

## 1 Morphological congruence?

Sagart (1994) considers morphological similarity between two (groups of) languages as key evidence for their genetic relatedness. He declares (p. 274) that "[i]t is generally considered that morphology is highly stable and not borrowable [...]. For that reason the diagnostic value of morphological congruence in determining genetic relationships is particularly high." (This view is in and of itself not uncontroversial, but let's believe him for the moment.)

In the case at hand, it might not be totally obvious that there is anything here that can be compared. While there is hardly any controversy about assuming a rich derivational morphology for Proto-Austronesian, it is only recently that the traditional view concerning Old Chinese morphology, which amounted to the belief that Old Chinese was lacking morphology completely, was challenged. Baxter & Sagart (1998) cite a proponent of this

<sup>1</sup>Citations consisting only of a page or section reference will therefore refer to Sagart 1994. Reconstructed forms and glosses are taken from that article as well, unless otherwise noted. Sagart's Old Chinese forms first mention Li Fāngguī's reconstruction (see for example Li 1982) and then in parentheses Baxter's (1992) as modified by Sagart.

belief and then move on to present a fair amount of evidence for a number of infixes and suffixes, whose existence had been assumed e.g. by Baxter himself for some time; for a larger class of prefixes than had previously been postulated; as well as for reduplication processes, whose existence is almost uncontroversial, since they are better reflected in the written texts, whereas the affixes are presumably not represented in the script. If the affixes really existed, it is for this reason that they have been ignored by traditional scholarly work on earlier stages of Chinese, which too often focused exclusively on the writing system and the phonological information that it could record.

I will first review Baxter & Sagart's (1998) arguments for a more elaborate Old Chinese morphology. The morphemes they propose subsume those in Sagart 1994, so any criticism applies to Sagart 1994 as well. Moreover, in some cases where Baxter & Sagart (1998) propose morphemes that had not been assumed previously, they have to modify Baxter's (1992) reconstruction creating structures incompatible with Sagart's (1994) claims. Still, their approach does bring a new quality into the existing efforts of reconstructing Old Chinese that serves both Sagart's and Baxter's agenda, which I take to be the following ones.

Sagart is trying to maximize the structural similarities between Old Chinese, which had occasionally been believed to be lacking any morphological processes at all, and Proto-Austronesian, which must have had a rather complex morphology, given the diverse affixation and reduplication processes found in its descendants.

Baxter on the other hand has for some time been trying to do away with the huge onset clusters that have been posited for the Old Chinese monosyllables: for example, whereas LI (1982) reconstructs \*hrjəgw as the Old Chinese reading of 收 (Mandarin *šow*l, 'receive'), Baxter (1992) has \*xjiw. Postulating more morphological processes than had previously been assumed for Old Chinese allowed Baxter to ignore certain irregularities in sound correspondences that had hitherto either prompted the reconstruction of onset clusters or gone unexplained.

For example, Baxter (1992) has 育 \*ljuk 'give birth, rear, raise, bring up, grow up, nourish' and 鞠 \*k(r)juk 'nourish' (glosses from Baxter & Sagart 1998). Baxter & Sagart (1998) however argue for a prefix \*k- that would explain both forms as stemming from a single root \*ljuk with 鞠 \*k-ljuk the derived form.

Although I tend to agree with Baxter's goals of cutting down on implausible consonant clusters in Old Chinese forms, I would only be convinced that postulating a richer morphology is the way to go if Baxter and Sagart were able to provide some content for their morphemes. After all, morphemes are minimal units of meaning, but Baxter & Sagart (1998) seem to treat morphemes rather as units of minimal meaning. About \*k- they say that its "function [...] is difficult to specify with certainty" (*op. cit.*, p. 47), and in the case of 育、鞠, "the form without \*k- seems general in reference, the form with \*k- more specific" (*op. cit.*, p. 49), which is too unspecific for my taste.

When Sagart (1994, §2) compares Proto-Austronesian and Old Chinese morphology, his analysis suffers from similar deficiencies. For example, he tries to establish a morpheme 'distributed action/object' whose Proto-Austronesian manifestation is the infix *\*-ar-* corresponding to Old Chinese *\*-r-*. In Old Chinese, for example, there is a verb 合 *\*gəp* (*gop*) that Sagart (p. 277) glosses as 'join (two things together)' in one case, and as 'join, unite' a few lines further on. However, a derived form corresponding to the first meaning is 洽 *\*grəp* (*g-r-op*) 'unite (more than 2 things)', whereas the second meaning of 合 combines with the infix *\*-r-* to form 𠔁 *\*gəp* (*g-r-op*) 'name of a sacrifice to ancestors taken collectively'. What seems questionable about this is whether there really were two words *\*gəp* (*gop*) that were (a) homophonous, (b) close in meaning, and (c) both written as 合, yet still distinct, for when each of them combines with *\*-r-*, the results are semantically quite distinct. If there was only one word 合 *\*gəp* (*gop*), one of the alleged derived form isn't really derived from it, or is derived via a different morphological process. The words denoting these distributed actions or objects derived by *\*-r-* are rather diverse in meaning anyway, perhaps pointing to more varied reasons for their existence. The case for Proto-Austronesian is not very clear in the first place (Blust 1995, p. 286f.), and perhaps Sagart is overly optimistic about having found its remote cousin.

The other infix that Sagart identifies is a nominalizer PAN *\*-in-/OC \*-j-* (p. 278f.). The problem with this is that the Old Chinese derived forms are much more specific than the Austronesian forms. Whereas Chamorro has *faisen* 'to ask' and *if-in-aisen* 'the question', Old Chinese has 火 *\*hmərx* (*hmoj?*) and 燧 *\*hmjərx* (*hm-j-oj?*) 'land cleared by fire' where 'land' and 'cleared' are overly specific. Blust (1995, p. 284ff.) provides a detailed description of the meaning and use of PAN *\*-in-* and finds only little overlap with the alleged functions of OC *\*-j-*. Even Starosta, who views Sagart's proposal much more positively, voices his doubts about the claimed functional similarity between the compared morphemes (Starosta 1995, p. 380f.).

Sagart notes (p. 275) that Austronesian and Old Chinese have in common the place where the infix is inserted into a stem, namely before the first vowel. As Sagart himself points out, PAN *\*-in-* has a prefix variant *\*ni-* (p. 278), which points to a typologically common alternation in infixhood vs. prefixhood that is determined by the shape of the morpheme: vowel-initial infixes can be seen as secondary prefixes, since they attach to a stem as far left as possible but without creating an onsetless syllable. The shared insertion site is thus consistent with typological generalizations (Ulan 1978) and does not necessarily point to a similarity that could only be attributed to a genetic relationship.

Finally, Sagart presents a stative/intransitive verb prefix PAN *\*ma-/OC \*N-* (p. 279ff.) and devotes a lengthy discussion to justifying the Old Chinese nasal prefix *\*N-*. On Sagart's interpretation, this prefix would have reflexes in Chinese loanwords in a number of Miao-Yao languages, where it occasionally corresponds to prenasalized stops. Although this argument is presented in much detail, one keeps wondering why it is not taken up or at least referred to by Baxter & Sagart (1998), who are primarily concerned with evidence



for Old Chinese morphology. Blust (1995, p. 287) finds the semantic categories associated with PAN *\*ma-* and OC *\*N-* to have little in common. Although Starosta (1995, p.383) shows how a difference in the reconstructed Austronesian form of the infix can lead to a closer functional similarity with the Old Chinese prefix, this cannot silence Blust's (1995) suspicion that the basis of comparison might have been rather narrow: It is interesting to compare the reconstruction of the Old Chinese prefix *\*N-* in Baxter & Sagart 1998 with that in Baxter 1992, where one finds *\*f-*, following earlier work by Pulleyblank, who joins the debate (Pulleyblank 1995) telling us that he now prefers to reconstruct a 'pharyngeal glide *\*ä-*' instead. The only concrete evidence for these three different reconstructions is a voicing contrast in the Middle Chinese stops still found today in the Wú 吳 languages. In light of this sparse evidence, Baxter & Sagart's (1998) reconstruction of a nasal prefix seems almost as arbitrary as the other two choices (a nasal could be seen to be less "marked"), but serves Sagart's agenda of making the Old Chinese prefix look more similar to the Proto-Austronesian one, while the alleged similarity amounts to at most one shared feature.

So an obvious shortcoming (quite literally) of Sagart's comparison of morphemes is the shape of the things under comparison: in most cases the observed similarities hold for exactly one segment, in the last case only for one feature of one segment. How much smaller can it get?

## 2 Evidence from the lexicon?

Sagart (1994, §3) discusses lexical similarities between Old Chinese and Proto-Austronesian extensively and regards it as one kind of evidence for their alleged relatedness. In doing the comparison he claims to have been guided by the following principles.

- "First, strict requirements have been applied to the circumstances of attestation of the lexical material under comparison, as a partial hedge against late innovations." (p. 281)

On the Chinese side, this means that entries from the post-Hàn rhyme dictionaries, which reflect Middle Chinese phonology, could not be used—or rather: could no longer be used, but had been earlier (Sagart 1993b).

For Austronesian, this means that only items belonging to the earliest and most inclusive level, Proto-Austronesian, could be used for comparison, but no forms that had only been reconstructed up to, say, Malayo-Polynesian, which again puts a limit on the number of comparable items that is not found in Sagart 1993b.

- "Second, comparisons involving onomatopes and words of expressive origin have not been considered for comparison." (p. 282) Sagart notes that "[t]his excludes 62 of the 231 roots" in Blust 1988, but does not mention that it also gets rid of 25–50 of

the 222 lexical matchings in Sagart 1993b, the exact first number depending on what one considers as onomatopoeic.

- "Third, close semantic matchings have been required. Where semantic shifts are needed, evidence that the shift occurred in other languages [...] is presented." (p. 282)

Sagart concludes his methodological preliminaries with the statement that "[a]s a result of applying these controls, only a small number of comparisons (56 altogether) can be presented here" (p. 282). Of course, 'only' and 'here' only make sense to the reader who is familiar with Sagart's (1993b) earlier list of more than 200 matchings.

Given these provisions, it would seem that Sagart is clearly not guilty of the kind of blind, multi-lateral comparison that Matisoff (1990) and others have been criticizing. However, while following the traditional scholarly approach to lexical comparison Sagart was not always very careful in observing his own guidelines set out before. Moreover, some of his suggestions cannot possibly count as evidence for a genetic relationship. Let me elaborate on this last point, before I return to the issue of how much care Sagart exercised in selecting matching forms.

Occasionally when there is a semantic mismatch between Old Chinese and Proto-Austronesian forms, Sagart cites similar semantic developments found in other languages, as noted in his third methodological remark cited above. Though Sagart never explicitly says that this should be seen as support for the relatedness of imperfect semantic matches, which could be reflexes of a semantic change just like a known change he cites, the very fact that he mentions these similar developments makes me think that he attributes some value to it.

He should not.

Something like the following might be regarded as a valid, but weak, argument supporting a particular sound correspondence that Sagart has to postulate: "Proto-Austronesian *\*-mp-* corresponds regularly to Old Chinese *\*p<sup>h</sup>-*, in which case one might take *\*-mp-* to be a direct reflex of a common ancestor and *\*p<sup>h</sup>-* as an innovation—in fact, the development of aspirated stops from earlier prenasalized unaspirated stops has been attested in a number of Bantu languages." (Based on, but not quoting, Sagart 1993b, p. 13.) Regular sound change, though not predictable, is based on presumably universal properties of the human physiology and cognitive abilities. If an observed phonetic similarity among one pair of languages constitutes a regular correspondence as a result of sound change starting from a stage of uniformity, then this is at least consistent with the human physiology etc. Thus if an analogous similarity is observed in another pair of languages, for instance Old Chinese and Proto-Austronesian, raising the status of this similarity to that of a regular correspondence cannot be dismissed outright.

For semantic similarities however, pointing out analogous correspondences in a pair of related languages does not lend any more plausibility to the claim that semantically similar items are in fact cognates. Even though semantic notions might be universal (especially if the concepts referred to are), the links among semantic concepts and those between semantic concepts and phonetic forms are not. In particular, if Sagart regards Proto-Austronesian *\*-luR* 'to flow' and Old Chinese 水 *\*hwrjidx* (*hl-j-uj?*) 'river; body of water; water' (§3.9, p. 285) as cognate forms, it is irrelevant that "[b]oth meanings of 'water' (as substance) and 'river; body of water' are *derivable* from an original meaning 'to flow': cf. IE *\*wedor* 'water', from a root *\*-wed* 'wet; to flow'" (*ibid.*, my emphasis). I do not object to the word for 'water' being *derivable* from the word meaning 'to flow'. However, unless this derivation is actually attested (or inferred indirectly based on solid evidence), this relationship remains a mere possibility. Moreover, the purported fact that a similar semantic correspondence can be found internally in (Proto-)Indo-European would only bear on this issue if semantic change were based on universal human traits and, therefore, largely regular not only within one language, but also across languages.

Similar remarks apply to Proto-Austronesian *\*q<sub>2</sub>asiN* 'salt' and Old Chinese 辛 *\*sjin* (*s-j-in*) 'hot-tasting, pungent, bitter', which are claimed to be cognates (§3.22, p. 288). It does not help if Sagart cites scholarly work that relates "Lithuanian *suurus* 'salt' [to] an IE word meaning 'sour'" (*ibid.*, another example follows). The fact remains that 'salt' does not mean the same as 'hot-tasting' or 'bitter'. Exploiting the diversity of the reconstruction sources, one might just as well relate Old Chinese *\*sjin* 'bitter' to Dempwolff's Proto-Austronesian *\*pahit* 'to be bitter' (Dempwolff 1938, p. 111), presumably from a hypothetical ancestor *\*pafin* and *\*f > \*x > \*h* together with denasalization in Proto-Austronesian, while in Chinese the first syllable *\*pa-* of the ancestor form was lost on the way to *\*jin*, which was then reanalyzed as derived from *\*sin* plus a palatal infix *\*-j-*. I do not seriously want to propose such an alternative view, but I do find it quite amazing how easily one can "establish" other "cognate" sets that all in all seem just as plausible as Sagart's, the only difference being that semantic similarity is enforced more strictly at the cost of phonetic similarity. In sum, the task of establishing cognate forms becomes a matter of balancing semantic and phonetic (dis)similarity. Sagart's strategy is to maximize overall phonetic similarity,<sup>2</sup> perhaps because there are less stringent requirements on semantic similarity in cognates.

This last point already illustrates one of the main defects of Sagart 1994, namely the amount of semantic mismatches between allegedly related forms, which goes against Sagart's own third methodological remark (see p. 5 above) requiring "close matchings". This is not to say that there cannot be semantic mismatches between forms that have independently been established as cognates. Strong independent evidence for two semantically different forms being cognates can come from an already established regular sound corre-

<sup>2</sup>cf. his *bon mot* "There is no sound methodological alternative to requiring regular sound correspondences in comparative work" (p. 275)

spondence, but in order to find regular sound correspondences one has to have identified semantically related pairs. However the starting point of such an admittedly circular process should leave as little doubt as possible about what forms are related. Therefore, we would require something close to semantic identity between lexical items from the two languages, only allowing for some inaccuracies in the glosses, in order to establish phonetic correspondences. That done, we might proceed to less clear semantic matches and look for corroborating evidence.

Since Sagart 1994 is at most a starting point (if not a dead end) for a comparison between Old Chinese and Proto-Austronesian, one would want to see very close semantic matches indeed. For my taste, this would specifically exclude pairs like 'neck'/'gullet' (§3.3, p. 284); 'parasitic plant species'/'an edible fern' (§3.16, p. 286f.); 'opposite shore'/'far demonstrative' (§3.19, p. 287); 'salt'/'hot-tasting, pungent, bitter' (§3.22, p. 288); 'grasp in the fist'/'catch' (§3.30, p. 289); 'hack, chop into pieces'/'to cut off' (§3.44, p. 293); 'insert, stick into a soft surface'/'pierce, prod, stab' (§3.45, p. 293); 'cram, crowd'/'stop up, block' (§3.46, p. 293); 'rice gruel; to mix'/'cooked rice or millet' (§3.53, p. 294f.); 'rice [as food]/'peeled grain, rice'<sup>3</sup> (§3.54, p. 295); and 'torch'/'fire' (§3.56, p. 295). If we exclude these 11 pairs from Sagart's list, we are down to 45 comparable items.

Let me now return to Sagart's second methodological point (see above), which requires that we discount onomatopoeic and expressive words. A few such items can still be found on Sagart's list. Sagart himself notes that the words for 'to suck', PAN *\*-sep*, OC *\*tsap* (*ts[i,u]p*) (§3.48, p. 293f.) and for 'to hammer, pound', PAN *\*tuqtuq*, OC *\*təgwɣ* (*tuʔ*) (§3.52, p. 294) are quite likely onomatopoeic. In addition to this, I would also consider as onomatopoeic or expressive PAN *\*u(n)taq* 'to vomit', OC *\*thagx* (*thaʔ*) 'to eject from the mouth' (§3.5, p. 284); PAN *\*tuktuk* 'beak of a bird; to peck', OC *\*tuk* (*tok*) 'to peck up' (§3.15, p. 286); PAN *\*ɲitɲit* '[to] gnaw', OC *\*ɲiat* (*ɲet*)<sup>4</sup> 'gnaw, crunch in the teeth' (§3.38, p. 291); and PAN *\*paqpaq* 'chew', OC *\*bagh* (*N+paʔ+s*) 'to chew, have food in the mouth' (§3.40, p. 292), which all refer to parts of the mouth (or the analog of it in other species), or activities involving the mouth (etc.) or throat. There may be arguments about the status of each of these forms, but if we are trying to maximize the quality of the comparison, we would want to exclude the more dubious cases. Excluding all of these pairs from Sagart's list leaves us with only 39 remaining items.

When Sagart notes the "scarcity of cultural items in the [...] list" (p. 295) he is referring to 4 items on his original list, namely those discussed in his §3.53–56, 'rice gruel/

<sup>3</sup>The descendants of Proto-Austronesian *\*imay* all refer to cooked rice or rice as food, which Sagart mentions in the section title, while the Old Chinese term 米 *\*midx* (*mijʔ*) refers to peeled grain, perhaps not necessarily rice. Note that in modern-age Mandarin, which, by the way, draws a clear distinction between rice on the field and rice on the table, 米 *mi* is a derivational suffix referring to all kinds of peeled food, e.g. 花生米 *xwəʔɣɲɲ mi* 'peeled peanuts' from 花生 *xwəʔɣɲɲ* 'peanut'.

<sup>4</sup>Especially since the initial segment is a velar, or – in traditional Chinese terminology – a 'molar' sound 牙音.

cooked rice', 'rice as food', 'house', and 'torch/fire', which he classifies (p. 282) as denoting "cultural notions of great antiquity". However, I excluded 3 of these 4 forms as imperfect semantic matches and find even the remaining one, 'house', questionable, since the Old Chinese word 廡 \*mjagx (*m-j-a?*) seems rather rare for something referring to an important cultural concept. However, Sagart's first methodological principle (see above) would require him "to exclude rare words" (p. 281).

Sagart instantly dismisses the absence of cultural concepts from his list as "not necessarily meaningful" (p. 295) and notes that "a majority of items in the list are not of a kind that would normally lend itself to borrowing in a limited contact situation" (p. 295f.). He also takes it to be significant that most words on his list are verbs, "a word class believed to resist borrowing well" (p. 296) as he claims. Sagart effectively forestalls certain complaints that personal pronouns and numerals are completely absent from his list when he declares that "[t]his is not abnormal in the East Asian context" (*ibid.*). Sagart correctly points out that addressing people directly is traditionally avoided in the macro-culture of this area, and personal pronouns have therefore often been replaced by hedging expressions. Moreover, there are a number of cases where numerals have been borrowed. However, the last argument can be turned against Sagart. If we had been thinking that numerals were resistant to borrowing but are shown cases where numerals might actually have been borrowed in the East Asian area (Sagart 1995b, p. 202ff.), why should we still believe that verbs are any more resistant?

### 3 Sound correspondences?

Sagart (1994, §4) includes a quick summary of the sound correspondences that hold between the Proto-Austronesian form and the Old Chinese form in each item on his list. The most striking aspect of this comparison is that Proto-Austronesian words are in general polysyllabic, whereas Old Chinese words are almost exclusively monosyllabic (at least all forms on Sagart's list are). He concludes that "[t]he ancestor language ("Proto-Sino-Austronesian" [...]) is assumed to have been polysyllabic [...]. The shift to Chinese monosyllabism occurred through the loss of nonfinal syllables" (p. 296). As an example for a similar development, he cites Huihui, a Chamic language spoken on Hainán 海南 island off the Southern coast of China, which is developing into a monosyllabic tone-language, presumably under regional influence from Chinese, from an earlier non-tonal, polysyllabic stage (more examples are provided in Sagart 1993a).

What this claim leaves unexplained is the alleged presence of remnants of Austronesian infixes in Old Chinese words. This was pointed out by Blust (1995, p. 286). He explains that PAN \*-in- attaches after the first consonant of a stem, so that one finds morphologically complex words of the shape \*C-in-VCVC in Proto-Austronesian. But if all syllables except the last one are lost on the way to Old Chinese, how did the infix survive?

The material that can be compared phonetically thus has the shape of Proto-Austronesian (final) syllables: CV(C). This is partly because "affixes, whether OC or PAN, have been disregarded" (p. 296). For the Old Chinese forms, disregarding affixes often implies getting rid of initial consonant clusters by identifying certain segments as (reflexes of) prefixes or infixes. For example, when comparing PAN \*paqpaq '[to] chew' with OC \*bagh ( $N+pa?+s$ ) 'to chew, have food in the mouth' (§3.40, p. 292), Sagart takes the final syllable \*-paq of the PAN word and compares it to the OC stem \*pa?. He has thus identified the sound correspondence \*q ~ \*? (p. 297), apart from the trivial correspondences.

Although Sagart does not quite engage in megalocomparison, parts of Matisoff's (1990) criticism of ill-founded comparative practice do apply in Sagart's case as well. In particular, Matisoff (1990, §4.1) criticizes Paul Benedict's conception of the development of the Tai-Kadai and Hmong-Mien branches from Proto-Austro-Tai: under areal influence of Chinese, the languages of these two branches have reduced the complex polysyllabic morphemes of the proto-language to tone-bearing monosyllables.<sup>5</sup> Given this disparity between the rich proto-forms and its impoverished descendants, Matisoff concludes (*op. cit.*, p. 116), "the etymological possibilities are endless."

Clearly, the same can be said about Sagart. Once the basis for comparison has been narrowed down to two or three segments, it is not surprising that he has found what he was looking for, especially since phonetic matches do not have to be perfect as long as the later forms are endpoints of a "natural" sound change leading away from a common ancestor.

The "adjustments due to change in the OC transcription system" and the "modifications and innovations, which will be fully justified elsewhere [probably Baxter & Sagart 1998]" (p. 296) are not as minor as Sagart makes them appear. Identifying a more elaborate Old Chinese morphology (as mentioned above) will of course result in a re-evaluation of the Old Chinese reconstruction efforts. Moreover, Sagart not only made some adjustments in his transcription system, he in effect switched from Li Fāngguì's reconstruction, on which Sagart 1993b was based, to a modified version of Baxter's (1992) system that incorporates the richer morphology described in Baxter & Sagart 1998. Although most of the sound correspondences that Sagart (1993b) had originally proposed could be maintained, the proposed correspondence PAN \*-s ~ OC \*-h is now no longer supported by Sagart's (1994) data, as he himself notes (p. 298). However, that correspondence had played a crucial role in Sagart 1993a, §2.1.3, where it was taken to explain the later development of the falling tone in Middle Chinese.

Recall that Baxter & Sagart (1998) proposed a prefix \*k- for Old Chinese, which I found somewhat dubious above (see p. 3). But even if we believe Baxter & Sagart (1998), their strategy may backfire, at least as far as Sagart's agenda are concerned. For instance, whereas Baxter (1992) reconstructs the Old Chinese reading of 結 as \*kit, Baxter & Sagart

<sup>5</sup>Incidentally, these examples are mentioned in Sagart 1993a in his discussion of monosyllabic reduction and tonogenesis.

(1998, p. 57) have *\*k-lit*, probably because of other characters with the phonetic component 吉, such as 繫 *\*li* (Baxter's reconstruction). But Sagart (1994) wants to relate 結 'to tie, knot' to Proto-Austronesian *\*SikeC* 'tie, attach to' (§3.50, p. 294), cf. also his earlier view (Sagart 1993b, p. 40) that Old Chinese 結 *\*kit* is related to Dempwolff's Proto-Austronesian *\*Ra(ŋ)kit* 'tie together, raft' and Blust's root *\*-kit* 'join along the length'. Since in other cases, Sagart disregards affixes in his comparisons, does this mean that in the present case we should ignore Old Chinese *\*k-lit* because it is morphologically complex and only let its stem *\*lit* enter into the comparison? If so, this pair involving the word for 'to tie' is off the list too.

While trying to make the morphological congruence between Proto-Austronesian and Old Chinese more plausible, Sagart postulated a richer morphological system for Old Chinese. However, that sometimes has led him to identify different stems for which the comparison with Proto-Austronesian suddenly breaks down. Sagart is apparently attempting a balancing act that makes his claims about morphology more plausible while trying to keep the lexical comparisons stable. But if Sagart's views on the reconstruction of Old Chinese phonology and morphology can and do change so easily—not that there's anything wrong with that—I would expect that he would be more cautious with his claims about Proto-Sino-Austronesian phonology. But the confidence with which Sagart has been advocating the Sino-Austronesian connection, while freely replacing the Old Chinese reconstructions he used with others, portray him to me as someone who thinks he knows what the truth is before he has found it.

How can we be sure that Sagart won't change his views in the future?

#### 4 Conclusions?

We can't.

Sagart (1994) ended his discussion by admitting that the Tibeto-Burman languages "may stand closer to Chinese (and to PAn) than I had originally assessed" (p. 303). In Baxter & Sagart 1998, n. 23 he has fallen back completely to the more traditional view that "Chinese and Tibeto-Burman are closer to each other than either is to Austronesian".

And rightfully so, since the evidence for Sino-Austronesian is still rather weak, to say the least. Even if we accept Sagart's entire list of matching words, we cannot be fully certain that the similarity is due to a genetic relationship and not to borrowing, especially since it is known that the Austronesians began their conquest of the Pacific islands from the Chinese coastal area and were therefore most likely in contact with the ancestors of the Chinese peoples. (See Matisoff 1992, p. 159 for the same argument.)

It is hardly worth reiterating that it cannot be proven that any two languages are unrelated. Unless we are presented good enough evidence to the contrary, we should assume

that Old Chinese and Proto-Austronesian are unrelated. Sagart's explanations for the absence of personal pronouns or numerals in his list (see p. 9 above) are therefore unnecessary, since the absence of anything would only need to be explained once there is compelling evidence in favor of a genetic relationship.

But there isn't.

There is at most evidence for maximally three reliable segments in Proto-Austronesian/Old Chinese word pairs in 38 (on my count) to 56 (on Sagart's (1994) count) cases, which should be subject to further scrutiny. I would think that chance (see Blust 1995, p. 286) and/or borrowing (see Li 1995, p. 95) stand a good chance of being borrowed as alternative explanations.

Unfortunately, Sagart (1994, §7) comes pretty close to "megalomania" when he considers the possibility of an "extended Austric superfamily" (p. 301). This is based on recent proposals for an Austric macro-family that would subsume Austronesian and Austro-Asiatic. But Sagart does not stop here, he also considers Tibeto-Burman as a likely candidate for membership (only to claim later (Sagart 1995b) that Chinese is more closely related to Austronesian than to Tibeto-Burman). Interestingly, in his assessment of the data used as evidence for a Chinese/Tibeto-Burman connection, Sagart (1994, p. 301f.) explains that the morphology shared between Chinese and Tibeto-Burman is "is limited, and the "basicness" of some of the [shared] lexical items, though suggestive, is no fool-proof guarantee against borrowing in an intimate, long-term contact situation [...]. The problem is compounded by poorly understood sound correspondences, which make it hazardous to distinguish between cognates, look-alikes, and loanwords." These are profound insights that seem to be completely absent from the earlier parts of his paper.

With so much proposed lumping going on already (see the discussion in Blust 1995, p. 291f.), it seems to be merely a matter of time until someone proposes an even bigger Asia-Pacific group that will include Sinitic, Austric, Tibeto-Burman, and in addition Miao-Yao (cf. Sagart 1994, p. 303), Tai and Hmong-Mien (Matisoff 1992, p. 159 mentions Paul Benedict's Austro-Tai hypothesis), Japanese (cf. Matisoff 1990, p. 115) on a proposed relationship between Japanese and Austronesian), Na-Dene (cf. Matisoff 1990, p. 118 on a proposal originally due to Sapir that relates Sino-Tibetan to Na-Dene), or even Indo-European (*ibid.*, citing Edwin Pulleyblank's hypothesis) as well. Next, one may believe that "genetic relationship is plainly transitive" (Greenberg's view, expressed in his *Language in the Americas*, cited in Matisoff 1990, p. 114) and obviously symmetric, and Sagart (1994, p. 300) apparently does subscribe to this when he says that "any relationship that is valid of either OC or PAn is valid of both". In this case one might add more languages and language groups based on a single hypothesis from the literature (e.g. Tamil, once claimed to be related to Japanese by Ohno, and hence Dravidian), which would ultimately lead to Proto-World. No-one who is sympathetic to this idea has to look very far to find forms that might make membership of Sino-Austronesian in Proto-World plausible: Sagart (1994,



§3.3, p. 284) has Proto-Austronesian \*[i]qeR 'neck' and (hence??) Old Chinese 咽 \*ʔin 'gullet', which are of course relatable to Greenberg and Ruhlen's Proto-World \**maliq'a* 'neck, to swallow'.

But then we might ask (to end on a low tone): so what.

### Acknowledgements

It's my pleasure to acknowledge the help of Janice Fon, Rich Janda, and Brian Joseph during various stages of the preparation of this paper. The usual disclaimers apply.

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### The Development of Counterfactuals with *thélo*: 'want' in Early Modern Greek

Panayiotis Pappas

#### 0. INTRODUCTION

This paper investigates the emergence of *tha*+IMPF as it is used in the apodosis of counterfactual conditionals in Modern Greek, for example in the sentence

- (1) *an eixa amaksi tha pe:gaina*  
 if have/1SG, IMPF car/ACC, SG go/1SG, IMPF

"If I had a car I would go"

This sentence expresses a meaning that is contrary to the real state of affairs (counterfactual), i.e. "I do not have a car, so I will not go".

*tha* is also known as the Future marker of Modern Greek as in the example

- (2) *tha pao: aurio*  
 FUT go/1SG, PERFVE tomorrow

"I will go tomorrow" (*tha* will be marked from now on as FUT in both cases).

in which case it developed from the construction *thelo*:+INF (e.g. *thelo: graphein*), also denoting future, via grammaticalization.

Grammaticalization is the process in which a content word (e.g. *boot*, *snow*, *play*) becomes a function word (e.g. *a*, *the*, and, *not*), according to Hopper and Traugott (1993); Bybee et al. (1994) describe it as the gradual development of grammatical morphemes out of lexical morphemes or combinations of lexical morphemes with lexical or grammatical morphemes. Thus, a content word can even become an affix through grammaticalization. It is a very common process in the world's languages. Examples can be found in Malaysian, Italian, Russian, Finnish, Inuit, Nung, Ewe etc.

This process has been arranged by most linguists on a cline, which is the succession of stages that a word goes through as it changes from a content word to an affix. Even

though there may be some disagreement about how the exact points on this cline should

be named, the following cline of grammaticality is, according to Hopper and Traugott, the best compromise:

content item > grammatical word > clitic > inflectional affix

In our case the content item is *thelo*: which eventually becomes the affix *tha*. It transpires (cf. Jannaris (1968), Horrocks (1997)) that at the same time (after the 4th century C.E.) that the present tense of the verb *thelo*: followed by the infinitive of another verb (e.g. *thelo: graphein* "I will write") was used as a future construction, the imperfect tense of the same verb (*e:thela*) followed by the infinitive of another verb (e.g. *e:thela graphein* "I would write") was used in the apodosis of counterfactual conditionals. The ways of expressing futurity and counterfactuality are then not almost identical in Modern Greek only, they also have extremely similar origins. While the development of *tha* in the future constructions has been well documented and discussed extensively in the literature, the development of the same form in the counterfactuals has not been thoroughly investigated; instead it has been generally assumed that its development has mirrored the process of the future constructions.

This paper investigates the development of *tha*+IMPF expressing counterfactuality and compares it to the development of *tha*+INFL futures<sup>1</sup>. The presentation proceeds in the following manner. First I review what is known about the development of the futures in order to give a point of comparison for the counterfactuals, and to establish what exactly this mirror-like development would entail. Then, I discuss the semantic association that exists between the futures and counterfactuals and which would lead us to believe that the two would have identical developments. Next, I present the results of a detailed analysis and a quantitative study of future and counterfactual forms found in documents spanning the 16th, 17th, 18th, and 19th centuries, after addressing some methodological issues concerning the documents and the various forms that were attested. I end by discussing how the results of the study lead to the conclusion that *tha*, as it is used in counterfactual constructions in Modern Greek, developed quite differently than the *tha* that is used in the future despite the strong formal and functional connection between the two; namely, *tha*+IMPF is not a direct development from *e:thela*+INF, and requires additional assumptions which are not needed in the explanation of the development of the future constructions.

## 1. HISTORY OF THE CONSTRUCTIONS

According to Browning (1983), in the later Post Classical period (ca. 300 C.E.) speakers of Greek abandoned the use of a synthetic future form (e.g. *grapso*: "I will write") began employing a periphrasis, in which the present form of the verb (*thelo*:<sup>2</sup>) in conjunction with the infinitive were used to denote the future tense:

(3)	1SG	<i>thelo:</i>	<i>graphein</i>	1PL	<i>theloume graphein</i>
		want/1SG	write/inf		
		"I will write"			

<sup>1</sup>Thus, constructions which did not play a role in the development of these two final Modern Greek forms will be mentioned, but their history will not be traced in any detail.

<sup>2</sup>As an anonymous reviewer correctly pointed out, the evidence for use of *thelo*: in this period is weak because the use of *exo*: "I have", *melo*: "I am about to", and *opheilo*: "I must" are more prevalent, and most *thelo*: examples can be interpreted as volitive (but see also Horrocks (1997:76) where *thelo*: is included in the list of future auxiliaries).

2SG	<i>theleis graphein</i>	2PL	<i>thelete graphein</i>
3SG	<i>thelei graphein</i>	3PL	<i>thelousi graphein</i>

This construction evolved further during the following centuries until reaching its present vernacular form sometime in the 19th century. The following is a schematic representation of this evolution (see Joseph (1990)):

The construction *thelo*:+INF with a regular loss of final [n] (ca. 10th c.) yielded

(4)	1SG	<i>thelo:</i>	<i>graphei</i>	1PL	<i>theloume graphei</i>
		want/1SG	write/INF		
		"I will write"			

2SG	<i>theleis graphei</i>	2PL	<i>thelete graphei</i>
3SG	<i>thelei graphei</i>	3PL	<i>thelousi graphei</i>

A reanalysis of *graphei* as a 3rd singular form of the verb and not as the infinitive (ca. 12th-14th c.) led to

(5)	1SG	<i>thelo:</i>	<i>grapho:</i>	1PL	<i>theloume graphoume</i>
		want/1SG	write/1SG		
		"I will write"			

2SG	<i>theleis grapheis</i>	2PL	<i>thelete graphete</i>
3SG	<i>thelei graphei</i>	3PL	<i>thelousi graphousi</i>

At the same time (starting around the 10th c.) due to replacement of the infinitive by finite complementation<sup>3</sup> the following construction appears:

(6)	<i>thelo:</i>	<i>hina</i>	<i>grapho:</i>
	want/1SG	that/comp	write/1SG
	"I will write"		

A further development, characterized by Joseph as "elimination of redundant person marking", took place in both of the above constructions and yielded (15th c.)

(7)	1SG	<i>thelei</i>	<i>grapho:</i>	1PL	<i>thelei graphoume</i>
		want/3SG	write/1SG		
		"I will write"			

2SG	<i>thelei grapheis</i>	2PL	<i>thelei graphete</i>
3SG	<i>thelei graphei</i>	3PL	<i>thelei graphousi</i>

which presumably coexisted with

(8) <sup>4</sup>	<i>thelei</i>	<i>na</i>	<i>grapho:</i>
	want/1SG	that/comp	write/1SG
	"I will write"		

<sup>3</sup>Although supported by Joseph (1990) and Holton (1993), the analysis whereby *thelo: hina grapho:* is a development of the *thelo*:+INF construction is not accepted by all researchers. Horrocks (1997), Jannaris (1968), and an anonymous reviewer claim that the *the+na*+INFL construction (see below) developed from the use of the subjunctive as a future, which was supplanted by the use of the *na*+indicative construction, and later 'strengthened' by the addition of *the*. Thus, the origin of *tha*+INFL future is a controversial subject. This question, however, should not affect the topic of this paper which is to compare the development of futures and counterfactuals after the 16th century, at which time, as Holton (1993:122) writes "There is no essential difference between the two future constructions *thelo*:+infinitive and *the na*+subjunctive...".

<sup>4</sup>This construction is not attested but presumed in this account as a bridge between *thelo*:+*na*+INFL and *the+na*+INFL.

(*na* developing from *hiná* by regular sound change after Ancient Greek *hína* shifted-

irregularly-to *hiná*).

A reduction of the verb *thelei* (only when used as a future marker) led to

(9a) *the grapho:*  
FUT write/1SG  
"I will write"

and (9b) *the na grapho:* (12th-14th c.)  
FUT that write/1SG  
"I will write"

This last construction through assimilation and elision also yielded

(10) *tha na grapho:* (*tha n grapho:*) (16th c.)  
*tha grapho:* (Standard Modern Greek)

On the other hand, the inflected imperfect of the same verb (*e:thelon*/1SG, or *e:thela*/1SG<sup>5</sup>) followed by the infinitival form of the verb, is heavily used in the Post Classical period as a counterfactual construction. The constructions that we are going to focus on appear in the second clause, the apodosis of the conditional. In Ancient Greek this meaning was expressed by the use of the potential indicative<sup>6</sup> of a historic (i.e. imperfect, past, or pluperfect) tense as can be seen in the following example from Plato's Apology:

(11) *ei touto eleges <sup>7</sup>he:martanes an*  
if this/ACC say/2SG, IMPF err/2SG, IMPF potential PRT  
"if you said this you would be mistaken" (but you did not say it, consequently you have not made a mistake) (Plato, Apology: 20.b).

According to Jannaris (1968), this construction was still available in the Post-classical period (300 C.E.), but thereafter it was replaced, since *e:thelon* (past of *thelo:*) replaces *an*<sup>8</sup>. Thus he contrasts the following sentences:

(12) *halkeus ei e:me:n e: tekto:n*  
blacksmith/NOM if be/1SG-IMPF or mason/NOM  
*ouk an me emime:sasthe*  
not PART I/ACC imitate/2PL-AOR

"If I were a blacksmith or a mason you would not have imitated me" (Callinicus<sup>9</sup>, Vita S. Hypatii:57, 6, ca. 450 C.E.).

vs.

(13) *ei me: iako:v prose:uksato e:thelen*  
if not Jacob/NOM pray/3SG-AOR want/3SG-IMPF

<sup>5</sup>The latter form is the result of the analogical spreading of the Aorist endings to the Imperfect.

<sup>6</sup>The "potential indicative" is an indicative followed by the particle *an*.

<sup>7</sup>The rough breathing mark will be transliterated for Ancient Greek and archaic usage only.

<sup>8</sup>As Horrocks (1997:175) notes, a bare imperfect is used in the Vita Hypatii as well, and in the later Medieval period this alternative was strengthened by the use of *na*. It is unlikely, however, that this construction played an important role in the development of *tha*+IMPF counterfactual, because both *the+na*+INFL and *tha+na*+IMPF are not attested. Thus, it is not directly relevant to our topic of investigation which is to focus on the development of *tha*+IMPF counterfactuals only.

<sup>9</sup>As I note later on, we cannot be certain that forms attested in texts of the 5th to the 10th century are truly representative of the spoken language; for the case of Callinicus, however, we do know that his education was not so great (cf. Bartelink's introduction to *Vie d' Hypatios* -1971)

*kurios anelein me*  
 lord/NOM destroy/INF I/ACC, SG

"if Jacob had not prayed the lord would have destroyed me" (Testaments of the twelve patriarchs, I:7, ca. 12th century C.E.).

What Jannaris fails to realize in this case is that not only is *an* replaced by *ethelon* but also the indicative is replaced by the infinitive.

By the 19th century the construction *tha+INFL, IMPF* of the verb is used in the apodosis of counterfactual sentences:

(14) *ean omo:s e:ton mera*  
 if but be/3SG, IMPF day/NOM  
*polla oligoi tha eglito:nan*  
 many/ADV few/NOM,PL FUT escape/3rdPL, IMPF

"But if it were day, very few (of them) would have survived" (Anthology, v.I:476)

Not much is known about the intermediate stages of the counterfactual construction but it is generally assumed (Joseph 1990) that it mirrored the development of the future. If we were to take this statement in its strongest form then we would expect the following constructions to exist for the counterfactual as well:

Counterfactuals	Futures	
<i>e:thela graphein</i>	<i>thelo: graphein</i>	[1] <sup>10</sup>
<i>e:thela grapho:</i>	<i>thelo: grapho:</i>	[2]
<i>e:thela (hi)na grapho:*</i>	<i>thelo: (hi)na grapho:*</i>	[3] (fossilized)
<i>e:thele grapho:</i>	<i>thelei grapho:</i>	[4]
<i>e:thele na grapho:*</i>	<i>thelei na grapho:*</i>	[5]

at this point however, the development in the counterfactuals becomes slightly more complicated. In order to reach the pre-Modern form, the precursor to *tha egrapha*, the complement of *e:thela* (e.g. *graphein*) has still to acquire not only the person marking (*grapho:*), but also the tense marking (*egrapha*). There are two stages where this could happen, before or after the change from *e:thela* to *e:thele*. Thus

<i>e:thela egrapha*</i> or <i>e:thele egrapha</i>	[6]
<i>e:thela (or e:thele) na egrapha*</i>	[7]

are also required stages in the grammaticalization process of the counterfactuals.

Then we should have

<i>the egrapha*</i>	<i>the grapho:*</i>	[8]
<i>the na egrapha*</i>	<i>the na grapho:</i>	[9]
<i>tha na egrapha*</i>	<i>tha na grapho:</i>	[10]

and finally the Modern Greek form

<i>tha egrapha</i>	<i>tha grapho:</i>	[11]
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The implicit assumption in this proposal is that *thelo:+INF* and *e:thela+INF* are, for all intents and purposes the same construction, and thus followed the same development. Such a position, however, seems problematic to begin with because both *e:thela egrapha* and *e:thele egrapha* cannot be the result of reanalysis as *thelo: grapho:* was, as there is no way to reanalyze the infinitive as identical with any of the forms of the imperfect tense. Moreover, the detailed analysis that was conducted in this study shows

<sup>10</sup>The numbers in bold will serve as codes for these constructions, especially in the statistical charts in section F. \* indicates forms that are not attested in the texts used in this study.

that the development of the counterfactuals was more complicated than this proposal

would have it.

## 2. THE CONNECTION BETWEEN FUTURITY AND COUNTERFACTUALITY

The expectation that the forms that denote counterfactuality would be similar to future-denoting forms is not unreasonable. As a matter of fact, the use of a construction similar to that of the future in the counterfactual is not as surprising as it may first seem. In order to understand this we must first recognize that the future can be thought of as a mood as well as a tense. The future as a mood denotes non-factivity (i.e. it makes no statement about the reality of the event described) because we cannot be certain of what will actually happen in the future. Even a statement like "as soon as I complete this sentence I will tap my foot" can have no certainty to it, even though it discusses the very near future. Lyons (1977:818) provides a good summary of the diachronic and synchronic considerations that would lead us to think of the future as a mood, and that show its connection to the counterfactuals. Among other things he states that in many languages, including English, the grammatical category of past tense is regularly used to convert a non-factive utterance into a counterfactual one. For example he says that "...the subjunctive was the mood of non-factivity..." and that

... in Latin, which, unlike [Ancient] Greek and Sanskrit, did not preserve a distinctive mood of contra-factivity and remote possibility (the optative), the past tense of the subjunctive could be employed in contrast with the present tense of the subjunctive to distinguish between non-factive and contra-factive statements.

It is crucial to note here that at the time that the forms under consideration were emerging (300 C.E.), Greek had effectively lost the category of the subjunctive which was phonetically merged with the indicative around 200 C.E. (see Jannaris 1968:§779, Horrocks 1997:75). Thus the future may have been the only clear expression of non-factivity, and that is why speakers used the format of the future-denoting constructions in conjunction with the past tense to express counterfactuality. This hypothesis seems all the more probable when we consider the following. Jannaris (1968:§553) reports that "Especially regular and common appears the use of the present for a less assertive future in the case of such verbs as included in themselves the inception of future.", *thelo*: being one of them (others are *mello*: "I am about to", *o:phelo*: "I must", *prepei*, "it is necessary", etc. Later he states that "it is obvious that the imperfect of the above verbs in G-B<sup>11</sup> perform the office of the potential indicative" (see also Horrocks 1997:76) i.e. were employed in the apodosis of counterfactuals. This demonstrates that the formal connection between *thelo*: and *e:thela* is not due to chance and that there is also some functional connection that supports it.

Finally this connection exists in other language as well. Bybee et al. (1994:233) state that in Armenian,

...the old Imperfect is used with the future prefix [k'] to form what is termed the Past Future by Fairbanks and Stevick (1958: 132). This form which is like English *would* in both form and function, is used in the apodoses of hypothetical and counterfactual conditionals and in phrases such as 'I would like...'

<sup>11</sup>G-B' stands for Graeco-Roman to Byzantine, roughly from the 2nd to the 7th c. CE.



Winford (1996) also reports that is Srannan the future marker *sa* in conjunction with the past marker *ben* can be used to denote counterfactuality among other things. Thus, the connection between the future and counterfactual is not restricted to Greek alone.

### 3. METHODOLOGY

#### 3.1. Textual evidence

The texts that were used in this research are:

*Ploutarxou Paidagogos* by Nikolaos Sofianos, a scholar and grammarian from Corfu (ca. 1544)

*Katzourmos* by Georgios Chortatzes, a "vulgar" comedy from Crete (ca. 1600)

*Ero:phile:* by Georgios Chortatzes, a drama (end of 16th, start of 17th century)

*Anthologia te:s de:motike:s pezografias* volumes 1, 2 edited by Giorgos

Valetas (an anthology of documents spanning from the 14th century, texts from 1550-1880 considered only).

A few words need to be said about textual references in this period of the development of the Greek language. Robert Browning (1983) correctly notes that textual evidence from the Medieval period (6th to 10th centuries) for Greek should not be taken at face value because of the influence that the Attic Greek purist prestigious model may have had on the authors of the texts under examination<sup>12</sup>. However, he also claims that it is safe to assume that textual evidence past the 15th century mirrors the spoken language of the time, especially the Cretan plays. In addition Valeta's introduction, in which he explains why he did not include any texts using the purist 'katharevousa' eradicated most of my apprehension. Furthermore, we should note that none of the constructions discussed here are continuations of a purist stereotype in the era that we are examining—even though they do become the accepted forms in the 'katharevousa' of the 19th century. The purist stereotype for the future in the 15th and 16th centuries is the Ancient Greek suffixal form usually distinguished by [s]-as in *luso:* from *luo:* "I untie" and the stereotype for the counterfactual was the potential indicative a construction composed of the verb (in imperfect, aorist or pluperfect tense) and the particle *an* as in *eluon an* "I would untie". For example, in J. P. Migne's *Patrologia Graeca* (v.159:1008A), a collection of ecclesiastical documents (which certainly follow the purist stereotype) we find:

(15)	<i>oudeis</i>	<i>soi</i>	<i>tanantia</i>
	noone/NOM, SG	you/DAT, SG	the opposite/ACC, PL
	<i>en toutois</i>	<i>apokrihe:setai</i>	
	in these/DAT, PL	respond/3SG, FUT	
	'nobody will oppose you in these (matters)-Josephus Methonensis Episcopus, ca. 1450.		

and for the counterfactual

<sup>12</sup>Ever since the spread of Attic Greek in the Hellenistic world by Alexander the Great, traditionalist grammarians tried hard to uphold the standard of the language as it was recorded in the writings of the 5th century BC., and which many subsequent writers attempted to emulate. This created a 'purist' prototype which became the official language of the Byzantine empire and the Orthodox church. After the end of the Turkish rule a new purist language ('katharevousa'), supported by many prominent intellectuals, became the official language of the nascent Greek state, continuing and promoting diglossia. This did not end until 1975 when, with the restoration of Democracy, the new Greek constitution made the Demotic (spoken) language as the state's official language.

(16)	<i>kai</i>	<i>ei</i>	<i>hoi</i>	<i>hemeteroi</i>	<i>ouk</i>	<i>ape:lthon</i>
	and	if	the	our	NEG	go
	<i>emeinen</i>		<i>an</i>	<i>gunnos</i>		<i>tes arhierosune:s</i>
	remain/3SG, AOR		PRT	naked/NOM, SG		the priesthood/GEN, SG
	"and if our (people) had not gone (away), he would have remained bereaved of the high-priesthood"-Josephus Methonensis Episcopus (Patrologiae Graeca:985, D)					

Thus the variation present in the Medieval texts examined here cannot be ascribed to influence by the diglossia present in Greek linguistic culture.

Another important point concerning the textual evidence is that the data from these three different sources were examined separately instead of as a whole. This was done for the following reasons. First, the Cretan plays could not be considered with the other two documents because they are poetry. Greek poetry of this era, which is best represented by the Cretan plays of the early 17th century, was written with strict metrical and rhyming requirements. Since many of the forms mentioned above differ in their number of syllables and in the position of the inflected word (which is very useful in rhyming) it was decided that the patterns of variation in poetry would be potentially influenced by the metrical and rhyming requirements and that it would be impossible to sort out this influence. The second consideration was the fact that in the anthology writers (and thus different styles and dialects) are represented by short samples, always under ten pages. If the other documents were examined together with the anthology, this would have biased the results of the statistical analysis in favor of the forms used by Sofianos and Chortatzes, whose texts are much longer.

### 3.2. Use of the Quantificational Analysis

It has always been said of historical linguistics that it is the science of making the best out of imperfect data. Often the facts that the historical linguists have at their availability are very limited in number, and one or even two forms get to play a special role in the assessment of a language's development. Fortunately the records for Greek are copious, even though the period investigated in this paper has produced the smallest amount of documents since the 5th century B.C. Thus we are given the opportunity to evaluate the data quantitatively, that is we have been able to subject it to statistical analyses (as these are used in quantitative sociolinguistics) in order to determine whether the amount of variation observed is significant. This does not mean however, that this research is constrained by the methods of Quantitative Sociolinguistics as research from present day data would be. I still acknowledge the fact that I am working with historical material and that sometimes a single instance of a form may prove to be significant for reasons other than statistical ones.

### 3.3. Accountability

A brief discussion of the principle of 'accountability' is required here, for as Winford (1990:227) states "... it is perhaps the single most important methodological maxim for studies of variability". What this principle amounts to is that the researcher must be true to his or her data set, should include all variants of a variable, and when excluding a variant must make this information explicit and give a reason for the exclusion. Thus, future researchers should be able get the same results if they follow the steps of a previous study. In the next few pages I provide an explicit account of which types of constructions were included in order to uphold the maxim of 'accountability'.

#### 3.3.1. Examples of types of constructions included in the data set:

For the future one finds:

*thelo*: (INFL)+INF [1]

- (17) *thelete evrei ek merous*  
 want/2PL, PRES find/INF from part/GEN, SG  
*mou time:*  
 me/GEN, SG honor/ACC, SG

"you will find from my part honor" (Anthology v.1:298)

*thelo*: (INFL)+INFL (AGR) [2]

- (18) *katho:stheleis mou*  
 as want/2SG, PRES me/GEN, SG  
*akouseis*  
 hear/2SG, PRES-PERFVE

"as you will hear from me" (Anthology v.1:253)

*thelei*+INFL [4]

- (19) *kataramenos thelei eisai*  
 curse/PART-PASS FUT be/2SG, PRES  
*eis te: xo:ra*

in the/ACC, SG land/ACC, SG

"cursed will you be in the land" (Anthology v.1:314)

*the+na*+INFL [9]

- (20) *kai su the na exeis*  
 and you/NOM, SG FUT PRT have/2SG, PRES  
*kinduno*  
 danger/ACC, SG

"and you will be in danger" (Anthology v.1:387)

*tha+na*+INFL [10]

- (21) *tha na kle:ronome:sou*  
 FUT PART inherit/3PL, PRES-PERFVE  
 "they will inherit" (Ero:phile: V, 626)

*tha*+INFL [11]

- (22) *to spiti tou tha kapsete*  
 the house/ACC, SG his FUT burn/2PL, PRES-PERF

"will you burn his house?" (Anthology v.1:278)

For the counterfactuals the attested constructions are:

*thelo*: (IMPF, INFL)+INF [1]

- (23) *oute kai auto to e:thelan pathenei,*  
 neither and this/ACC, SG it/ACC, SG want/3PL,IMPF suffer/INF  
*an den e:thelan kai atoi tous*

if NEG want/3PL,IMPF and the selves/NOM,PL they/GEN,PL

"and they would not even have suffered this, if they did not want it themselves" (Anthology v.1:153)

*thelo*: (IMPF, INFL)+PRES, INFL [2]

- (24) *an den me to elege,*  
 if NEG I/ACC,SG it/ACC, SG say/3SG, IMPF  
*e:thela eimai eis to skotos*  
 want/1SG, IMPF be/1SG, PRES in the darkness/ACC, SG

"if he had not told me I would be in the dark" (Anthology v.1:464)

*e:thela*+IMPF, INFL [4]

(25)	<i>an</i>	<i>den</i>	<i>eudokimouse</i>		
	if	NEG	prosper/3SG, IMPF	<i>the</i> /NOM, SG	ship/NOM, SG
	<i>e:thela</i>		<i>eipoun</i>		
	want/3SG, IMPF		say/3PL, PERF		
	"if the ship had not fared well they would have said" (Anthology v.1: 502)				

*tha*+IMPF, INFL [11]

(26)	<i>ean</i>	<i>omo:s</i>	<i>e:ton</i>	<i>mera</i>	
	if	but		be/3SG, IMPF	day/NOM
	<i>polla</i>	<i>oligoi</i>	<i>tha</i>	<i>eglito:nan</i>	
	many/ADV	few/NOM, PL	FUT	escape/3rdPL, IMPF	
	"but if it were day, very few (of them) would have survived" (Anthology v.1:476)				

### 3.3.2.1. Regarding both the *thelo*:+INF and the *e:thela*+INF constructions.

There were several types and tokens of constructions that were excluded from the data set. First of all, we must note that there is in the corpus another variant of the future construction number [5]-*thelei* + *na* plus an inflected form of the verb (cf. section 1). This variant was excluded because it only appears four times, three of which are tokens of the type "the world will come apart" *thelei na xalasei o kosmos*, which suggests that it may be simply a fossilized expression.

Another problem concerning the variants is caused by the syncretism of the infinitive with the 3rd active present singular of the verb, plus the fact that the impersonal form *thelei* (or *e:thela* in the counterfactuals) is identical with the 3rd present singular of *thelo*: (*e:thela*) (cf. the full paradigms in section 1). Thus, the 3rd person singular forms for *thelo*:+INF, *thelo*:+INFL, and *thelei*+INFL are identical. The way around this problem was to determine what the most prevalent type of construction was within a passage and then classify any 3rd person singular tokens of that type. In most passages the use of one of these types is categorical so making a decision was not very problematic. In the end, no more than 5 tokens of 3rd person singular were placed using such subjective criteria.

Finally, tokens of a particular writer, Korais, were excluded because we know that he was a language former. As a leading intellectual at the time of the Greek revolution, Korais took an active part in the language debate and had formed a model of what he felt the national language should be like, mixing Ancient Greek and Demotic Greek forms (Browning, 1983). Indeed, his language formed the basis for the development of the 'katharevousa' later on in the 19th century. Also, tokens appearing in a formal oath for induction to the 'Philike: Etaireia' ("Society of Friends", a secretive society that organized the Greek revolution of 1821) were left out, because of the obvious stylistic influence-oaths and prayers usually tend to be in more archaic language.

### 3.3.2.2. Regarding the *e:thela*+INF constructions only.

#### 3.3.2.2.1. Tokens excluded because they denote a different meaning.

Regarding the use of the *e:thela*+INF constructions, except for the problem of the syncretism between the third person and the impersonal form, there are other problematic cases as well. These problematic cases stem from the fact that this construction (along with some of its variants) is used not only to express counterfactuality, but other

meanings as well. Since the focus of the quantitative method of investigation is examining the different ways (phonetic, phonological, morphological, or syntactical) in which a unique meaning can be expressed (in this case counterfactuality), it is important to give a list of all the types of constructions which were excluded because they do not denote counterfactuality.

'Hypothetical Future'

One of these different meanings that can be expressed by the *e:thela*+INF constructions is a 'hypothetical future' a statement about the future that has even less certainty than an ordinary one. The first such case were constructions that look like the counterfactual constructions but are not true counterfactuals. For example, Sofianos frequently uses:

- (27) *e:thela*                      *eipei*  
want/1SG, IMPF              say/INF

"I would say" (Ploutarxou Paidago:gos:4)

This does not mean, of course, that he is not saying what he is saying; it is simply a device to weaken the strength of his statement, to be more polite.

A second instance of this meaning is when in a conditional whose apodosis is the *e:thela*+INF construction, the hypothesis uses the present-perfective stem of the verb instead of the imperfect tense. Thus we get the contrast between

- (28) *an*              *genei*                                      *touto* ...  
if              becomes/3SG, PRES-PERFVE              this/NOM, SG  
*e:thelan*                      *luthro:thei*  
want/3PL, IMPF              save/INF

"if this happens ... they would be saved" (Ploutarxou Paidago:gos: 2)

where this is not a true counterfactual, but simply denotes a remote possibility, with

- (29) *ean*              *e:ton*                      *dunato*  
if              be/3SG, IMPF possible/NOM, SG  
*e:thele*                      *anevei*  
want/3SG, IMPF              climb/INF

"if it were possible ... he would have climbed" (Ploutarxou Paidago:gos:9)

which is a true counterfactual.

A third instance in which the *e:thela*+INF construction can be used to express "hypothetical future" is when it occurs after the following conjunctions:

(*op*)*otan* ("when", "whenever")

- (30) *otan*              *e:thele*                      *gurisei*                      *graios*  
when              want/3rd, SG, IMPF              turn/INF                      southeastern wind/NOM, SG

"when it would turn into a southeastern wind" (Anthology v.1:129)

*na* (when an attainable wish is expressed)

- (31) *na*              *e:thela*                                      *sou*                                      *griko:*  
PART want/1st, SG, IMPF              you/GEN, SG              hear/1st, SG, PRES

"if only I could hear from you" (Anthology, v.1:255)

*me:po:s* ("in case")

- (32) *me:po:s*                      *tous*                                      *e:thele*                      *phoneusei*  
in case                      they/ACC, PL              want/3rd, SG, IMPF              kill/INF

"in case he would kill them" (Anthology v.1:135)

*opou (na)* ("that")

- (33) *opou ta*                                      *e:thelan*                                      *erme:neusei*

that they/ACC, PL want/3rd, PL, IMPF instruct/INF

"that they would instruct them" (Ploutarxou Paidago:gos:8)

*o:sa na* ("as if")

- (34) *o:sa na e:thele daneisthei*  
 as-if PART want/3SG, IMPF borrow/INF  
 "as if he had borrowed" (Anthology v.1:274)

#### 'Inferred Certainty'

Some variants of the *e:thela*+INF construction express 'inferred certainty' (from Bybee et al., 1994:44). In the Cretan plays, and in the early period in the Anthology (1600-1771), this is expressed by the construction *the+na*+INFL. This construction carries only the meaning of 'inferred certainty', and is never used to denote counterfactuality, a fact that simply has not been noticed in the literature.

- (35) *the na e:tan paignidi*  
 FUT PART be/3SG, IMPF game/NOM, SG  
 "that must have been a game" (a joke) (Anthology v.1:279)

- (36) *to mantato to priku*  
 the message/ACC, SG the bitter/ACC, SG  
*the na xei mathe:meno*  
 FUT PART have/3SG, PRES learn/PCPL  
 'he must have learned the bitter news' (Ero:phile: I, 45)

In the late period in the Anthology (1821-1880) we find that *tha*+AOR can express 'inferred certainty':

- (37) *tha ekatalavan*  
 FUT understand/3PL, AOR  
 "they must have understood" (Anthology v.1:481)

#### 'Future in the Past'

There are also constructions in which *e:thela*+INF is used to denote 'future in the past':

- (38) *thoukudide:s ... eksistorise ton polemo...*  
 Thucydides... recount/3SG, AOR the war/ACC, SG  
*kai thareuontas oti e:thele apovei*  
 and assume/PRCL that want/3SG, IMPF turn-out/INF  
 'Thucydides... recounted the war, ... and assuming that it would turn out...' (Anthology v.1:408).

There are instances where one cannot tell if the intended meaning is a counterfactual or a 'future in the past'. Ben-Mayor (1980:88) reports that speakers of Modern Greek cannot always make the distinction between the two; this confusion is limited to cases of future in the past that may be interpreted as counterfactuals, not vice versa—true cases of counterfactuals are not mistaken for future in the past constructions. This is not surprising as we have seen that a future in itself cannot declare anything with certainty (cf. section 2). Perhaps the formal connection between the future in the past and the counterfactual construction adds to the hearer's confusion—especially when in the present he or she knows that the prediction made in the past was not borne out. Even though these tokens were not numerous, I decided not to take them into account, neither as counterfactuals nor as futures, in order not to bias the statistical analysis.

#### 3.3.2.2.2. Tokens excluded because they belong to a different set of variants.

Finally one more set of the *e:thela*+INF construction occurrences was excluded from the data set. This set is composed of instances of the construction which appear in the hypothesis of a conditional clause which expresses counterfactuality, e.g.

- (39) *an den e:thele prophthasei* ...  
 if NEG want/3SG, IMPF arrive-on-time/INF  
*e:thele thanato:sei*  
 want/3SG, IMPF kill/INF  
 "if (the message) had not arrived on time he would have killed" (Anthology v.2:82).

Even though these instances of *e:thela*+INF do denote counterfactuality-the meaning in the sentence above is that the message did arrive on time-they were not included in the data set, because this environment involves different variants. Instead of finding *e:thele*+INFL or *tha*+IMPF, in the hypothesis of these conditionals we find the IMPF as can be seen from the following example

- (40) *an den eudokimouse t o karavi*  
 if NEG prosper/3SG, IMPF the/NOM, SG ship/NOM, SG  
*e:thele eipoun*  
 want/3SG, IMPF say/3PL, PERF  
 "if the ship had not fared well they would have said" (Anthology v.1:502)

The instances of *e:thela*+INF or any of its variants in the apodosis were, of course included in the data set.

#### 4. ANALYSIS OF THE DATA

##### 4.1. Ploutarxou Paidago:gos (ca. 1544)

In Ploutarxou Paidago:gos we find that the construction for the future is exclusively of type[1]: Inflected form of *thelo:* followed by the infinitival form of the verb (*thelo*:+INF)

- (41) *kai me perissotere: epimeleian*  
 and with much/ACC, SG care/ACC, SG  
*thelousi ta anathrepsei*  
 want/3PL, PRES they/ACC, PL raise/INF  
 "and they will raise them with more care" (Ploutarxou Paidago:gos:6)

and only one token of the inverse order, i.e. INF+*thelo:*

- (42) *parado:sei thelo: ton logon ...*  
 deliver/INF want/2SG, IMPF the reason/ACC, SG  
 "I will deliver the reason ..." (Ploutarxou Paidago:gos:18)

The same two patterns exist for the counterfactuals as well:

a) inflected form of the imperfect of *thelo:* plus the infinitive of the verb (*e:thela*+INF)

- (43) *ean e:ton dunato*  
 if be/3SG, IMPF possible/NOM, SG  
*e:thele anevei*  
 want/3PL, IMPF climb/INF  
 "if it were possible ... he would have climbed" (Ploutarxou Paidago:gos:9)

b) infinitive of the verb followed by the imperfect of *thelo:* (INF+*e:thela*)

- (44) *klausein e:theles ean o:rgizomoun*  
 cry/INF want/2SG, IMPF if become angry/1SG, IMPF

"You would have cried ... if I had become angry" (Ploutarxou

Paidago:gos:25)

However, the inverse order is more numerous in the counterfactual construction where the infinitive precedes the *thelo*: form in 7 out of 23 tokens instead of 1 out of 24 as we observe in the future constructions. The fact that the order in the counterfactual is freer than the order in the future indicates that the counterfactual construction has a greater degree of independence and that *e:thela* has not advanced along the grammaticalization cline as much as *thelo*: has.

#### 4.2. The Cretan Plays (Katzourmpos and Ero:phile:, ca. 1600)

In the Cretan plays of the early 17th century the future constructions are

*thelo*:+INF (66 tokens)

(45) *o:s thelte dei*  
as want/2PL, PRES see/INF  
"as you will see" (Ero:phile: I, 101)

*thelo*: (INFL)+INFL (AGR) (7 tokens)

(46) *kai thes ts eipeis*  
and want/2SG, PRES she/ACC, SG say/2SG, PRES-PRFVE  
"and you will tell her" (Ero:phile: IV, 95)

*the+na*+INFL (43 tokens)

(47) *the na malo:so:*  
FUT PART fight/1SG, PRES-PERFVE  
"I will fight" (Katzourmpos:II, 5)

*tha+na*+INFL (1 token)

(48) *tha na kle:ronome:sou*  
FUT PART inherit/3PL, PRES-PERFVE  
"they will inherit" (Ero:phile: V, 626)

*tha*+INFL (67 tokens)

(49) *tha piaso:*  
FUT grab/1SG, PRES-PERFVE  
"I will grab" (Katzourmpos:II, 121)

However, in the counterfactual construction only the following variants are available:

*thelo*: (IMPF, INFL)+INF (10 tokens)

(50) *po:s e:thelame paxunei*  
how want/1SG, PL fatten/INF  
"how we would get fat" (after contemplating the possibility of eating  
1,000 lambs-Katzourmpos:III, 135)

*thelo*: (IMPF, INFL)+INFL (3 tokens)

(51) *aniso:ski ekoudounize ...*  
if and ring/3SG, IMPF  
*deis e:theles*  
see/2SG, PRES-PERF want/2SG, IMPF  
"if he were to ring ... you would see" (Katzourmpos:I, 197)

The absence of *tha*+IMPF for this construction should be especially noted here, since it clearly shows that *thelo*: has moved to *tha* in the future construction before it has moved to *tha* in the counterfactual construction.



The data from Ploutarxou Paidago:gos and the Cretan Plays already show that the development of the future and counterfactual construction are not mirror images of each other. In the next section a quantificational analysis of the much larger corpus from the Anthology of Demotic Greek provides us with a clear picture of the ways in which these constructions differ.

#### 4.3. Anthology of Demotic Greek (1550-1880)

In the anthology of Demotic Greek we find the following constructions for the future:

*thelo*: (INFL)+INF

- (52) *thelete eurai ek merous*  
 want/2PL, PRES find/INF from part/GEN, SG  
*mou time:*  
 me/GEN, SG honor/ACC, SG  
 "you will find from my part honor" (Anthology v.1:298)

*thelo*: (INFL)+INFL (AGR)

- (53) *katho:s theleis mou*  
 as want/2SG, PRES me/GEN, SG  
*akouseis*  
 hear/2SG, PRES-PERFVE  
 "as you will hear from me" (Anthology v.1:253)

*thelei*+INFL

- (54) *kataramenos thelei eisai*  
 curse/PART-PASS FUT be/2SG, PRES  
*eis te: xo:ra*  
 in the/ACC, SG land/ACC, SG  
 "cursed will you be in the land" (Anthology v.1:314)

*the+na*+INFL

- (55) *kai su the na exeis*  
 and you/NOM, SG FUT PRT have/2SG, PRES  
*kinduno*  
 danger/ACC, SG  
 "and you will be in danger" (Anthology v.1:387)

*tha*+INFL

- (56) *to spiti tou tha kapsete*  
 the house/ACC, SG his FUT burn/2PL, PRES-PERF  
 "will you burn his house?" (Anthology v.1:278)

For the counterfactuals the following constructions are witnessed:

*thelo*: (IMPF, INFL)+INF

- (57) *oute kai auto to e:thelan pathenei,*  
 neither and this/ACC, SG it/ACC, SG want/3PL,IMPF suffer/INF  
*an den e:thelan kai atoi tous*  
 if NEG want/3PL,IMPF and the selves/NOM,PL they/GEN,PL  
 "and they would not even have suffered this, if they did not want it themselves" (Anthology v.1:153)

*thelo*: (IMPF, INFL)+PRES, INFL [2]

- (58) *an den me to elege,*  
 if NEG I/ACC,SG it/ACC, SG say/3SG, IMPF

- e:thela*                      *eimai*                      *eis*                      *to*                      *skotos*  
 want/1SG, IMPF              be/1SG, PRES in              the                      darkness ACC, SG  
 "if he had not told me I would be in the dark" (Anthology v.1:464)
- e:thele*+IMPF, INFL [4]  
 (59) *an den eudokimouse to karavi*  
 if NEG prosper/3SG, IMPF the/NOM, SG ship/NOM, SG  
*e:thele eipoun*  
 want/3SG, IMPF say/3PL, PERF  
 "if the ship had not fared well they would have said" (Anthology v.1: 502)
- tha*+IMPF, INFL [11]  
 (60) *ean omo:s e:ton mera*  
 if but be/3SG, IMPF day/NOM  
*polla oligoi tha eglito:nan*  
 many/ADV few/NOM,PL FUT escape/3rdPL, IMPF  
 "but if it were day, very few (of them) would have survived" (Anthology v.1:476)

#### 4.3.1. The quantitative analysis

The comparison between the grammaticalization of *thelo*: in the constructions denoting futurity and the grammaticalization of *e:thela* in the constructions denoting counterfactuality is based on a preliminary study of the variation in the constructions denoting futurity in early Modern Greek. In this paper I simply present the factors that proved to be significant in the variation of the future and then test the variation in the *e:thela*+INF constructions and compare them with the results for the future.

The preliminary study showed that in the variation for the future two factor groups were significant. The time period in which the constructions are used, and whether the sentence was affirmative or negative, i.e. the 'sentence polarity'. The following pages include a number of tables and charts, obtained by using the variable rule application GoldVarb, version 2.0 (Rand and Sankoff 1990), which confirm this statement. I will take a moment here to discuss how the tables can be read. There are two kinds of tables. Some simply present the raw number of occurrences of a construction type and the percentage of distribution that these occurrences amount to. Most of the charts, however, present the results of a variable rule analysis of the data, which enables us to discern whether a set of parameters (factor group) effects the distribution of the construction types. This is a list of what the column titles refer to.

Under 'Group' the factor groups are listed, and within these the individual factors that we believe influence the distribution of the data (listed under 'Factor').

'Input probability' is the probability of a particular construction occurring even if the specified factors are not present.

'Weight' is the probability of the construction occurring due to a specific factor.

'Input and Weight' is the combined effect of the last two on the probability that the construction will occur.

'Applications' is the number of occurrences of the construction under analysis.

'Expected' is the predicted number of occurrences of the construction under analysis.

'Error' is the value that indicates the discrepancy between the predictions of the model and the actual occurrences.

'Chi-square per cell' is a measure of the independence of the factors and should be less than 1.5 for the model to have a good fit.

'Log likeli-hood' is a number that gives us a way of comparing models; the greater the number (i.e. the smaller the negative number) the better the model.

'Stepwise Regression Analysis' refers to a series of tests run by the program in order to determine which factor groups effect the variation significantly.

#### 4.3.1.1. The Futures

Only one application value at a time can be tested because the Varbrule program cannot execute multinomial analyses (analyses of more than two application values at a time). Thus, in order to give an accurate depiction of how the variation of the future construction is affected five separate runs (one for each attested construction type) would be needed. However, as can be seen in the following table (1) certain constructions are not as prominent as others.

Table (1). Distribution of constructions denoting Future according to time period

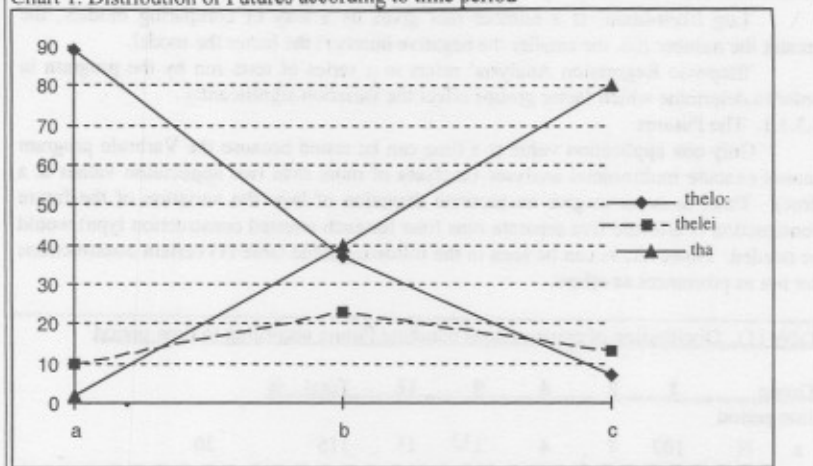
Group		1	2	4	9	11	Total	%
time period								
a	N	102	7	4	1 <sup>13</sup>	1 <sup>8</sup>	115	36
	%	89	6	3	1	1		
b	N	43	8	19	10	37	117	37
	%	37	7	16	9	32		
c	N	6	2	9	3	66	86	27
	%	7	2	10	3	77		
Total	N	151	17	32	14	104	318	
	%	47	5	10	4	33		

In order to reduce the number of knock out cells (as well as make the presentation less cumbersome), type *thelo*:+INFL was grouped with type *thelei*+INFL and type *the+na*+INFL with type *tha*+INFL. Even though this grouping was done in order to facilitate the statistical analysis it is also validated by the character of the constructions grouped. Both *thelo*:+INFL and *thelei*+INFL have a full form of *thelo*: and an inflected form of the verb, while *the+na*+INFL and *tha*+INFL both have a reduced form of *thelo*:

The following chart (chart 1) shows the comparative distribution of these constructions according to time period.

<sup>13</sup>This token is not actually witnessed, but was inserted in the data set as "ghost tokens", in order for the Varbrul program to operate. Instances where "ghost tokens" are inserted will be marked ▲.

Chart 1. Distribution of Futures according to time period



The general picture presented here is that in the first period type *thelo*:+INF is the dominant construction. In the second period two constructions *thelo*:+INF and *tha*+INFL are in strong competition, and type *thelei*+INFL is also involved in the competition but not as heavily. In the third period the competition is resolved in favor of type *tha*+INFL, the decline of type *thelo*:+INF use is quite radical, while for *thelei*+INFL it is more gradual.

Why should these particular subdivisions of the time period apply? Browning (1983) identifies 1821 as the starting point of the Modern period, so this would be a valid division. But he does not offer any break up of the period 1550-1821, and the period 1771-1821 seems to be a strange cut-off point mainly because it is so short. Valetas in his introduction of the anthology suggests that the period around the French Revolution (1789) brought turmoil to the Greek language, mainly by upsetting the status quo of the noble patrons of education. Clogg (1986) makes explicit mention that from the middle of the 1700s there is a boost in the establishment of Greek schools in the Ottoman empire, coupled by an extreme increase of the publication of secular books. These factors may have colluded to bring about the change we observed above at the end of the 18th century. This correspondence may be significant and should be further pursued. The fact is that this division of the time period is necessitated by the data. In the preliminary study different divisions of the time span 1550-1863 were tested but they all proved to be insignificant.

On the other hand, my choice of the factor group "sentence polarity" is based on the observation that in some texts *tha*+INFL construction is used only in affirmative sentences. For example, in the writings of Kalaras (Anth., v.1: 448-451, ca. 1815) we find *tha* constructions only in affirmative sentences:

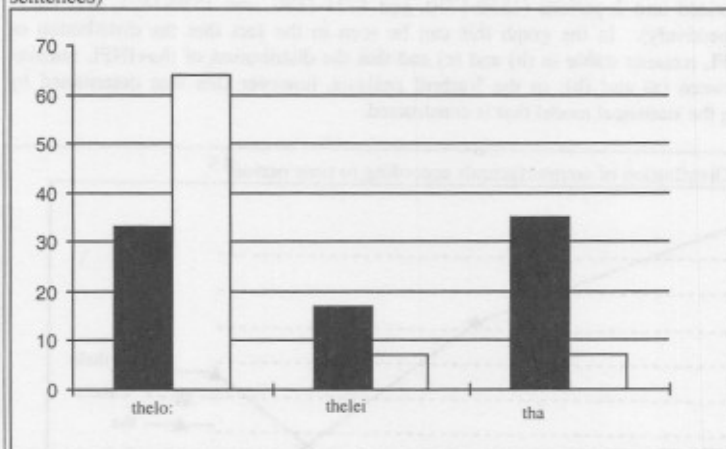
- (61) *tha phtuso:*  
FUT spit/1SG, PRES-PERFVE  
"I will spit"
- (62) *tha planometha*  
FUT wander/1PL, PRES  
"We will wander"

but *thelo*+INF constructions in both affirmative and negative sentences

- (63) *de thelete anapneusei*  
NEG want/2PL, PRES breathe/INF  
"You will not breathe"
- (64) *thelousi ekchuthei*  
want/2PL, PRES overflow/INF  
"They will overflow"

As I have stated earlier, the importance of 'sentence polarity' as a factor group is restricted to the second period. Since negative future clauses disfavor the use of *tha*+INFL future, as can be seen in chart (2), we can speculate that 'sentence polarity' is the factor that brings about the competition between the three types of constructions in the second period.

Chart 2. Distribution of futures according to 'sentence polarity' (black: affirmative sentences)



#### 4.3.1.2. The Counterfactuals

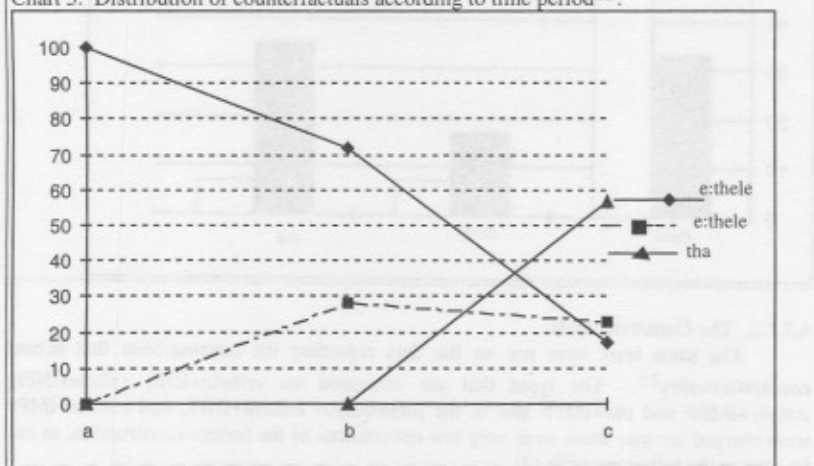
The same tests were run on the data regarding the constructions that denote counterfactuality<sup>14</sup>. The types that are witnessed are *e:thela*+INF, *e:thela*+INFL, *e:thele*+IMPF and *tha*+IMPF and in the presentation *e:thela*+INFL and *e:thele*+IMPF were merged because there were very few occurrences of the former construction, as can be seen in the following table (2).

<sup>14</sup>The time span researched in this case was expanded to 1880 in order to determine at what point the dominance *tha*+IMPF was established.

Table (2). Distribution of constructions denoting counterfactuality by time period

Group		1	2	4	11	Total	%
time period							
a	N	45	1	1 <sup>s</sup>	1 <sup>s</sup>	48	42
	%	92	2	2	2		
b	N	22	1	7	1	31	27
	%	69	3	22	3		
c	N	6	1 <sup>s</sup>	9	19	35	31
	%	17	3	25	53		
Total	N	73	3	17	21	114	
	%	62	3	15	18		

In the constructions that denote counterfactuality we see (cf. Chart 3) that while the use of type *thelo*:+INF constructions can be divided into 3 periods (1550-1770, 1771-1821, and 1821-1880), the use of the type *thelei*+INFL and type *tha*+INFL constructions can be divided into 2 periods (1550-1770, and 1771-1880, and 1550-1821, and 1821-1880, respectively). In the graph this can be seen in the fact that the distribution of *thelei*+INFL remains stable in (b) and (c) and that the distribution of *tha*+INFL remains stable between (a) and (b); in the Varbrul analysis, however this was determined by examining the statistical model that is constructed.

Chart 3. Distribution of counterfactuals according to time period<sup>15</sup>.

We notice in the model (cf. Table 3), that the probabilities between the second and third period are very similar (0.696 and 0.666). This gives us reason to think that the two periods should not be separated from each other. When factors (b) and (c) are collapsed,

<sup>15</sup>In the chart the actual percentage of *e:thele*+IMPF and *tha*+IMPF are depicted; the values in Table (2) are different because of the inserted tokens.

the probabilities of the two factors are significantly different and the log-likelihood of this new model is only slightly worse than the previous one (-44.944 vs. -44.964, cf. Table 4), a result that validates collapsing factor (b) and (c).

Table (3). Model of *e:thele*+IMPF used as Counterfactual by time period  
Input 0.129

Group time period	Factor	Weight	App/Total	Input&Weight
	a	0.243	0.05	0.05
	b	0.692	0.25	0.25
	c	0.666	0.23	0.23
Cell	Total	App'ns	Expected	Error
c	35	8	7.999	0.000
b	32	8	7.999	0.000
a	44	2	2.003	0.000

Total Chi-square = 0.0000  
Chi-square/cell = 0.0000  
Log likelihood = -44.944

Table (4). Model of *e:thele*+IMPF used as Counterfactual when periods (b) and (c) are collapsed into (b')

Group time period	Factor	Weight	App/Total	Input&Weight
	a	0.243	0.05	0.05
	b'	0.678	0.24	0.24
Cell	Total	App'ns	Expected	Error
b'	67	16	15.997	0.000
a	44	2	2.003	0.000

Total Chi-square = 0.0000  
Chi-square/cell = 0.0000  
Log likelihood = -44.964

For the construction *tha*+IMPF we notice that the probabilities of periods (a) and (b) are also very similar (0.241 and 0.308 in Table (5)).

Table (5). Model of *tha*+IMPF used as Counterfactual by time period  
Input 0.131

Group	Factor	Weight	App/Total	Input&Weight
time period	a	0.241	0.05	0.05
	b	0.308	0.06	0.06
	c	0.899	0.57	0.57
<u>Cell</u>	<u>Total</u>	<u>App'ns</u>	<u>Expected</u>	<u>Error</u>
c	35	20	19.994	0.000
b	32	2	2.003	0.000
a	44	2	2.006	0.000

Total Chi-square = 0.0000  
Chi-square/cell = 0.0000  
Log likelihood = -39.518

Once these two periods were collapsed and a new model was constructed, its log-likelihood was very slightly worse than in the previous one (-39.518 vs. -39.569 in Table 6) and the two factors (the period from 1550-1821 and the period from 1821-1880) have very different probability values. Again this is the preferred model.

Table (6). Model of *tha*+IMPF used as Counterfactual by time period when periods (a) and (b) are collapsed into one (a')

Input 0.132

Group	Factor	Weight	App/Total	Input&Weight
time period	a'	0.269	0.05	0.05
	c	0.898	0.57	0.57
<u>Cell</u>	<u>Total</u>	<u>App'ns</u>	<u>Expected</u>	<u>Error</u>
c	35	20	19.994	0.000
a'	76	4	4.009	0.000

Total Chi-square = 0.0000  
Chi-square/cell = 0.0000  
Log likelihood = -39.569

If we compare the results of the variation analysis for the constructions that denote future with the results of the analysis for the counterfactuality denoting constructions we note the following differences:

1. The use of all future denoting constructions can be divided into three periods (1550-1770, 1771-1821, and 1821-1863) while in the constructions that denote counterfactuality only the use of type *e:thela*+INF constructions fits this division; the use of type *e:thela*+IMPF constructions is divided into two periods 1550-1770, and 1771-1880, and so is the use of type *tha*+IMPF constructions, although this is a different division: 1550-1821, and 1821-1880.



2. In the second period for the futures, the main competitor of type *thelo*:+INF constructions is type *tha*+INFL, while for the counterfactuals the main competitor of *e:thela*+INF is the type *e:thela*+IMPF.
3. In the second period, "sentence polarity" determines the use of constructions denoting futurity by favoring the use of type *thelo*:+INF against the use of type *tha*+INFL, while it is not a factor in the variation of the counterfactuals in any period.

## 5. DISCUSSION

The evidence presented in the two previous sections establishes that, contrary to the prevalent assumption, *tha*+IMPF for the counterfactual developed differently than *tha*+INFL in the future. The differences we have observed can be summarized as follows.

1. The *e:thela*+INF constructions lag behind the *thelo*:+INF in their development. In the Cretan plays this was evident in the absence of *tha*+IMPF constructions for the counterfactuals. In the anthology this is seen in the late emergence of *tha*+IMPF in the third period, whereas *tha*+INFL emerges in the second period, which gives us a time difference of about two generations.

2. The distribution of *thelo*:+INF in the second period in the anthology is constrained by 'sentence polarity', while the distribution of *e:thela*+INF is not.

Considering these differences we can say that with respect to these constructions, *thelo*: and *e:thela* cannot be considered as two different forms of the same verb anymore, they are two separate lexical items undergoing their own developments. Even though it is difficult to specify how much time must elapse between two changes in order for them to be considered separate, I assume that once they transcend a generation the changes cannot be connected in a direct fashion since the second change is implemented by an entirely new set of speakers. As Janda (1996) states:

...later speakers never have access to the grammars of preceding generations, and so they cannot know-either consciously or unconsciously- if the status that earlier speaker assigned to a particular linguistic element was lexical or grammatical, much less the precise extent to which it was either of these.

The changes however, may be connected in an indirect fashion; thus, once a change has congealed in the grammar of speakers it may influence the development of other changes. The abrupt emergence of the *e:thela*+IMPF in the second period of the anthology and of *tha*+IMPF in the third period are probably due to such an indirect change. They can be seen as forms that became variants for the counterfactual *e:thela*+INF once their future counterparts were established as viable variants for *thelo*+INF. The development of *e:thela*+IMPF and of *tha*+IMPF are probably due to some sort of analogical influence from the constructions available for the future.

When use of the *thelo*:+INF constructions in the future started to decrease in the second period, we may hypothesize that speakers felt the pressure to change the construction they used in the counterfactuals, especially as this change interacted with their use of new types for Future in the Past, and, perhaps more importantly, signaled a step away from the use of infinitival forms. The two most prominent alternatives would have been constructions of type *e:thela*+IMPF or type *tha*+IMPF. The strength of the type *thelo*:+INF constructions in the counterfactuals, however, would have equally prohibited speakers from moving too far from it, i.e. to type *tha*+IMPF. Type

*e:thela*+IMPF would serve as an excellent compromise between these two pressure

points; it eliminates the use of the infinitive but, at the same time, retains *e:thela* which seems to have taken over the denotation of counterfactuality. In addition it avoids the problem presented by the fact that negative clauses in the future do not select *tha*+INFL. In the third period when the use of type *tha*+INFL constructions became categorical for the future tense, speakers would eventually use this construction in the counterfactuals as well.

What is extraordinary about this development is that despite the obvious connection between *thelo*: and *e:thela* both formally (as present and past tenses of the verb 'to want') and functionally (through the semantic association between futures and counterfactuals), this connection did not prove significant enough to keep the two constructions on a parallel track of development. In the constructions under investigation speakers treat the two forms as separate lexical items. Nevertheless, we see that later on in their development the forms denoting counterfactuality (*e:thela*+IMPF and *tha*+IMPF) are constructed on the basis of the forms that denote futurity, even though by this time the formal connection is much more obscure. In this respect the pattern that we have observed is paradoxical. Why should the development of these forms diverge when they are as close *e:thela*+INF and *thelo*:+INF are, only to be merged again at a later stage? Though this may be an intriguing question it should not be the focus of our attention. Instead we should recognize that the paradox itself is very revealing, as it is a clear indication that speakers do not make grammatical associations along the same lines that linguists or grammarians do, and that the linguistic behavior of speakers, however erratic, is what ultimately shapes language change.

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## APPENDIX

Table 1: abbreviations

1, 2, 3 etc.	Person Markings
ACC	Accusative
AGR	Agreement Between Forms
FUT	Future Marker
GEN	Genitive
INF	Infinitive
INFL	Inflected
NOM	Nominative
PRCL	Participle
PASS	Passive
PERFVE	Perfective Aspect
PL	Plural
PRES	Present Tense
PRT	Subordinating Particle
SG	Singular
IMPF	Imperfect

The Present and Past tense in Modern Greek can have two aspects: Perfective and Imperfective. The Perfective Past is here labeled as Aorist (AOR); the Imperfective Past as Imperfect (IMPF). The Imperfective Present is labeled Present and the Perfective Present simply Perfective Present (PRES-PERF).

Proto-Buli-Kɔnni	Buli	Kɔnni	Other Gur	Other Gur	Other Gur	Other Gur
*p	h	y	w			
*t	t	t				
*k	k	k				
*g	g	g				
*d	d	d				
*n	n	n				
*m	m	m				

### Triple Splits from Proto-Buli-Kɔnni

Proto-Buli-Kɔnni	Buli	Kɔnni	Other Gur	Other Gur	Other Gur	Other Gur
*p	h	y	w			
*t	t	t				
*k	k	k				
*g	g	g				
*d	d	d				
*n	n	n				
*m	m	m				

Michael Cahill

### 1. Introduction

Buli and Kɔnni are Gur languages spoken in northern Ghana, in the Oti-Volta subgroup of Gur (Naden 1988, 1989), and comprise a subgroup of their own (Naden 1988:18). Kɔnni has approximately 2500 speakers, Buli about 80,000. Surrounding languages (Sisaala, Kasem, Mampruli) are all larger ones, each having over 100,000 speakers. Buli has a relatively long history of linguistic study (e.g. Manessy 1975, Prost 1974) while Kɔnni is comparatively unknown. Naden wrote a summary of a preliminary linguistic survey (Naden 1986). The only other published language data on Kɔnni are by Cahill (1991, 1992, 1994, 1996, to appear a, b, etc.).

Among the interesting phenomena in the reconstruction of the consonants of Proto-Buli-Kɔnni (hereafter PBK) is a pair of triple splits. In the first split, \*p has reflexes of h, y, w in modern Kɔnni. In the second split, \*t has reflexes of t, ɟ, h in modern Kɔnni. Interestingly, both of these sound changes are conditioned by the particular vowel following the consonant in question. The purpose of this paper is to document and examine these changes. Naden (1986) noted in passing that in words in which other Gur languages contained p, Kɔnni had corresponding y, w. These and the additional reflex of h from \*p were first fully reported and analyzed in Cahill (1991). Both triple splits examined here are also discussed in the larger context of PBK consonants in Cahill (1995).

The Kɔnni data in this paper is from my own field notes and recordings, mostly from the village of Yikpabongo. The Buli data is from Kröger's 1992 Buli-English Dictionary, supplemented by an unpublished phonology by Todd Poulter and a tape recording of the Swadesh 100 list.

Below are given the phonemic inventories of modern Kɔnni and Buli.

(1) Kɔnni Phonemic Consonants

	Labial	Alveolar	Palatal	Velar	Labial-velar	Glottal
stops	p, b	t, d		k, g	kp, gb	
affricates			tʃ, dʒ			
fricatives	f, v	s, z				h
liquids		l, r				
nasals	m	n	ɲ	ŋ	ŋm	
glides			y	w		

(2) Buli Phonemic Consonants

	Labial	Alveolar	Palatal	Velar	Labial-velar
stops	p, b	t, d		k, g	kp, gb
affricates			tʃ, dʒ		
fricatives	f, v	s, z			
liquids		l, r			
nasals	m	n	ɲ	ŋ	ŋm
glides			y	w	

As can be readily seen, the Buli and Kɔnni phonemic inventories are almost identical, the only difference being that Kɔnni has /h/ while Buli does not. This bare fact is somewhat misleading; in previous research, I document that Kɔnni has undergone approximately fifteen consonantal changes from PBK, while Buli has undergone only five (Cahill 1995).<sup>1</sup> That Kɔnni is the more innovative language will be seen in this paper as well.

Note that [r] is actually an allophone of /d/ in both Kɔnni and in Buli, but is orthographic in both, and will be transcribed as such in the data below.

## 2. The proto-stop triple splits

### 2.1 Triple Split from \*p

This section is a refinement of the analysis in Cahill (1991), including here more accurate and relevant vowel data and reconstructions. In that work, data from fourteen other Gur languages was examined to establish the existence of \*p in the relevant forms

<sup>1</sup> This case neatly reminds us that simply comparing phonemic inventories of related languages can be quite misleading in working out language relationships. Correspondences among segments in specific lexical items and other morphemes are what is crucial when comparing languages.

in proto-Gur. Here I assume the existence of \*p in the inventory of PBK, as represented by the relevant Buli forms below.

Tone is contrastive in both languages, but is omitted here as irrelevant to the matter under discussion. Kɔnni forms listed are phonemic. Buli forms are as reported in Kröger's dictionary, with the addition of a phonetic vowel notation to the right where different than his dictionary entry. The phonemic status of some Buli vowels continues to be a point of uncertainty, with Kröger assuming a 5-vowel inventory, and others including at least /ɔ, ɛ/ in the set of vowels.

Words illustrating three Kɔnni correspondents of Buli /p/ are given below. Singulars and plurals are separated by single slashes.

(3)	gloss	Kɔnni	Buli
<b>h~p</b>	bark	-haŋ / -hagɾɪ	pauk / pakta
	new	-haalɪŋ	paalik
	debt	hamɪŋ	pami/pama
	strength	hagɪɾɪŋ	pagra
	woman	hɔɡu / hɔɔba	ni-pok / ni-pooba [ɔ]
<b>y~p</b>	white	-yeɛɪŋ / -yeɛla	pieluk / pielita
	bright, to be	yɛntɪ	piɛnti
	sheep (pl)	yise	piisa
	ash	tanyeeliŋ	tampelem
	arrow, nail	yɪɪŋ/yɪɪma	pēi/piema
<b>w~p</b>	Frafra potato	yɛsɪŋ	piesiri – piesi/piesa
	flowers	wute	puuta
	rotten	-wuusɪŋ / -wuusa	poosidi / poosa
	shave head, to	wuŋ	poni
	shell, to	wurɪ	poti
	stir porridge, to	wɔɔɾɪ	porɪŋi
	rubbish heap	taggwɔŋ	tampoi

The single Buli stop /p/ corresponds to Kɔnni /h, y, w/. If we consider these three correspondences, it is clear that the vowel following \*p is the factor which conditions the particular reflex which occurs in Kɔnni. If the \*p occurs before /a/ or /ɔ/, the Kɔnni reflex is /h/, if before /i, ɪ, e, ɛ/, the Kɔnni reflex is /y/, and before /u, ɔ, o, ɔ/, the Kɔnni reflex is /w/.

The same change apparently took place word-medially as word-initially (see *tanyeeliŋ-tampelem* 'ash' and *taggwɔŋ-tampoi* 'rubbish heap'). In 'rubbish heap,' there is the further change that [w] has become [gw]. The [g] is inserted as a strengthening, or possibly an emergent stop, to use Ohala's terminology. There is no other [ŋw] sequence between vowels in my data.

The exact formulation of the sound change is dependent on the reconstruction of the proto-vowels, since there is an overlap of the conditioning vowels to consider. When

the vowel following the \*p is [ɔ], we have in some cases 'woman' and in other cases \*p > w, as in wɔɔri/poriji 'to stir.' The conundrum is solved when we examine the vowels of PBK more closely. In Cahill (1991), the cognate forms for 'woman' in various languages are quite mixed in whether they have a, ɔ, or o. Due to governmental policies in the past, the orthographic [o] in some of these languages may actually represent phonemic /ɔ/.<sup>2</sup>

(4) 'woman' in various Gur languages

Kɔnni	hɔɔ	Birifor	pɔɔ
Buli	ni-pɔk	Yom	pɔga
Mampruli	poga	Bimoba	poo
Dagbani	paga	Vagla	haaŋ
Kusaal	poʔa		

I contend that 'woman' in proto-Gur had \*a rather than a rounded vowel. The fact that so many Gur languages in different branches of Gur have either ɔ or o is explained by the idea of the central vowel a becoming backed before the back velar consonant, a very natural and common sound change. (It is not irrelevant to note that many of these languages are in rather close contact, and the influence of neighboring languages cannot be lightly dismissed.) So in many languages, including both Buli and Kɔnni, the sound change \*a > ɔ before velars occurred. With this background in place, we see that the sound change of \*p > h occurred only before \*a. We are left with the following sound changes.

(5) Sound changes from PBK \*p to Kɔnni:

- a. \*p > h before a
- b. \*p > y before i, i, e, e
- c. \*p > w before u, u, o, ɔ

Given that \*p weakened severely into a glide, the nature of that glide is quite naturally taken from the adjacent vowel. A front vowel gives rise to a front glide y, a round vowel gives rise to a rounded glide w, and the pharyngeal vowel a gives rise to the glottal h. Conceptually, \*p weakened to the point of deletion, but native Kɔnni contentive words are required to be consonant-initial, so the weakest consonant - a glide nearest in value to the following vowel - is the result.

One other complication must be considered before leaving the subject of \*p. There are a number of cognates in Buli and Kɔnni which *both* have /p/, as below.

<sup>2</sup> In past years (though not so much at the present time), governmental policy was to avoid non-English-appearing orthographies. For vowels, the English {a,e,i,o,u} were the only ones used in several languages which demonstrably had the vowels /i, u, e, ɔ/. For /ɔ/, orthographies generally wrote the grapheme {o}.



(6)	English	Kɔnni	Buli
<b>p~p</b>	peel, to	paɪ	piesi
	roof, to	pili	pili
	log	dampali / dampala	dampali / dampala

There are a number of possible explanations for this correspondence. If we do not hold to a strong version of the regularity of sound change, we may merely consider these words as some which did not undergo the sound changes noted above. However, other more attractive explanations are at hand.

Three alternate hypotheses can be considered. The first is that there were actually *two* sources of present Gur /p/, possibly a *fortis* and *lenis* \*p or an ejective vs. non-ejective \*p. However, there is no evidence for this across other Gur languages. A second hypothesis is that there is some conditioning factor not considered above which prevented some words from undergoing the above sound changes. I see no evidence of any such conditioning factor. Neither of these hypotheses has any evidence in its favor; both are discussed more fully in Cahill (1991).

What remains is that the **p~p** correspondence can be accounted for by borrowings. In this scenario, these words were borrowed into both languages after they were differentiated and the "massive P shift" took place in Kɔnni. If they had been present in the proto-language before the split of \*p, these would also have undergone the split. It is quite possible, given the sociological setting, that these were first borrowed into Buli and then transmitted to Kɔnni, but this remains speculative. More investigation is required to specifically find the source of these borrowings, but this remains the most plausible source of the **p~p** correspondence.

## 2.2 Triple Split from \*ŋ

Parallel to the triple split that \*p has undergone, PBK \*ŋ also has undergone a split into three reflexes. As before, it is the Kɔnni which has undergone the changes, while Buli has retained the proto-form of the consonant. Data is much more limited for this set of correspondences, as seen below.

(7)	English	Kɔnni	Buli
<b>h~ŋ</b>	boat	haartŋ	ŋaarŋ
	'black-berry' tree	haartŋ	ŋaarŋ
	the (definite suffix)	-ha	-ŋa
	them	ha	ŋa
<b>ŋ~ŋ</b>	neck	- / ŋie	ŋiri/ŋie
	things	ŋinti	ŋanta
<b>ŋ~ŋ</b>	chew	ŋɔbi	ŋobi [ɔ]

Words illustrating the full set of vowels are not available but it is probable that

as in the case of \*p, the vowel following the \*ŋ is the factor which conditions the change (or lack of change, in the ŋ~ŋ correspondence). The documented sound changes are limited to the three vowels a, i, ɔ:

(8) Documented sound changes of \*ŋ into Kɔnni:

- a. \*ŋ > h / \_\_a
- b. \*ŋ > ɲ / \_\_i
- c. \*ŋ > ŋ / \_\_ɔ

In spite of the limited data, it is not unreasonable to assume the same sets of conditioning vowels as were active in the sound changes from \*p discussed above. If so, then there are two sets of sound changes conditioned by the same sets of vowels following.

### 3. Further discussion

#### 3.1 The Kɔnni [h]

As noted above, /h/ is the only consonantal phoneme of Kɔnni that Buli lacks. This Kɔnni /h/ had its sources in \*p and \*ŋ, as discussed above, but there are two other sources of synchronic Kɔnni /h/. The table below gives examples of all of these.

(9)	English	Kɔnni	Buli
<b>h-p</b>	new	-haalŋ / -haala	paalik
	strength	hagrŋ	pagra
<b>h-ŋ</b>	boat	haarŋ	ŋaarŋ
	'black-berry' tree	haarŋ	ŋaarŋ
<b>h-s</b>	bush	haagrŋ	sagi
<b>!</b>	leopard ("bush-dog")	haŋ-gbaaŋ	goa-biak
<b>h-w</b>	grass	huuŋ / huuti	wuuk / wuuta

Though a weakening of s to h is not uncommon (and the related Gur language Dagbani even has -hi and -si as allomorphs of one of the plural morphemes) the h-s correspondence between Buli and Kɔnni is limited to this one item in my data, and other \*sa... items have reflexes of sa... in both languages. The word for 'bush' above therefore remains a unique correspondence.<sup>3</sup>

<sup>3</sup> The words for 'bush' and 'leopard' are interesting. 'Bush' in both Kɔnni and Buli refers not to a plant, but to an area outside a village, e.g. "He's gone to the bush" means he has gone to the farm or hunting or gathering firewood. Buli has two words for 'bush': sagi is the area used for farming, and goai is the more remote area for activities such as hunting and gathering grasses. Kɔnni has only one word haagrŋ which includes both ranges of meaning of 'bush,' but which is cognate to the 'near-bush' word sagi in Buli. The word for 'leopard' in both languages is a compound word, literally 'bush-dog.' (In a Kɔnni compound, the

As with the **h-s** correspondence, the **h-w** correspondence is unique to one word, and must remain so for the present.

### 3.2 The Nangurima dialect of Kɔnni

Though it is a small language group, Kɔnni is not a homogeneous language. Discussion thus far has centered on the more prestigious and more widely-spoken Yikpabongo dialect. However, one village, Nangurima, speaks a distinctly different dialect, and one of the most salient features of this dialect is pertinent to the discussion here. The Nangurima dialect followed the Kɔnni split of **\*p > y, w** as did Yikpabongo, but wherever a Yikpabongo word has **h**, Nangurima has **ŋ**.

(10)	gloss	Yikpabongo Kɔnni	Nangurima Kɔnni	Buli
<b>h/ŋ~p</b>	bark	-haŋ	-ŋaŋ	pauk
	new	-haalŋ	ŋaalŋ	paalik
	woman	həgu	ŋəgu	ni-pək
	strength	hagɪŋ	ŋagɪŋ	pagra
	arrive, reach	haarɪ	ŋaarɪ	paari
<b>h/ŋ~ŋ</b>	boat	haarŋ	ŋaarŋ	ŋaarun
	them	ha	ŋa	ŋa

Thus Nangurima Kɔnni completely lacks the /h/ phoneme of Yikpabongo Kɔnni, and resembles Buli in that respect. It is relevant to note that Nangurima is the Kɔnni-speaking village closest to the Buli area and under the most Buli influence.

**\*ŋ** and **\*p** are documented outside PBK, back to at least Proto-Central Gur (Naden 1989), which includes most of the Gur languages. Proto-Central Gur lacked the phoneme **\*h**, and this is another reason why its presence in Kɔnni must be attributed to a recent innovation.

### 4. Conclusion

A completely certain account of the development of the reflexes of **\*ŋ** is perhaps beyond reach, but a reasonable scenario can be proposed. In this scenario, when Buli and

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first noun is often reduced, typically to one syllable, thus **haŋ**- rather than **haalŋ**- in 'leopard'.) But while Buli naturally uses the 'far bush' term in compounding 'leopard', Kɔnni, not having an option, uses the only word for 'bush' available. What is interesting is that, at least at the time the proto-language made this split, the components of *meaning* of 'leopard,' that is, 'bush-dog,' were more important than the sounds involved. The Kɔnni word has no relation to the initial sounds of Buli **goa-biak** at all, but it is a direct translation of the component morphemes. The word for 'leopard' in PBK was probably similar to the Buli form.

Konni split, \*ŋ changed to ɲ before front vowels and remained as ŋ elsewhere. But when the Yikpabongo dialect split off from Nangurima, the Yikpabongo dialect lost the nasalization before **a** and changed ɲ to **h** in this environment. Here, Nangurima and Buli preserve the older forms. To sum up the changes:

- (11) a. \*ŋ > ɲ before front vowels in Konni  
 b. \*ŋ > **h** before **a** in Yikpabongo Konni

The development of the reflexes of \*p is more complex, and more uncertain as well, due to the Nangurima data. What is certain is that in the Buli/Konni split, Konni changed \*p > **y, w, X**, conditioned by the following vowel. The question is the nature of **X**, since Nangurima Konni has ɲ for these reflexes, but Yikpabongo Konni has **y** for them.

If the **X** is ɲ, and the sound change was \*p > ɲ, then Yikpabongo had a further change of ɲ > **h**, a not unreasonable change. We know from (11b) that the sound change ɲ > **h** did occur in Yikpabongo Konni, and this scenario would unify these changes. However, several intermediate stages would be needed for the initial change of \*p > ɲ; it is too large a change to happen in one step.

A preferred scenario is that **X** is **h**, and the sound change was \*p > **h**. This has the advantage of being more in parallel with the sound changes that produced the other glides, as well as being attested in other languages besides the Gur group. However, we must then account for the change in Nangurima Konni of **h** > ɲ. Though spontaneous nasalization is possible and indeed well-attested cross-linguistically<sup>4</sup>, another plausible motivation for the correspondence is the Buli influence on Nangurima, with Buli's pervasive ɲ as a model for change. These sound changes stemming from \*p may be summed up as:

- (12) a. \*p > **y, w** in Konni before front and back rounded vowels, respectively  
 b. \*p > **h** in Konni before **a**  
 c. **h** > ɲ in Nangurima Konni

<sup>4</sup> For example, the Avestan reflex of PIE \*s between two a's is apparently an aspirated engma. Since \*s > h almost everywhere else in Avestan, it is likely that \*asa passed through a stage of **aha** before the final outcome of **ah<sup>h</sup>a**, the last sound change being h > ɲ<sup>h</sup>, similar to what I am positing here. Thanks to Brian Joseph for pointing out this example.

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**The Mind and Spirit of Old English *mōd* and *fer(h)ð*:  
The Interaction of Metrics and Compounding**

Thomas W. Stewart, Jr.

### 1. Introduction

In discussing the language of poetry there can be some question of the relative roles of metrical form and semantic content in determining the poet's choice of words and/or phrasing at particular points. The picture becomes especially clear when the words in question are unattested outside the genre of poetry, some unattested outside a particular poem. The present paper examines a cohesive, though originally somewhat arbitrarily selected subset of compound words attested in Old English alliterative poetry. Through an examination of features such as distribution with respect to metrical patterns, it concludes that there is far more influence from metrical concerns than from semantic intentions in the creation and use of at least some compound forms.

#### 1.1. Old English alliterating poetry

The style of poetry written in Old English (OE) which survives in manuscripts has been analyzed, in metrical terms, into half-lines. In edited texts, these half-lines are typically presented in pairs as long-lines, which are visually separated by a space, the caesura. The motivation for this sort of analysis is the rhythmic and alliterative patterns that apparently operate over these domains—the half-lines are united by regular alliteration and a largely regular pattern of lexical stress. In any metrical scheme, there must be certain constraints on a poet's word choice, and these constraints presumably tighten in proportion with the rigidity of the metre. In OE, this led to the development of sets of synonyms or near-synonyms with different initial segments. This was presumably to allow the poet some flexibility in accommodating the alliterative pattern while permitting the intended message to be expressed (Gneuss, 38, 48; Carr, xix).

### 1.2. Defining "compound"

OE poetry has a number of formulaic aspects to it, not the least of which is the vocabulary. Words, once created for poetic purposes, could be appropriated by later poets, as circumstances demanded. Such appropriation, to the degree that it exists, allows scholars to identify mini-citations of other poems<sup>1</sup>, especially in the case of unusual words, like compounds. The present paper may contribute in the long run to this identification process, selecting as it does two relatively frequent synonymous nouns which poets used in building a remarkable number of compounds (see Appendix).

There is some recurrent discussion as to when a collocation of two or more lexical items may reliably be called a compound, as distinct from a syntactic phrase. In languages with inflectional morphology for case, compounds can be safely identified when one of the words which should be in an agreement or case relationship with the other does not show the expected morphological marking. This, however, does not represent more than a sufficient condition for diagnosing compounding. Modern German compounds, for example, are typically written as single words (*Auslautverhaertung*), whereas Modern English compounds may be written separately (*lottery ticket*), connected by a hyphen (*Indo-European*), or together as a single unit (*baseball*) (Carr, xxiii). The relative age of the compound in Modern English often correlates with the degree of orthographic connectedness, with the oldest and best established compounds almost invariably being written as single words.

OE records do not provide as much assistance in this area as might be hoped, because scribes from the period were quite variable in their practice of orthographic connection. Occasionally, demonstrably separate words, i.e., words from distinct syntactic phrases, are written as single units in manuscripts (Mitchell & Robinson, 25), while at the same time there are other words with inexplicable, seemingly random spaces in them (Carr, xxiii). Modern editorial practice has generally been sensitive to this ambiguous relationship between the written record and actual OE morphology, but in cases of indeterminacy, editors still have to impose a choice on the text. It cannot be assumed that edited versions of manuscripts, as we have them today, have managed to capture the set of all and only the intended compounds of the OE poets.

The formal attributes of a compound, so far as they can apply generally in OE, are as follows:

- (1) They are composed of at least two content morphemes, i.e., each part must be attested as a simplex form elsewhere in the language;
- (2) The right-most member in the compound is interpreted as the head of the compound, determining morphological and syntactic category membership for the whole, and the head is inflected as its simplex form would be;
- (3) The remaining portion of the compound is interpreted as the determinant of the compound, describing or delimiting the head in a systematic fashion; and
- (4) The elements contained in a compound could be recast in a grammatical syntactic phrase with a minimum of reordering, inflection, or insertion of function words

<sup>1</sup> Particularly innovative or striking wording in a well-known poem, if re-employed in a later poem by the same or another author, can serve as an allusion to the original work.



such as prepositions, determiners, or auxiliaries.

A suitable definition of compound could be, therefore, a "terse, unified substitution for a syntactical phrase" (Gardner, 13).

As suggested above, compounds abound in the Germanic languages. Compounding is an extremely productive word-formation process, and apparently this has been the case in Germanic since the earliest times for which records survive. Some scholars who have tried to analyze compounds in the past have run into trouble either with logically incompatible or excessively vague categorization schemes, proving unsatisfactory in both semantic-compositional terms on the one hand, and in syntactic-formal terminology on the other (Gardner, 17-39).

Attribute (4) in the preceding section claims that a grammatical syntactic phrase may readily be constructed containing the head and determinant of any compound, and that this phrase should retain the semantic interpretation of the compound. In fact, an entire analytical framework for compounds has been developed based on the syntax of the "equivalent" Modern English phrases, clearly an unfortunate and avoidable methodological confound (Reibel 1963, described in Gardner, 31-32). Robinson as well recommends this approach to analyzing compounds, yet although his examples employ OE vocabulary, the syntax smacks of Modern English (17). I place "equivalent" in quotation marks and refer to a confound because a system of categorization based on strict modern notions of English grammatical categories ignores discrepancies arising from semantic and syntactic changes between the OE and ModE synchronic grammars. This suggests that an alternative method be found.

There will be no attempt made here, however, to replace or edit previously devised systems of classification. Rather the focus will be on issues related to a very small portion of the lexicon, the OE words *mōd* and *fer(h)ð* and the set of attested poetic compounds derived from them, with the hope that any small generalizations which fall out on this micro-level may prove useful when a larger context is addressed.

### 1.3. Motives for compounding in Germanic poetry

Gardner summarizes the question quite nicely:

Where a poet used a compound and a syntactical phrase (often just a few lines apart) with identical or almost identical meaning, there must have been a reason for it (11).

This returns to the point made earlier, that subscribing to an alliterating metrical framework constrained a poet's choices. Gardner uses the following example from *Solomon and Saturn*:

<i>domdaeges dynn</i>	"Judgement Day's din"	Sol 273
<i>on domes daege</i>	"on Judgement's day"	Sol 337

There are two forces conspiring against the alternative *\*domes daeges dynn* "Judgement's day's din" for line 273: not only is this construction "metrically unfeasible" because of triple alliteration within the half-line, but also the nested genitive construction was apparently strongly



disfavored (Gardner, 11).

Carr, on the other hand, would ascribe the frequency of compounds to the Germanic poets' more general attitude of valuing description over plot (xviii). By this account, the narrative element of Germanic poems is merely a medium in which the poet may display descriptive virtuosity. Carr's claim to this knowledge of past mindsets of course rests on dubious ground, but it is at least worth considering whether it is the poet or the form which is truly the master.

## 2. *Mōd* and *fer(h)ð*: A case study<sup>2</sup>

Two synonymous nouns in OE, *mōd* and *fer(h)ð*, which both may be glossed as "mind" or "spirit," are frequently pressed into service in poetic compounds. The present paper has grown out of a study of the limited corpus of lexical items in Timmer's (1966) edition of the poem *Judith*. The glossary provided for the poem contains just over one hundred compound nouns and around sixty compounds that were categorized by the editor as adjectives.

The comment "categorized by the editor" is telling, perhaps, since it suggests some possible alternative analysis of the "adjectives" in question. The head elements of a number of these compounds are not originally adjectives, suggesting the zero-derivation of nouns into adjectives, i.e., derivation with no overt change in form (Lee, 5). Every instance of an "adjective" compound in *mōd* or *fer(h)ð* in *Judith* would apparently be the result of just such a zero-derivation, all the more so given the attested derived adjective *mōdig* and compounds derived from it such as *felmōdig*, *tilmōdig*, and *mōdigwæg* (Bosworth & Toller, 694-95).

Another point at which the categorization found in Timmer's glossary is perhaps inadvertently misleading is that for each of the citation forms (lexemes) relating to *mōd* and *fer(h)ð*, the actual forms that appear in the text of the poem are inflected for the various cases. Of course, in a case-marking language such as OE, it is absolutely to be expected that a noun or adjective will inflect for case as is contextually appropriate. The problem, if indeed it is a problem, arises due to an idiosyncrasy of OE morphology. Synchronically, there may be said to be two suffixes *-e*, one, an inflection that marks the dative and instrumental cases in the singular, the other, a derivational affix which converts adjectives to adverbs. Diachronically it seems clear that the instrumental forms came to be reanalyzed as adverbs. In the case of *mōd* and *fer(h)ð*, then, we have attested instances of original nouns shifted to adjectives analyzed as adverbs, for example:

*oð hie glædmōde gegan hæfdon* (Jud 140)  
...until they, with cheerful spirits, had reached...

The compound *glædmōde* might be translated by any number of Modern English phrases, but ultimately, which phrase we choose is insignificant, because the OE word *glædmōde* is marked in the oblique, so it does not directly agree with the nominative (plural!) subject *hie*. The next question is whether *glædmōde* is to be interpreted as a noun phrase or as an adverb derived from a zero-derived adjective. It is just the ambiguity that this synchronic syncretism of *-e* creates that

<sup>2</sup> Glosses for OE words listed in Bosworth and Toller (1989) may be found in the appendix.

causes confusion in taking a definitive stand in assigning grammatical categories to OE lexical items. Timmer (43) makes the bold categorization, *adj.* To be fair, the editor had pedagogical reasons for making a unitary classification, but this notwithstanding, there are both formal and functional reasons to take Timmer's categorization with a grain of salt.

### 2.1. The distribution of *mōd* and *fer(h)ð* as independent lexical items

Both *mōd* and *fer(h)ð* consist of quantitatively heavy syllables, and as such typically carry lexical stress. For the purposes of OE alliterative metre, this means that they are candidates for participation in the alliterative pattern of any long-line in which they occur. Using the *Concordance to the Anglo-Saxon Poetic Records* (Bessinger & Smith, 1978; henceforth *CASPR*), a limited token-counting exercise was undertaken, including all and only the instantiations of the lexeme *[mōd]* which are either zero-inflected (*mōd*) or marked with a suffix *-e* (*mōde*, whatever its morphological or semantic value). Of the 157 instances of *mōd*, there are a total of 36 instances where the word does *not* participate in the alliteration of its local long-line. On the other hand, of the 194 instances of *mōde*, there are 79 instances where the word does not participate in the alliteration pattern of the long-line. This means that for these two examples, the word in question actually *does* participate in the local alliteration 77% of the time and 60% of the time, respectively. These proportions can be taken provisionally as base rates of alliteration participation for stressed words.

### 2.2. *Mōd* and *fer(h)ð* as heads in compounds

There are fifty distinct compound lexemes listed in Bosworth & Toller (1898/1921) which have *-mōd* as their head (693). Ten of these lexemes appear within the text of *Judith*, in a total of 13 inflected instantiations. For each of the ten lexemes, every single inflected instantiation participates in the alliteration pattern of the long-line which contains it. Moreover, when examined in *CASPR*, every attested token of these words in the entire corpus participates in the alliteration pattern of their respective long-lines (Bessinger & Smith, 481, 649, 1127, 1168, 1250, and 1278). There are two marginal cases involving *[geōmormōd]*:

*geōmormōde ofgiefan sceoldan* "...with sad mind had to abandon..." Phx 412  
*geōmormōdum ludith behead* "...with sad minds Judith commanded..." Jud 144

It still seems quite clear, however, that the intended alliteration is between [y] sounds, despite the prepositional prefix *of-* in the former and the lack of an orthographic <g> in the latter. If these two examples are allowed, and they are not without supporting precedent, then it may be maintained that there exists a perfect correlation between the appearance of compounds and an alliteration scheme which requires exactly their initial sound at a particular point.

There are sixteen distinct compound lexemes listed in Bosworth and Toller which have *-fer(h)ð* as their head (282). Of these, five are present in the text of *Judith*, in a total of seven inflected instantiations. Again, each of these instantiations participates in the alliteration pattern of the long-line which contains it, and once more *CASPR* shows clearly that every instantiation of these five lexemes in the corpus of OE verse duly participates in their respective local alliteration patterns (Bessinger & Smith, 171, 1092, and 1417).

### 2.3. *Mōd* and *fer(h)ð* as determinants in compounds

*Mōd*- appears as the initial element, or determinant, in thirty-nine distinct compounds (Bosworth & Toller, 693-95). Of these, six are not represented in *CASPR* (832-36), while two additional compounds are listed in *CASPR*, *mōdgode* ("with divine spirit") and *mōdheap* ("bold troop or host"), which are absent from Bosworth & Toller. The total number of inflected instantiations of such compounds attested in OE verse is 128. Without exception, each of these compound forms participates in the alliteration pattern of its local long-line, independent of its position therein.

The story with *fer(h)ð*- is much the same, although with fewer examples in every respect. Fourteen are listed in Bosworth & Toller (282), of which only one (*ferhðufe*) does not appear in *CASPR* (313-14). Present in *CASPR* but not in Bosworth & Toller are *fer(h)ðgefeonde* ("with rejoicing heart or spirit") and *ferðweg* ("the way or manner of the heart or spirit"). The total number of inflected instantiations of such compounds in OE verse is 32. Again, without exception, each of these forms participates in the alliteration of its respective long-line.

These facts, taken together with those of the previous section, suggest that the correlation between a word's being a compound and its fulfilling a local demand of the metrical scheme is more than coincidence. The deviation from the base rate of participation presented in section 2.1 clearly points to the role of metrical constraints in the poet's choice whether to employ a compound form instead of an equivalent syntactic phrase.

### 2.4 Reversible compounds?

Among the compound forms under consideration are seven pairs which have the same two component parts in opposite orders, namely, *ferhðgleāw* & *gleāwferhð*, *ferhðwērig* & *wērigferhð*, *mōdgeomor* & *geomormōd*, *mōdglæd* & *glædmōd*, *mōdgleāw* & *gleāwmōd*, *mōdhwæt* & *hwætmōd*, and *mōdswið* & *swiðmōd*. Under normal circumstances, a change in the head of a compound results in a change in the semantic interpretation of the whole. Observe, for example, the contrast between the Modern English compounds *housecat* and *cathouse*, or *showboat* and *boat show*. There is, however, no such semantic contrast within the pairs listed here. The words are defined as synonyms, and therefore should have been able to be used interchangeably. The attested distribution is therefore quite telling: within each pair, each compound begins with a different sound, and which of the two compounds appears in a given line is directly correlated with the local alliteration pattern. It would be difficult to dismiss the conclusion that the choice is slavishly determined by the metre.

### 3. Conclusion

Section 2.1 demonstrated that potential participants in alliteration need not alliterate in their every appearance, yet the distribution of compounds observed in sections 2.2-3 make a clear implication: If a compound, then alliterate. Section 2.4 presents the transitory nature of OE poetic compounds in the attested variable ordering of the component parts while maintaining the identical composite semantics. The conclusion is clear, at least for the compounds presented here (and most likely more generally as well). Metrical concerns drive compounding, with the potential to override semantic conventions. The place of poetic compounds in the OE lexicon, therefore, is ambiguous at best.

Brady (1979) warns against assuming that a given compound is "demanded by the exigencies of alliteration" just in case the word in question "itself sets the alliterative pattern of the line in which it stands" (88). In support of this point, she uses examples parallel to the above, where the compound in question is the first alliterating element in the long-line. The claim that such positions "set" the local alliterative pattern presupposes a strict left-to-right process of composition. There is no evidence that the first of two paired half-lines in linear sequence was necessarily created first temporally, and this is especially unlikely where each of the half-lines belongs to a different clause. Brady's overall emphasis on the semantic content of compounds is likely behind the poem-as-unfolding-narrative perspective, whereas the present paper's metrical hypothesis abstracts away from the narration to focus on undeniable patterns of rhythm and sound with respect to these same objects, the OE poetic compounds.

In sum, there does indeed seem to be some substance to the suspicion that compounds are created or chosen in lieu of simplex forms in syntactic phrases according to metrical convenience. Depending on one's perspective this can be seen either as taking away a measure of spontaneity on the part of the poet, or as providing additional stimulation to and recognition of the poet's ingenuity. In light of the formulaicity of the enterprise in general, and the amount of variation actually achieved, visible even in these two lexical sets, there remained a significant opportunity for creativity, resourcefulness, and self-expression available to the OE poet.

#### Appendix

Forms marked with \* have a corresponding compound with the component parts in the opposite order with no apparent change in the meaning of the whole. Glosses are from Bosworth and Toller (1898).

##### With *-ferhþ*, *-ferþ*, or *-fyrhþ*:

<i>collen-</i>	"fierce-minded, bold in spirit"
<i>dreorig-</i>	"sad in soul"
<i>freorig-</i>	"sad in soul"
<i>gāl-</i>	"mind-lustful, licentious"
<i>gamol-</i>	"'old-souled', advanced in age, aged"
* <i>gleāw-</i>	"of wise mind, sagacious"
<i>sār-</i>	"sore at heart, wounded in spirit"
<i>sārig-</i>	"sad in soul"
<i>stærced-</i> (= <i>sterced-</i> )	"having the mind strengthened, stouthearted, courageous"
<i>stearc-</i>	"of harsh or stern soul"
<i>stīþ-</i>	"of firm, strong mind"
<i>sweorcend-</i>	"with the mind growing gloomy"
<i>swīþ-</i> (= <i>swyþ-</i> )	"of strong mind or soul"
<i>swoncen-</i>	(??) if a variant of <i>swoncer-</i> , then "life having failed, i.e., dead"; if a variant of <i>sworcen-</i> , then "with darkened soul, i.e., dead"
* <i>wērig-</i>	"weary-hearted, disconsolate, depressed"
<i>wīde-</i>	"long life, an age," thus "for a long time, forever"

##### With *ferhþ-* or *ferþ-*:

<i>-bana</i>	"life-destroyer, murderer"
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- <i>cearig</i>	anxious in soul
- <i>cleōfa</i> (= - <i>cōfa</i> )	"the mind's cave, i.e., the breast"
- <i>frec</i>	"bold in spirit"
- <i>friðende</i>	"life-saving"
- <i>geniþla</i>	"life-enemy, deadly foe"
- <i>gewit</i>	"mental wit, understanding"
*- <i>gleāw</i>	"prudent in mind, sagacious"
- <i>grim</i>	"fierce of spirit"
- <i>loca</i>	"soul inclosure, bosom, body"
- <i>lufe</i>	"soul's love, mental love"
- <i>sefa</i>	"mind's sense, intellect"
*- <i>wērig</i>	"soul-weary, sad"

With -*mōd*:

<i>ācol-</i>	"of fearful, timid mind"
<i>an-</i>	"steadfast, eager, bold, courageous, daring"
<i>ān-</i>	"of one mind, unanimous"
<i>ættren-</i>	"of poisonous mind"
<i>æwisc-</i>	"disgraced in mind, ashamed, abashed"
<i>blīðe-</i>	"of joyful mind"
<i>deōr-</i>	"bold of mind, brave"
<i>dreōrig-</i>	"sad of mind"
<i>eād-</i> (= <i>eāð-</i> )	"humble, meek, mild, lowly, obedient"
<i>forht-</i>	"mind-frightened, timid, pucillanimous"
<i>freōrig-</i>	"sad in mind"
<i>gāl-</i>	"light-minded, licentious"
<i>gealg-</i>	"sad in mind, gloomy"
* <i>geōmor-</i>	"sad of mind, sorrowful"
<i>gewealden-</i>	"subdued of mind, self-controlled"
* <i>glæd-</i>	"glad-minded, cheerful, joyous, pleasant, kind, courteous"
* <i>gleāw-</i>	"of wise mind"
<i>gūð-</i>	"of warlike mind"
<i>heāh-</i>	"of high, lofty mind; noble, proud, haughty"
<i>heān-</i>	"dejected, humiliated"
<i>heard-</i>	"of hard, unyielding spirit; self-confident, stouthearted, brave"
<i>hreōh-</i>	"savage, fierce of mind, ferocious, troubled in mind"
<i>hreōwig-</i>	"sad at heart"
* <i>hwæt-</i>	"stouthearted, bold"
<i>irre-</i>	"of angry mood, angry-minded"
<i>lāðwende-</i>	"evilily or hostilely disposed"
<i>leōht-</i>	"of light or cheerful mind, light-hearted, easy-tempered"
<i>meagol-</i>	"of earnest mind, earnest, strenuous"
<i>meaht-</i>	"strong feeling, passion"
<i>micel-</i>	"having a great mind, magnanimous"
<i>ofer-</i>	"pride, arrogance, overconfidence"
<i>or-</i>	"without courage, hopeless, despairing"

<i>reomig-</i>	(variant of <i>reōnig-</i> )
<i>reōnig-</i>	"sad at heart, weary"
<i>rēðe-</i>	1) pejorative "of fierce or savage mind" 2) "of [justly] stern or severe mind, wroth"
<i>rēðig-</i>	"of fierce or savage mind"
<i>rūm-</i>	"of liberal mind, liberal in giving"
<i>sārig-</i>	"sad-hearted, of mournful mood"
<i>sceōh-</i>	"fearful (wanton?) of heart"
<i>stifð-</i>	1) "of constant mind, resolute" 2) "stern, of stern mind" 3) "of violent or fierce mind" 4) "stubborn, of stubborn mind, obstinate"
<i>styrn-</i>	"stern of mind"
* <i>swifð-</i>	1) "great-souled, magnanimous, stouthearted" 2) "of violent mind, arrogant, haughty, high-minded"
<i>þancol-</i>	"having the mind addicted to thought, of acute mind, wise, intelligent"
<i>þearl-</i>	"of severe mind" (in either a positive or negative sense)
<i>til-</i>	"noble-minded"
<i>torht-</i>	"glorious, illustrious"
<i>torn-</i>	"having the mind excited to anger; having rage in the heart"
<i>wērig-</i>	"weary in spirit"
<i>wrāð-</i>	"angry-hearted, incensed"

With *mōd-*:

<i>-blind</i>	"having the mind's eye darkened, undiscerning"
<i>-blissende</i>	"rejoicing at heart"
<i>-bysgung</i>	"anxiety of mind"
<i>-cearig</i>	"anxious of heart"
<i>-cearu</i>	"sorrow of heart, grief"
<i>-cræft</i>	"mental power or skill"
<i>-cræftig</i>	"possessing mental power, intelligent, skillful"
<i>-cwānig</i>	"sad at heart"
<i>-earfoþ</i>	"travail of soul, distress of mind"
<i>-gehygd</i>	"thought"
<i>-gemynd</i>	"mind, thought, intelligence"
* <i>-geōmor</i>	"sad at heart, of mournful mind"
<i>-geþanc</i>	"mind, thought, thoughts"
<i>-geþoht</i>	"mind, thought"
<i>-geþyldig</i>	"patient of soul"
<i>-gewinna</i>	"a foe of the mind," thus "care, anxiety"
* <i>-glæd</i>	"of gladsome mind"
* <i>-glædw</i>	"wise of mind"
<i>-hete</i>	"hostility of mind, hate"
<i>-hord</i>	"the mind"

*-hwæt	"strong of soul, courageous, brave"
-leās	"spiritless, dull"
-least	"want of courage, pusillanimity"
-leōf	"dear to the heart, beloved"
-lufu	"heart's love, affection"
-sefa	(poetic syn. for <i>mōd</i> ) "the inner man, mind, spirit, soul, heart"
-seōc	"sick at heart"
-seōcness	"disease of the heart"
-snotor	"prudent of mind, wise, sagacious"
-sorh	"care or sorrow of mind, sorrow of soul"
-staþol	"foundation on which the mind rests"
-staþolfæstness	"stability of mind"
*-swið	"strong of mind or soul"
-þracu	"impetuosity of mind, impetuous or daring courage"
-þreā	"pain or torment of mind"
-þryǣu	"violence of mind"
-þwære	"gentle, meek, mild"
-þwærness	"gentleness, meekness, patience"
-unmeht	"disease of the heart" (see <i>mōdseōcness</i> )
-welig	"rich in spiritual or mental gifts"
-wēn	"hope entertained by the mind"
-wlanc	"proud, haughty, of high courage"

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**Ringe Revisited:  
Comments on Ringe's Probabilistic Comparison Method\***

Pauline Welby, Neal Whitman

**0. Introduction**

Ringe (1992) seeks to design a "completely objective criterion of proof" (p. 80) for eliminating the "factor of chance" in investigating possible genetic relationships between languages. The method is presented as a *necessary* starting point in language comparison: "It is urgently necessary to subject all controversial 'demonstrations' of language relationship to investigation by the probabilistic method, so as to prove the truth of those claims or show that they are beyond objective proof." (p. 81) Ringe clearly states that it is only worthwhile to apply the traditional comparative method if the probabilistic method yields a positive result. Of a comparison of English and Latin, Ringe states:

[t]o be sure, the probabilistic method does demonstrate that English and Latin are related, and such a demonstration *is necessary* before we can embark on further meaningful comparative work. (emphasis added) (p. 47)

A negative result, by contrast, signals that any relationship between two languages is not demonstrable and that therefore no further comparison should be attempted. Ringe is motivated by the claims of Nostraticists and Proto-World linguists who argue for the provability of long-distance relationships among language families or indeed among all spoken human languages. These linguists challenge the view of traditionalists who argue

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\* This paper is the result of combining and refining two individually written manuscripts on this topic. We wish to thank Brian Joseph for inviting us to contribute to this volume, which is what prompted us to pool our previous work. We also thank Keith Johnson for useful discussion of statistical issues. Any errors are, of course, our own.

that even if all (spoken) human languages do share a common ancestor, such relationships cannot be proven because evidence of relatedness has been obliterated by time. Ringe's stated goal is "to provide an objective test of the validity of such challenges" (p. 1). He argues that proponents of the provability of long-distance relationships have failed to adequately discount the possibility that sound correspondences used as evidence for a genetic relationship are actually chance resemblances.

In this paper, we summarize several of the language comparisons that have been conducted using Ringe's method, including the comparisons in Ringe's original paper, those reported in Baxter and Manaster Ramer's (1996) review article, and a number of comparisons that we have done ourselves. We make two primary criticisms of the Ringe method. First, the method yields results whose interpretation is not clear. Second and more importantly, the distance of relationship between two languages has little bearing on the strength of the result returned by Ringe's test.

## 1 Outline of the method

### 1.1 Compilation of a Swadesh list

Ringe compares the phonological similarity of words from two languages that share a common meaning. In designing a list of words, one must eliminate words whose phonological shape is non-arbitrary — nursery words and onomatopoeic words. Words that are not inherited but borrowed from another language are also excluded. Ringe stresses the importance of not admitting words of related but non-identical meanings into a comparison (contra the practices of Nostraticists and Proto-World linguists). To reduce the possibility of including borrowings, Ringe uses Swadesh lists<sup>1</sup> of 100 and 200 basic meanings, reasoning that this basic vocabulary is less likely to be borrowed.

### 1.2 Applying the method

Use Swadesh lists for two languages to conduct pairwise comparisons of corresponding forms (i.e., words sharing the same meaning) in two languages. This process is illustrated below with a comparison of English and Hawaiian.

Step 1: Choose a word position to examine. We examine here segments occurring in word-initial position.

Step 2: For each language, calculate the probability that a word from the Swadesh list (not from a larger sample of the language's vocabulary) will have a given segment in that position. The English Swadesh list (see Appendix) has 17 possible initial consonants (counting  $\emptyset$  as a consonant for vowel-initial words). For each of the 17 consonants, the number of times that consonant appears in word-initial position must be tabulated. For example, the phoneme /h/ appears in initial position 8 times in the English Swadesh list. To obtain the probability of initial /h/, 8 is divided by the number of words in the list, in

<sup>1</sup> A list of core vocabulary presumed to be resistant to borrowing, named for Morris Swadesh.

this case 100. The resulting number, here 0.08 or 8%, is the probability that the given consonant will appear by chance in the chosen position.

Step 3: Calculate the probability of all possible correspondences between phonemes in the two languages in the chosen position. Since the English list has 17 possible initial consonants and the Hawaiian list has 9, the probabilities for the 153 (i.e., 17 x 9) correspondences have been calculated, and are shown in Table 1.

**Table 1:** Expected number of matches for English-Hawaiian initial phonemes

		Haw								
		p	k	m	n	h	w	l	ʔ	∅
Eng	∅	.64	.64	1.04	.4	.64	.48	.88	1.92	1.2
	b	.8	.8	1.3	.5	.8	.6	1.1	2.4	1.5
	d	.24	.24	.39	.15	.24	.18	.33	.72	.45
	f	.64	.64	1.04	.4	.64	.48	.88	1.92	1.2
	g	.24	.24	.39	.15	.24	.18	.33	.72	.45
	h	.64	.64	1.04	.4	.64	.48	.88	1.92	1.2
	k	.4	.4	.65	.25	.4	.3	.55	1.2	.75
	l	.4	.4	.65	.25	.4	.3	.55	1.2	.75
	m	.4	.4	.65	.25	.4	.3	.55	1.2	.75
	n	.64	.64	1.04	.4	.64	.48	.88	1.92	1.2
	p	.08	.08	.13	.05	.08	.06	.11	.24	.15
	r	.48	.48	.78	.3	.48	.36	.66	1.44	.9
	s	1.12	1.12	1.82	.7	1.12	.84	1.54	3.36	2.1
	θ	.64	.64	1.04	.4	.64	.48	.88	1.92	1.2
	ð	.16	.16	.26	.1	.16	.12	.22	.48	.3
	w	.56	.56	.91	.35	.56	.42	.77	1.68	1.05
	y	.64	.64	1.04	.4	.64	.48	.88	1.92	1.2

Selecting one example out of these 153, consider the probability for a correspondence between English initial /h/ and Hawaiian initial /h/. The probability for initial /h/ in English is 0.08; the probability for initial /h/ in Hawaiian is 0.11. The probability that for a given meaning the English word will have initial /h/ and the Hawaiian word will have initial /h/ is the product of these two probabilities (0.08 x 0.11 = 0.0088) or 0.88%. To obtain the number of expected matchings, this probability is multiplied by the number of words on the list. Our list has 100 words, so about one match is to be expected by chance alone (0.0088 x 100 = 0.88).

Step 4: Count the actual number of correspondences for every combination of phonemes. Results are shown in Table 2 (boldfaced and underlined entries will be discussed shortly). As it happens, there are three instances of English initial /h/ corresponding to Hawaiian initial /h/: hair [of head]/lauoho, hand/lima, hear/lohe.

**Table 2:** Actual number of matches for English-Hawaiian initial phonemes.

		Haw								
		p	k	m	n	h	w	l	?	Ø
Eng	Ø	1	0	1	0	1	0	2	1	<b>8</b>
	b	0	1	1	<b>2</b>	0	0	0	4	2
	d	0	0	<b>2</b>	0	0	0	0	0	1
	f	1	0	1	0	1	1	1	1	2
	g	0	0	1	0	1	0	0	1	0
	h	2	1	0	0	0	<b>2</b>	<b>3</b>	0	0
	k	1	0	1	0	1	0	0	0	2
	l	0	0	1	0	0	0	2	1	1
	m	0	1	2	1	0	1	0	0	0
	n	1	1	0	0	1	0	0	3	2
	p	0	0	0	0	0	0	0	0	<b>1</b>
	r	<b>2</b>	1	0	0	0	0	0	1	2
	s	1	1	1	1	1	0	1	5	3
	T	0	0	0	1	1	0	2	0	1
	D	0	<b>2</b>	0	0	0	0	0	0	0
	w	0	1	1	0	1	<b>2</b>	0	1	1
	Y	0	0	1	0	0	0	0	1	0

Step 5: For each phoneme correspondence, check using the binomial distribution to see whether the observed number of matches could be expected to occur by chance. Ringe includes only matches whose number of occurrences is in the 99th percentile. That is, if there is less than a one in 100 chance that the observed number of correspondences could occur by chance ( $p < 0.01$ ), the match is included. Binomial distributions can be calculated using published charts or statistical software packages. Ringe gives a number of binomial distribution charts. A sample of binomial distribution charts (from Ringe) is given in Table 3.

**Table 3:** Sample binomial distribution chart

p < 0.0088	
Number of matches (out of 100 word pairs)	percentile
5	0.99972680
4	0.99801103
3	0.98794550
2	0.94119314
1	0.77998859

For example, the probability of there being a match between English /h/ and Hawaiian /l/ by chance is 0.0088. The actual number of matches is 3. The chart shows that there is therefore a 99% chance (0.98794550, rounded to two significant digits) that these three matches did not occur by chance alone. This set of matches meets Ringe's standard.

Step 6: Count the number of matchings that meet the 99th percentile criterion. For convenience, we will follow Baxter and Manaster Ramer (1996) in referring to this number as M. In addition to the /h/:/l/ matches in the English/Hawaiian comparison, there are 8 other matches that meet the 99th percentile criterion: /r/:/p/, /b/:/k/, /d/:/m/, /b/:/n/, /h/:/w/, /w/:/w/, /Ø/:/Ø/, /p/:/Ø/. These are indicated in Table 2 above, underlined and in boldface type.

Step 7: Repeat Steps 1 - 6 using a different word position. Ringe does not specify a minimum number of positions to examine before drawing a conclusion.

Step 8: Draw a conclusion based on the results.

## 2 Evaluation of the method

### 2.1 How "high" numbers are interpreted

Using this simple procedure, we compute one number for each word position in each two-language comparison. Ringe, however, gives us no real criteria for interpreting these numbers, and does not indicate how many word positions must be examined before a conclusion is drawn. We therefore closely examine the conclusions Ringe draws from his own comparisons.

Ringe starts with a comparison of two closely related languages, Standard American English and Standard High German. The results of a comparison of a single word position are enough to convince him the two are related: "... there are sixteen [initial position matches that meet the 99th percentile criterion]. That alone would be enough to demonstrate beyond a reasonable doubt that English and German are related languages." (p.23) Although he draws the conclusion that the two languages are related from the comparison of a single word position, Ringe does go on to apply the method to several other word positions, noting that the results provide overwhelming support for his initial conclusion. "The probabilistic method of investigation employed here clearly provides massive evidence of the close relationship between English and German." (p. 35).

In a comparison of English and Latin, languages whose relationship is more distant, but also well documented, Ringe finds seven word-initial consonant matches that meet the 99th percentile criterion. He remarks "[t]hat is far fewer than in the case of English and German, and it shows that English and Latin are not nearly so closely related." (p.42) The interpretation of this statement is not clear. Ringe may mean that the finding of seven word-initial consonant matches indicates that English and Latin are definitely related or he may mean that if English and Latin are related, they must be less closely related than English and German. He also examines matchings between second-position consonants, concluding "[t]he numbers found look absolutely random, except for [one matching]." A comparison of first-syllable vowels finds no matches meeting the 99% percentile. A comparison of consonants immediately following first-syllable vowels

finds two matches meeting the 99<sup>th</sup> percentile, English /r/: Latin /r/ (six word pairs) and English /t/: Latin /d/ (six word pairs). Ringe notes that "the lexical correlation of matchings is not impressive" since only seven word pairs have significant matches for more than one word position. He nevertheless concludes "[t]o be sure, the probabilistic method does demonstrate that English and Latin are related, and such a demonstration is necessary before we can embark on further meaningful comparative work." (p. 47) He notes that while the mathematical method finds the English:Latin matching *t:d* to be significant only for the position immediately after the first vowel, the comparative method reveals that correspondence exists word-initially (as in *two: duo*) and postconsonantly (as in *heart: cord*-). Ringe seems to offer this as an example of how the probabilistic method offers a starting point to the comparative method, stating "this case, then, also demonstrates that the probabilistic and comparative methods complement each other, each contributing something of value" (p. 47).

To summarize, Ringe's treats the discovery of sixteen matches reaching the 99<sup>th</sup> percentile criterion in a one-position comparison of English:German as evidence "beyond a reasonable doubt" that the two languages are related. Although the probabilistic method finds for the English and Latin comparison only seven matches reaching the 99<sup>th</sup> percentile criterion for one position and two matches reaching the criterion for another position (with only nine word pairs with two matchings reaching the 99<sup>th</sup> percentile), Ringe concludes that the method proves that the two languages are related. If a comparison of two other languages yields similar results, we should therefore be able to conclude that those two languages are related.

We do in fact find results that may be comparable to those found in the English:Latin comparison. Recall that a comparison of English and Hawaiian, two languages generally believed not to be related, yielded nine word-initial matches reaching the 99<sup>th</sup> percentile criterion: /Ø/:/Ø/ (eight word pairs); /h/:/h/ (three word pairs); /r/:/p/, /ð/:/k/, /d/:/m/, /b/:/n/, /f/:/w/, /w/:/w/ (two word pairs each), and /p/:/Ø/ (one word pair). The number of matches is eight if we follow Ringe in excluding the /p/:/Ø/ match since it occurs in only one word pair. In another comparison of two languages not known to be related, Ringe examined English and Navajo initial consonants, vowels, and non-initial consonants, finding no matchings and concluding: "[t]hus the probabilistic method asserts unequivocally that English and Navajo are not demonstrably related. The comparative method concurs" (p. 54). In fact, our own computations show that there are 9 English: Navajo word-initial matches that meet Ringe's criteria (in the 99<sup>th</sup> percentile, match is found in more than a single word pair). These matches are /t/:/c/, /h/:/d/ (three word pairs each); /d/:/c/, /Ø/:/h/, /f/:/t', /k/:/k', /b/:/b/, /Ø/:/y/, /s/:/s/ (two word pairs each). When we consider first syllable vowels, we find 4 matches meeting Ringe's criteria, namely /e/:/i/, /ɔ/:/á/, /ɔ/:/áá/, /u/:/oo/. There are also 17 other matches which meet the 99<sup>th</sup> percentile criterion, but are found only in single word pairs ("single matches") and so would be discarded by Ringe. A comparison of consonants occurring immediately after the first vocalic nucleus yielded one match (/n/:/h/) which meets Ringe's criterion. In addition there are 15 single matchings which meet the 99<sup>th</sup> percentile criterion. If we exclude single matches, there is no English:Navajo word pair



which contains criteria-reaching matches in more than one word position. The results of the English:Navajo comparison seem similar to those of the English:Latin comparison.

	word-initial consonant	vowel of first syllable (V1)	consonant following V1	word pairs with >1 significant match
English/Latin	7	0	2	7
English/Navajo	9	4	1	0

What conclusion are we to draw from these results? Ringe's interpretation of the English:Latin comparison suggests that we might conclude from the results of these two comparisons that English and Hawaiian are related and that English and Navajo are related, and that it is therefore worthwhile to apply the comparative method to explore the details of their relationship.

## 2.2 How "low" numbers are interpreted

Even if the method sometimes yields "high" numbers which cannot be reliably interpreted as proof of a language relationship, it might be worthwhile to apply the method if it consistently yielded "low" numbers *only* in comparisons of two unrelated languages. Unfortunately, as Baxter and Manaster Ramer point out, comparisons of demonstrably related languages sometimes yield numbers between zero and two.

Comparisons of two languages not known to be related do often yield lower numbers than do comparisons between two languages whose relationship is well-established. Ringe compares English and Turkish, two languages not generally believed to be related. He finds two initial consonant matchings (English /b/: Turkish /k/ and English /j/: Turkish /s/) that meet the 99% percentile.<sup>2</sup> He examines the eight word pairs involved in these matchings, using what we know about the history of the two languages, and concludes that there is no historical relationship between the pairs of words. That result, combined with the absence of matchings in comparisons of first syllable vowels and consonants immediately following the first syllable vowel, leads Ringe to conclude that English and Turkish are not demonstrably related. Remarking on the fact that two, rather than zero, word-initial matches reaching the 99<sup>th</sup> percentile criterion were found, Ringe writes "...two numbers of matchings in the 99<sup>th</sup> percentile of their expected ranges will not be remarkably high. It follows that two 99<sup>th</sup>-percentile numbers of matchings for a single phonotactic position in a single list-comparison *must not* be taken as evidence for linguistic relationship without further investigation. Random chance does not present us with such cases very often, but it does so occasionally" (p. 51, emphasis in the original). Ringe seems to be cautioning us that we do not necessarily need to obtain a result of zero for every comparison in order to conclude that two languages are not demonstrably related. Indeed, Baxter and Manaster Ramer's (1996) comparison of word-initial

<sup>2</sup> In fact, according to our calculations, there are two other matches, English /s/: Turkish /k/ and English /t/: Turkish /d/, that meet the 99<sup>th</sup> percentile criterion when rounded to two significant digits. These two matches bring the total number of word-initial consonant matches to four. It is unclear how this number would be interpreted.

consonant matches in a Dutch and Hebrew, two languages not generally believed to be demonstrably related, yielded a result of two.

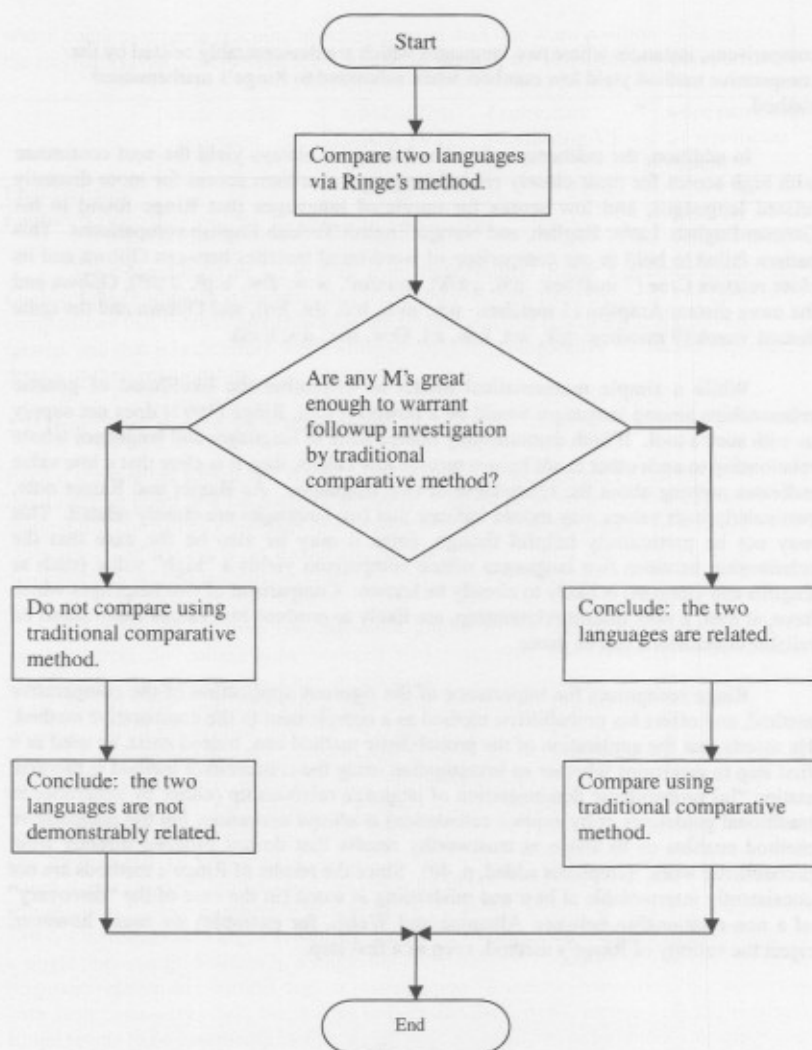
The same low numbers are found, however, in comparisons of languages known to be demonstrably related. Baxter and Manaster Ramer (1996) found no matches meeting the 99% percentile criterion in their comparison of initial consonants in Modern Hebrew and Hausa, two Afro-Asiatic languages, and only one such match in a comparison of Albanian and Welsh, two Indo-European languages. Baxter and Manaster Ramer observe that "[s]uch a situation illustrates the crucial mathematical problem with Ringe's method: though he wishes to use the statistic *M* to determine whether observed similarities are significantly greater than expected by chance, he does not give any method of determining how likely a particular value of *M* is to occur by chance, either in general or in any specific comparison. Without this information, there can be no test of the significance of any particular result..." (p. 377).

### 3 Conclusion

The following flowchart illustrates, to the best of our understanding, Ringe's proposal for the interaction of the comparative method and his probabilistic method. The method is presented as a litmus test — if the method yields a positive result, investigation by the comparative method is warranted; if the method yields a negative result, the conclusion is drawn that two languages being compared are not demonstrably related and the investigation stops before the comparative method is applied.

Our criticisms begin at the decision diamond, with the question, "Are any matches meeting the 99<sup>th</sup> percentile criterion great enough to warrant followup investigation by the traditional comparative method?" It is unclear what it means to answer 'yes' to this question. How many matches are enough? How many word positions must be examined? The finding of 16 matches for word-initial consonants led Ringe to conclude that English and German are related "beyond a reasonable doubt" (p. 23). For the English:Latin comparison, however, it took 7 word-initial matches and three additional matches at other positions to determine that the two languages were related. If the mathematical method does not clearly indicate whether we can draw similar conclusions of relatedness in comparisons like English:Hawaiian and English:Navajo, then what is its value?

According to Ringe, if we decide that a mathematical comparison of two languages has been successful and choose "yes" at this point in the chart, we can claim to have proved a relationship and should follow up with the comparative method to learn more details of the relationship. If we decide that the comparison has failed and choose "no" at this point in the chart, we conclude that the two languages are unrelated and end the investigation, thus saving valuable time which might otherwise have been spent on exploring dead ends. After all, if the comparative method is to be invoked whether the answer is yes or no, Ringe's method would offer little if any benefit. We have seen, however, in Baxter and Manaster Ramer's Hebrew:Hausa and Albanian:Welsh



comparisons, instances where two languages which are demonstrably related by the comparative method yield low numbers when submitted to Ringe's mathematical method.

In addition, the mathematical method does not always yield the neat continuum with high scores for most closely related languages, medium scores for more distantly related languages, and low scores for unrelated languages that Ringe found in his German:English, Latin: English, and Navajo:English/Turkish:English comparisons. This pattern failed to hold in our comparison of word-initial matches between Ojibwa and its close relative Cree (7 matches: n:n, g:k/k<sup>h</sup>, m:m/m<sup>h</sup>, w:w, z:w, b:p<sup>h</sup>, d:t/t<sup>h</sup>), Ojibwa and the more distant Arapaho (5 matches: n:n, m:b, b:č, d:t, š:s), and Ojibwa and the quite distant Yurok (9 matches: g:k, w:t, k:w, z:t, Ø:w, m:r, d:s, k:m).

While a simple mathematical model to determine the likelihood of genetic relationships among languages would be a powerful tool, Ringe (1992) does not supply us with such a tool. If both demonstrably related pairs of languages and languages whose relationship to each other is not known receive low values, then it is clear that a low value indicates nothing about the relatedness of two languages. As Baxter and Ramer note, particularly high values may indeed indicate that two languages are closely related. This may not be particularly helpful though, since it may be also be the case that the relationship between two languages whose comparison yields a "high" value (such as English and German) is likely to already be known. Comparison of two languages which have, at best, a very distant relationship, are likely to produce low values from which no reliable conclusions can be made.

Ringe recognizes the importance of the rigorous application of the comparative method, and offers his probabilistic method as a complement to the comparative method. He asserts that the application of the probabilistic method can, indeed *must*, be used as a first step to determine whether an investigation using the comparative method is merited, stating "[a] probabilistic demonstration of language relationship (either by adherence to traditional guidelines or by explicit calculation) *is always necessary*, but the comparative method enables us to arrive at trustworthy results that do not proceed directly from probabilistic work" (emphasis added, p. 40). Since the results of Ringe's methods are not consistently interpretable at best and misleading at worst (in the case of the "discovery" of a non-relationship between Albanian and Welsh, for example) we must, however, reject the validity of Ringe's method, even as a first step.

## Appendix: Swadesh lists

	English	Hawaiian	Ojibwa	Cree	Arapaho	Yurok
1	all (pl)	apau	gkɪnə	kʰəkɪy-	he:yow	ču
2	ashes	lehu	bgwɪ	pʰɪku-	čeʔiθe:	pontet
3	bark	ʔili	nəgek	wəyəkəsko	nó ox	-rkʰeč
4	belly	ʔooppuu	məsəd	mətəy	nót	-yah
5	big	nui	gčɪ	mɪsɪkɪt-	be	peloy-
6	bird	manu	bneši	piyesɪw	ni:ʔehi	č'uč'is
7	bite	nahu	dkuŋəd	makʰwə-	tó:yo-	čeykum
8	black	ʔeleʔele	mkəde	kəskɪt-	wo:ʔte:	loʔogey-
9	blood	koko	mškɪwɪ	mʰɪko	beʔ	pekoyek
10	bone	iwi	kəne	oskən	hix	wʔɪkʔ
11	breast(s)	uu	dodoš	to'tos	θen	newon
12	burn [intr]	ʔaa	zkɪdeg	pəsi-	nonð:té:ʔ	loʔop
13	claw	mikiʔao	škaž	skəsi	ʔox	wetketeg
14	cloud	ao	ankod	wəsko	hɪ:mo:nóʔet	leptenok
15	cold	anu	dkaba	tʰəkɪs-	to:yò:	sa:won
16	come	hele mai	bɪyad	ɪtot-	héič-	nes-
17	die	make lao	nbud	nɪp-	če:tóʔo-	kmoyl-
18	dog	ʔ	nɪm	ətɪm	heθ	čišah
19	drink	inu	mɪkɪwed	mɪnʰɪkwewɪn	bé:ne-	rekʰoh
20	dry	maloʔo	bateg	pʰako-	heni'xo'	čeʔloy-
21	ear	pepeiao	təwəg	towəki	notono	čpegaʔr
22	earth	lepo	žəški	əstɪski	bɪtoʔowuʔ	tkel
23	eat	ʔai	wɪsnɪd	mɪčɪ-	bɪθɪh-	nep-
24	egg	hua	wawən	wawi	nom	wɪʔɪ
25	eye	maka	škižɪg	kɪsɪk	sɪsɪeʔ	-lin
26	fat [nn]	momona	wɪnən	pɪmɪ	ni:nen	pemey
27	feather	hulu	mɪgu	opiwəy	bɪi	regoʔ
28	fire	ahi	škude	ɪskut-	sɪte:	meč
29	fish	iʔa	gɪgo	kɪnuseo	neb	nepeʔwis
30	flesh	ʔiʔo	wɪyas	wɪyas	θebéx	tewon
31	fly [vb]	lele	bɪmɪdeg	pɪmɪy-	čebihʔohú-	la:yol
32	foot	waawae	zɪd	sɪt	ʔo:θ	čkah
33	full	piha	moškneɟ	sakəskɪn	heni:	kohčewe
34	give	haaʔawi	mɪgwed	mek-	bɪ:n-	čɪn-
35	good	maikaʔi	nɪwakad	mɪyo	hi:θéih-	skuyep-
36	green	ʔoomaʔomaʔo	žawškuzɪd	əskɪtəko-	če:mé:te:	wʔh-
37	hair [of head]	lauoho	nɪnzɪs	stəkəy	θeʔe:	leptoy
38	hand	lima	nɪŋ	čʰiči	čét	čewes
39	head	poʔo	ndɪb	tɪkwən	kuhúʔe	mołkʰohl
40	hear	lohe	nodəŋ	pʰətow-	nɪ:tone-	koʔm-
41	heart	puʔuwai	de	teh	te:	čekʰs



42	horn	kiwi	deškən	eskən	nimis	sʔečph
43	hot	wela	gʒizud	kɪsi-	wotéʔ	taʔanoy-
44	human	ʔ	nɪsna	inɪnrw	ʔ	ʔo:l
45	I	au	nɪn	nɪyə	nino:	nek
46	kill	pepehi a make	nɪrwed	nɪpeh-	noh-	sɪmɪt
47	knee	kuli	ŋgəɔdɔg	čikwən	čeʔiteyéi	ʔrkɪ
48	know	ʔike	gkedəŋ	kɪskeyim-	he:ʔino-	kom-
49	leaf	lau	nibiš	ni:pi	bi:či:s	ka:p'
50	lie	moe	ggiwid	pɪmi-	séʔis-	kmoyl
51	liver	ake	kun	oskun	his	wɪlkun
52	long	loa	gnag	kɪnos-	he:yo:ʔ	noʔom-
53	louse	ʔuku	koʒis	hɪkwe	tei	mohkoh
54	man	kane	nɪnɪ	napeo	hinen	pegrk
55	many	nui	nɪbwə	mičat	ʔ	ten-
56	moon	mahina	gɪzɪs	-pɪsɪm	bi:kóusɪ:s	wonewsleg
57	mountain	mauna	kuʒɪgən	wəči-	hóheʔ	mɪk*ʔɪ
58	mouth	waha	do	tun	ti:	luɪ
59	name	inoa	nazwɪn	ɪssɪnɪka-	ni:sihʔɪt	hew
60	neck	ʔaaʔii	kwegən	kwəyaw	sonon	pahtun
61	new	hou	ški	oski-	wono:méih-	čaʔanar
62	night	poo	ɔbɪk-	tɪpɪsk-	téčeʔ	nahsčewen
63	nose	ihu	jaž	kut	iʔis	hɪpɪʔn
64	not	ʔaʔole	ga	nəmə	ho:wūni	mos
65	one	ʔekaahi	bežɪg	peyək	če:sey	koh-
66	path	ala	mɪknas	meskənaw	bóo	la:yeɔ*
67	rain [nn]	ua	zɪg	kɪmiwən	o'só	ten-
68	red	ʔula	msku-	mʰiko-	be:é	ɪrkɪy-
69	root	aʔa	jɪbɪk	očepɪk	θeiciʔ	wɪʔɪpɪrk
70	round	poepoe	wawye	wawɪye-	če:teyó:ʔ	ɪɪhɪpɪh
71	sand	one	negəw	yekaw	né:í	čaɪ
72	say	ʔoolelo	kɪdud	əyəm-	nɪhɪ-	nahč-
73	see	ʔike	wabɪd	wapəm-	nono:ho-	new-
74	seed	ʔanoʔano	mika	kɪstɪkan-	θó:xú:	holéɪ
75	sit	noho	nɪmɔɔbɪd	əp-	čénók-	ček-
76	skin	ʔili	nəgʔəy	əsəkəy	nóx	wɪskun
77	sleep	moe	nbad	nɪp-	no:kohu-	čkey
78	small	iki	bbiwag	əpɪsɪs-	čes-	čeyk-
79	smoke	uahi	bkwene	pʰəst-	čét:té:	mera:
80	stand	ku	nɪnɪbwɪd	n:pow-	θɪ:ʔo:ku:	ko:ʔ-
81	star	hookuu	nəŋ	ecəkus	hóθoʔ	hogeč
82	stone	poohaku	si	əstɪni	hoʔnó:ke-	haʔa:g
83	sun	laa	gɪzɪs	-pɪsɪm	hi:sɪ:s	kečoyɪn hego:
84	swim	ʔau	bgɪzod	pɪmatak-	wóuwú-	kepoyur
85	tail	huelo	zow	mɪsəy	tɪhi	wɪɪry

86	that (nt.)	keelaa	ow	owə	núhu?	wek
87	this (nt.)	keeia	iw	ənə	híne'	wek
88	tongue	alelo	denniw	teyoni	iθon	hipl
89	tooth	niho	ibid	pít	ičíθ	rpeł
90	tree	laa?au	mtig	hatrk	hoho:t	tepo:
91	two	lua	niž	ni:su	ni:s	na?-
92	walk	hele waawae	sed	pim <sup>o</sup> t-	čebis-	heg-
93	water	wai	nbi	nipi	neč	pa?ah
94	we	maakou	ninwi	kiyan-	no?	nekah
95	what	aha	wag <sup>o</sup> nen	kekwan	hitóu	ti?n
96	white	ke?oke?o	wab	wap-	nóno:k	munče-
97	who	wai	wene	owenə	héne:?	ti?now
98	woman	wahine	kwe	iskweo	hisei	wenčok*s
99	yellow	melemele	zaw	osaw-	ní:ho:	ti?mpel-
100	you (sg.)	?oe	gin	kiyə	nin	ke?l

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### Etymology of 'volga'

James Weller

#### 0. INTRODUCTION

The Volga river area in the northwest of Russia has been inhabited by different groups of people throughout the millennia. Finno-Ugric tribes were among the earliest to settle the region stretching from the Volga to the Oka River before the first millennium BCE. By the beginning of the first millennium BCE the Finno-Ugrians had come into contact with East Balts who spread out thinly beyond the upper reaches of the Volga and more heavily into the Volga-Oka interriver region. The next arrivals were the Slavs (chiefly the Krivichians) who began settling the upper reaches of the Volga in the eighth century CE. (Tret'jakov 1966: 286, 297) In the latter half of the eighth century CE Scandinavians extended their presence from Lake Ladoga as far south as the Volga-Oka interriver region along what would come to be known as the Baltic-Volgaic Route<sup>1</sup>, an important route in the silver trade. (Nosov 1992: 103) By the advent of written records, the name of this important waterway had come to be 'Volga'.

#### 0.1 DISPUTED ETYMOLOGY OF 'VOLGA'

As is often the case with the names of places inhabited by successive waves of people, the etymology of the name Volga is disputed. Most linguists point to four possible sources: Slavic \*vŭlga 'moist, wet' (Vasmer 1986: vol. 1, 337), East Baltic \*ilga 'long' (Gimbutas 1963: 33), Volga-Finnic \*jŭly 'river' (Mikkola 1929: 27), and Baltic Finnic \*valga 'white'. (Preobrazhenskij 1959:91) Although in general great care has been taken to show how, via certain sound changes, the name Volga derives from a given source, I found that most of the etymologies operate without sufficient consideration for

<sup>1</sup> I take responsibility for translating 'Baltijsko-volzhsij put' as 'The Baltic-Volgaic Route.'

the *chronology* of the sound changes or their *conditioning environments*. In two instances I noted a reliance on sporadic sound changes and unexpected reflexes.

## 0.2 AN APPROXIMATE CHRONOLOGY OF THE RELEVANT SLAVIC SOUND CHANGES AND HISTORICAL EVENTS

Prompted by these observations, I decided to test what has been put forth in each etymology against the known chronology of the sound changes and their conditioning environments. To this end I compiled an approximate chronology of 15 sound changes and historical events based chiefly on Shevelov (1964) and Tret'jakov (1966).

	<u>Sound Change/Historical Event</u>	<u>Approximate Date</u>
1.	Finno-Ugrians settle Volga region	Before 1 <sup>st</sup> millenium BCE
2.	Initial Baltic-Finnic contacts in Volga Region	By start of 1 <sup>st</sup> millenium BCE
3.	Merger of <i>o&gt;a&lt;a</i>	6 <sup>th</sup> -5 <sup>th</sup> century BCE
4.	Prothesis of <i>j</i> and <i>v</i>	1 <sup>st</sup> -5 <sup>th</sup> century CE
5.	First delabialization <i>jũ&gt;jĩ</i>	6 <sup>th</sup> -7 <sup>th</sup> century CE
6.	Initial Slavic-Baltic Contacts	6 <sup>th</sup> -8 <sup>th</sup> century CE
7.	Initial Slavic-Finnic Contacts	7 <sup>th</sup> , 8 <sup>th</sup> century CE
8.	Rise of Yers ( <i>CũC</i> , <i>CĩC</i> preserved, where C=consonant, l=r,l)	Early 9 <sup>th</sup> century CE
9.	Merger of <i>CũC/CĩC</i> in the type <i>C̣lC</i>	Early 9 <sup>th</sup> to 10 <sup>th</sup> century CE
10.	Rise of <i>o</i> from Short <i>ã</i>	Middle of 9 <sup>th</sup> century CE
11.	Pleophony	mid-9 <sup>th</sup> century CE
12.	Dissolution of Slavic Linguistic Unity	mid-9 <sup>th</sup> century CE
13.	Svarabhakti	ca. Late 9 <sup>th</sup> century CE
14.	Second Pleophony	No sooner than 9 <sup>th</sup> century to before the loss of the Yers
15.	Loss of Yers	Mid-12 <sup>th</sup> -mid-13 <sup>th</sup> century CE

I will evaluate the four cited etymologies against these criteria using, when applicable, historical and archeological evidence that may be relevant. My purpose is neither to promote nor disprove a given etymology, but rather to evaluate the likelihood of what has been put forth.

### 1.0 ETYMOLOGY 1: SLAVIC \*v̥lga

Vasmer believed that the origin of the name Volga could be determined from the presence of the word-internal tautosyllabic segment *-ol-*. (Vasmer 1986: vol.1 337) Because pleophony had resulted in the change *ColC>ColoC* (or *CalC>CalaC* which later becomes *Coloc*), the name Volga could not have originally contained the sequence *-ol-*. Its presence in the modern word indicates that just prior to pleophony, the name Volga

must have been \*vǫlga (CǫC>CoIC). Furthermore, because the merger of CǫC and CǫC in the shape of CǫC (where the initial consonant was not an alveopalatal fricative) was critically ordered in a feeding relationship with the change of CǫC>CoIC, Vasmer viewed it as possible to reconstruct the earlier stage \*vǫlga.

### 1.1 INTERNAL EVIDENCE FOR SLAVIC \*vǫlga

The internal reconstruction \*vǫlga has supporting evidence from within Russian. If the \*vǫlga etymology correct, then the most likely word root would be \*vǫlg- (parsing the word otherwise would result in an altogether unknown Slavic root). This root \*vǫlg- resembles the Old Russian word *vǫlg-ъкъ* 'humid, damp.' This root *vǫlg-* (which itself is reconstructed as \*vǫlg-) is the undisputed zero grade of the word *volǫga* (<pre-pleophonic \*volga or \*vǫlga) 'liquid fat used as a seasoning' and the Church Slavic borrowing *vǫlga* 'moisture, liquid' which assumed the original meaning of *volǫga*.

A second derivational relationship which argues strongly in favor of the \*vǫlga reconstruction is the term *vǫlozhka* 'arm of the Volga formed usually after a flood'. (Vasmer 1986: vol.1 337) Because the North Russian sound change of second pleophony resulted in the duplication of a yer vowel after the sonant when in tautosyllabic position (CǫC>CǫC), the etymology of *vǫlozhka* is believed to be the selfsame \*vǫlg- (>\*vǫlg->\*vǫlg->volog-). The occurrence of the root *volog-* in *vǫlozhka* and in other North Russian words of the meaning 'moist' or 'liquid' suggests that the root of Volga could well be the Russian \*vǫlga.

### 1.2 ADDITIONAL EVIDENCE FOR SLAVIC \*vǫlga

Additional evidence for the Slavic \*vǫlga etymology is found in Slavic and in Baltic. In Polish the cognate form of the zero-grade root \*vǫlg- is *wilgi* 'damp'. In Lithuanian the cognate root is *vilg-* as in the word *vilgyti* 'to moisten'. The presence of these forms strongly suggests that the root \*vǫlg- was indeed native to Slavic as well as Baltic.

### 1.3 THE VOLGA AS THE 'WET' RIVER?

If the Slavic root \*vǫlg- is the correct etymology of Volga, then the name Volga means essentially 'the wet' or 'the moist' river. That the Volga is laden with water, more so than any other river in the northwest region of Russia (or in all of Europe for that matter) is beyond dispute. The Volga is fed by melting snow in the spring and considerable (though varying amounts of) rain in the summer. (Bol'shaja sovietskaja encyklopedija 1958: 604)

The question that arises is whether the Slavs would have named a river 'wet' or 'moist'. Dr. Daniel Collins has suggested (personal communication) that not far to the north of the Volga flows another major river, the Suhona, the etymology of which contains \*suh-, the Slavic root meaning 'dry.' Like the Volga, the Suhona is an important waterway, but unlike the Volga, navigation is interrupted in the summer due to shallow

depths in certain areas. (BSE 1958: 333) Perhaps for this reason the river was named 'dry'. Consequently, if a river could be named 'dry' for its shallow levels in the summertime, it is not impossible that a river allowing for uninterrupted summertime navigation was named 'wet' (i.e., the Volga).

#### 1.4 SLAVIC \*vīlga AND CHRONOLOGY AND CONDITIONING ENVIRONMENTS

Vasmer's etymology passes through two stages: the merger of CǫC and CīC in the form CǫC (\*vīlga>\*vǫlga); and the yer shift (\*vǫlga>Volga). His etymology is therefore wholly unproblematic as the two sound changes he employed were indeed ordered in a feeding relationship. The approximate dates of the first sound change involved, early 9<sup>th</sup>-10<sup>th</sup> century CE, accord with the growing Krivichian presence in the upper reaches of the Volga (eighth century CE and onward) and the subsequent Slavic expansion into the Volga-Oka interriver region. Therefore, it is very possible that as the Slavs settled the Volga river region, initially in areas only sparsely populated, they gave the river a Slavic name.

#### 2.0 ETYMOLOGY 2: EAST BALTIC \*ilga

Although the Slavic etymology seems compelling, some linguists argue that the name Volga must be of Baltic origin, because pockets of East Balts were known to have inhabited the upper reaches of the Volga, the very area that was later settled by the Krivichians. (Tret'jakov 1966: 286) According to this theory, \*vīlga is taken to be the correct etymology, but at a later stage. It is argued that the East Baltic word \*ilg- 'long' was given to the river because of its enormous length. (Gimbutas 1963: 33) According to this theory, when Slavic settlers finally arrived in the upper reaches of the river and encountered Baltic elements there, they borrowed a Baltic name for the river as \*īlga.

Proponents of the Baltic etymology then point to the well-known Slavic sound change of glide prothesis. Accordingly, it is argued that \*īlga attracted the labial glide w- (which later became v-) for the form \*vīlga. From this point on \*vīlga passes through the stages outlined in the Slavic etymology: \*vīlga>\*vǫlga and \*vǫlga>Volga.

#### 2.1 GLIDE PROTHESIS

Slavic and Baltic contacts took place in the northern region between the sixth and eighth centuries CE. (Tret'jakov 1966: 296) Glide prothesis in Slavic is believed to have taken place in four stages, the first (and for our purposes most applicable) of which occurred between the first and fifth centuries CE. (Shevelov 1964: 246) It was at this time that word-initial *i-* and *u-* attracted the glides *j-* and *w-* (>*v-*) respectively. Glide prothesis then continued in three subsequent stages over the next four hundred years, including the period of Slavic-Baltic contacts. (Shevelov 1964: 246) Nevertheless, the conditioning environments for all stages of prothesis can be summed up in the following manner: a front vowel (*i* or *e*) attracted the palatal glide *j-*, whereas a back vowel (*u* or *o*) attracted the labial glide *w-* (which later became *v-*). (Townsend and Janda 1996: 68)

Consequently, although the chronology of the Baltic etymology is possible (Slavic-Baltic contacts coincide with subsequent stages of glide prothesis), the choice of prothetic element (\*ɪlga > \*vɪlga) violates the conditioning environment for prothesis: *v*-prothesis before a front vowel is unknown in Slavic.<sup>2</sup> The expected result of glide prothesis would be \*ɪlga > \*jɪlga, and \*jɪlga, not meeting the qualifications for the subsequent merger of CɪC and CjC in the form of CɹC, would have become \*ɪlga. (Shevelov 1964: 467-8)

## 2.2 EAST BALTIC \*ilga AND CHRONOLOGY AND CONDITIONING ENVIRONMENTS

The time frame for \*ilga to have been borrowed (6<sup>th</sup>-8<sup>th</sup> centuries CE) presents no problem. But as I stated above, the Baltic etymology violates the known conditioning environment for glide prothesis. One alternative put forth by some linguists is that the Baltic root in question possessed a word-initial *j*- which was subsequently lost in Baltic languages. (Gimbutas 1963: 33) According to this etymology, the starting point would have been \*jɪlga. This, however, raises an equally problematic question: how did the glide *v*- come to replace *j*-? If \*jɪlga became \*vɪlga by a spontaneous glide-switch, it may be the sole occurrence of such a sporadic change in Russian.

Dr. Daniel Collins (personal communication), while himself not espousing the Baltic etymology, suggested that some may argue for the change \*ɪlga > \*vɪlga followed by *v*-prothesis for the form \*vūlga (>Volga). This possible 'way out' seems plausible at first glance, but it misses a crucial step. The argument starts from the premise that hard (velarized) *l* was responsible for the backing of *ɪ* to *ɤ*. The common textbook example is \*vɪlkū 'wolf' > vɫkɤ. Bearing this in mind, it would follow that there exists the possibility for \*ɪlga to have become \*ɤlga prior to receiving a prothetic element. When prothesis did finally occur, it involved the expected labial glide *w*- (\*wɤlga > \*vɤlga).

As I stated above, this second alternative is also problematic. For it to work, prothesis and backing need to have been ordered in a feeding relationship. Backing of short *ɪ* took place during the ninth and tenth centuries CE, whereas glide prothesis was an ongoing process during the entire period of Slavic-Baltic contacts (sixth to eighth centuries CE). Since Slavs reached the Volga by the eighth century CE, it stands to reason that a Baltic name would have been borrowed at that time. Consequently, at least with regards to Slavic-Baltic contacts, the two sound changes involved were ordered in a counter-feeding relationship. This means that prothesis would have been instantiated (\*ɪlga > \*jɪlga) prior to the ninth century change of CɪC > CɤC. Unless some other explanation can be found to reconcile the etymology of \*ilga to the conditioning environments for the changes it was a candidate for, its likelihood is significantly weakened.

<sup>2</sup> The glide *v*- did indeed occur before front vowels in Slavic, but for all such examples it can be demonstrated that the word-initial *v*- was etymologically present: Slavic \*vɪdova 'widow' vs. Old Indic vidhā:va; Slavic \*vɪlkū 'wolf' vs. Latv. vilks 'wolf', et al.

### 3.0 ETYMOLOGY 3: VOLGA-FINNIC \*jyl̥

Still other linguists feel that a Finnic source for the name Volga must exist inasmuch as the Finno-Ugrians had settled the region long before either the Balts or the Slavs. Mikkola sees in the Old Mari name for the Volga, \*Jyly 'river', the possible origin of the name Volga. (Vasmer 1986: vol.1 337) He proposes that the name for the river was \*jyl̥ in Volga-Finnic. As Slavs came into contact with Finno-Ugrians they borrowed the Finnic name mapping their short *ū* for the Finnic short *u* and a voiced velar stop *g* for the Volga-Finnic *γ* rendering the form \*jūlga.<sup>3</sup> He then posits a "glide switch" \*jūl̥ga>\*vūlga. (Mikkola 1929: 127) From here on out \*vūlga follows the same development as put forth in the Slavic and Baltic etymologies, \*vūlga>\*v̥lga> Volga.

#### 3.1 A POSSIBLE GLIDE SWITCH?

As I noted above in the case of Baltic \*jilga becoming \*v̥lga, such a glide switch would probably have been the only instance of this sporadic sound change. In the Baltic case, the switch was from the wholly acceptable Slavic sequence \*j̥l̥- to \*v̥l̥- without any strong motivation for it. In the Volga-Finnic case, however, the change involves modifying what had come to be an unacceptable sequence in Slavic \*jū- to the more acceptable sequence vū-.

Mikkola offers an innovative explanation. Prior to Slavic-Finnic contacts (seventh to eighth centuries CE), Slavic had delabialized *ū* and fronted it to *ɪ* when it occurred immediately after an alveopalatal consonant. (Mikkola 1929: 127) That this delabialization had been completed prior to Slavic-Finnic contacts is demonstrated by the Common Slavic word \*jūga 'yoke.' Prior to Slavic-Finnic contacts, the short *ū* was delabialized and fronted rendering \*j̥ɪga. It was borrowed into Finnish from the oblique cases (i.e. gen.sg. *ižese*) as *ies* (gen. *ikeen*). (Shevelov 1964: 267; Vasmer 1986: vol.2 115-6) Therefore, Mikkola was correct to point out that the sequence *jū-* would have become unnatural in Slavic just prior to the period of Slavic-Finnic contacts. (Mikkola 1929: 127)

The question that arises is how Slavic would have treated the foreign sequence #jū- after delabialization (and fronting) had removed the sequence from the native lexicon. Mikkola, of course, argues that Slavic would have replaced the glide *j-* with the labial glide for the acceptable sequence vū-. However, inasmuch as the change jū->j̥ɪ- was phonetically motivated (*ū*, a back vowel, assimilated to the place of articulation of *j-* thereby fronting and delabializing to become *ɪ*), I feel that the expected development would have been for \*jūlga to become \*j̥ɪlga. My conclusion is supported by the North Russian borrowing *ikumálka* 'drive used for fastening traces to the fore-part of Saami sleds'. The word derives from Finnish *jukko* 'yoke' (itself of Scandinavian origin) and

<sup>3</sup> Presumably the final -a would have been added by the Slavs by analogy to numerous other river names in the area that end in -a.

*malka* 'pole'.<sup>4</sup> (Vasmer 1986: vol.2 126) In this borrowing we see that the Finnish word-initial *ju-* must have undergone the development *\*ju->\*jī* and finally *\*jī-* became *i-* even *after* the sound change proper of delabialization had taken place.

### 3.2 VOLGA-FINNIC \*jūly AND CHRONOLOGY AND CONDITIONING ENVIRONMENTS

The etymology of the Volga-Finnic \*jūly involves a putative glide switch which would have occurred during the initial period of Slavic-Finnic contacts (eighth to ninth centuries ū). Although the time frame for the etymology is unproblematic, the result of \*vūlga from \*jūly is unexpected unless a sporadic change is posited for this single occurrence. As demonstrated with *ikumalka*, a word with the sequence #jū- could enter the language after delabialization had removed the sequence from native words. However, positing a glide switch rather than assimilation to the place of articulation presents an unnecessary complication (Occam's Razor). On this basis alone it would seem that the Volga-Finnic etymology \*jūly is unlikely.

### 4.0 ETYMOLOGY 4: BALTIC FINNIC \*valga

With the Slavic, Baltic, and Volga-Finnic etymologies aside, there remains the Baltic Finnic etymology of \*valga 'white'. The Russian scholar Preobrazhenskij is the most prominent of the supporters of this etymology. (Preobrazhenskij 1959: 91) Preobrazhenskij purports that the Baltic Finnic word *valkea* meaning 'white' was employed in naming the river. (Preobrazhenskij 1959:91) Schramm also sees in Volga the Finnic root *valg-* (Schramm 1973: 122) According to these scholars, the Finnic name was borrowed directly by the Slavs in the form of \*valga.

However, for this etymology to work, the form \*valga needs to have been borrowed *after* pleophony took place (mid-ninth century CE) but *prior* to the rise of *o* from short \*ā (also mid-ninth century CE). In this way \*valga would have become Volga. Because the window of opportunity for \*valga is so narrow (mid-ninth century), it is also possible that Volga passed through a stage of svarabhakti in the form \*voľ̥ga during the late ninth century. If so, it would not have been until after the yer shift that \*voľ̥ga would finally have reached its present form of Volga.

### 4.1 THE 'WHITE' RIVER?

One question that immediately arises is why the Volga would be called the 'white' river. This in and of itself is not problematic. There was a tendency among ancient cultures (the Finns, Slavs, and Balts included) to name geographical features in close proximity to each other in pairs of opposite colors. (Ludat 1953: 139) The color black was often applied to mark a northern geographical feature and white a western one. (Prištak 1954: 377) The Volga river stretches to the southwest from another river known

<sup>4</sup> The lack of the reflex of pleophony in the word *malka* and the apparent reflex of a back yer in *iku* suggest that this word was borrowed no sooner than the late ninth century CE but no later than the mid-twelfth century.

to have a Finnic name, the *Msta*. The name of this river derives from the Finnish word *musta* 'black'. Therefore, there is no immediate difficulty with the Volga being named 'white' in context of the 'black' river.

#### 4.2 CHALLENGES FACING THE BALTIC FINNIC ETYMOLOGY

Aside from the semantics of the name Volga, little else will prove unproblematic. Four challenges face this etymology: 1) the critical ordering of pleophony and the rise of *o* in a counter-feeding relationship; 2) the borrowing taking place in the mid-ninth century; 3) the proposition itself that a Baltic Finnic root is the source of the name Volga; and 4) the very shape of the root itself. Of these four concerns, two will prove particularly troublesome.

#### 4.3 ORDERING OF PLEOPHONY AND RISE OF SHORT *o*

There is an abundance of evidence to place both pleophony and the rise of *o* in the mid-ninth century. (Shevelov 1964: 384, 416) However, there may be only one form that could be used to order them relative to each other in the area of Slavic-Finnic contacts. The Karelian word *tarboin* 'stick for stirring' was borrowed into North Russian dialects as the verb *torbat* 'to drive fish with sticks' and into Old Russian in *torbnaja lovija* 'fishing with sticks.' (Shevelov 1964: 384; Vasmer 1986: vol.4 82) The Old Russian form shows the reflex of pleophony *torob-* which is inconclusive—whether pleophony came first (*\*tarb->\*tarab->torob-*), or the rise of *o* first (*\*tarb->\*torb->torob-*), or both of them simultaneously, the same form *torbnaja* results.

The North Russian form with the root *torb-*, however, might suggest that pleophony took place and was completed *prior* to the rise of short *o*. In this way the root *tarb-* would have escaped pleophony only to fall subject to the rise of *o*. Still another interpretation would be that the North Russian form *torbat* was reshaped from *\*torobat* by analogy to the Karelian form *tarboin* which continued to be in use among Karelians in the region. Equally valid is the possibility that pleophony and the rise of short *o* were spreading concomitantly, but that in some speech communities pleophony took place before the rise of short *o* while in other communities, the opposite occurred. Consequently, it is not possible to determine conclusively whether one change preceded the other but only that, in the very least, both changes took place at more or less the same time. The example of *torbat* at least allows for the argument that, in the case of \*volga, pleophony could have come first.

#### 4.4 A NINTH CENTURY BORROWING?

Slavic settlers established themselves in the upper-reaches of the Volga by the eighth century. (Nosov 1992: 102) By the latter half of the eighth century, the trade route known as the Baltic-Volgaic Route was in use not only for the transport of silver to Rus', but for the proliferation of Scandinavian influence. (Nosov 1992: 102, 103) The chief participants in trade with the Scandinavians (as well as conflict) were the Slavic Krivichians and Slovianians and the Finnic Merjas. (Nosov 1992: 102) The Baltic-



Volgaic Route connected the Neva, Volxov, Lake Il'men', Pola, Msta, the upper-Volga and the middle-Volga. (Nosov 1992: 102) Therefore, it stands to reason that the name for the Volga river would have become established by that time (eighth century CE). Consequently, to posit that \*valga is the source of the name Volga obligates one to believe that Slavic speakers borrowed a Baltic Finnic name for the river decades into conducting international trade along it. Although not impossible, this scenario strikes me as rather unlikely. I find it more plausible that the name \*valga would have been borrowed in the mid- or late eighth century, early into the establishment of the trade route.

#### 4.5 THE LACK OF PLEOPHONY

If international trade being conducted from the Baltic Sea to the Volga river is grounds for assuming that the name Volga would have been borrowed by the latter part of the eighth century, then the immediate difficulty facing the Baltic Finnic etymology is its failure to show the reflex for pleophony (mid-ninth century). According to this sound change, the vowel *a* (or *o*) or *e* immediately preceding tautosyllabic *r* or *l* was duplicated after the sonant. Therefore, just as Slavic \*galva: 'head' became Russian *golová*, and \*vertena became *veretenó*, one would expect that \*valga would have yielded \*vologa. (Vasmer 1986: vol.1 337)

#### 4.6 A BALTIC FINNIC NAME FOR THE VOLGA?

Perhaps a more critical issue facing the \*valga etymology is the proposed root itself. That a Volga-Finnic name would be proposed as the source of the name Volga seems only natural. The question that arises is whether the Volga could have a *Baltic* Finnic name derived from the Finnish *valkea*. The Finnic peoples of the Volga region most likely to have had initial contact with the Slavs were the Merja, a people whose exact identity is unknown. (Tret'jakov 1966: 287) However, adjacent to the Merja lived the Baltic Finnic Veps. (Strumin'skij 1996: 272)

Since no records of the Merja language exist to establish whether they spoke a Volga-Finnic or Baltic Finnic language, any classification of the Merja language would be speculative. However, Bohdan Strumin'ski's analysis of Merja loanwords in Russian suggests that Merja had a decidedly Baltic Finnic affinity. (Strumin'skij 1996: 272-86) When placed into the context of the Merja's close proximity to the Baltic Finnic Veps, it does seem possible that the Merja were part of an early continuum linking Baltic Finnic with Volga-Finnic. (Strumin'skij 1996: 272)

If we then assume a Baltic Finnic presence along the northern Volga (possibly via the Merja), it would follow that a Baltic Finnic name could have been given to the river. As stated earlier, Preobrazhenskij proposes that Russian borrowed the root \*valg- 'white' from Finnish *valkea*. (Preobrazhenskij 1959: 91) However, the Finnish adjectival ending *-ea* derives from an earlier *-eä*. (Institut Jazyka, Literatry i Istorii 1955: 115-6) It would then follow that the Baltic Finnic root in question would be not \*valga but rather \*valgeä, at least as far as Finnish is concerned. The comparative method can be

employed to help establish what the root for 'white' might have been in the Baltic Finnic languages of the region. I have included Mordvin and Mari, Volga-Finnic languages, inasmuch as any Baltic Finnic contingent in the region was likely part of a continuum with Volga-Finnic.

Finnish	valkea<* walkeða	
Estonian	walge	
Livonian	va:lda	
Veps	våged	
Mordvin	valda	(Volga-Finnic)
Mari	valgede	(Volga-Finnic)

On the basis of these attested forms, it seems rather safe to conclude that the root in question would have been \*valgVða. Therefore, one would expect the name of the Volga to have been \*Vologda, \*Volgda, \*Vologeda, or \*Volgeda depending on when it was borrowed, that is, depending on whether pleophony preceded the rise of short *o* and whether a vowel separated the velar from the dental stop.

#### 4.7 SUPPORTING EVIDENCE FOR THE \*valgVða RECONSTRUCTION

The possibility remains for Slavs to have truncated \*valgeða to \*valga, or for the Finnic peoples to have done so themselves prior to the arrival of the Slavs. After all, the earliest attestations of Finnish already show the loss of *ð* (Itkonen, Joki, and Peltola 1975: vol.5 1620) However, evidence suggesting that \*valgeða or \*valgVða was in use in its full form during initial Slavic and Finnic contacts comes from the North Russian river Vologda. The name of this river was borrowed from the Baltic Finnic word for 'white' and it shows both *-d-* in the suffix and the expected reflex of pleophony (*-olo-*). This form casts serious doubt on the possibility of Volga deriving from the truncated form \*valga.

Further supporting evidence is found in a Baltic Finnic toponym northwest of the Volga. Baltic Finns came into contact with the Komi-Permians in the tenth and eleventh centuries CE. (Savel'jeva 1992: 129) One of the toponyms attributed to Baltic Finnic peoples is the name of the right tributary of the Northern Dvina, the Vycheгда.<sup>5</sup> (Savel'jeva 1992: 129) Although the exact meaning of this river is unclear, its shape (*-gda*) recalls that of Vologda. One characteristic of Baltic Finnic adjectives bearing the suffix *-ða* is a preponderance for velar stems. (Institut Jazyka, Literaturny i Istorii 1955:116) Vycheгда shows both the *d* and the velar that are typical of a class of Baltic Finnic adjectives. Therefore, on the basis of Vologda and Vycheгда it follows that if the

<sup>5</sup> Room proposes that the etymology of Vycheгда is the old Mansi *vich* 'damp meadow' and *jegda* 'river.' If so, that would not diminish from the fact that Slavs had no problem incorporating the foreign sequence *-gda* into their language at least so far as hydronyms are concerned.

name of the Volga did indeed derive from the Baltic Finnic word for white, \*valga is not the correct reconstruction.<sup>6</sup>

#### 4.8 BALTIC FINNIC \*valga AND CHRONOLOGY AND CONDITIONING ENVIRONMENT

As with the Volga-Finnic etymology, it seems only logical that the name of Volga would go back to a Finnic source: Finno-Ugrians inhabited the Volga region long before the arrival of the Balts and the Slavs. Nevertheless, the \*valga etymology falls short on two critical points: 1) it fails to demonstrate the reflex for an important sound change it was a clear candidate for (pleophony), because the name would likely have been borrowed well before pleophony took place. Furthermore, the comparative method raises serious doubts as to whether the proper root was reconstructed in the first place. For \*valga to work, one must posit that Slavs borrowed the name decades into conducting important trade along the river and that, in this one instance, they trimmed *-eda* off of the Finnic name. Therefore I conclude that the Baltic Finnic etymology \*valga is unlikely.

#### 5.0 CONCLUSION

After examining the four most accepted etymologies put forth for the Volga (Slavic \*vŭlga, East Baltic \*ilga, Volga-Finnic \*jŭly, and Baltic Finnic \*valga) against the sound changes they involve, I find that only one etymology is likely: Slavic \*vŭlga 'wet' or 'moist.' This etymology shows the expected reflexes for the sound changes it was a candidate for and its meaning is understandable in terms of the favorable water levels present throughout the summer months. Furthermore, historical evidence may give a clue as to why this river was given a Slavic name--despite the heavy presence of Finno-Ugrians in the mid-Volga region and some pockets of Balts towards the northern reaches of the river, Slavs established large settlements in the rather unpopulated region of the river: the source.

The remaining etymologies violate the conditioning environments for the sound changes they require and/or fail to show reflexes for changes for which they were clear candidates. The lesson is clear: deriving etymologies requires a careful study of the facts in order to determine what is possible and what is not. Attention must be given to the chronology of the sound changes, their exact conditioning environments, and the history and archeology of the region if plausible conclusions are to be drawn.

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<sup>6</sup> The truncated root *valk-* is found in several Finnish words: *valkaista* 'to bleach', *valkoinen* 'white', *valko-* 'white' in compound words, et al. However, I consider it risky to cite such forms as a source of the name of the Volga. To do so would be to posit that Slavs extracted the root *valko-* and disregarded the rest of the name in this one instance while clearly not in others.



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Schwyzerdütsch: Ein Überblick über die Grammatik

Grammatik

Verben

1.1

1.1.1

Schwyzerdütsch: Ein Überblick über die Grammatik

The following section discusses the use of the verb "go" in the Schwyzerdütsch dialect. It is noted that the verb "go" is used in a variety of ways, and that the short form "go" is used in a variety of contexts. The following examples illustrate the use of the verb "go" in the Schwyzerdütsch dialect.

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### Short-form "Doubling Verbs" in Schwyzerdütsch

Charlotte Christ Schaengold

The following section discusses the use of the verb "go" in the Schwyzerdütsch dialect. It is noted that the verb "go" is used in a variety of ways, and that the short form "go" is used in a variety of contexts. The following examples illustrate the use of the verb "go" in the Schwyzerdütsch dialect.

Certain verbs in Schwyzerdütsch appear twice in a sentence without a corresponding repetition of meaning (Nübling 1995:173). This paper explores some possible explanations of this "doubling verb" phenomenon, using the current literature on the subject and interviews with native speakers.<sup>1</sup>

The first appearance of the "doubled" verb in a sentence is an inflected form of the verb, and the second appearance is a short form CV stem. The sentences below illustrate this phenomenon; the short forms are in bold. In the first example "go" is the short form and "gang" is the inflected 1st person form of the verb "goo" (*gehen* in Modern High German) 'go.' There are four verbs in Schwyzerdütsch which can be "doubled" in this way, with each "doubled" verb showing different patterns of usage and frequency of use; there are also historical differences and differences in use among the different regional dialects of the country. e.g.:

1. i gang **go** schwimme 'I'm going swimming'  
I go(1st sg) go(stem) swim(1st sg)
2. es kunnt **ko** räagne 'it's about to rain'  
it come(3rd sg) come(stem) rain(1st sg)
3. si foot a **fo** schaffe 'she's beginning to work'

<sup>1</sup> Native-speaking experts: Jakob Christ, age 70; Rike Christ-Kutter, age 63; Max Reif, age 45; Christa Baltzer, age 35; Daniel Sachs, age 27; Rosina Christ, age 20.



4. er loot      lo      grüesse      'he sends greetings'  
 he let(3rd sg) let(stem) greet(Inf)

The histories of the "doubled" verbs are diverse: while the doubled use of "go" is found already in Middle High German literature and is used in modern times all over German-speaking Switzerland, as well as in parts of Southern Germany, the doubled use of "afo" and "lo" is found only in certain Swiss dialects. The doubled use of "ko" is harder to trace, as in several dialects it is homophonous with "go."

All four of these "doubling" verbs undergo the same phonological reduction in the unstressed position before the infinitive main verb, losing length and individual word accent. In this process the meaning is also reduced, necessitating another token of the same verb to produce the desired meaning. Thus, it is the first, inflected form of the verb which is the repeated or "doubled" form, not the second, short stem form. Even so this is not really a productive operation with predictable results, and the grammatical result of each "doubled" form is different.

In Schwyzerdütsch as in Modern High German there are weak (regular) verbs, strong (irregular) verbs, and modals. The short form verbs represent another class of verbs in Schwyzerdütsch. In Modern High German the weak and strong verbs are conjugated differently, but in Schwyzerdütsch the weak and strong verbs are conjugated similarly; the real differences in the conjugations are between the set of strong and weak verbs and the set of modals and the short-form verbs. The indicative is formed in the weak and strong verbs by adding the personal endings to the verb stem; in the short form and modal verbs it is formed with a vowel change in the plural in addition to the personal endings. The conditional is formed in the weak and strong verbs with the addition of [t] and the conditional person endings to the verb stem; in the short form and modal verbs the stem also undergoes vowel change, see following charts adapted from Suter 1976:108.

**weak verb 'to seek'**

*infinitive:* sueche *imperative:* suech!  
*past participle:* gsuecht

	<u>present indicative</u>	<u>conditional</u>
i	suech	i suechti
de	suechsch	de suechtisch
er	suecht	er suechti
mer	sueche	mer suechte
er	sueche	er suechte
si, Si	sueche	si, Sie suechte

**short form verb 'to go'**

**strong verb 'to sing'**

*infinitive:* singe *imperative:* sing!  
*past participle:* gsunge

	<u>present indicative</u>	<u>conditional</u>
i	singe	i singti
de	singsch	de singtisch
er	singt	er singti
mer	singe	mer singte
er	singe	er singte
si, Si	singe	si, Si singte

**modal verb 'to want'**

*infinitive:* goo *imperative:* gang!  
*past participle:* gange

*infinitive:* welle  
*past participle:* welle

<u>present indicative</u>	<u>conditional</u>	<u>present indicative</u>	<u>conditional</u>
i gang	i giengti	i wott	i wetti
de goosch	de giengtisch	de woosch	de wettisch
er goot	er giengti	er wott	er wetti
mer geen	mer giengte	mer wään	mer wette
er geen	er gientet	er wään	er wettet
si, Si geen	si, Si giengte	si, Si wään	si, Si wette

There are some other syntactic differences between Modern High German and Schwyzerdütsch; one of them is the order in which adjacent verbs appear in a sentence, here shown with parallel modal constructions in the two languages. Whereas in Modern High German the modal appears after the complement verb in a dependent clause, in Schwyzerdütsch the modal (in bold) verb appears after the meaning-bearing verb.e.g.:

5. "I haa nit **kenne** schlooffe" (Ich habe nicht schlafen **können**) 'I couldn't sleep' (1976:191)<sup>2</sup>
6. "Mer hänn **deerfe** zueluege" (Wir haben zuschauen **dürfen**) 'We were allowed to watch' (Suter 1976:105)

Also similar to the modals, in some adjacent verb pairs in Schwyzerdütsch, no "zu" is needed for the verbal complement, though in the Modern High German sentences the "zu" may be obligatory. e.g.:

7. "S foot aa räagne" (es fängt an zu regnen) 'it's starting to rain' (Suter 1976:149)
8. "Heer uff dumm schwätze!" (hör auf, dumm zu schwatzen!) 'stop talking dumb!' (pg. 149)

Schwyzerdütsch also does not use the German um...zu construction for purpose clauses. The short-form "go" can be used instead of "zu" when the explicit meaning 'go' is not available, though there seems to be some persistence of meaning even with this usage; there is an implied "going" in the verb 'to visit' in the following sentence:

9. S isch uns koo go bsuche (sie ist uns besuchen gekommen) 'she has come to visit us'

Some specific form of the verb 'to go' has acquired a grammatical meaning in many languages. In Swiss-German the verbal particle "go" indicates immediate future, similar to *gonna* in English and to forms of "aller" in the *futur proche* in French. This is the verb found "doubled" not only in Swiss-German but also in Southern German dialects. When it is used thus as a grammatical element it does not require the use of the inflected form of the same verb, but can also be used with other verbs. e.g.:

<sup>2</sup> The orthography of Schwyzerdütsch and the sentences in quotes are from Suter's 1976 *Baseldeutsch-Grammatik*. Modern High German translations are in parentheses, English glosses are in single quotes.

10. "Si isch uff Floränz graist go di naischte Modäll bschaue" (sie ist nach Florenz gereist, um die neuste Modell zu beschauen) 'she has traveled to Florence to look at the latest fashions' (184)

It seems that this "doubling" structure may be spreading from "go" to other verbs within the language; at the same time these short verb-stem forms are acquiring grammatical meaning. Nübling notes that short forms of "goo" are among the first attested forms of this type. (Nübling pg.167) The verb "go" has been used with doubled forms at least since the 16th century in German. e.g.:

11. (Lötscher 1993: 183) "Ich gan weder gan bredigen noch gan taufen" 'I go neither to preach nor to baptize' (literally "I go, neither to go preach nor to go baptize")

The process of change from an inflected "regular" use of these verbs can be observed in their varied usage; while the use of the short form "go" requires the inflected form of the same verb to get the actual meaning 'go' in a sentence, the rules regarding the other three verbs in this set are not as clear. As the verb 'go' is used as an immediate future or *futur proche*, "ko" is used to mark inchoative aspect. The verb "afo" is developing an inceptive aspectual meaning, and "lo" is used to mark a change in voice, from active to passive.

The "ko" form is not used in as many dialects and its use has not been attested for as long a period of time as "go." When it is "doubled" it must be used with the inflected form of the same verb, "koo." The doubled use of this verb is not obligatory except in cases of impending weather. e.g.:

12. si kunnt uns ko bsueche ~ si kunnt uns bsueche 'she's coming to visit us'  
 13. es kunnt ko räagne \*Es kunnt räagne 'it's about to rain'

The doubled use of "anfangen" is not obligatory in either direction: both the inflected form and the short form may be used without the other in a sentence.

14. si foot aa schaffe ~ si foot afo schaffe (sie fängt an zu schaffen) 'she is beginning to work'  
 15. "Er wott afo studiere" (Er will zu studieren anfangen) 'He wants to begin to study' (150)

There has been a reanalysis of verbs used together with "lassen," to mean "get VERBed." e.g.: "loreinige" 'get X cleaned'; "loflicke" 'get X repaired.' The "thing" denoted by X may not appear between the prefixed "lo" short form and the verb. These verbs do not require "lassen" as the inflected verb in the sentence, and the inflected forms of "lassen" and the short form "lo" are not immediately perceived to be parts of the same verb paradigm. In the Schwyzerdütsch spoken in Basel when "lassen" denotes 'leave X to be...(cleaned, fixed, etc.)' sentences with only the inflected form of the verb "lassen"



are ungrammatical. In the Schwyzerdütsch spoken in Zürich as in Modern High German these sentences are grammatical and have the same meaning as the present tense sentences with the doubled use of the verb, e.g.:

16. er loot di schue flicke ~ er loot di schue lo flicke 'he leaves the shoes to be fixed'

In Basel and other parts of Switzerland the doubled form is required in the present tense; to get the desired meaning there must be some inflected verb and the short form element "lo." e.g.:

17. \*si loot di hosen reinige \*'she lets the pants clean'

18. \*er loot di schue flicke \*'he lets the shoes fix'

19. a. er hat di hosen lo reinige 'he has left the pants to be cleaned'  
 b. er wird di hosen lo reinige 'he will leave the pants to be cleaned'  
 c. er duet di hosen lo reinige 'he is leaving the pants to be cleaned'

20. a. si hat di hosen lo flicke 'she has left the pants to be fixed'  
 b. si wird di hosen lo flicke 'she will leave the pants to be fixed'  
 c. si duet di hosen lo flicke 'she is leaving the pants to be fixed'

When greetings are "left" the analysis is ambiguous; it is not clear if the doubling is not obligatory, or if in one case the substantive 'greetings' are meant and in the other case the verb 'greet' is being used, as the verb and noun in this case are homophonous.

21. er loot grüesse ~ er loot lo grüesse (er lässt grüssen) 'he leaves (sends) greetings'<sup>3</sup>

The historical facts about the language offer an explanation for these constructions. In Middle High German there were two competing forms of many verbs, a long form and a contracted form, e.g.: *la:zen* ~ *la:n* 'let'; *haben* ~ *ha:n* 'have'; *slahen* ~ *sla:n* 'hit' (Nübling 1995:169). The present-day conjugations of these verbs in Swiss-German are formed on the pattern of the short forms, and the Modern High German conjugations are patterned after the long form conjugations. The infinitives and past participles of the short-form verbs in Swiss-German are monosyllabic and end with a long vowel.

In the time of Middle High German there is evidence of some variation in word order within the sentence; modals and auxiliaries could appear before or after the main complement verb in a dependent clause. Swiss-German now uses the pattern modal-main

<sup>3</sup> Changing the word order to try to find a final -n in the infinitive does not provide a solution, as it would in Modern High German, as in Schwyzerdütsch there is an epenthetic -n between vowels regardless of the part of speech, e.g.:

i. I due glette (Ich tue/mache glätten) 'I am ironing'

ii. Das duen I nit (Das mache ich nicht) 'That I don't do'

verb in this environment, and Modern High German uses the pattern main verb-modal

e.g.:

22. a. (from the *Nibelungenlied*) "Ir sult wol lazen schouen..." 'you should show...'  
(modal-verb)  
b. (Paul 1953:147 MHD) "...sünde hat begangen" '...had committed sins'  
(aux-verb)  
c. (Paul 1953:147 MHD) "...er uns hat geschaffen" '...he has made us'  
(aux-verb)  
d. (Paul 1953:143 MHD) "...er hat sich leben lazen" '...he let himself live'  
(verb-modal)

The main meaning-bearing verbs are those following the modals and auxiliaries in Swiss-German; for that reason they receive a heavier accent. In contrast to the main verbs, the modals and auxiliaries are unaccented and reduced. Because of their position in the sentence the short-form verbs lose length and individual word accent, changing from the syllable structure CV: to CV. In this process the meaning is also reduced, and another token of the same verb may be required to produce the desired meaning. Thus it is the first, inflected form of the verb which is actually "doubled" or repeated, not the second, reduced verb form. It is not the case that "doubling" has become some sort of productive process in Swiss-German, rather the phenomenon has been called "doubling" because linguists can still recognize the relationship between the regular inflected forms of the verbs and the particles that were once part of those verb paradigms. Grammaticalization of certain forms of these verbs has taken place; the resulting reduced forms no longer carry the same meanings as the inflected forms. The preverbal verb forms "go," "ko," and "fo" yield aspectual differences in the verbs, and the prefixed "lo" element changes the valence, or the relationship of the argument to the verb.

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**PROGRESSIVE CONSTRUCTIONS IN THE SPANISH SPOKEN IN  
PUERTO RICO<sup>1</sup>**

Michelle F. Ramos-Pellicia

**0. Abstract**

The continuous contact between English and Spanish in the Puerto Rican society has resulted in a situation that promotes the use of English as a linguistic model for imitation. Researchers such as Vázquez (1974: 75-77) have posited that interpolation of adverbial phrases in progressive constructions and other variations in the use of the gerund in Puerto Rican Spanish are the direct result of interference from English. Others such as Morales (1986: 41) have pointed out that in the linguistic interferences in the Spanish spoken in Puerto Rico, the speaker selects syntactic rules or processes from the English language. Therefore, it can be hypothesized that those speakers with the greatest amount

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<sup>1</sup> Portions of this paper are based upon Ramos-Pellicia (1997) and on Muller and Ramos-Pellicia (1997) and was presented at the 7th University of New Mexico Conference on Ibero-American Culture and Society and the 16th Conference on Spanish in the United States. I wish to thank Brian D. Joseph, Don Winford, Norma Mendoza-Denton, Craige Roberts, Jennifer Muller, Idalia Cordero Cuevas, Ana Celia Zentella, Carmen Silva-Corvalán, Rena Torres-Cacoulllos, Luis Ortiz López, Ana Teresa Pérez-Leroux, Rob Smead and those at the Conference who helped me with their insightful comments and recommendations. All shortcomings are, of course, mine.

of contact with English will produce these innovative forms with greater frequency than those with limited English interference.

This paper tests these hypotheses through the examination of the following structure: forms with semantically telic verbs in a progressive construction (e.g. 'Estaba disparando un tiro.' 'S/He was firing a shot.'). These progressive constructions were obtained through sociolinguistic interviews of 18 speakers of Puerto Rican Spanish. The participants also completed a questionnaire which revealed the level of contact with English that each speaker had. A total of 501 tokens from their speech was examined. These tokens were correlated with a variety of factors based on the information collected by the questionnaires. The quantitative analysis of these factors was accomplished by using the statistics package for sociolinguistic analysis, GoldVarb 2.0.

The main results were that those speakers who had no exposure to English in the United States have a positive correlation with usage of telic verbs in progressive constructions, while those with more exposure to English at higher levels of education have a negative correlation for usage of telic verbs in progressive constructions. On the other hand, those speakers who used Spanish as well as English outside the home have a higher frequency of usage of telic verbs in progressive constructions than those who only used Spanish in the same linguistic domains.

### 1. Introduction

"El Presidente está enviando un mensaje de que en esta actividad, claramente celebrando el reclamo de la estadidad para los puertorriqueños, aquí está su representante personal y está claro el compromiso que está implicando esa presencia y que reafirma lo que él ha dicho en numerosas ocasiones: que él favorece que los puertorriqueños se expresen y que él apoya esa expresiones. (El Nuevo Día. 7/28/1995. p.5.)<sup>2</sup>

<sup>2</sup> "The President (of the United States) is sending a message that this activity, clearly celebrating the claim of the statehood for the Puerto Ricans, here is the personal representative and it is clear the compromise

The linguistic and political situation in Puerto Rico has unique characteristics that are different from those of other Latin American countries. In Puerto Rican Spanish, progressive constructions, consisting of *estar* 'be' plus the gerund verbal form in *-ndo*, can be formed with telic<sup>3</sup> verbs, and so contrasts with Peninsular Spanish, which only admits non-telic verbs in such constructions. The above extract from the *El Nuevo Día*, a Puerto Rican newspaper, shows this with constructions such as: "El Presidente está enviando un mensaje. . ." 'The President is sending a message. . .' and ". . . está claro el compromiso que está implicando esa presencia. . .", 'it is clear the compromise that that presence is implying. ..'. The progressive is allowed in Peninsular Spanish only with non-telic verbs, but --in these cases-- a user of Puerto Rican Spanish has selected telic verbs. The question naturally arises as to why Puerto Rican and Standard Spanish differ.

The coexistence of Spanish and English in Puerto Rican society<sup>4</sup> results in a supposed bilingual situation, yet little research truly supports this idea. On the other hand, the continuous contact between the two languages has resulted in a situation that promotes the use of English as a linguistic model for imitation.

Even if it is assumed that Puerto Rican speakers are primarily monolingual, in that they most frequently use the Spanish language, they do borrow words, phrases and sometimes grammatical constructions from English. This language contact situation is assumed to result in a high level of interference in Spanish. Consequently, the structure of spoken Spanish in Puerto Rico may be altered due to contact with English.

The purpose of this paper is to examine the use of the progressive constructions in the Spanish of Puerto Rico to determine if extensive contact with English has resulted in innovative forms or semantic change. In particular, the following forms will be examined: forms with semantically telic verbs in a progressive construction that is non-telic in aspect:

(1)

Estaba disparando un tiro.  
"S/he was firing a shot."

(2)

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that that presence is implying and that that reaffirms what he has said in various occasions: that he favours that the Puerto Ricans express themselves and that he supports those expressions."

<sup>3</sup> A telic situation involves the achievement of a goal or some resulting state. Please refer to my discussion on how to distinguish telic from non-telic constructions in section 2.1.

<sup>4</sup> In Puerto Rican society, there are English monolinguals and to make sure that these individuals receive current information, there are also English media outlets: radio stations, Cable TV and a newspaper. These English media outlets coexist with media outlets in Spanish that are the norm all around the island.

El Presidente está enviando un mensaje. . ."

"The President is sending a message. . ." (El Nuevo Día, 7/28/1995, p.5)

My working hypothesis is that speakers with the greatest amount of contact with English will produce these innovative forms with greater frequency than those with more limited opportunity for English interference.

## 2. Literature Review

In Peninsular Spanish, when a verb in its gerund form is used in a periphrastic construction, the gerund gives a durative sense (i.e. a telic aktionsart). With verbs of momentary action (i.e. the achievement of a goal or some resulting state), "estar + gerund" has two meanings: a durative one or a habitual action in progress. But, with a restrictive modifier that refers to a momentary action to the verbal phrase, the resulting sentence is unacceptable in Standard Spanish because the momentary action is incompatible with the semantic nature of the gerund (Real Academia Española 1986: 448).

Under the assumption that the Peninsular Spanish situation reflects the older state of affairs, the Puerto Rican Spanish acceptable use of telic verbs in the progressive gerund construction represents an innovation, one for which a cause needs to be sought.

The function of the gerund to be considered in this paper is that which denotes a non-telic action<sup>5</sup>. This use of the gerund in Spanish has been studied by, among others, Alarcos Llorach (1995: 142-43) and Gili Gaya (1970: 63-64).

The specific construction of the gerund under investigation here is what is referred to in the literature as "the verbal periphrastic" construction, which contains an auxiliary verb followed by a verb in the gerund. It is noted that this construction differs semantically from a similar construction that does not contain the gerund. For example,

(3)

Comí.

"I ate."

differs from:

(4)

Estuve comiendo.

"I have been eating."

in that the first denotes an action that was completed, while the second denotes one that is ongoing.

<sup>5</sup> Differentiated from the functioning of a verb as a noun, as in English V. "write", N. "writing".



Due to the semantic nature of this type of construction, it is assumed that verbs that are semantically telic (i.e. those that denote an action that occurs at just one point in time), such as:

(5)

El cazador está disparando un tiro.

"The hunter is firing a bullet."

may not be used in the gerund form; a mismatch of meaning would result. For instance, in a construction such as:

(6)

"... uno se está graduando ya de sexto grado y, pues, uno se cree grande y todo eso..." (M, 6)

"... one is graduating from sixth grade and therefore one believes himself to be old enough and all that..."

the telic verb "graduarse" ("to graduate") surfaces in the progressive form, thus denoting ongoing action.

In the data, which consists of 501 tokens of *estar* 'be' plus the gerund verbal form in *-ndo*, constructions with non-telic aktionsart verbs as well as telic aktionsart verbs were observed. It has been assumed that this innovative structure is the result of English interference by grammarians such as Gili Gaya (1970: 63-64) as well as La Real Academia Española (1986: 448). On the other hand, Lapesa (1986: 133) has pointed out that in the Spanish spoken in Latin America, the verbal periphrasis of *estar* + gerund frequently competes in use with the simple forms without any differentiation in the meaning. Kany (1960: 179) attributes the change in meaning of these progressive forms to influence of Indian languages:

Indian loans and calques are numerous in various regions of Spanish America. In Andean regions, for instance, popular "estar + gerund" may replace any simple verb form, such as *estar teniendo* for *tener* and *estás pudiendo* for *puedes*. Such usage may be considered an extension of Old Spanish progressive constructions carried to unsuspected limits under the influence of local Indian tongues and displaying a variety of new shades of meaning.

Researchers such as Morales (1986: 41) have pointed out that in the linguistic interferences in the Spanish spoken in Puerto Rico, the speaker selects syntactic rules or processes from the English language. Mentioned among these interferences is the use of the gerund with punctual action (i.e. telic action), among others in the verbal phrase.

Pérez Salas (1971: 49) contradicts the above assumptions and claims that variation in use of the progressive constructions in Spanish may not be due to English interference. Moreover, Pérez Salas (1971: 91) also claimed that there is no justification for the claim that constructions such as:

(7)

Le estamos incluyendo.

"We are enclosing."

are abuses of the use of the progressive constructions, although in English it is a very well accepted syntactic construction. However, this researcher does not support his claims with quantitative results.

### 2.1. The use of the progressive with telic vs. non-telic constructions

For my analysis of the aktionsarten of the verbs used in the progressive forms, I will use the analysis proposed by Dowty (1985: 1-35) and further elaborated for the Imperfecto (Imperfect) and Pretérito (Simple Past) in Spanish by Cipria & Roberts (1996: 43-70). Aktionsarten are a classification of states or events. These are classified as either telic or non-telic, where the former involves the achievement of a goal or some resulting state and the latter are either states or processes. I argue here that the changes observed in the gerund constructions can be explained by reference to the aktionsart of clauses in English and that contact with English promotes the change in the aktionsart of the gerund constructions in the particular dialect of the Spanish used in Puerto Rico.

In Spanish, as well as English, the progressive forms will always imply non-telicity because their truth holds in their subintervals (the sub-interval property). Therefore, if (8) holds at an interval of time:

(8)

(a) Carmen está caminando de 7:00 a 8:00 de la mañana.

(a') Carmen is walking from 7:00 to 8:00 a.m.

then it also holds at some sub-interval of that interval as in (9) (distributivity):

(9)

(b) Carmen está caminando de 7:00 a 7:30 de la mañana.

(b') Carmen is walking from 7:00 to 7:30 a.m.

Moreover, if this truth also holds at some sub-interval, then it will also hold at some super-interval:

(10)

(c) Carmen está caminando de 7:00 a 9:00 de la mañana.

(c') Carmen is walking from 7:00 to 9:00 a.m.

In other words, the progressive form "Carmen is walking." entails that "Carmen has walked.". This situation, however, does not hold for telic aktionsarten. For instance, if we have a sentence like the following:

(11)

(a) El ladrón está disparando un tiro.

(a') "The thief is firing a shot."

the truth will not hold in its subintervals:

(12)

(b) El ladrón está disparando un tiro de 7:00 a 8:00 de la mañana.

(b') The thief is firing a shot from 7:00 am to 8:00 am.

or at some sub-interval of that interval:

(13)

(c) El ladrón está disparando un tiro de 7:00 a 7:30 de la mañana.

(c') The thief is firing a shot from 7:00 am to 7:30 am.

Neither does this truth holds for a super-interval:

(14)

(d) El ladrón está disparando un tiro de 7:00 a 9:00 de la mañana.

(d') The thief is firing a shot from 7:00 am to 9:00 am.

Moreover, the fact that a verb has a certain aktionsart does not mean that its clause has to share the same aktionsart. Certain arguments can yield aktionsarten different from the verb when it is in isolation.

In Spanish there is also a difference between the progressive form and the Imperfecto. The progressives focus on the internal processes and they appear in all tenses. However, the Imperfecto is limited to the past tense and presents the events as continuing and open. The progressive viewpoint has some meanings that the Imperfecto does not have. Progressive constructions are often associated with nuances of activity, dynamism and vividness. Also, progressive constructions invoke change and activity. (Smith, 1991: 113). The progressive viewpoint may be used to present a state as an event: a marked aspectual choice. The Imperfecto, on the other hand, lacks the dynamism that is typical of the progressive viewpoints, but appears with all situation types.

## 2.2. Classifications of the progressive forms

During my analysis of the progressive constructions, I identified some differences in the semantics of these clauses. These differences are mainly related to the aspectual characteristics of the different progressive forms collected in the 18 interviews: (1)

Extended Sense of the Progressive, (2) Habitual, (3) Future Sense and (4) Past Tense Reference.

### 2.2.1. Extended sense of the progressive

The following example expresses an extension of the process of "giving books". The speaker is handing more than just a physical object; he is also handing the knowledge contained in those books:

(15)

"Les estoy dando estas novelas porque son. . . se pueden conseguir. . ."

(M, 2)

I am giving you these novels because they are. . . they can be found. . ."

Notice the extension of the action towards the future, since --in this case-- the speaker has already distributed the books.

### 2.2.2. Habitual

In the habitual reading of the progressive construction, the speaker is giving this construction a property of the simple present. In other words, the progressive form cannot be used in situations where the simple present is used. As Cipria (1996: 53-54) states: "We cannot use the present progressive to describe situations that refer to a habitual or generic situation". Among the instances gathered under this type of classification are the following:

(16)

"... desde el año pasado para acá no solamente está yendo a Estados Unidos todos los años. . ." (M, 2)

"...since last year, she not only goes to the United States all the years..."

The Telic verb is used in a Non-Telic construction, and further the adverbial phrase "desde el año pasado" ("since last year") is also a Non-Telic phrase.

(17)

"Entonces, yo se la enseñé a ella, y --hasta ahora-- yo soy la única que está cantando." (F, 10)

"Then, I showed it to her and until now I am the only one who is singing."

The Telic verb is used in a Non-Telic construction, and further the adverbial phrase "hasta ahora" ("until now") is also a Non-Telic modifier.

In the above cases, the simple present form would have been more adequate as far as Peninsular Spanish is concerned: "... desde el año pasado para acá no solamente va a Estados Unidos todos los años. . ." ("... since last year, she not only goes to the United

States all the years. . ."); "Entonces, yo le enseñé a ella, y --hasta ahora-- yo soy la única que *canta*. . ." ("Then, I showed to her and until now I am the only one who sings.").

### 2.2.3. Future sense

The future sense of the progressive refers to an inertia situation where a situation begins and continues without any interference in the course of events. With all things being equal, the situations described by these progressive forms are perceived as being in progress at the evaluation point where the sentence was uttered and continuing beyond it.

The future sense that is given to the progressive constructions in the sentences collected in my data has been pointed to be inadequate by different grammarians such as Bello (1970: 162), Alarcos Llorach (1995: 146), among others. Such constructions, they claim, seem inadequate to express posteriority, consequence or effect. However, the following progressive constructions were produced by my interviewees.

(18)

"... uno se está graduando ya de sexto grado y, pues, uno se cree grande y todo eso. . ." (M, 6)

"... one is graduating from sixth grade and therefore one believes himself to be old enough and all that. . ."

The Telic verb is used in a Non-Telic construction that has a future sense, to give an informal characterization of "inertia" semantics: "if all things remain the same in the coming months, I will graduate. . .".

(19)

"Entonces, por ejemplo, a mí me pasaría de 17 todavía están en cuarto año, ponle, porque están saliendo de cuarto año. . ." (F, 11)

"Then, for example, it would happen to me: people that are 17 years old are in 12 grade because they are finishing 12 grade. . ."

The Telic verb is used in a Non-Telic construction that has a future sense: "if all things remain the same in the coming months, they will leave. . .".

(20)

"... y te pones 23 que estamos saliendo de universidad, o sea, esto es terrible porque de verdad vienen de las escuelas con unos vocabularios tan extraños. . ." (F, 11)

"... and suppose people who are 23 (years old) who are finishing college, like, this is terrible because they come from schools and they have such strange vocabularies. . ."

The Telic verb is used in a Non-Telic construction that has a future sense: "if all things remain the same in the coming months, we will graduate. . .".

(21)

"... y, pues estamos participando con dos personas más y la profesora y un grupo en unas competencias que se van a hacer a nivel internacional, se van a celebrar en Tailandia. . ." (F, 15)

"... and we are participating with two more persons and the professor and a group in some competitions that are going to be held at the international level and they are going to be held in Thailand. . ."

The Telic verb is used in a Non-Telic construction that has a future sense: "if all things remain the same in the coming months, we will participate. . .".

(22)

"... porque sería, porque sería, o sea, sería aquí mismo --entonces-- me estaría atrasando un semestre más. . ." (M, 3)

"... because it would be because it would, be I mean, it would be here, then, I would be behind another semester. . ."

The Telic verb is used in a Non-Telic construction that has a future sense: "if all things remain the same in the coming months, I will be behind another semester. . .".

All these constructions seem to violate the normative use of the Spanish progressive constructions because these are not supposed to refer to an action that extends into the future. Progressive constructions in Spanish presuppose an action that originated in the past and continues in the present.

#### 2.2.4. Past tense reference

The following are constructions that express dynamic, instantaneous events that have already occurred in the past. However, they are better expressed in Spanish by the simple past since the action under discussion here is instantaneous and incompatible with the nature of the progressive construction. Gili Gaya (1973: 63-64) as well as the Real Academia Española have criticized such constructions. In them, the Telic verb is used in a Non-Telic construction that refers to an action that has already occurred:

(23)

"... porque con la prisa y eso, y después no, caí en cuenta que no, que era que mi mentor me estaba mandando la carta de recomendación que tenía que mandar a Duke. . ." (F, 13)

"... because with the rush and that and later I realize that it was my

mentor that was sending me the letter of recommendation that I needed to send to Duke. . ."

(24)

"... estaba ese verano en Nueva York y estaba yendo a San Patricio a la Iglesia y mi papá parece que iba también a esa misa, cuando ella está entrando, ella oye que alguien la llama y entonces ella dice: 'bueno, ¿quién me conoce aquí...?'" (F, 16)

"... she was that summer in New York and she was going to Saint Patrick, to the church, and my father seems that he went there too to that mass, when she was entering [the church] she heard that someone was calling her and then she says: "Well, who knows me here?"

### 3. Methodology

This study examines the use of the gerund by 18 speakers of Puerto Rican Spanish. The data were taken from transcripts of interviews that were performed by Ramos-Pellicia at the Río Piedras Campus of the University of Puerto Rico in 1994.

Half of the speakers were male and the other half female. The participants all ranged in age from 18-23. They came from various parts of the island of Puerto Rico, and therefore, do not represent just the dialect of the metropolitan area of San Juan.

The participants in the study completed a questionnaire that included questions about the speaker's level of education, at what age the speaker had begun education in English, whether the speaker attended a private or public school, and whether the speaker had education experience in the United States. Additionally, the questionnaire asked about the educational level of the speaker's family, and which languages were used with the family. A variety of questions targeted the socioeconomic status of the speaker. Further, the speaker was asked how much English they used outside the home, at work, with friends, or at school.

The participants were then interviewed while being recorded. The interviews consisted of informal questions and answers, depending upon the interests of the speaker and the flow of the conversation. Each interview lasted approximately one hour.

Each of the interviews was transcribed; all uses of progressive constructions were individually isolated, classified and grouped according to type of construction.

As mentioned before, the types of constructions that this study aims to investigate are those with telic verbs. In order to focus the scope of this study, only those constructions that consisted of:

(25)

estar + X + gerund

were examined, where X could be any interpolated material.

A total of 501 tokens were examined; each token was classified according to speaker. Additionally, each verb in isolation was noted as either being of non-telic or telic aktionsart.

In addition to noting the aktionsarten of the tokens, a variety of factors based on the information from the questionnaires was included. Specifically, the intent was to include factors that would reveal the level of contact with English that each speaker had. Therefore, the speaker's educational experiences in English were taken into account, as well as individual experience in an English-speaking environment (i.e. either in the United States or in a home where English was the predominant language). The speakers were grouped as to whether they have had (1) extensive educational experience in the United States, (2) some educational experience in the United States, or (3) no educational experience at all in the United States.

Additionally, the socio-economic status of each participant was included as a factor. The status of each speaker was evaluated by examining the level of income and education of the family as a whole, as well as the number of family members. It is assumed that Puerto Ricans of middle or upper middle class have more exposure to English, based upon the educational situation on the island<sup>6</sup>. In general, wealthier children are consistently taught in English, while less wealthy children are not.

Another factor that was also included in the analysis was gender.

The quantitative analysis of these factors was accomplished by using GoldVarb 2.0, the statistics package for sociolinguistic analysis. Eight factors groups were included in the coding system<sup>7</sup>.

#### 4. Results

From the 18 speakers, 501 tokens of *estar + X + gerund* were coded. The female participants had 277 tokens while the males had 224 tokens. The results of these analyses

<sup>6</sup> In the public schools of Puerto Rico and in many middle income private schools, English is taught as a second language from the primary level until 12th grade in high school. Therefore, all classes are conducted in Spanish except for the English class. Some private schools of the upper middle class have all classes (except for the Spanish class) conducted in English since first grade.

<sup>7</sup> It should be noted that some of the factor groups that were originally considered in the statistical runs were discarded since they did not reflect any variation (they were knockouts). Most of these factors were assumed to be peripheral to the focus of the analysis.



are shown on Tables (1) and (2).

From the GoldVarb analysis of use of telic verbs in gerund constructions, I obtained the following results: (a) the number of non-applications (i.e. progressive constructions with non-telic verbs) is greater than the number of applications.

With regard to (b) education in English in the United States, the group with no educational experience in the mainland United States showed a high probability of using telic verbs in progressive constructions, as indicated by the relatively high weight value (0.615). The group with primary school experience showed a moderate probability (0.438) of using this construction, while the group with significant educational experience in the United States showed a surprisingly low probability of telic verb use, as indicated by the low weight value (0.295).

**Table 1. Factor Group Weights of Years of Education in English in the United States and Use of Telic Verbs in Progressive Constructions.**

Factors	Weights	Applications/ Total	Input & Weights
College and High School	0.295	0.82	0.76
Primary School	0.438	0.82	0.86
None	0.615	0.89	0.93

Total chi-square= 11.1951

Chi-square/cell= 0.7463

Log likelihood= -186.627

p= 0.09

In looking at other factor groups, such as (c) Language/s spoken outside Home, it seems that those speakers who used both Spanish and English produced, with a higher frequency (0.634), the telic verbs in progressive constructions.

**Table 2. Factor Group Weights of Language Spoken outside Home and**

## Use of Telic Verbs in Progressive Constructions.

Factors	Weights	Applications/ Total	Input & Weights
Spanish Only	0.480	0.85	0.88
Both Spanish and English	0.634	0.95	0.93

Total chi-square= 11.1951

Chi-square/cell= 0.7463

Log likelihood= -186.627

p= 0.01

### 5. Discussion

The results of the quantitative analysis appear to indicate that the factor "Educational experience in the United States" at any level will have a lower probability of producing a telic verb in a progressive construction. This seems like a puzzling result, for the expected outcome of an education completely in English is to have the most interference with Spanish, as opposed to those individuals who have some or none educational experience in the mainland. Moreover, my reanalysis of the data using a Formal Semantics approach reveals that those speakers with the least education in English will produce this innovative form more than those speakers with more education in this language in the mainland. The fact that those speakers with least education in English use the telic verbs in progressive constructions with a higher frequency seems to be a contradiction to the exposure of this group to the education in English. Nonetheless, this can be explained by the imperfect learning of English as a second language. Thomason and Kaufman (1988: 39) have pointed out that: ". . . interference through imperfect learning does **not** begin with vocabulary: it begins instead with sounds and syntax, and sometimes includes morphology. . .".

The fact that those speakers who used both languages outside the home have a higher frequency of use of telic verbs in progressive constructions can be explained by the fact that these are speakers who used the resources that are not available to them in the linguistic system of Spanish, but that they have at hand in English. The use of telic verbs in the progressive constructions, then, is a marked aspectual choice for these speakers.

However, even though the results are suggestive of English interference, it must be noted that it is possible that the use of telic verbs is the result not only of contact with English, but also of an internal change in the language system. It could also be that the factors selected for this quantitative analysis do not reveal as clear a picture of English interference as possible. For example, I have assumed that extensive education in and exposure to English are the only factors that necessarily imply significant English interference. However, it is possible that other factors contribute to interference, such as language attitudes, a factor that was not investigated.

### 6. Conclusions

Looking closely at differential access to education in this particular sample from Puerto Rico, I argue that higher level of achievement in English is directly correlated with lower frequencies of telic verbs in progressive constructions, while a lower level yields a higher frequency of progressive constructions with telic verbs. As I had stated before, it would be reasonable to explain this by imperfect learning of English as a second language. Otherwise, how could one explain the fact that a person with a lack of sufficient exposure to a language as English can be producing these constructions with telic verbs?

Speakers who use English as well as Spanish outside the home are using the telic verbs in progressive constructions to present a state as an event. Therefore, this option for them is a marked aspectual choice that is not available in the Spanish system and gets incorporated by transferring the strategy available in the English system into their L1.

On the other hand, there are progressive constructions with telic verbs in Spanish, that, when used in English, sound very odd. I argue that this is the result of an extension of a syntactic rule of English that by interference is selected by Spanish and extended to other constructions in English and Spanish that are not allowed to use this type of aktionsart.

The biggest difficulty confronted in the statistical analysis of these factors is the low frequency of those structures that are prone to change. However, this is the only way available of documenting the slight changes that are occurring through time in the Spanish spoken in Puerto Rico. It is only when more accurate methods are developed that these changes will be as obvious as they are in the daily language spoken in the Island.

Moreover, there is also the possibility that other factors such as: language attitudes, community networks, among others could provide a clearer picture of the language situation in the island of Puerto Rico. Still, this study documents an effect that

English seems to be having on Spanish in Puerto Rico, and thus contributes to our understanding of contact-induced change in the island.

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### Tracing the Functional Expansion of the Self-pronoun

Karin Golde

#### 0. Introduction

This paper aims to explore the development of various functions of the English self-pronoun in light of their present day constraints, focusing in particular on the reflexive and intensive functions.<sup>1</sup> Starting life as an intensive in Old English (OE), the morpheme *self* came to be fused with pronouns to form the compound self-pronoun which today performs a variety of functions, including intensification and marking reflexivity. After giving a brief outline of this process, I will attempt to pinpoint the constraints on Present Day English (PDE) intensives, showing how this enables us to better understand the function of OE intensives and their extension to a reflexive use. I will then discuss other PDE uses of self-pronouns, suggesting possible origins, and raising questions for further research.

<sup>1</sup> The term "self-pronoun" refers to the morphological forms *himself*, *herself*, *themselves*, etc.

## 1. Formation of the self-pronoun

In the PDE pronominal system, we have both personal pronouns and self-pronouns, the latter type having, among other functions, the ability to mark reflexivity or intensification. The term "reflexive" is used here to mean a pronoun which is coindexed with a less oblique argument of the same head, as in the following examples:

- (1) a. John likes \*him<sub>i</sub> / himself<sub>i</sub>.  
 b. Agatha<sub>i</sub> relies on \*her<sub>j</sub> / herself<sub>i</sub>.  
 c. Mary posted the children's<sub>i</sub> drawings of \*them<sub>j</sub> / themselves<sub>i</sub>.

In (1b,c) the preposition can be considered a case marker, making little or no semantic contribution. Thus the self-pronouns are still co-arguments of the subject and the possessive, respectively.

The term "intensive" refers to a self-pronoun which is an appositive to an NP or personal pronoun. As will be discussed in §2, intensives mark either external prominence (i.e. high rank or importance), or discourse prominence and contrast.

- (2) a. The governor himself will be at the rally.  
 b. Louise's brother likes downhill skiing, but she herself prefers cross country.

However, OE had only personal pronouns, which unlike their PDE counterparts, could be used as reflexives.

- (3) he hine > ær hwile reste  
*he him there meanwhile rested*  
 "he rested (himself) there meanwhile"

[Visser 1963, §432a]

The morpheme *self* served alone as an intensive, apparently with much the same purpose as the PDE intensive.<sup>2</sup> The following examples are taken from Keenan (1994).

- (4) ond he geseah flone hælend sylfne standan on his godflrymme  
*and he saw the(acc) Lord self(acc.m.sg.) standing in his divine glory*  
 [Mart.8; =Keenan's (30)]
- (5) fla forborn flas cyninges heall mid callum his spedum, ond his sunu awedde, ond he sylf ahreofodee...  
*then the king's hall burned down with all his treasures, his son went mad, and he self(nom.m.sg.) became a leper*  
 [Mart.174; =Keenan's (35)]

<sup>2</sup> This will be explored in more detail in §4.

As these examples illustrate, *self* was usually declined to agree in number, case, and gender with the noun it modified.

The sequence of pronoun+*self* eventually gave rise to the PDE compound self-pronoun, a process which according to Visser (1963) was completed by the 15th century.<sup>3</sup> During this time, pronoun+*self* came to be used as a reflexive as well as an intensive. The remainder of this paper will explore this change in function during OE and Middle English (ME) without going into further details of the morphological developments. Thus I will speak of both *self* and self-pronouns with the understanding that the latter is a direct descendent of the former.

## 2. Constraints on PDE intensives

Ideally, we would like to determine the constraints on OE intensives, but given the subtleties of intensives, it is useful to begin with a precise account of PDE, and compare those results with the available examples in OE texts. In this section I will concentrate on the analysis of Baker (1995), who provides us with just such an explicit set of criteria for the appropriate use of intensives.<sup>4</sup> His analysis will require some fine-tuning which will result in a more accurate description of the constraints on PDE intensives.

Baker distinguishes between two necessary conditions on intensives; the intensive NP must be contrastive and it must have some sort of discourse prominence. The first condition is stated as follows:

- (6) CONTRASTIVENESS CONDITION: Intensive NPs are appropriate only in contexts where emphasis or contrast is desired. [=*Baker's (19)*]

Discourse prominence is a more nebulous concept, related to the centrality of a figure with respect to the rest of the narrative.

- (7) CONDITION OF RELATIVE DISCOURSE PROMINENCE: Intensive NPs can only be used to mark a character in a sentence or discourse who is relatively more prominent or central than other characters. [=*Baker's (24)*]

There are several sources of discourse prominence; for example, the intensive NP can refer to the directly responsible agent, as in (8a), or the directly affected patient, as in (8b). In each case, I have underlined the NP with which the intensified NP is being contrasted.

<sup>3</sup> See Visser §426-453, Mitchell (1985) §472-500, Ogura (1989), and references therein for details on the complex set of motivations for this change.

<sup>4</sup> Baker relies primarily on British literature for his data, but also constructs some of his own examples. I will be citing Baker's data, as well as my own excerpts from the Brown corpus and other literature.

- (8) a. The obstacle that Bill tried to set up for the opposing lawyer ultimately caused difficulties for Bill himself.  
[=Baker's (29a)]
- b. Of all the people who were in the courtroom yesterday morning, only the defendant herself remained completely calm when the judge handed down her sentence.  
[=Baker's (31a)]

In both cases the contrast is with a character or characters who are less directly involved in the relevant scene. Note that it is not necessary for the contrasted NP to appear in the sentence; for example, if the first PP is removed from (8b), the intensive NP is still acceptable since the presence of others in the courtroom may be understood in context.

A more general source of prominence is being the "primary topic of concern." For example, the following is an excerpt from a review of a performance by violinist D'Albert which at one point names him in a list of other composers.

- (9) Works by Dohnanyi, Hubay, Mr. D'Albert himself, and Paganini indicated that the violinist [D'Albert] had some virtuoso fireworks up his sleeve as well as a reserved attitude toward a lyric phrase.  
[Brown 180021]

Note that again there is contrast, this time with the other composers, who are more incidental to the story.

Another way to be prominent is to be the subject of consciousness; that is, to be the person whose point of view is being represented. This is the case in the following example from Jane Austen's *Sense and Sensibility*, where Elinor Dashwood is the subject of consciousness.

- (10) She, [Elinor Dashwood] could not but smile to see the graciousness of both mother and daughter towards the very person—for Lucy was particularly distinguished—whom of all others, had they known as much as she, did, they would have been most anxious to mortify; while she, herself, who had comparatively no power to wound them, sat pointedly slighted by both.  
[Austen 239; =Baker's (37a)]

Here Elinor is being contrasted with another character whose point of view is not represented.

A common source of prominence is for the intensive NP to be contrasted with something defined relative to it. That is, if the intensive NP is X, then what it is being contrasted with is defined as X's Y.



- (11) On each side of the motor well there's storage for battery, bumpers, line, and spare props with six-gallon gas tanks below. The well itself is designed to take two Merc 800's or 500's if you wish, and there's room for a 25-gallon long cruise gas tank below it.

[Brown 297323]

Here the contrast is between the motor well and the areas defined as being on either side of the well.

So far, Baker's conditions seem correct in that prominence and contrast are the key to the function of intensives. However, the fact that prominence and contrast are separated into two conditions presents a minor problem. Consider the following dialogue:

- (12) Agatha: I wonder what John's been up to.  
 Bernice: I don't know; I haven't seen him lately.  
 Candice: I saw John yesterday.  
 Agatha: You saw Sean yesterday? Who's that?  
 Candice: No, I said I saw John (#himself) yesterday.

When Agatha misunderstands, Candice emphasizes the correction, contrasting it with *Sean*. Since John is also the primary topic of concern in this conversation, Baker's conditions for the intensive are met. Yet the inclusion of *himself* yields an anomalous sounding sentence.

Based on this observation, we can conclude that contrast and prominence are not sufficient in and of themselves. Instead, the contrast must be with a member or members of a comparison set consisting of figures which are less prominent in the relevant sense. In (12) the contrast is not with a character who is peripheral to the topic at hand, since Sean may or may not exist. The contrast is rather with the misunderstood version of a name.

This problem with Baker's conditions can be solved by combining them into the following unified constraint:

- (13) **INTENSIVE NP CONSTRAINT: (preliminary version)**  
 Intensive NPs are appropriate only where there is contrast with a member of a relevant comparison set which has less discourse prominence.

This constraint also differs from Baker's in that it leaves out the term "emphasis." To make sure that this does not cause the wrong predictions to be made, we want to check for cases in which an intensive NP could be considered emphatic, but not contrastive.

This situation does in fact arise with another source of prominence not yet discussed—what Baker calls "high external rank." I will refer to this as "external prominence," in contrast with the previously discussed types of discourse prominence. Such prominence relies not on the structure of the discourse, but rather on shared

assumptions about rank at various levels. Deities, religious figures such as saints, and high-ranking politicians are common examples of characters with external prominence.

There seem to be two types of situations in which the intensive emphasizes external prominence. In the first, the intensive NP serves as an exemplar of a quality relevant to the situation, with its appropriateness justified by shared cultural assumptions rather than by contextual clues. For example, the devil may be invoked as the highest member on a hierarchy of evil beings.

- (14) Cried one professor after a few months of Student Schiele's tantrums and rebellion: "The devil himself must have defecated you into my classroom!"  
[Brown 166871]

Abstract natural forces may also qualify as having high external rank. In the following example, *life* is referred to as a paragon of persistence, an assumption which the reader may readily accommodate given the context of animals in the wild.

- (15) There is a patience in the wild—dogged, tireless, persistent as life itself—that holds motionless for endless hours the spider in its web, the snake in its coils, the panther in its ambushade...  
[London 3265]

In the second type of situation, the externally prominent NP can perhaps be paraphrased as "the one and only X" or "the great X."

- (16) a. The award for volunteer work was presented by President Clinton himself.  
b. The children saw Santa Claus himself at the mall.

Again, the intensive NP refers to a figure which has a high rank on some externally available hierarchy. However, note that while the rank has external justification, it must also be contextually appropriate.

- (17) Agatha: Guess who's going to be presenting the award for volunteer work?  
Bernice: Who—Mayor Brown?  
Agatha: No, Governor Wilson himself!

In the above dialogue, Bernice mentions a figure lower in the political hierarchy than the state governor, so that it is appropriate for Agatha to name the governor as someone whose rank justifies the use of the intensive.

But in (18), Bernice invokes a more extensive hierarchy by mentioning the president.

- (18) Agatha: Guess who's going to be presenting the award for volunteer work?  
Bernice: Who—President Clinton?  
Agatha: No, Governor Wilson (#himself).

Thus Agatha is no longer able to use the intensive, since the governor is ranked lower on this hierarchy than the president.

Similarly, it is important that the hierarchy exist for at least some of the people in the text or dialogue. In (16b) the intensive is justified because we can imagine that the children place Santa Claus high on a list of powerful or influential figures in their lives. This is why the following sounds odd:

(19) The teenagers saw Santa Claus himself at the mall.

Once children reach a certain age, we know that Santa Claus becomes demoted in their pantheon of heroes.

In both types of cases, whether the intensive NP marks an exemplar of a quality or an individual with certain prestige, it might be argued that an implicit contrast exists between the high-ranking figures and the figures which rank below them. In this case, we could simply remove the word "discourse" from the constraint in (13) and have a reasonably accurate constraint covering both discourse and external prominence.

But it would be misleading to characterize contrast as a necessary condition on intensives with external prominence. Instead, any contrastiveness seems to be an incidental byproduct rather than the speaker's intention, since it cannot be divorced from the notion of high rank in the first place. On the other hand, an NP with discourse prominence can be referred to without any such contrast being implied.

Furthermore, in the next section I will argue that there is independent evidence for a distinction between discourse prominence and external prominence. Thus there is no need to conflate these two types of intensives to begin with. Instead, the constraint in (13) can be augmented as follows:

- (20) INTENSIVE NP CONSTRAINT: (final version)  
 Intensive NPs are appropriate only where:
- (a) there is contrast with a member of a relevant comparison set which has less discourse prominence, or
  - (b) the referent is understood to have the highest rank on a relevant hierarchy of external prominence.

This distinction between two types of prominence is further supported by both synchronic and diachronic evidence, to be discussed in the following section.

### 3. Evidence for two distinct types of prominence

#### 3.1 Synchronic evidence

In PDE, intensives with external prominence can be shown to behave differently than those with discourse prominence. First, Baker observes that it is impossible for more than one NP to be an intensive within a short passage; only one NP at a time is able to have discourse prominence.

- (21) a. Do you want to speak to Barbara, or would you rather deal with Martha?  
 b. Do you want to speak to Barbara herself, or would you rather deal with Martha?  
 c. Do you want to speak to Barbara, or would you rather deal with Martha herself?  
 d. ??Do you want to speak to Barbara herself, or would you rather deal with Martha herself?
- [=Baker's (41)]

Thus either Barbara or Martha can be interpreted as having discourse prominence, but not both in the same passage.

However, the following attested example shows that two intensive NPs are possible if one has external prominence.

- (22) Those social, civilizational factors not rooted in the human spirit of the group, ultimately cease to exist. Civilization itself—tradition—falls out of existence when the human spirit itself becomes confused.
- [Brown 525005, 525017]

The first intensive NP is being contrasted with the "social, civilizational factors" mentioned in the first sentence, and gains discourse prominence by virtue of its contrast with something defined in terms of it. The second intensive NP is not being contrasted, but has external prominence as an abstract concept, like that in (15). The result is a piece of prose which may not be stylistically appealing, but is admitted by the grammar in a way that (21d) is not.

While only one figure may have discourse prominence within a given passage, we would expect it to be possible in principle to have more than one intensive NP with external prominence. To my knowledge there are no attested examples, but the following sentence certainly sounds better than (21d).

- (23) In Alfred's vision of the apocalypse, everyone good and evil was assembled together, from Christ himself right down to the devil himself.

Thus Baker's observation seems to be correct, but only if limited to intensives with discourse prominence rather than external prominence. This difference in behavior suggests that the two types of prominence should be treated as distinct.<sup>5</sup>

### 3.2 Diachronic evidence

There is also evidence that the original function of the intensive *self* was to mark external prominence, and that the marking of discourse prominence followed later. The former function was by far the most common in OE, according to Keenan's data. In his corpus, he finds that almost all (71 out of 74) full NPs modified by an intensive refer to the holy trinity, a superhuman figure, or a person of high rank. Below are two examples of full NPs with external prominence.

- (24) ond he geseah thone haelend sylfne standan on his godthymme  
*and he saw the Lord self standing in his divine glory*  
 [Mart.8; =Keenan's (30)]
- (25) menn tha gearwiath clane wununga on heor heortum Criste sylfum  
*men who prepare a clean habitation in their hearts for Christ self*  
 [BiHom.VI.73; =Keenan's (31)]

Neither of these examples appears to involve contrastiveness, just like the PDE intensive NPs with external prominence. Interestingly, only about half of all intensified pronouns (34 out of 77) refer to an exalted figure, a fact that will be returned to in the next section.

The distribution of intensives suggests that their primary function was to mark external prominence, perhaps periphrastically. Both Keenan and Peitsara find that local object binding is much more likely to be expressed by a self-pronoun in religious texts, in OE and ME respectively. Peitsara believes this is related to the general use of periphrases in religious texts to affect a solemn tone. Other such periphrastic conceits include the unemphatic *do*-periphrasis, pleonastic *that* with subordinators, and lengthened prepositions. Thus the use of self-pronouns was strongly linked to a sense of formality, even after they had come to mark reflexivity as well as to intensify. This is consistent with the hypothesis that intensive *self* had its start simply marking external prominence.

In the following section the development of OE intensives into markers of discourse prominence will be examined in some detail.

<sup>5</sup> Another potential source of evidence lies in the intonational differences between the two (pointed out to me by Carl Pollard, p.c.). I will leave this line of inquiry open for future research.

#### 4. A closer look at OE intensives

The function of the OE intensive is often simply described as “emphatic”; Mitchell cites Penning (1875:20), Wülfing (1890:i, §239,242), Farr (1905:22,25), and Quirk and Wrenn (1958:72) as all agreeing on this point. But the data suggest a slightly more complex picture.

Recall that Keenan found the following distribution among instances of intensive *self* in his OE corpus:

referent has...	external prominence	discourse prominence
full NP	74	3
pronoun	34	43

**Table 1: Distribution of *self* in OE**

If the primary function of *self* was to mark external prominence, then the question is why it would also be used with personal pronouns with undistinguished referents. I would suggest that in using *self* in apposition with personal pronouns whose referents had high status, another more utilitarian function emerged, that of clarification of reference.

- (26) *tha forborn tha cyninges heall mid eallum his spedum, ond his sunu awedde, ond he sylf ahreofodee...*  
*then the king's hall burned down with all his treasures, his son went mad, and he (him)self became a leper*

[Mart.174; =Keenan's (35)]

Note that the appositive not only marks the high status of the referent, but also alerts the reader that it is the king rather than the king's son who is being referred to. It would not be a very big step from there to the present use of appositives. Rather than necessarily indicating high status, the intensifier would mark a pronoun whose referent is the most prominent individual in the discourse, contrasting it with other potential referents.

This is in fact already the case in (27).

- (27) *Abraham sodlice ymbsnad hys sunu Ismahel on thone ylcan daeg, swa swa God him bebad. & he sylf weard ymbsniden fla he waes nygan & hundnygantig geara.*  
*Abraham verily circumcised his son Ishmael on the same day as God bade him, and he (him)self was circumcised when he was nine and ninety years old.*

[ÆGen16.24; =Keenan's (50)]

All of the characters involved have special status as biblical figures, with God having the highest status. Yet it is Abraham as the main character whose pronominal representation is marked with *sylf*, in contrast with his son Ishmael.

From there the intensive's function would extend to prominent full NPs which were being contrasted. Thus the original function of the intensifier as a marker of external prominence has remained, with no requirement of contrast, as in (24) and (25). The extended function has been to mark the contrast of a discourse prominent NP. In both cases, if a pronoun was being modified in OE, it potentially had the ancillary function of disambiguating pronominal reference.

This secondary function has been recognized in PDE intensives as well. For example, Bickerton (1987) points out that in the following dialogue, the appositive restricts the antecedent of the pronoun to Susan:

- (28) A: How will Mary<sub>i</sub> do on the exam?  
 B: I don't know, but Susan<sub>i</sub> says that she<sub>ex</sub> / she<sub>ex</sub> herself will pass.  
 [=Bickerton's (5)]

Since it is Susan's point of view which is being taken in B's report, the constraint in (20) accurately predicts that the intensive is appropriate; presumably Susan is being contrasted with the other exam takers. It is possible that Mary could also have discourse prominence, perhaps as the primary topic of concern, but there is not enough context to establish this. There is also no one in the context that Mary could be contrasted with.

Bickerton claims that it is the c-commanding potential antecedent which blocks coreference with *Mary*, but McKay (1991) shows that this cannot be right by adding some more context to the dialogue.

- (29) A: Mary<sub>i</sub> has been concerned about her friends. Susan<sub>i</sub> said that several were going to fail the course, and Susan might be right. But Mary should think more about her own work. How will Mary do on the exam?  
 B: I don't know, but Susan<sub>i</sub> says that she<sub>ex</sub> / she<sub>ex</sub> herself will pass.  
 [=McKay's (10)]

McKay asserts that the difference lies in the establishment of a comparison set for Mary by adding other students to the context, and I would agree. The implication is that Susan is aware of the whole situation, (making B's statement relevant to A's question), and thus is contrasting Mary, the primary topic of concern, with the other students in the course. Alternatively, the intensive pronoun could still refer to Susan as the subject of consciousness if she was contrasting herself with both Mary and the rest of the students. Thus, depending on the particular context, the intensive does have the potential to disambiguate a pronoun's reference.

In general, the PDE functions of intensive *self* appear to have emerged by the end of OE; marking external prominence, marking discourse prominence and contrastiveness,

and clarifying pronominal reference when the other conditions are met. In the next section the emergence of the self-pronoun as a reflexive marker will be discussed in light of these developments.

### 5. From intensive to reflexive

Now that we have clarified the functions of the intensive and their development, it is possible to reexamine the emergence of the self-pronoun as a reflexive. What feature or features of the intensive allowed its function to be extended to that of marking local object-binding? It has been commonly assumed (Levinson 1991, Faltz 1985, Peitsara, among others), that the shared characteristic of intensives and reflexives is "unexpectedness" or "remarkability." This stems from the observation that self-pronouns were first used as reflexives with verbs where the coreference is marked.

I will argue that this analysis is slightly inaccurate. In order to have a more precise account of this development, we should take care not to equate unexpectedness with either contrast or external prominence. Levinson (p.31), for example, lists as a feature of intensives "a contrastive, contrary-to-expectation element," as if the two terms described the same situation. Similarly, Peitsara (p.299) interprets the use of self-pronouns to denote Christ as partly due to the fact that "anything done by Christ is a remarkable act," a statement which misses the generalization we arrived at previously about the marking of external prominence.

The reason that self-pronouns came to be used as reflexives, the commonly accepted argument goes, is that like the intensives, they are marking a situation in which the referent is unexpected. However, as Baker cites König (1991:89) as pointing out, nothing we know about intensives suggests that one of their functions is to signal unexpectedness per se. Recall that in §2 the following constraint was determined.

- (30) INTENSIVE NP CONSTRAINT: (final version)  
 Intensive NPs are appropriate only where:
- (a) there is contrast with a member of a relevant comparison set which has less discourse prominence, or
  - (b) the referent is understood to have the highest rank on a relevant hierarchy of external prominence.

This is not to say that the intensive may not incidentally mark an NP with an unexpected referent.

- (31) Ed was always bragging about how much money he made. Yet not only was Ed's mother seen picking up food from the local charity, but Ed himself was known to use food stamps at the grocery store.

Note that in (31) Ed is both discourse prominent and contrasted with his mother. It is also unexpected that Ed would use food stamps on a large income.



However, if the referent is only unexpected, but not contrastive, then the intensive is no longer appropriate.<sup>4</sup>

- (32) Ed was always bragging about how much money he made. Yet Ed (#himself) was seen using food stamps at the grocery store.

In Peitsara's detailed study, she confirms previous assertions that throughout ME, locally bound personal pronouns were most common with verbs for which coreferential arguments were the rule rather than the exception. In particular, these verbs were either "essentially reflexive" or "predominately reflexive."<sup>7</sup> The first type includes verbs with pleonastic objects, such as *go*, *bethink*, *repent*, *govern*, *obey*, and *busy*.

- (33) homward he him spedde  
*he sped homeward*  
 [Chaucer 359k]

- (34) This knight avyseth him and sore syketh  
*This knight pondered and sighed wretchedly*  
 [Chaucer 372w]

Most of these verbs, such as the two above, have lost this pleonastic object in PDE.

The predominately reflexive verbs occur most frequently with coreferential arguments, but also allow a non-coreferential object, and include *shave*, *arm*, *clothe*, and *bless* ("cross oneself").

- (35) And cladde him as a povre laborer  
*And [he] clothed himself as a poor laborer*  
 [Chaucer 551k]

On the other hand, Peitsara finds that self-pronouns were more often used to mark coreference on the "accidentally reflexive" verbs, those which generally have non-coreferential arguments. These include verbs denoting destructive or undesirable behavior (*deceive*, *kill*), as well as recommendable actions (*understand*, *offer*, *overcome*). Context naturally plays a role; locally bound pronouns which refer to exalted figures, or which are contrastive tend to be expressed as self-pronouns.

Having carefully examined the functions of intensives, we can now interpret Peitsara's findings in terms of their transition to reflexives. First, the use of self-pronouns

<sup>4</sup> This example can be improved by imagining a contrast between Ed and the people he brags to. In fact, it is difficult to construct an isolated example in which there is no way to construe some sort of contrast.

<sup>7</sup> Peitsara uses the term "reflexive verb" as an abbreviation for "the reflexive use of a verb"; i.e. one in which the objects are coreferential. In the case of the essentially reflexive verbs, however, the object is pleonastic and thus not referring.

both contrastively and with externally prominent referents marks a direct extension of their intensive functions.

As for the verbs denoting either destructive or recommendable behavior, there are two observations to be made. First, the verb's subject frequently represents a directly responsible agent, while the direct object will be the most directly affected patient.<sup>8</sup> Since each has the same referent, two of the conditions requisite for discourse prominence will often be met within the context of the verb's action, when only one is actually necessary.

It is also natural that essentially reflexive and predominately reflexive verbs generally did not take a self-pronoun. Essentially reflexive verbs do not even have a referential object, thus there is no referent to have prominence or to be contrasted. It is conceivable that the objects of predominately reflexive verbs were treated as pleonastic as well, given that they often alternated with a middle voice construction in which the object was deleted. For example, Visser (§161) finds many verbs in ME (such as *bathe, clothe, conform, humble, submit, and pride*) which appear both with and without a reflexive object, and have a reflexive interpretation. At any rate, there was no need to disambiguate the reference of the object of a predominately reflexive verb, since the coreference was understood to be the unmarked case.

## 6. Further developments

So far we have seen that the original function of the intensive *self* as marking external prominence has been expanded to allow it to mark discourse prominence and contrast, as well as local object-binding. As is typical of semantic change, these developments have been additive, with the result that all of the steps represent a function still present in PDE. These developments are summarized below.

### (36) HYPOTHESIZED DEVELOPMENT OF *SELF*:

- STEP 1: *self* marks external prominence on NPs and pronouns
- STEP 2: *self* disambiguates reference of externally prominent pronouns; there is implicit contrast with other potential referents
- STEP 3: *self* disambiguates reference of discourse prominent pronouns; there is implicit contrast with other potential referents
- STEP 4: *self* marks discourse prominence and contrast on full NPs
- STEP 5: *self* disambiguates reference of locally bound objects; referent is typically externally prominent and/or discourse prominent and contrastive

By no means do these steps represent all of the PDE functions of the self-pronoun. It is not within the scope of this paper to fully examine the remaining functions and their origins, but a representative sample will be discussed below.

<sup>8</sup> See, for example, Dowty (1991).

### 6.1 Locally free reflexives and their relatives

Baker focuses a great deal of attention on what he terms "locally free reflexives" (LFRs) in British literature.<sup>9</sup> These are self-pronouns which are not locally bound, although they may be subcategorized for.

- (37) a. She perceived him soon afterwards looking at herself, and speaking familiarly to her brother...  
 [Austen 253; =Baker's (10d)]
- b. The Miss Dashwoods had no greater reason to be dissatisfied with Mrs. Jennings' style of living, and set of acquaintance, than with her behavior to themselves, which was invariably kind.  
 [Austen 183; =Baker's (15a)]

According to Baker, LFRs are subject to just the same constraints as intensives. In the above examples, each referent is the subject of consciousness in its respective passage, and each is being contrasted with the referents of the underlined NPs. The self-pronoun is essentially equivalent to a pronoun followed by an appositive.

This results in one of the most interesting aspects of LFRs from a historical point of view—their ability to have independent reference. Reflexives, the most common instantiation of self-pronouns, are by definition coindexed with another argument, and thus referentially dependent. And intensives, being modifiers, are not referring at all. Thus it would be interesting to discover the details of the LFR's introduction to English, as it constitutes a significant addition to the self-pronoun's repertoire.

The use of the self-pronoun alone is quite possibly the result of avoiding the repetition that would occur if *her herself* or *them themselves* were used.<sup>10</sup> This type of phenomenon has also been noted by Stemberger (1981). If this was the case, then the question is, when was the pronominal head dropped? Baker claims that LFRs are found only in British English, but is not explicit about whether they occur in certain dialects or registers of British speech as well as literature. If they can occur in speech, or if they could at some point in the past, then did they at any time appear in American English? And how does this relate to the timing of the elision of the pronominal head?

In searching for the answers to these questions, we also want to take into consideration the use of self-pronouns in contrastive predicates, a use which seems to simply be a sub-type of LFR, although Baker does not state this explicitly. The contrastive predicates are the ones headed by terms like *except*, *like*, *as*, *but*, or *than*. The

<sup>9</sup> Baker uses the term "reflexive" to refer to what is here called a self-pronoun, nonetheless recognizing that LFRs are not reflexives in the sense used here.

<sup>10</sup> In this regard, it is also interesting to note that while in ME the subject pronoun was not always overt, the intensive could still show up on its own. In other words, *he himself* was sometimes expressed as *himself*, something which might have facilitated the development of LFRs as a productive phenomenon.

individual conjuncts of coordinate NPs may also be considered contrastive with respect to one another.

- (38) a. John wants to hire someone as smart as himself.  
 b. Bridget feels that Todd is more skilled than herself.  
 c. According to the mayor, investigators have spoken with both herself and her secretary.

Unlike the LFRs in (37), these are acceptable in American English (AmE) as well as British English (BrE), although they may be perceived as formal or stilted. One of the differences between the two is that the LFRs in (37) are selected by a head with a less oblique argument also present. In (37a), *look* has as its arguments both (*at*) *herself*, and the less oblique *him*, while in (37b), *behavior* has (*to*) *themselves* and the less oblique *her*.

In (38), however, the pronoun can be analyzed as not being selected by a head with a less oblique argument. According to Pollard and Sag (1994), in (38a,b) the whole phrases *as himself* and *than herself* constitute the arguments of the respective comparative predicates, so that the pronouns themselves do not have any coarguments. Similarly, in (38c), the head *speak* selects the entire coordinate NP as an argument, not the individual conjuncts. For ease of exposition, I will be referring to the former situation as "locally o-commanded" and the latter as "locally o-free," using terminology from HPSG.<sup>11</sup>

Pollard and Sag claim that this is part of a larger generalization about where non-locally bound self-pronouns can occur in AmE. As long as a self-pronoun is locally o-free, it is acceptable given that the appropriate non-syntactic conditions are met. This has the effect of unifying the self-pronouns in (38) with those in picture NPs (to be discussed further in §6.2).

However, Baker objects to this analysis, on the basis of his belief that not all speakers who use self-pronouns in picture NPs also use them in contrastive predicates. Instead, he suggests that the American speakers who use the LFRs in (38) simply have a few idiosyncratic rules which allow them to be used in these very specific contexts. This usage reflects the remnants of the more general distribution of LFRs in BrE.

Baker is very likely correct that the more general British and the more restricted American uses of LFRs are related, given that they have the same constraints and that one is a subset of the other. But there is support for the argument that the LFRs in AmE are not simply fossilized remnants, but productively available in locally o-free environments.

This support comes from examples of LFRs in other contexts in AmE:

<sup>11</sup> See Pollard and Sag (1994) for precise definitions of these terms.

- (39) John swears that he didn't find a snake next to Mary, he only found one near himself.

In this variation on the common "snake" example the self-pronoun is in a PP adjunct, and is locally o-free because that adjunct has no less oblique argument. It meets the definition of an LFR in that it refers to a discourse prominent person (in this limited context) and is contrasted with another referent, *Mary*. Unless a better analysis exists, this type of pronoun is evidence of the productive use of LFRs in locally o-free contexts other than contrastive predicates.

Again taking a diachronic perspective, it would be enlightening to know how and when this restriction on locally o-commanded non-reflexive self-pronouns arose. In AmE this restriction has virtually no exceptions (but see the discussion of hypercorrective self-pronouns below), leading us to wonder if this has always been the case in AmE.

Finally, there is another use of self-pronouns which is evidently related to LFRs, and generally considered hypercorrective by prescriptive grammarians. For the speakers that use them, they appear most frequently in coordinate NPs. The following examples are from the OJ Simpson criminal and civil trials.

- (40) a. Detective Phillips, this purports to be a telephonic communication between a deputy coroner by the name of Paul Willis and yourself, and I'll ask you if you recognize the voices at some point, okay?  
[OJS 2/16, 1891]
- b. Myself and Ms. Mazzola were the ones who were collecting the items of evidence away from the body.  
[OJS 11/5, 269]
- c. There is an extensive colloquy between himself and counsel in which he refers to one glove that he saw at the foot of Ron Goldman and he says that repeatedly, and yet taking one statement out of context they seek to draw inferences from it.  
[OJS 1/24, 783]

As these examples show, the self-pronoun may be in first, second, or third person, in a subject or an object, and can be the first or the second conjunct. The first two self-pronouns here fit the profile of an LFR, since they refer to a subject of consciousness (the speaker or hearer), and are in a coordinate NP, which can be considered a contrastive environment. But in (40c), the referent of *himself* is not as easily construed as discourse prominent; he is neither directly responsible nor affected by an action, and is not the primary topic of concern (which instead seems to be the opposing legal team the speaker is attacking).

The hypercorrective self-pronoun is also common in other locally o-free environments such as those in (41), including the passive *by*-phrase.

- (41) a. Q. And who was present?  
 A. Just myself. [OJS 1/15, 3731]
- b. The copy is a digitized copy that was made by myself to reduce some background noise, hiss sounds. [OJS 11/20, 131]
- c. A. Yes. We had dinner together at LaQuinta Hotel.  
 Q. Just the two of you?  
 A. No; it was a party of, I would guess, ten people.  
 Q. Friends of both of yourself [sic]? [OJS 12/3, 5622]

The first example is construable as an LFR, since there is contrast between the speaker and others who were not present. But the lack of contrast in (41b,c) shows that these are not LFRs.

It is even possible for this self-pronoun to appear in a locally o-commanded position, although this is much less common.

- (42) a. As the Westec sergeant was passing myself, I stopped him and I said, "is there anybody that's supposed to be at home now," ... [OJS 3/10, 1912]
- b. Please hand your tickets to myself as you board the plane.<sup>12</sup>  
 [United Airlines flight attendant]

The self-pronoun is locally o-commanded in (42a) by the subject (*the Westec sergeant*), and in (42b) by the implicit second person subject.

As was noted above, the American LFRs in (38) have a formal ring to native speakers. Most likely, the people who produced the utterances in (40)-(42) were attempting a high register by using as many self-pronouns as allowed by the grammar, plus a few that might otherwise be considered impossible because of their locally o-commanded position.

In particular, these self-pronouns tend to be in the first person. This could be related to the fact that first person pronouns always represent a subject of consciousness, and therefore meet the discourse prominence requirement for LFRs. Thus they are the most familiar to these speakers as a "formal pronoun."

<sup>12</sup> Thanks to Frederick Parkinson for passing this example on to me.

It is not certain whether these hypercorrective self-pronouns will continue to spread among speakers, or whether their syntactic distribution will be further extended into locally o-commanded positions. Either way, they show us that their formal connotation continues to play a role in the development of self-pronouns, just as it did in OE.

## 6.2 Picture NPs

Like LFRs, self-pronouns in picture NPs may also be referentially independent, but the constraints on their distribution are further removed from those of intensives. For some time it has been recognized that self-pronouns in picture NPs require their referent to be some sort of subject of consciousness. The following examples from Cantrall (1974) show that if a referent is unaware of the circumstances, a self-pronoun does not sound acceptable.

- (43) a. Funny stories about himself won't restore Tom to good humor.  
 b. \*Funny stories about himself won't restore Tom to life.  
 [=Cantrall's (13),(14)]

Cantrall notes that (43b) is improved if one believes that Tom's spirit is hovering nearby, underscoring the fact that Tom's point of view must be available for the self-pronoun to be used.

Similarly, Kuno (1987) constructs examples showing that the referent of the self-pronoun must be construable as the experiencer of the situation being described. (The judgments below are Kuno's.)

- (44) a. ?The minister was worried by the fact that there were pictures of himself with a prostitute in circulation.  
 b. \*The minister was implicated by the fact that there were pictures of himself with a prostitute in circulation.  
 [=Kuno's (11.12)]

In (44a) the verb *worry* suggests that the minister's point of view is being taken, making the self-pronoun acceptable, while the objective presentation of information in (44b) has the opposite effect.

Furthermore, the self-pronoun does not need an antecedent within the sentence, as the following attested example shows.

- (45) After the meeting, Rama returned to his latest project: staging a national, six-month, six hundred and fifty thousand dollar "Zen" seminar promotional campaign. The effort included the placement of a two-page spread in the Sunday New York Times. One page was a photo of himself; the other advertised his free talk on Zen and success at Alice Tully Hall, Lincoln Center.

[Laxer 7507]

This passage concerns cult leader Rama's plans, and is presented from his point of view. These kinds of examples show that the reference of self-pronouns in picture NPs is determined entirely by non-syntactic factors.

While the final word is yet to be said about the constraints on self-pronouns in picture NPs, point of view and subjects of consciousness are a recurring theme in their analyses. This constraint is most likely related to the discourse prominence constraint for intensives, although we cannot rule out the possibility that it arose independently without first knowing exactly how and when picture NPs took their present form. As with the LFRs, further research is needed to determine their origins, and their relationship with other self-pronoun developments.

### 6.3 Locative PP arguments

As we have seen, in PDE local object binding must be marked with a self-pronoun. This is generally true whether there is a direct object or an object in a PP argument:

- (46) a. John<sub>i</sub> likes \*him<sub>i</sub> / himself<sub>i</sub>.  
 b. Mary<sub>i</sub> relies on \*her<sub>i</sub> / herself<sub>i</sub>.  
 c. Mary<sub>i</sub> talked with John about \*her<sub>i</sub> / herself<sub>i</sub>.

However, locative PP arguments behave differently in that they generally allow either a personal pronoun or a self-pronoun.

- (47) a. John<sub>i</sub> put a coat around him<sub>i</sub> / himself<sub>i</sub>.  
 b. Lori<sub>i</sub> put the candles around her<sub>i</sub> / herself<sub>i</sub> to keep the mosquitoes away.  
 c. Patrick<sub>i</sub> spilled the coffee all over him<sub>i</sub> / himself<sub>i</sub>.  
 d. Cindy<sub>i</sub> tattooed a little angel on (?)\*her<sub>i</sub> / herself<sub>i</sub>.

Part of an explanation for this could be that the locative PP sometimes forms an independent predicate which has the verb's direct object as its subject. Thus the PP's object is not always locally bound by the verb's subject.

Rather than go any further into the details of a syntactic account of this distribution, I will instead note some pertinent facts about the connotations of the self-pronoun for native speakers. Kuno suggests that the use of the self-pronoun in these contexts signals that the referent is a "target" of the verb's action. By this he means perhaps among other things) that the referent is either more physically involved in the action, or is the goal of an intentional action.



In Golde (1998) I conduct an extensive survey of native speakers, testing their reactions to the naturalness of each pronoun in various contexts.<sup>15</sup> My results indicate that intentionality does not play a role in acceptability, but that the more physically involved a referent is in the action, the more acceptable speakers find the self-pronoun to be.

For example, subjects found the self-pronoun in a sentence like (47a) to be more natural than that in (47b). In the former, John is more physically involved because he ends up in contact with the blanket, while in the latter, Lori only has brief contact with the candles during their placement. These kinds of contrasts show that it is not the verb which is affecting choice of pronoun, but the context.

In terms of the overall development of self-pronouns, one question is where the connotations of physical involvement arose for these locative PP objects. While there may be no definitive answer, the most likely connection is with the reflexive use of self-pronouns. Since reflexives are by definition part of the verb's argument structure, then if the verb represents some physical action, the reflexive will denote a physically involved referent:

(48) John killed / maimed / poked / hit / stepped on / rubbed / tickled / scratched himself.

When the self-pronoun is used in (47), it already resembles a reflexive in that it has an antecedent within the minimal clause. Thus hearers may infer that the choice of a self-pronoun over a personal pronoun is intended to invoke another feature of reflexives, their referent's physical involvement in the verb's action.

It would also be useful to know more about the development of clause-bound self-pronouns in PPs. Most research up to this point has focused on self-pronouns as the direct objects of verbs (e.g. Ogura and Peitsara). One question that still needs to be addressed is whether these locative PP arguments have always allowed both types of pronoun to be clause-bound, and whether there has always been a preference for self-pronouns in the more physically involved contexts. It is conceivable that the personal pronoun has been generally preferred in the past, and that the use of the self-pronoun to connote physical involvement is a new development. Clarifying the history of pronouns in this position will be difficult, due to the relative infrequency of this exact configuration, but will potentially illuminate other aspects of the development of self-pronouns as well.

<sup>15</sup> Other variables mentioned in this paper as affecting acceptability of self-pronouns, such as point of view and contrast, were held constant by embedding the relevant sentences in dialogues.

## 6.4 The big picture

We have by no means exhausted the functions of PDE self-pronouns,<sup>14</sup> but the preceding discussion should be sufficient to demonstrate that many questions remain if we are to map out their development. Furthermore, I believe that it would ultimately be a mistake to attempt to trace the development of one function while ignoring the development of others. While the various uses of self-pronouns may be considered syntactically and semantically distinct, there is significant overlap of certain features, such as prominence, contrast, and local binding.

This overlap is reminiscent of the morphological "constellations" discussed in Janda and Joseph (1995). A constellation is defined as a grammatical construct which captures the fact that certain morphological rules may have striking similarities to one another which affect their historical development, yet are not similar enough to be collapsible into one basic rule. In the case of self-pronouns, we may find that we are dealing with a sort of lexical constellation. While the wide variety of functions served by self-pronouns precludes their being captured by a single lexical entry, the use of one self-pronoun, say the intensive, may still have the power to influence the development of another self-pronoun, perhaps the picture NP type. Only further research will reveal whether this is the case, but it remains an important possibility, and is a good reason for investigating the development of self-pronouns as a group rather than in isolation.

## 7. Conclusion

This paper has examined the extension of the *self* morpheme from intensive to reflexive, and hypothesized a sequence of small steps leading to this change. In doing so, I have illustrated the need for precision and clarity when dealing with complex functions like intensification. The further developments of self-pronouns, such as LFRs and picture NP objects, also involve subtle constraints requiring careful investigation. With future research, we may be able to identify those constraints as well, and use them to help trace the development of the myriad PDE uses of self-pronouns.

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<sup>14</sup> For example, we should not overlook the adverbial use (*John talked to Mary himself*), the "possessive+self" use (*John is finally back to his old self*), or various idiomatic uses (*John did it by himself*, *Determination in (and of) itself isn't enough*).

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### RULING OUT CHANCE, UNIVERSALITY, AND BORROWING: AN ALTERNATIVE TO RINGE\*

GWANG-YOON GOH

#### 1. RINGE'S (1992) PROBABILISTIC METHOD

Ringe (1992) proposes a mathematical method of determining whether the similarities between the basic vocabularies of putatively related languages are the result of chance or not.<sup>1</sup> Although he claims that his method provides "a completely objective criterion of proof" of putative relationships (p.80), his approach has many problems from both linguistic and methodological standpoints some of which are so serious that they render his method partially or even totally incapable of producing accurate or meaningful results.

Above all, although his method can calculate the probability of there being a particular number of matchings of the same kind between the two relevant sounds in a comparable position, he doesn't provide a way of determining how likely a particular number of recurrent matchings (RMs) are to occur by chance. Thus, his method can only give us some sort of strong impression about language relationships. For example, 16 RMs occurring in the first positions of 70 word pairs of an English and German 100-word list might be impressive enough to make anybody believe the close relationship between English and German. However, it cannot answer what this high number (of RMs) means probabilistically or how different numbers of RMs from different pairs of languages can be compared.<sup>2</sup> This problem forces Ringe to appeal to historical arguments to explain the unexpected two RMs found between English and Turkish, making his argument rather circular (pp. 49-50).

\* An earlier version of this paper was presented at the 24th annual meeting of Berkeley Linguistics Society, University of California, Berkeley, February 1998. I am grateful to the participants for helpful comments. I would also like to thank Mary Beckman, Steve Keiser, and Craig Hilts for their comments on earlier versions of this paper.

<sup>1</sup> He applies his method to four different pairs of languages, i.e., English-German, English-Latin, English-Turkish, and English-Navajo (Ringe 1992). He also uses his method with some revisions to test the controversial Nostratic hypothesis (Ringe 1995), to debunk Greenberg's multilateral comparison of 'Amerind' family (Ringe 1996), and to suggest a genetic relationship between Indo-European and Uralic language families (1998).

<sup>2</sup> For a more detailed discussion of this problem, see Baxter and Ramer (1996).

Furthermore, Ringe's method can easily result in an undesirable conclusion, because his evidence is based on the number of RMs, whose probabilities cannot be nicely combined. For example, if closely-related languages show a smaller number of RMs than distantly-related languages, his method will give us a wrong prediction about the given relationships. Such a case is actually found in Welby & Whitman (1999 in this volume), which adopts Ringe's method and applies it to three pairs of Algonquian languages: remotely-related Ojibwa and Yurok show 8 RMs, while closely-related Ojibwa and Cree on the one hand and Ojibwa and Arapaho on the other show 3 RMs and 4 RMs, respectively. Such problems can be solved by fully appreciating the wisdom of the traditional comparative method.

In short, Ringe's approach, despite its pioneering role, fails to attain its main goal of computing the chance probability for the relationship between putatively related languages. In this paper, I will propose an alternative method, which bases its probabilistic evidence mainly on the convergence of 'similarities'.

## 2. MAKING THE BEST OF PROBABILISTIC EVIDENCE

### 2.1. THE DESCRIPTION OF THE ALTERNATIVE METHOD

For a detailed description and explanation of the general probabilistic methods and procedures involved, which are summarized in (1), I refer to Ringe (1992).

#### (1) General probabilistic methods and procedures involved

- (a) Compile a (Swadesh) word list for the two languages to be compared.
- (b) Choose positions for comparison.
- (c) Calculate the probability of all possible segment correspondences.
- (d) Tabulate the actual matchings.
- (e) Calculate the binomial distribution for  $n$ , a given number of trials, and  $p$ , the probability of a segment correspondence on any trial.<sup>3</sup>
- (f) Sort out RMs.

In this paper, "a set of singular facts" and "the convergence of singular facts" in the traditional comparative method (Meillet 1967: 14) are interpreted as a set of RMs and occurrences of multiple matchings [MMs] (i.e. the occurrence of more than one instance of RMs in a given word pair), respectively. Furthermore, the concept 'similarity' is defined probabilistically. Two sounds in a comparable position are 'similar' if their matching turns out to be an RM (i.e. if it falls in the 99th percentile of their expected range), because their correspondence is very difficult to explain unless they are assumed to be reflexes of the same sound.

The alternative method differs from Ringe's mainly in that RMs and their non-chance probabilities, which, in Ringe's method, are intended to be the main evidence for non-chance relationships, are mainly used here to identify 'similar' sounds between two compared languages. Thus, the non-chance probability of RMs, which cannot be

<sup>3</sup> Probability (=  $P$ ) that there will be  $k$  matches in  $n$  random trials, for any number  $k$ :

$$P = \frac{n!}{k! \times (n-k)!} \times P^k \times (1-P)^{n-k} \quad (\text{cf. Paulos 1988})$$

$n$  = the total number of trials (i.e. the number of word pairs in a given list).

$P$  = probability of a segment correspondence (i.e. probability that two sounds will match) in a comparable position on any trial.

determined against the given total word list, will be reflected in the calculation of the probability of MMs.

The further steps in the alternative method are as follows: first, using the method employed in Ringe (1992), determine what sounds are 'similar' on the basis of RMs actually found; second, determine how many MMs a given pair of languages show; third, calculate the probability of the convergence of 'similarities', on the basis of the frequencies of the sounds in each comparable position, the 'similar sounds' between two languages, and the number of MMs; finally, provide a probabilistic interpretation of the putative relationships.

### 2.3. THE PROBABILITY OF THE CONVERGENCE OF 'SIMILARITIES'

The probability that a MM occurs in a word pair can be calculated, as in (2). Moreover, the probability that a particular number of MMs will occur in the given 100-word list can be calculated by the formula for binomial distribution in (3).

(2) Probability [= P(n)] of an n-tuple RM in a word pair

(a) **P(0)** = probability that no RM occurs in any position

$$= P_{12345} = (1-P_1) \times (1-P_2) \times (1-P_3) \times (1-P_4) \times (1-P_5) \\ = P_1 \times P_2 \times P_3 \times P_4 \times P_5$$

• **P<sub>n</sub>** (or **P<sub>n</sub>**) = probability that an RM (or no RM) occurs in n-th position

e.g. if the first positions show three RMs: d-t, s-s, k-k,

$$P_1 = (\#d/100 \times \#t/100) + (\#s/100 \times \#s/100) + (\#k/100 \times \#k/100)$$

(b) **P(1)** = probability that any RM occurs in a word pair.

$$= P(2) + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345}) = 1 - P(0)$$

• **P<sub>12345</sub>** = probability that we have an RM only in the first position

$$= P_1 \times (1-P_2) \times (1-P_3) \times (1-P_4) \times (1-P_5)$$

(c) **P(2)** = probability that any multiple RM occurs in a word pair.

$$= P(3) + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345} + \\ P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345}) \\ = \{1 - P(0) - P^*\}, \text{ where } P^* \text{ is the probability of any non-multiple RM.} \\ = (1 - P_{12345} - [P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345}])$$

(d) **P(3)** = probability that any more-than-double RM occurs in a word pair.

$$= P(4) + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345} + \\ P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345})$$

(e) **P(4)** = probability that any more-than-triple RM occurs in a word pair.

$$= P(5) + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345})$$

(f) **P(5)** = probability that any quintuple RM occurs in a word pair.

$$= P_{12345} = P_1 \times P_2 \times P_3 \times P_4 \times P_5$$

- (3) Probability that a particular number of MMs occur in the given 100-word list

$$\frac{100!}{x!(100-x)!} \times P^x \times (1-P)^{100-x}, \text{ where } P = P(n) \text{ and } x = \text{the number of MMs.}$$

### 3. INVESTIGATION AND RE-INTERPRETATION OF THE DATA

#### 3.1. ENGLISH-GERMAN

The first 100-word list of English and German (cf. Ringe 1992: 83-85) shows many 'similar' sounds in each of the comparable positions, and the corresponding probabilities P1, P2, P3, P4, and P5 (i.e. probability that any real match occurs in each chosen position in a word pair) can be given, as in (4)-(8):<sup>4</sup>

- (4) First position [cf. Ringe 1992: 19, 24]

similar sounds (frequencies)	number of matches
[1] s - s (14 - 8)	5
[2] s - z (14 - 7)	6
[3] b - b (10 - 8)	5
[4] h - h (9 - 9)	6
[5] Ø - Ø (8 - 9)	8
[6] n - n (8 - 5)	5
[7] f - f (8 - 11)	8
[8] w - v (7 - 8)	4
[9] l - l (5 - 5)	4
[10] m - m (5 - 4)	3
[11] t - c (5 - 3)	3
[12] k - k (5 - 7)	3
[13] r - r (4 - 5)	3
[14] d - t (4 - 3)	2
[15] g - g (3 - 5)	3
[16] Ø - d (2 - 2)	2

Total: 16 pairs of similar sounds (= recurrent matchings) in 70 word-pairs

$$\bullet P1 = (\#s/100 \times \#s/100) + \dots + (\#Ø/100 \times \#d/100) = 768/10000 = 0.0768$$

- (5) Second position [cf. Ringe 1992: 25-6]

similar sounds (frequencies)	number of matches
[1] l - l (7 - 7)	5
[2] r - r (5 - 7)	4
[3] t - t (3 - 4)	3

Total: 3 pairs of similar sounds in 12 word-pairs

$$\bullet P2 = (7/100 \times 7/100) + (5/100 \times 7/100) + (3/100 \times 4/100) = 96/10000 = 0.0096$$

<sup>4</sup> The comparable compositions are determined, as in Ringe (1992): first position = initial consonants; second position = second consonants of the initial clusters; third position = consonants right after the first-syllable vowel nucleus; fourth position = second consonants after the first-syllable vowel nucleus; fifth position = final syllables.



## (6) Third position [cf. Ringe 1992: 27-31]

similar sounds	(frequencies)	number of matches
[1] Ø - Ø	(18 - 10)	7
[2] n - n	(15 - 17)	9
[3] t - s	(13 - 11)	8
[4] r - r	(10 - 13)	7
[5] l - l	(8 - 7)	4
[6] d - t	(6 - 7)	3
[7] m - m	(5 - 5)	3
[8] s - z	(3 - 3)	2
[9] s - s	(3 - 3)	3
[10] ʒ - ʒ	(3 - 3)	3
[11] v - b	(2 - 2)	2

Total: 11 pairs of similar sounds in 51 word-pairs

$$\bullet P3 = (\#Ø/100 \times \#Ø/100) + \dots + (\#v/100 \times \#b/100) = 862/10000 = 0.0862$$

## (7) Fourth position [cf. Ringe 1992: 32-3]

similar sounds	(frequencies)	number of matches
[1] d - d	(6 - 8)	3

Total: 1 pair of similar sounds in 3 word-pairs<sup>5</sup>

$$\bullet P4 = 48/10000 = 0.0048$$

## (8) Fifth position (= final syllable) [cf. Ringe 1992: 34-5]

similar sounds	(frequencies)	number of matches
[1] əɾ - əɾ	(4 - 4)	4

Total: 1 pair of similar sounds in 4 word-pairs

$$\bullet P5 = 16/10000 = 0.0016$$

On the basis of the probabilities for any RMs in each of the positions determined above, we can calculate the probability for convergence of similarities. For English and German, all the probabilities for all the different kinds of MMs are provided in order to show how each probability is calculated. However, P(2) (= probability that any multiple match occurs in a word pair) alone will often be enough to provide the probabilistic interpretation of the putative relationship. Thus, P(2), P(3), P(4), and P(5) are given in (9), (10), (11), (12), respectively.

## (9) P(2) (= probability that any multiple RM occurs in a word pair)

$$\begin{aligned} P(2) &= P(3) + \text{Probability of any double RM in any positions} \\ &= 1 - \{P(0) + P''\}, \text{ where } P'' \text{ is the probability of any non-multiple RM.} \\ &= 1 - \{P_{12345} + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345})\} \\ &= 1 - 0.990935836 = 0.009064164 < 0.009 < 0.01 \end{aligned}$$

<sup>5</sup> This number of matches falls just below the 99th percentile. See Ringe (1992: 33).



$P_{12345} = 0.0783120573$ ;  $P_{12345} = 0.0040040859$ ;  $P_{12345} = 0.0013304174$

(10) P(3) (= probability that any more-than-double RM occurs in a word pair)

$$\begin{aligned} P(3) &= P(4) + \text{Probability of any triple match in a word pair} \\ &= P(4) + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345} + \\ &\quad P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345}) \\ &= 0.00011586 < 0.00012 \end{aligned}$$

(11) P(4) (= probability that any more-than-triple RM occurs in a word pair)

$$\begin{aligned} P(4) &= P(5) + \text{Probability of any quadruple matching} \\ &= P(5) + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345}) \\ &= 0.00000045 < 0.0000005 \end{aligned}$$

(12) P(5) (= probability that we have any quintuple RM in a word pair)

$$\begin{aligned} P(5) &= P_{12345} = P_1 \times P_2 \times P_3 \times P_4 \times P_5 \\ &= 0.000000004880911565 < 0.000000005 (= 5^{-10}) \end{aligned}$$

The first 100-word list of English and German shows 55 MMs: 3 word pairs show quadruple RMs, 17 word-pairs show triple RMs, and 35 word pairs show double RMs (cf. Ringe 1992: 35-7). The binomial distribution for each type of MMs with P(2), P(3), and P(4), respectively, can be computed by using the formula in (3), as in (13):

(13) The binomial distribution for each type of MMs with P(2), P(3), and P(4)

(a) Distribution for a MM of P(2) [0.01]

no. matches	%	cumulative
0	36.60323413	36.60323413
1	36.97296377	73.5761979
2	18.48648188	92.06267978
3	6.099916581	98.162596361
4	1.494171486	99.656767847
5	0.2897787124	99.9465465594
6	0.04634508026	99.99289163966
7	0.006286345663	99.999177985323
8	0.0007381693771	99.9999161547001
9	0.0000762195092	99.9999923742093
10	0.000007006035694	99.999999380244994
11	0.0000005790112144	99.9999999592562084
12	0.00000004337710276	99.99999999263331116
:	:	:

(b) Distribution for a MM of P(3) [0.00012]

no. matches	%	cumulative
0	98.80710014	98.80710014
1	1.185827501	99.992927641
2	0.007044660715	99.999972301715
3	0.00002761838421	99.99999992009921
4	0.00000008037914355	99.9999999.....
:	:	:

## (c) Distribution for a MM of P(4) [0.000005]

no. matches	%	cumulative
0	99.99500012	99.99500012
1	0.004999752506	99.999999872506
2	0.0000001237439364	99.9999999962499364
3	0.000000000002021152	99.999999996251957552
:	:	:

As we can see from the given binomial distributions, the probability of the 55 MMs occurring with the probability P(2) [0.01] is extremely small so it can exclude almost any possibility of chance. This then is precisely the probabilistic evidence for the close non-chance relationship between English and German which can replace the 'strong impression' about the closeness of the language relationship. Furthermore, this evidence is much more decisive than the probabilistic evidence (based on the number of RMs) which Ringe (1992) attempts in vain to provide.

## 3.2. ENGLISH-LATIN

The numbers of the 'similar' sounds found in the first 100 words of the Swadesh list for English and Latin are given in (14)-(17).<sup>6</sup> The corresponding probabilities P1, P2, P3, P4, and P5 can be calculated in the way described in (2a), as follows:

## (14) First position

- 7 pairs of similar sounds in 31 word-pairs

$$\begin{aligned} \bullet \text{P1} &= (8/100 \times 22/100) + (9/100 \times 14/100) + (14/100 \times 9/100) + (8/100 \times \\ &8/100) + (8/100 \times 7/100) + (4/100 \times 3/100) + (5/100 \times 2/100) \\ &= 570/10000 = 0.057 \end{aligned}$$

## (15) Second position

- 1 pair of similar sounds in 2 word-pairs

$$\bullet \text{P2} = 3/100 \times 2/100 = 6/10000 = 0.0006$$

## (16) Third position

- 2 pairs of similar sounds in 12 word-pairs

$$\bullet \text{P3} = (10/100 \times 16/100) + (13/100 \times 10/100) = 290/10000 = 0.029$$

## (17) Fourth and other positions

- No matches

$$\bullet \text{P4} = 0; \quad \text{P5} = 0$$

On the basis of the probabilities for any RM in each position, we can calculate the probability that any MM occurs in a given word pair for English and Latin. Here, the

<sup>6</sup> For the first 100-word list, see Ringe (1992: 83-85). As for the 'similar' sounds found in each comparable position, on the other hand, refer to Ringe (1992: 41, 14, 44-47).

calculation of  $P(2)$  alone is enough to provide the necessary probabilistic interpretation of the putative relationship.

(18)  $P(2)$  (= probability that any multiple RM occurs in a word pair)

$$\begin{aligned} P(2) &= P(3) + \text{Probability of any double RM in any positions} \\ &= 1 - \{P(0) + P^*\} \\ &= 1 - \{P_{12345} + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345})\} \\ &= 1 - 0.998297384 = 0.001702616 < 0.002 \end{aligned}$$

The first 100 words of the Swadesh list for English and Latin show 9 MMs (cf. Ringe 1992: 47). The binomial distribution for a MM with  $P(2)$  [0.002] can be computed, as in (19).

(19) The binomial distribution for a MM with  $P(2)$  [0.002]

no. matches	%	cumulative
0	81.8566805	81.8566805
1	16.40414438	98.26082488
2	01.627264824	99.888089704
3	0.106527691	99.994617395
4	0.005176947	99.999794342
5	0.000199193147	99.999993535147
6	0.000006320424	99.99999855571
:	:	:

The probabilistic interpretation of 9 MMs in the English and Latin word is that the occurrence of the given number of MMs is extremely difficult to explain by chance. Even though the non-chance probability for those 9 MMs is not so big as the one for the 55 MMs from English and German, it is still big enough to exclude almost any possibility of chance. Such a probability can be said to reflect our strong impression about the closeness of the non-chance relationship between English and Latin as well as the difference we feel between the relationships of the two pairs of languages (i.e. English and German, on the one hand, and English and Latin, on the other).

### 3.3. ENGLISH-TURKISH

The first 100-word list of English and Turkish (cf. Ringe 1992: 86-89) shows several 'similar' sounds in some of the comparable positions (i.e. the third and fourth positions). They are given in (20) through (23), along with the corresponding probabilities  $P_1$ ,  $P_2$ ,  $P_3$ ,  $P_4$ , and  $P_5$ .

(20) First position (cf. Ringe 1992: 14, 48-9)

- 2 pairs of similar sounds in 8 word-pairs: 6 [b-k] (10-17); 2 [y-s] (2-6)
- $P_1 = (10/100 \times 17/100) + (2/100 \times 6/100) = 182/10000$

(21) Second position

- No similar sounds<sup>7</sup>
- $P_2 = 0$

<sup>7</sup> No RMs are found since there are no initial clusters in Turkish.

The sounds in the third position and their frequencies are given in (22a), the expected chance matchings for all possible pairs are given in table 1 in (22b), and the numbers of matchings actually found are given in table 2 in (22c), as follows:

## (22) Third position

## (a) Frequencies of consonants

English		Turkish	
Ø	18	r	15
n	15	l	13
t	13	Ø, n, m	9 each
r	10	ğ	8
l	8	t	7
d	6	s, z	6 each
m	5	k	4
k	4	ç, p	3 each
θ, s, ş, g, ğ	3 each	j, d	2 each
v	2	b, v, s, h	1 each
p, f, ð, z	1 each		
total	100	total	100

## (b) Expected chance matchings in the third consonants, English-Turkish

Tk Eg	r (15)	l (13)	Ø, n, m (9 each)	ğ (8)	t (7)	s, z (6 each)	k (4)	ç, p (3 each)	j, d (2 each)	b, v, s, h (1 each)
Ø (18)	2.7	2.34	1.62	1.44	1.26	1.08	0.72	0.54	0.36	0.18
n (15)	2.25	1.95	1.35	1.2	1.05	0.9	0.6	0.45	0.3	0.15
t (13)	1.95	1.69	1.17	1.04	0.91	0.78	0.52	0.39	0.26	0.13
r (10)	1.5	1.3	0.9	0.8	0.7	0.6	0.4	0.3	0.2	0.1
l (8)	1.2	1.04	0.72	0.64	0.56	0.48	0.32	0.24	0.14	0.08
d (6)	0.9	0.78	0.54	0.48	0.42	0.36	0.24	0.18	0.12	0.06
m (5)	0.75	0.65	0.45	0.4	0.35	0.3	0.2	0.15	0.1	0.05
k (4)	0.6	0.52	0.36	0.32	0.28	0.24	0.16	0.12	0.08	0.04
θ (3)	0.45	0.39	0.27	0.24	0.21	0.18	0.12	0.09	0.06	0.03
s (3)	0.45	0.39	0.27	0.24	0.21	0.18	0.12	0.09	0.06	0.03
s (3)	0.45	0.39	0.27	0.24	0.21	0.18	0.12	0.09	0.06	0.03
g (3)	0.45	0.39	0.27	0.24	0.21	0.18	0.12	0.09	0.06	0.03
ğ (3)	0.45	0.39	0.27	0.24	0.21	0.18	0.12	0.09	0.06	0.03
v (2)	0.3	0.26	0.18	0.16	0.14	0.12	0.08	0.06	0.04	0.02
p (1)	0.15	0.13	0.09	0.08	0.07	0.06	0.04	0.03	0.02	0.01
f (1)	0.15	0.13	0.09	0.08	0.07	0.06	0.04	0.03	0.02	0.01
ð (1)	0.15	0.13	0.09	0.08	0.07	0.06	0.04	0.03	0.02	0.01
z (1)	0.15	0.13	0.09	0.08	0.07	0.06	0.04	0.03	0.02	0.01

Table 1

(c) Numbers found for matchings of the third consonants, English-Turkish<sup>4</sup>

Tk Eg	r (15)	l (13)	Ø (9)	n (9)	m (9)	ğ (8)	t (7)	s (6)	z (6)	k (4)	ç (3)	p (3)	j (2)	d (2)	b, v, s, h (1 each)
Ø (18)	3	2		3	2	1	2		3	1	1				
n (15)	4	1	1	1	2	2		2		1					1 (v)
t (13)			3	1	1	2	1			2			[2]		1 (s)
r (10)	1	2		2				2			1	1			1 (b)
l (8)	3	2				1	1				1				
d (6)			1	1	1			1	1						1 (h)
m (5)		1					1		1				[2]		
k (4)	2		1		1										
θ (3)		1				1		1							
s (3)				1	1		1								
s (3)			[2]				1								
g (3)			1		1							1			
ŋ (3)		1			1				1						
v (2)	1					1									
p (1)			1												
f (1)												1			
ð (1)			1												
z (1)	1														

Table 2

(d) Similar sounds based on RMs

similar sounds (frequencies)	number of matches
[1] t - j (13 - 2)	2
[2] m - d (5 - 2)	2
[3] s - l (3 - 13)	2

Total: 3 pairs of similar sounds in 6 word-pairs

$$\bullet P_3 = (13/100 \times 2/100) + (5/100 \times 2/100) + (3/100 \times 13/100) = 75/10000$$

As for the sounds in the fourth position, their frequencies, the expected chance matchings for all possible pairs, and the numbers of matchings actually found are given in (23), as follows:

(23) Fourth and other positions &gt; no RM &gt; no similar sounds

$$\bullet P_4 = 0; P_5 = 0$$

(a) Frequencies of consonants

English		Turkish	
Ø	86	Ø	79
d	6	m	12
t	3	n	3
n, k	2 each	d, r	2 each
θ	1	z, k	1 each
total	100	total	100

<sup>4</sup> The numbers in brackets are the matchings (i.e. RMs) which cross the 99th percentile threshold.

## (b) Expected chance matchings for the fourth consonants, English-Turkish

Eg	Tk	Ø (79)	m (12)	n (3)	d, r (2 each)	z, k (1 each)
Ø (86)		67.94	10.32	2.58	1.72	0.86
d (6)		4.74	0.72	0.18	0.12	0.06
t (3)		2.37	0.36	0.09	0.06	0.03
n (2)		1.4	0.24	0.06	0.04	0.02
k (2)		1.4	0.24	0.06	0.04	0.02
ø (1)		0.79	0.12	0.03	0.02	0.01

Table 3

## (c) Numbers found for matchings of the fourth consonants, English-Turkish

Eg	Tk	Ø (79)	m (12)	n (3)	d (2)	r (2)	z (1)	k (1)
Ø (86)		69	9	3	2	1	1	1
d (6)		5	1					
t (3)		3						
n (2)		1						
k (2)		1						
ø (1)						1		

Table 4

On the basis of the probabilities for any real matches (i.e. P1, P2 and etc.) in the first 100 words of the Swadesh list for English and Turkish, we can calculate the probability that any MM occurs in a given word pair, as in (24) below:

(24) P(2) (= probability that any multiple RM occurs in a word pair)

$$\begin{aligned}
 P(2) &= P(3) + \text{Probability of any double RM in any positions} \\
 &= 1 - \{P(0) + P''\} \\
 &= 1 - \{P_{12345} + (P_{12345} + P_{12345} + P_{12345} + P_{12345} + P_{12345})\} \\
 &= 0.0001365 < 0.00014
 \end{aligned}$$

$$P_{12345} = 0.9744365; P_{12345} = 0.0180635; P_{12345} = 0 (< P2=0)$$

$$P_{12345} = 0.0073635; P_{12345} = 0 (< P4=0); P_{12345} = 0 (< P5=0)$$

No MM is found in the first 100 words of the Swadesh list for English and Turkish and this can be verified by the fact that every pair of similar sounds found in the list occurs in a different word pair, as in (25).

(25) Word pairs which show similar sounds

(a) First position (2 pairs of similar sounds in 8 word-pairs)

word pair	meaning	matching	word pair	meaning	matching
bark - kabuk	'bark'	(b - k)	blood - kan	'blood'	(b - k)
belly - karin	'belly'	(b - k)	bone - kemik	'bone'	(b - k)
bird - kus	'bird'	(b - k)	yellow - sari	'yellow'	(y - s)
black - kara	'black'	(b - k)	you - sen	'you'	(y - s)

### (B) Second position (3 pairs of similar sounds in 6 word pairs)

word pair	meaning	matching	word pair	meaning	matching
night - gece	'night'	(t - j)	fish - balik	'fish'	(s - l)
hot - sıcak	'hot'	(t - j)	ash - kül	'ash'	(s - l)
human - adam	'human'	(m - d)			
woman - kadın	'woman'	(m - d)			

The binomial distribution for a MM with P(2) [0.00014] is given, as in (26):

#### (26) Binomial distribution for a MM with P(2) [0.00014]

no. matches	%	cumulative
0	98.60965778	98.60965778
1	1.380728511	99.990386291
2	0.009569788351	99.999956079351
3	0.00004377196013	99.99999985131113
:	:	:

What this binomial distribution in (26) means is that the occurrence of one MM is more than 98.6 % non-chance, which means that out of 1000 hundred-word lists (or out of 100000 word pairs) we can expect one MM in 14 lists. This again means that although finding a MM or two in some of the lists is possible and expected, it will still be very difficult. This is compatible with the fact that no MM was found in the given 100-word list.

In addition, the given probabilistic interpretation supports our expectation about the putative relationship between English and Turkish based on the comparative method. Thus, unlike Ringe (1992), we don't have to appeal to any extra-probabilistic arguments such as historical arguments for English and Turkish.

#### 4. CONCLUSION

In this paper, I have proposed an alternative probabilistic method for determining the (non-)chance relationship between putatively related languages. In particular, the probabilistic evidence (especially, P(2) and the corresponding binomial distribution) based on MMs representing the convergence of similarities has been used to provide a better probabilistic prediction and to deal with problems remained unsolved in Ringe (1992).

In short, supporting and debunking claims or hypotheses about language relationships should be one of the main goals of the probabilistic methods. Considering the fact that the probabilistic method is rarely necessary for closely related languages such as English and German, the further demonstration of validity of the current method as a sifting device in more difficult cases is a pressing need.

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**Evaluating Semantic Shifts: The Case of Indo-European  
\*(s)meuk- and Indo-Iranian \*muč-<sup>1</sup>**

Brian D. Joseph & Catherine S. Karntis

**1. INTRODUCTION**

Progress towards a general theory of semantic change has always been slow, and thus an independent characterization of what constitutes a "natural" semantic change has proven somewhat elusive. It is not clear, for instance, if there is a well-enough articulated theory of lexical semantics to allow for conclusions about changes in the signified associated with a given signifier, and if there is such a theory, it has not been well-integrated into historical studies. Nor is it clear that there is any sort of independent corroboration available to back up any tentative conclusions one might reach about the naturalness of a given semantic change.

The situation is different with change in other components of the grammar. For instance, sound change has phonetics as a point of reference to provide guidance on naturalness, as work such as Ohala 1993 has demonstrated. Morphological change has its own set of naturalness constraints based on assumptions about human cognitive faculties, as is evident in work such as Anttila, 1989 and Dressler et al. 1987. Finally, syntactic change has the benefit of a well-worked-out theory<sup>2</sup> that can provide the limits on possible syntactic changes, as the work of Lightfoot, e.g. Lightfoot 1979, 1991, among others, has shown.

<sup>1</sup>This paper was conceived, researched, and drafted while the second author was at Ohio State, though she is now at the Muktabodha Indological Research Institute in Ganeshpur, India. A version of this paper was presented at the annual meeting of the Linguistic Society of America in Los Angeles, in January 1999. We would like to thank Richard Janda, Robert Rankin, Martha Ratliff, and Elizabeth Traugott for helpful comments on previous versions. All remaining errors are our own.

<sup>2</sup>Indeed, in the case of syntax, we might well say that there are *several* well-worked-out theories!

To some extent, what has made the task so tricky with regard to semantic change is that so much of what goes on with changes in the meaning of a word is tied to the socio-cultural setting for a language (cf. Fortson 1999). The classic example of English *bead* changing in meaning from 'prayer' to 'small round glass object' is a case in point, for it is only in the context of counting of prayers on rosaries that the reanalysis that led to this innovation in the *signifié* for the *signifiant bead* makes sense.

Even the recent attempts within what has come to be called "grammaticalization theory" to invest changes in the grammatical status of morphemes with a cognitive dimension that correlates with the typical changes from concrete to abstract meanings, from lexical to grammatical meanings, etc. would founder on examples such as *bead*.

As a result, much of traditional work on semantic change has used the methodology of giving primacy to matches in form, and letting semantic mismatches between putative cognate forms be handled by searching for parallels, on the assumption that what can happen (presumably) independently twice or more has a chance of being a natural development, a better chance really than any isolated unique events in this domain.<sup>3</sup>

Actually, the same methodology is used in investigations into change in other components, even though they have other ways of gauging naturalness, as noted above. That is to say, working towards understanding of change in *any* component requires some searching for parallels -- the more examples we find of *t > ts > s* before *i*, or of the elimination of allomorphy in paradigms, the safer we feel in believing such changes to be natural, and thus ones that a theory of change ought to be able to account for, and if it turns out that there is corroborating evidence in articulatory phonetics, or in X-bar theory, or whatever, so much the better.

In this paper, we present the results of an investigation, largely following this traditional methodology of looking for parallels to get a handle on the wide range of semantic extension, into the semantics of two Indo-European roots. Using such a methodology has proven very fruitful in opening up a particularly interesting line of inquiry into the semantic history of these two roots. Thus, to the extent that these findings have some value, they provide some justification for the utility of this method; it may be an imperfect methodology, but it can be useful. At the same time, though, the limitations of this methodology emerge from these studies.

<sup>3</sup>Sihler (1995:163) offers a good example of such methodology. In talking about the Proto-Indo-European root for 'snow', as in English *snow*, Latin *nix*, etc., he says that the Vedic root *\*snh-*, occurring in Skt. *o*-grade *o*-stem *sneha-* and a sizable family of derivatives, reconstructs flawlessly to a root *\*sneygʰ-*. But the meanings -- 'be sticky, viscid; feel affection for', *sneha-*, 'greasiness; love(!)' -- are hard to reconcile with 'snow'. Nevertheless, given the quality of the formal fit, the connectoin would be likely however improbable the semantics; and besides there are actual In[do-]Ir[anian] attestations of the usual 'snow' sort in Av[estan] *snaēza-* v[er]b., Prāk[rit] *siṅha* n[oun]". We are grateful to Rich Janda for bringing this passage to our attention.

## 2. A CASE-STUDY: PROTO-INDO-EUROPEAN \*(s)meuk-

The first part of our case-study is the root \*(s)meuk-. This root shows a broad semantic range across the whole family, taking in (so Pokorny 1959, Mayrhofer 1956) forms with meanings pertaining to SLIPPERINESS (Pokorny's meaning group A) and to RELEASE (Pokorny's group B) that are rather semantically divergent, e.g. Latin *mūcus* 'mucus', *ē-mungere* 'blow one's nose'; Sanskrit *muc-* 'release, loosen'; Lithuanian *maukti* 'strip off, wipe' *mūkti* 'slip away, escape', Latvian *muki* 'flee, disengage'; Tocharian A *muk-*, Tocharian B *mauk-* 'let go, give up, abandon'; Greek ἀπο-μύσσειν 'wipe the nose'. There are also forms with clear initial *s-* that apparently belong here, such as Greek σμύσσειται 'wipes the nose' and Old Church Slavonic *smykati se* 'creep', and provide a basis for a reconstruction \*smeuk- that goes along with the form \*meuk- that other forms point to; we thus cite the root simply as \*(s)meuk-.<sup>4</sup>

Moreover, the derivatives of \*(s)meuk- in individual languages also show a broad semantic range; our particular focal point here is Sanskrit *muc-*, whose meanings appear to mostly fall on the RELEASE side of \*(s)meuk-, though some of the Classical Sanskrit meanings admittedly may not obviously fit that characterization, a point taken up in greater detail later on. For Vedic Sanskrit, a basic meaning for *muc-* has been given variously as 'untie, unfasten, loosen' (Grassmann 1872, 'losmachen, losbinden') or 'strip off' (Mayrhofer 1956, 'abstreifen'), and Grassmann 1872 gives the meanings for the active voice of 'untie, loosen, unfasten, set free; release (streams); undo, dissolve; cause to disappear' and for the middle voice, 'disengage'. For Classical Sanskrit, Monier-Williams 1899, MacDonnell 1929, and Apte 1912 give a range of meanings covering 'loosen, set free, release, let go, let loose, deliver; relax (the throat); slacken (reins); let live, spare; leave, abandon, quit, give up; quit (the body), die; set apart; dismiss, send away; cast, throw, hurl, discharge; emit, drop, shed, let fall; utter, give forth; give away, grant, bestow; put on (middle voice); void (excrement); sacrifice; deceive, cheat'.

Sanskrit also offers the possibility of the addition of adverbial modifiers (so-called "preverbs") to roots, and with *muc-*, various preverbs allow the root to cover an even greater conceptual space, including some highly specialized senses. For Vedic, the sense of 'loosen' shows several instances with preverbs specialized to meanings of 'loosen (a mantle or garment) by motion' (e.g. RV 1.116.10: object *drapim* 'garment', preverb *pra*) and the opposite 'put on (a garment)' (e.g. RV 4.53.2: object (again) *drapim*, preverb *prati*). For Classical Sanskrit, combinations as in Vedic occur, as well as plus 'put on (garments, shoes)', with the preverb *bupa-*, and 'take off (shoes)' with the preverbs *vi-ava-*.

There is relevant evidence bearing on the meaning of Sanskrit *muc-* from Iranian. As it happens, there are no verbal forms that survive in Iranian from Proto-Indo-Iranian \*muč-, but some frozen, isolated derivatives occur, and interestingly, these show, as do forms in later Sanskrit, foot-related

<sup>4</sup>The presence versus absence of *s-* in these forms is of course a significant concern, one for which we offer no explanation, except to note that it seems to be part of the same well-known but poorly understood phenomenon, the so-called "s-mobile", that numerous other roots in Indo-European show.

specializations: Avestan (Old Iranian) *fra-muxti* 'taking off of footwear', *paiti-šmuxta-* 'having shoes'; Pahlavi (Middle Iranian) *močak* 'boots', *mozag* 'shoe'; Modern Persian *moza* 'leather socks'. These forms, as the only testimony for this root on the Iranian side of the Indo-Iranian branch to which Sanskrit belongs, allow for the interesting hypothesis that the foot specialization may be of Proto-Indo-Iranian age, despite the lateness of its emergence in Sanskrit, but such an hypothesis does not really explain this specialization. Alternatively, these Iranian forms could be independent instances of the foot-specialization, and thus provide parallels to the development in later Sanskrit, about which more below.

### 3. SOME QUESTIONS

The facts in §2 raise several interesting questions. The first is a general one, namely whether all the various Indo-European meanings — the "slippery" ones as well as the "release" ones — are really to be related to a single root, and even if they can be combined (as suggested above), whether they should be combined or instead separated into two distinct but homophonous roots. Relevant here is the suggestion of Grassmann 1872 that two different "Grundbedeutungen", and thus possibly two distinct roots, are involved, based on the fact that Old Church Slavonic forms that seem to be related have meanings pertaining to 'moistness' (*moknati* 'become moist', *močiti* 'moisten', *mok-rū* 'moist').

Two other questions pertain to some of the specialized senses discussed above. In particular, can the opposite meanings of Sanskrit *muc-* with garments, 'put on' as well as 'take off', be reconciled with one another? Also, is it possible to make sense of the specialization to foot-related meanings that some instances of *muc-* in Sanskrit show, and to determine their relation to the meanings of Iranian *(s)mucē-*?

Finally, a question having to do with a specialized meaning but similar to the general question of how many \*(s)meuk-'s there are arises with the meaning 'cheat' found for *muc-* in later Sanskrit. In particular, is the 'cheat' meaning from the same root as the 'release' meanings, or is this *muc-* instead an alteration of a different root *mac-*, as Monier-Williams 1899 suggests?

### 4. TOWARDS SOME ANSWERS — SOME PARALLELS

For some of these issues, a tentative answer can be arrived at simply by attempting to provide a rationale for the developments in question. For instance, with regard to the general question of whether the basic meanings of derivatives of \*(s)meuk-, the SLIPPERY and the RELEASE meanings, can be joined into a single meaning, reasoning alone suggests a formulation that covers both apparent senses of \*(s)meuk- in a unified way: 'set something into a (relatively) frictionless motion; cause a change of state or position through a medium with minimal resistance'.

Similarly, to deal with the issue of opposite meanings, the 'put/take on/off' meanings can be attributed to the contribution of the preverb modifiers, and make sense in combination with various preverbs. For instance, *vi-muc-*, meaning 'take off' has the preverb *vi-*, whose usual meaning involves dispersal away from or division; on the other hand, *prati-muc-* 'put on' has

the preverb *prati-*, whose usual meaning involves direction towards. This account is in keeping with the above suggestion of a basic sense for PIE \*(s)meuk-, and thus presumably for Sanskrit *muc-* originally at first, having to do with unimpeded motion, that is, more a matter of the manner of motion than the direction or intensity or the like — the preverbs then modify this basic sense by giving a directionality, 'motion onto' vs. 'motion off of'.

Even so, being able to come up with rationalizations such as these is not the same as showing that meanings of these sorts can be instantiated in natural language; thus an empirical basis is needed for any answers to any of these questions. Indeed, such a basis can be found by the traditional methodology of looking for parallels for each of the semantic developments that give cause for the questions in §3, and importantly, there are several rather direct parallels, some of them quite striking.

In particular, with regard to the semantic specialization to foot-related meanings, once the meanings of 'put on/take off garments' are available, a specialization to a particular type of apparel, namely apparel for the feet, is not unthinkable a priori, and moreover it has parallels, both within Indo-European and outside of Indo-European. Within Indo-European, the root \*eu- 'put on' (Pokorny 1959:346) is the basis for the semantically general Armenian *aganim* 'put on clothing' and Latin (*ind*)-*uō* 'put on (a garment)' as well as two foot-specific forms, Avestan *aōbra* 'shoes' and Lithuanian *aiti* 'to put on shoes'. If the original meaning is the broad one that Pokorny suggests, then the Avestan and Lithuanian forms show a specialization in meaning similar to the Indo-Iranian ones with \*(s)meuk-. It is true, though, that Beekes (1995:36) gives the specialized sense of 'put on footwear' as the Proto-Indo-European meaning of \*eu-; if he is right, then the general sense found in Armenian *aganim* and Latin (*ind*)-*uō* would be the innovation — a broadening — and not the specialization seen in the Avestan and Lithuanian forms. Still, in that case, though there would no longer be a direct parallel, these forms taken together would still constitute a family of words in which both footwear-specific meanings and general dressing meanings coexist, suggesting a naturalness to connections between such meanings.

Moreover, there is a parallel outside of Indo-European, suggesting a naturalness to the posited specializations with \*(s)meuk-. In Turkish, the verb *çekmek* means 'to draw, pull, tug; put on (clothing)' and it has a derivative *çekme* which means 'trousers' but also, significantly for the case in question here, 'boots'. The basic sense of the derivative is apparel that one draws on oneself, and that meaning apparently has been specialized to a type of foot-apparel, as with \*(s)meuk- in Indo-Iranian. This example, by the way, raises the possibility of a Central Asian semantic extension of PUT ON → PUT ON SHOES that may have diffused from Iranian into Turkic, or vice-versa, though it could very well just represent an independent extension in each of these groups.

##### 5. A VERY STRIKING PARALLEL: ENGLISH *slip* (PIE \*(s)lei-b-)

The search for similar semantic changes yields a strikingly uncanny and thorough-going parallel for the extensions of meaning seen with \*(s)meuk-

think (as Grassmann suggested) that there must be two roots intertwined in \*(s)meuk-.

Finally, regarding what to make of the 'cheat' meaning of *muc-*, *slip* shows that separating *muc-* 'cheat' from other *muc-*'s need not be done if this decision is based solely on the disparity in meaning (which apparently was Monier-Williams' basis).

The extent of the parallel between *slip* and *muc-* is especially striking, for the agreement covers virtually all aspects of the words' meanings. This makes the particular conjunction of meanings seen in these words seem all the more natural, for it is not just one isolated piece of the semantic picture but a whole nexus of meanings.

#### 6. CONCLUSION

By way of conclusion, a comment on methodology is in order: as noted in §1, searching for parallels is a part of all attempts at gaining a handle on naturalness for any component of language, and, as here, it can be a productive strategy in comparative semantics that can lead to interesting results, even if it is necessarily carried out in a seemingly ad hoc, case-by-case basis, and does not lead in itself to a general theory of semantic change. Still, as a method, it also has its limits, for even though there is a parallel, it seems that the 'cheat' meanings of *slip* are tied up in part to the culture-specific context of playing cards, and so may not be so directly a parallel to this sense of Sanskrit *muc-* as one would like.

That is, slipping while one is walking or moving in an unimpeded manner is likely to be part of the universal repertoire of human activities, so parallels in those domains might well be candidates for naturalness in semantic development, but card-playing per se, while widespread, is not a necessary part of human existence, and so semantic changes that are linked to that context are more likely not to be repeatable, or if so, are more likely to be accidental rather than evidence of something "natural".

This case-study, then, does not really vindicate the methodology, but it does show that the search for parallels in semantic developments can be a productive way of proceeding, in at least some circumstances.

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popularity of the work, i.e. how often it has been cited by others as an acceptable explanation of the question; and second, the testability of the hypothesis presented.

## 2. Elaboration of the Problem

Many Slavic languages express imperfective futurity by means of an analytic construction formed by the future form of the verb 'be' plus an imperfective infinitival or participial complement. Table 1 gives examples of this construction from several of these languages.

<u>West Slavic</u>	Czech	<i>ja budu psátí</i>
	Polish	<i>ja będę pisać / ja będę pisał</i>
<u>East Slavic</u>	Russian	<i>ja budu pisat'</i>
	Ukrainian	<i>ja budu pisati</i>
<u>South Slavic</u>	Slovene	<i>ja bom písal</i>

Table 1. The expression 'I will write' in selected Slavic languages.

The combination of the future auxiliary *budu*<sup>1</sup> with verbal infinitives is not found in the oldest attested Slavic language, Old Church Slavonic (OCS), nor does it exist in most of the modern South Slavic languages: literary Serbo-Croatian, Macedonian, or Bulgarian. Thus, we must assume that it did not exist as an available construction in the reconstructed proto-language, Common Slavic, but rather appeared after individual Slavic languages began to distinguish themselves.

Each language that has the be-future marks a different date for the earliest attestations of the construction. The earliest appearances of a be-type future construction in Slavic are found in Czech, which shows examples dating from the fourteenth century (Lehr-Splawiński 1957:141). In Polish, the construction is found in the earliest texts, dating from the second half of the fourteenth century, but a lack of extant texts from any earlier period makes it difficult to establish a definitive *terminus post quem*. For Slovene and Croatian, early data are extremely scarce; the earliest attestations in both areas are from the mid-sixteenth century (Rösler 1952:120, 114, respectively).

Texts in the East Slavic languages show the construction appearing later than in West Slavic, although the data are unclear. Belarusian and Ukrainian texts, for example, show the be-future consistently only from the fifteenth or even sixteenth centuries; there may be, however, isolated examples from earlier.

The construction begins to appear in texts of Russian provenance relatively late. Examples are attested for western Russian texts from the late fifteenth century, but this area was heavily influenced by Polish and thus we cannot assume a comparably early development of the be-future in so-called "Great", or central, Russian. Only in the eighteenth century does the be-future become the overwhelmingly dominant means of expressing imperfective futurity in Russian (Kuznecov 1959:246).

Prior to the appearance of the be-future, Slavic texts attest several strategies for expressing imperfective futurity. We find constructions formed with auxiliaries based on the verbs 'want', 'have', and 'begin' plus an imperfective infinitive. In Ukrainian, the

<sup>1</sup> For the purposes of this paper, *budu* should be understood as a cover form for all inflected forms of the future auxiliary in all of the languages that employ the be-future construction.

'have'-type future developed into a grammaticalized future suffix that attaches to the infinitive. This construction coexists with the be-future in the modern literary language (Rusaniv'skij 1971:250).

There also existed in Common Slavic a system of "perfect" tenses, which were formed with tense forms of the verb 'be' in combination with the *l*-participle, or resultative participle, of verbs of either aspect. One of these constructions, the future perfect, was formed with the same auxiliary later used with the be-future, *budu*. Polish and Kashubian, moreover, have developed a variant of the be-future that is formally identical to the future perfect, although it is formed with participles of imperfective verbs only.

We can conclude from this data that the be-future was not inherited from Common Slavic, and none of the earlier constructions expressing futurity can be seen as a source from which the be-future developed directly through regular phonological or morphological change. Thus, scholars have sought to explain the development of the be-future as either the result of borrowing from outside of Slavic, or as a result of innovations within the Slavic languages themselves.

### 3. Methodology

In recent years, much work has been done in linguistics that has immediate relevance for an analysis of the development of the Slavic be-future. The nature of the future as a verb tense, the process by which lexical items in a language come to be used as grammatical markers, and the mechanisms of syntactic change are subjects which have received considerable attention within the last two decades. This primarily theoretical work provides an essential background for evaluating the viability of theories regarding the origins of the be-future. By comparing the situation in Slavic to what is found among all of the world's language, it becomes possible to assess whether a particular description of the development of the be-future is "reasonable", both typologically as well as in terms of how we understand language change.

In this section, I will present a brief overview of the theoretical framework upon which this study is based.

#### 3.1 The Nature of the Future Tense

Perhaps the most fundamental assertion linguists make regarding the future tense is about its place in the tense system of language. Future tense is used to describe events that take place after the moment of speech, and thus, as Ultan (1978:105) points out, the future contains a degree of uncertainty that separates it from other tenses. As a verbal category, the future tense contains elements of both modality and temporality.

The unique semantics of the future tense is often reflected in a unique form. Russian, for example, has the analytic (periphrastic) imperfective be-future while other tenses are formed synthetically, through the addition of various suffixes. Indeed, according to Bybee and Dahl (1989:56), slightly more than half of the world's languages employ an analytic future tense construction.

This conclusion has direct relevance to the problem of the Slavic be-future, because some scholars have justified their arguments regarding the development of the construction on the assumption that its formal isolation implies a non-Slavic origin. Bonfante (1950:96), for example, argues that Slavic must have borrowed its periphrastic future constructions from Greek, since the Slavic languages had no native analogue upon which such a future

could have been modeled. By analyzing the formation of future tense expressions from a

cross-linguistic perspective, however, one concludes that analytic future tense constructions, including those found in Slavic languages, are not typologically marked.

### 3.2 Grammaticalization

In recent years, there has been a great deal of attention paid to grammaticalization, a theory which attempts to define the process by which lexical items come to be used as markers of grammatical categories. The development of future tense constructions has been a fruitful source of data for scholars interested in grammaticalization, because future markers (whether auxiliaries or suffixes) are often young enough that their lexical origins have not been obscured by subsequent linguistic changes.<sup>2</sup>

While there is some debate regarding the validity and importance of many of the claims of grammaticalization theory, the literature has a great advantage in providing a broad, cross-linguistic perspective on future constructions. In this capacity, grammaticalization theory provides useful data against which one can evaluate the Slavic be-future.

For example, some scholars have argued that the semantic similarity between the Slavic be-future auxiliary and the German future auxiliary *werden* indicates that the two future constructions must share a single source.<sup>3</sup> Bybee et al. (1991:18), however, demonstrate that this is not necessarily valid. Their cross-linguistic study of future expressions shows only a very small number of possible lexical sources for future markers, increasing the probability that coincidence is at work when two genetically unrelated languages share a similar future construction.

### 3.3 Mechanisms of Syntactic Change

The development of the Slavic be-future concerns matters of syntactic change, an area which is relatively unexplored in Slavistics. Thus, many of the descriptions of the development of the be-future have been written from a very narrow perspective that does not attempt to compare or reconcile Slavic data with data from other languages.

Recent scholarship in general linguistics has produced important works in this area. One of them, Harris and Campbell 1995, seeks to define the basic mechanisms of syntactic change. Of these, two are relevant for this study: reanalysis and extension.

Reanalysis, as defined by Harris and Campbell (ibid.:61) is a type of change which affects underlying structure without producing a visible change in surface structure. In the case of the Slavic be-future, one can speculate that the change of the verb 'be(come)' into a purely grammatical marker of futurity is a type of reanalysis.

Extension, on the other hand, is a change that affects surface structure (ibid.:97). This concept is similar to analogy, although Harris and Campbell draw a distinction between them (ibid.:51). For Slavic, extension can be used to describe how the be-type future became the standard means of describing imperfective futurity.

By seeking to describe syntactic change in terms of these specific mechanisms, Harris and Campbell allow for a more systematic and consistent analysis of changes. In the case of the be-future, we shall see below that their theories are a useful tool for evaluating

<sup>2</sup> For a discussion on determining the age of a future construction, see Bybee et al. 1991.

<sup>3</sup> See section 4.2.1 for a more detailed discussion of this argument.

the feasibility of paths of development that have been proposed.

In the following sections, we turn to the main purpose of this paper: to evaluate the theories which have been proposed to describe the development of the be-future in Slavic.

#### 4. Previous Scholarship: The Borrowing Hypothesis

Many scholars who have explored the nature and origin of the Slavic imperfective be-future have concluded that the construction arose as a result of the influence of a non-Slavic language. The work of Rösler (1952) provides the most detailed argument for this position and has been cited by several sources as the most convincing explanation for the presence of be-futures throughout Slavic.

##### 4.1 The Slavic be-Future as a German Borrowing

Rösler (1952) acknowledges that the be-future cannot be traced back to Common Slavic. Moreover, he points out that as independent verbs both *werden* and *budu* express change-of-state meaning (although *budu* has since lost this meaning<sup>4</sup>). Given other evidence of German linguistic influence on Czech, and his position that a be-type future is typologically rare, Rösler argues that the Slavic be-future must have its origins as a borrowing from German.

Central to Rösler's argument is the question of chronology. He argues that since German developed its future construction with *werden* plus the infinitive in the eleventh to thirteenth centuries, while the Czech be-future begins to be attested in the fourteenth century, there clearly existed a window of time large enough for the German construction to be adopted into Czech. Rösler estimates the time frame of the borrowing to be the late thirteenth century (*ibid.*:142).

In order to explain the presence of be-type futures across all of North Slavic territory, Rösler then postulates a successive sequence of borrowings from Czech into Polish, Polish into Belarusian and Ukrainian, and thence into Russian. Given that the first be-futures do not appear in Russian until the seventeenth century, Rösler argues that he has a sufficiently late endpoint to his proposed sequence of borrowings.

##### 4.2 Counterarguments to the German-Borrowing Hypothesis

Rösler's hypothesis has provoked a great deal of discussion among scholars, and has among its supporters Cocron (1962), and apparently Vlasto (1988). There are serious flaws in his argument, however, many of which have been addressed in Kurz (1952), Křížková (1960), and Leiss (1985). This section discusses the most salient counterarguments against Rösler, with an emphasis on recent conclusions drawn by grammaticalization theory as well as dialect studies.

###### 4.2.1 The Perceived Rarity of a be-Type Future

Rösler's initial assumption regarding the development of the Slavic be-future is that because it shares the same lexical source as the German future, its origins must be linked to the German future. In other words, he finds it highly unlikely that these two similar futures developed independently of each other.

<sup>4</sup> In the interests of space, a discussion of the semantics of *budu* must be reserved for another time; for information regarding its earlier, change-of-state meaning, see Dostál 1954:147.

In order to assess the validity of Rösler's assumption, we must determine not only whether a be-type future is actually rare, but also whether there is such diversity in sources of future constructions that two similar constructions are likely to be related. In a series of cross-linguistic studies of the future tense, Bybee and Pagliuca (1987) examine a representative sample of the world's languages in order to analyze the most typical kinds of future tense formations. By their assessment, there are in fact only a small number of lexical sources for future tense constructions, and one of those sources is verbs of existence or coming into existence (ibid.:111). One sees this type of future, for example, in the Classical Latin future with the suffix *-b-*, and according to Ultan (1978:110), such futures are also found in Cuna, Upper Chinook, and some Celtic languages.

These data invalidate the assumption that the German and Slavic be-futures must be related. It does not, however, demonstrate that they are not related. A more detailed analysis of Rösler's theory, as well as the counterarguments against him, however, sheds more light on this question.

#### 4.2.2 Reliability of Textual Evidence

In his presentation of evidence to support his claim, Rösler makes the mistake of relying on a single source of data to draw his conclusion: the date of the earliest attestation of a be-type future in each language. This is inadequate for several reasons. First, we cannot assume that the earliest extant text containing a particular linguistic feature is the earliest text which contained that feature. Many texts have not survived through the centuries to be available to us now. Second, many texts available to us have survived only in copies made a century or more later than the original. The process of copying the text may have resulted in contamination of the original language with newer linguistic material. Third, one must make a distinction between the language of written texts, which generally reflects a more conservative linguistic system, and the contemporaneous spoken language. Because of the gap between spoken and written variants of a language, textual evidence cannot indicate the absolute *terminus post quem* for the appearance of linguistic features; it can only indicate that the feature existed *at least as early* as the date of the text. This suggests that the origins of the be-future in a particular language must be found earlier than the first written attestation of the construction.

If we assume that such a time lag existed in the relationship of texts to the spoken language in the early Slavic languages, we begin to see flaws in Rösler's description of events. With only written attestations in mind, Rösler has drawn a picture of events that relies on a very narrow window of opportunity for a large number of independently occurring linguistic borrowings. Let us take an example. Rösler states that the earliest appearance of the be-future in Polish is in the middle of the fourteenth century, but places the entry of the construction into Czech in the middle to late thirteenth century. In essence, Rösler is suggesting that the construction appeared in Czech and managed to spread to dominance quickly enough to enter into Polish as a prestige borrowing in only one hundred years. Moreover, the construction managed to establish dominance in Polish quickly enough to be borrowed into western East Slavic within another hundred years. Such speed for a linguistic change involving syntax is highly improbable.

The example of Polish brings up another interesting problem with regards to establishing a chronology. Rösler has overlooked the fact that for Polish, the earliest extant texts are dated relatively late – from the mid-fourteenth century (Rospond 1971:36). We

can only speculate as to the composition of Old Polish before the earliest extant text, and we cannot know whether the be-future was newly borrowed in the fourteenth century or whether it had already been in use for a century or more.

#### 4.2.3 Dialect Evidence from Ukrainian

The transmission of the be-future from Polish into the western languages of East Slavic, Belarusian and Ukrainian, is an especially problematic part of Rösler's hypothesis. Rösler (1952:144-5) argues that the borrowing of the be-future was simply one more instance of Polish exerting influence over Ukrainian. However, the relationship between Polish and Ukrainian is far more complex than Rösler suggests. As Šerech (1952:348-9) argues, only after the sixteenth century was the general direction of influence from Polish into Ukrainian. During the earliest period of contact between Polish and Ukrainian peoples, from the tenth to the fourteenth centuries, Ukrainian influence on Polish was much more significant.

Data from modern Ukrainian dialects also suggest that Rösler's characterization of events is inaccurate. The Ukrainian dialect atlas (Zakrevska et al. 1988:II, map 244) reports that *budu* in combination with the *l*-participle (hereafter "*budu + l*") is found in a large area of southwestern Ukrainian dialects, extending from the Polish border in the west deep into the areas associated with the Dnister and Podillja dialect groups. Most of this area also uses *budu* with an infinitival complement ("*budu + Inf.*") as a variant, but there are significant islands around L'viv, Ternopil', and south of Xmel'nic'kyj where only *budu + l* is found.

According to Rösler, Polish influence on Ukrainian led to the latter's adoption of a *budu + Inf.* type future. The dialect map shows, however, that Polish influence appears to be reflected as the use of a *budu + l* future. If this is true, then the *budu + Inf.* type either arose independently in Ukrainian or was borrowed from Russian or Belarusian. At any rate, it appears that one of the steps of Rösler's proposed spread of the *budu + Inf.* future may not have taken place.

#### 4.2.4 The Chronology of German *werden*

The chronological problems of the borrowing hypothesis are not restricted to the Slavic side of the equation. In his review of Rösler's article, Kurz (1952) is the first to point out errors regarding the development of the *werden* future in German. According to Kurz as well as Leiss (1985), Rösler is inaccurate when he states that the *werden*-future was available for borrowing as early as the thirteenth century. In reality, Leiss argues, examples of *werden + Inf.* are almost nonexistent before the thirteenth century, and the scattered examples from earlier texts have been characterized by some scholars as scribal errors (ibid.:257). With the earliest Czech examples dating from the thirteenth century, it is impossible to argue that *werden + Inf.* was already available for borrowing at an early enough time.

According to Leiss, Rösler also overlooks the very strong possibility that German actually borrowed its future construction from Czech. Supporting evidence includes the fact that Czech texts show the consistent use of be-futures far earlier than German texts show constructions of *werden + Inf.* (ibid.:258). Also, the spread of the German be-future was from the east to the west (ibid.:265-6). In her analysis, Leiss effectively demonstrates

that Rösler's initial assumption, that Slavic borrowed its be-future from the German

construction with *werden*, is incorrect.

In this section, I have described the main points that have led some to conclude that the Slavic be-future originated as a borrowing. Given the amount of evidence that contradicts that position, we are left to demonstrate that the Slavic be-future construction developed from a native Slavic source. This is not a new idea – in the next section we will discuss a number of arguments that reach this conclusion – but it remains to be seen whether scholars have successfully described the process by which a be-future could have developed within Slavic.

### 5. Previous Scholarship: Alternatives to the Borrowing Hypothesis

For many scholars, there is no question that the Slavic be-future developed out of linguistic material native to Slavic. After all, Common Slavic can be shown to employ all the necessary components of the be-future constructions: a future auxiliary based on the verb 'be', a resultative participle with the formant *-l-* which can serve as the antecedent for the Polish future with the *l*-participle complement, and a precedent of analytic future tense constructions using the infinitive with various verbal auxiliaries. Common Slavic and the earliest attested Slavic languages also used a fairly diverse collection of future periphrases; Old Church Slavonic, Old Czech, and Old Russian, for example, all show a number of competing constructions. The essential problem to be solved, then, is in providing a description of how the situation reflected in the oldest texts evolved into the modern situation.

In the interest of space, I have selected two works to evaluate as representatives of the viewpoint that the be-future is a native Slavic construction: Lomtev (1952) and Křížková (1960). Both present detailed arguments which can be carefully analyzed.

#### 5.1 Lomtev 1952

Although Lomtev discusses only the Russian be-future, his perspective is characteristic of the type of discussion one finds in earlier works on this issue. He argues that there are two possible sources of the Russian be-future (*ibid.*:251-2). The first possible source is the future perfect construction *budu + l*-participle, with the be-future developing via a replacement of *l*-participles by the infinitive. The second possible source is the future constructions which were in use in Old Russian with 'have', 'want', or 'begin'-type auxiliaries plus the infinitive. The required change would be a replacement of these auxiliaries by the auxiliary *budu*.

According to Lomtev (*ibid.*), only the first proposed path of development produces, or can produce, the proper outcome for modern Russian. By his way of thinking, the second proposed path is not possible. Whereas the earlier auxiliaries could combine with infinitives of either aspect, the be-auxiliary is only attested with imperfective infinitives. If the second path were the correct one, an intermediate stage is predicted where the be-auxiliary is attested with infinitives of both aspects. Since this intermediate stage is not found, Lomtev argues that the source for the modern Russian be-future must be the future perfect construction.

There are several flaws in Lomtev's argumentation. First, when describing the behavior of Old Russian future constructions prior to the be-future, he fails to make the distinction that phasal verbs, or verbs describing the beginning, continuation, or ending of

actions, do not combine with perfective infinitives in Slavic. While 'have' and 'want' auxiliaries did combine with infinitives of both aspects in Old Russian (Kuznecov 1959:234), inceptive auxiliaries are attested always with imperfective infinitives (*ibid.*:235). This distinction suggests that Lomtev's hypothesis is based on an initial assumption that is false.

Second, Lomtev's view of what constitutes syntactic change is unsophisticated by a more modern standard. Harris and Campbell (1995:50-ff.), for example, demonstrate that syntactic changes can be categorized into a small number of general types. Changes are not simply haphazard reshufflings of linguistic material, but rather are guided by the constraints of specific mechanisms like reanalysis and extension. In this sense, Lomtev's description of the reformulation of the Old Russian future perfect construction into the modern Russian future is inadequate. He does try to identify an intrasystemic trend – by tracing the be-future from the future perfect, he draws a link between the development of the *l*-participle into a past tense marker and the development of the be-future – but he does not, however, define an underlying mechanism at work.

Lomtev also is obliged to add a stipulation to his hypothesis stating that only imperfective *l*-participles were replaced by infinitives to form the modern be-future. Without this condition, one would again expect to find the be-auxiliary in combination with infinitives of either aspect. There is, however, no clear explanation for why *l*-participles of different aspects would develop in different ways. Lomtev is drawn to his conclusion only by his belief that the other possible path of development is impossible, as well as a desire to present the development of the future and past tenses as two sides of the same coin.

In summary, it appears that Lomtev rejects one possible path of development based on a suspect premise, and favors a hypothesis that seems considerably more problematic. Upon closer examination, the path of development that Lomtev rejects is actually a much more compelling hypothesis. In her book of only a few years later, Křížková (1960) presents an interesting argument linking the be-future with the previously used future constructions, especially those using auxiliaries from inceptive verbs.

## 5.2 Křížková 1960

The work of Křížková (1960) is the first to present a solution for the problem of why the be-future, unlike earlier future constructions, is used only with imperfective verbs. To explain this behavior, she quite correctly points out that one can see an analogue in phase verb constructions (*ibid.*:100). As mentioned above, phase verbs in Slavic combine with only imperfective infinitives. Moreover, inceptive verbs, such as those formed with the root *-čn-* 'begin', were used as future expressions from the earliest Slavic texts.

Given these facts, Křížková argues that the following process led to the genesis of the be-future. First, the present tense forms of 'begin'-type verbs came to be used as future auxiliaries. Such verbs retained their lexical meaning in other tenses, however, and thus Křížková argues that this made them unsuitable for use as purely grammatical markers. She claims that the be-auxiliary then came to be used because it made a more suitable future auxiliary, already free of other nuances (*ibid.*:101).

Křížková makes significant progress towards a comprehensive description of the development of the be-future; her insight in finding a connection between phase verbs and the be-auxiliary is essential to a satisfactory solution. There are, however, some problems with her hypothesis. Some of these are discussed by Kiparsky (1967:234-5) and do not



need to be reiterated here. In light of more recent scholarship, however, one particular error

stands out.

A crucial part of Křížková's theory is her description of how the be-auxiliary came to be the regular future auxiliary. According to her argument, *budu* was used because it was a more suitable auxiliary in that it carried no lexical nuances. Such reasoning, however, contradicts what scholars have found on a cross-linguistic scale regarding the grammaticalization of verbs into auxiliaries. Bybee et al., for example, argue that in general, future auxiliaries develop from verbs whose meaning suggests a secondary implication of intention or prediction on the part of the speaker (1994:254). If this is the case, the older auxiliaries of 'have', 'want', and 'begin' were no less "suitable" for use as future markers than 'become'. Many languages, after all, employ futures that have developed from these verbs. Moreover, the be-auxiliary certainly existed as a fully lexical verb in its earliest stages, and there are similar instances of the grammaticalization of 'be(come)' in other languages (see section 4.2.1).

The use in Common Slavic of *budu* in a future perfect construction, however, appears to support Křížková's assumption that the auxiliary *budu* already existed in a usage free of nuances. But for many of the languages, there is doubt that the future perfect coexisted in time with the be-future.<sup>5</sup> In Old Church Slavonic, the future perfect is attested only seven times (Lunt 1974:99). Even in Polish, which employs be-futures with both infinitival and participial complements, the latter construction is extremely rare in the earliest texts (Górecka and Śmiech 1972:13). The use of the future perfect in Slavic has always been very limited, if only due to its semantics.

Ultimately, it may not be possible to explain why the be-auxiliary became the regular marker of the imperfective future, but only how. Křížková provides an important clue towards answering this question in her discussion of the similar behavior of inceptive verbs and the be-auxiliary, but does not pursue the discussion.

## 6. Conclusion

It is clear from examining the previous scholarship on the development of the Slavic be-future that an entirely satisfactory description of events has not yet been written. It has been argued convincingly that the be-future was not borrowed into Slavic from German, but there is as of yet no description of the evolution of the construction within Slavic that takes advantage of recent progress regarding the nature of morphosyntactic change. There are clues present in the work of Křížková 1960, however, that point towards a possible solution: her description of the analogical relationship between the be-future and phase verb constructions. I believe it is possible to show how *budu* could have undergone a semantic shift into a phasal verb capable of taking an imperfective infinitive complement,<sup>6</sup> but a comprehensive analysis remains to be done.

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<sup>5</sup> For Russian, see Nikiforov 1952:182-3; for Czech, see Lehr-Splawiński 1957:141.

<sup>6</sup> Details can be found in Whaley 1998.

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**THE ADVENT OF THE ENGLISH PREPOSITIONAL PASSIVE:  
A MULTI-FACETED MORPHOSYNTACTIC CHANGE**

GWANG-YOON GOH

**1. Introduction<sup>1</sup>**

In general, Old English (OE) had two kinds of ways to represent passive.<sup>2</sup> First, there was one verb which had a synthetic passive, that is, *hatte* 'is/was called'. Second, OE also had a syntactic passive like Modern English (MnE). The norm for this OE passive is for the accusative object of the active verb to become the subject of the passive, which is called 'personal passive'. Otherwise, the impersonal passive was the rule, in which there is no (nominative) subject. That is, when an active verb takes a dative or genitive NP, the NP remains in the oblique case without becoming a subject of the passive, as follows:

- (1) *Him weortheð blæd gifen!*  
him [dat] became glory given  
'he was given glory' (Christ 877)<sup>3</sup>
- (2) *Forðæm se ðe his ær tide ne tiolað,*  
because his [gen] before time not provide (for)  
*thonne bið his on tid untilad,*  
then (it) is his [gen] on time unprovided  
'because they will not provide for him before time  
then it will be unprovided in respect of him when the time comes'  
(Bo 67.11 [Mitchell 1985: §849])<sup>4</sup>

<sup>\*</sup> I am grateful to Brian Joseph, Bob Kasper, and Alan Brown for their encouragement and invaluable comments on various points. Of course, none of them are responsible for any errors.

<sup>1</sup> OE (ME, MnE) = Old (Middle, Modern) English, DO = direct object, P = preposition, V = verb, P-V CV = preposition-verb compound verb, PP = prepositional phrase, PreP = prepositional passive, PO = prepositional object, OPO = occurrence of an overt PO in a non-canonical position.

<sup>2</sup> Impersonal *man* for indefinite agency was often used in the nominative singular with an active verb form as an equivalent of the passive voice.

<sup>3</sup> For identification of the OE texts and examples, I follow the system of Venezky and Healey (1980).

<sup>4</sup> Example (2) may be problematic because the word *untilad* can be regarded as an adjective rather than a past participle. Unlike the impersonal passive for the dative object, examples of the impersonal passive for

On the other hand, OE did not have the passive type *He was laughed at*. This type of passive, called the prepositional passive (PreP) or pseudo-passive, in which the subject of the passive corresponds to the object of a preposition (P) in the active, began to appear in the early 13th century, but remained rare until the end of the 14th century (Mustanoja 1960: 440-1, Denison 1985, 1993: ch.7).<sup>5</sup>

(3) Bot nu than am i *after send*

'but now when I am after sent (= sent for)'

(a1400 (a1325) Cursor 14216 [Denison 1993: 126])

(4) Litel is he loud or *lete by* that suche a lessoun techith  
'he is little loved or  
thought of who teaches such a  
lesson'

(c1400(a1376) PPl. A(1) 11.29 [Denison 1993: 126])

The PreP is not found in what Denison calls 'Standard Average European', though there is something similar in mainland Scandinavian languages. Prepositional stranding (P-Stranding), of which the PreP is one special kind, is extremely rare among languages of the world (Croft 1990: 10) and also freer in modern English than in most other European languages (Denison 1993: 125). Thus, the advent of the English PreP constitutes an interesting question in English historical syntax.

## 2. Major Earlier Studies and Their Contributions <sup>6</sup>

The advent of the PreP has been the main focus of many studies in English historical linguistics and various proposals have been made, considering almost every identifiable important factor involved for a more satisfactory account of the change. In this section, I will re-examine major proposals from the earlier works in order to see what indispensable insights we can derive from them and what (logical) gaps still to be filled.

Some studies attribute the advent of the PreP to a change in the nature of the English passive rules or in the scope of their application. Lightfoot (1979) claims that OE and Middle English (ME) had only a lexical passive rule but English came to have a transformational passive in the early MnE period (15th to 16th century).<sup>7</sup> Lightfoot (1981) says that OE had no PreP (or indirect passive (IP)) because the movement for such a passive would cause a conflict in the case of the moved NP (i.e. base-assigned oblique case vs. structurally assigned nominative case) and that the loss of base-generated oblique case in ME made the PreP possible. Even if the problems with his dates and 'catastrophic' explanation can be ignored and his rules and case distinction are taken for granted,<sup>8</sup> Lightfoot still has to explain why OE (and even ME, according to his claim) only had a

the genitive object are rare (McLaughlin 1983: 62). This rareness is compatible with the distinction between dative and genitive, which is reflected in the obliqueness hierarchy proposed in this paper.

<sup>5</sup> Another type of passive that OE didn't have is the indirect passive (IP), which takes as its subject an NP (BENEFACTIVE) corresponding to the indirect object in the active (e.g. *I was given the book*). It became a feature of English usage in the 15th century (Mustanoja 1960).

<sup>6</sup> Denison (1985, 1993: chs. 6 & 7) has a good discussion of various approaches and other relevant factors.

<sup>7</sup> A lexical passive is derived by a local rule which permits only the verbal argument NP (= DO) to be moved into the subject position, whereas a transformational passive is derived by a more general syntactic process that moves an NP from the VP into the subject position.

<sup>8</sup> The dates of Lightfoot (1979) do not correspond to the rise and development of the PreP (and IP) (Lieber 1979, Denison 1993: 156). Moreover, his 'catastrophic' explanation, which predicts near-simultaneity in the developments of the new passives, is not compatible with their gradual development.

lexical passive (as in Lightfoot (1979)) and why even the impersonal PreP didn't occur in OE (cf. Mitchell 1985: §855), since this passive form would not create the case conflict assumed in Lightfoot (1981).

On the other hand, Bennett (1980) claims that English has always had both rules for lexical and transformational passives but that the scope of the relation 'direct object' (DO) was extended to some prepositional objects (PO).<sup>9</sup> Above all, though, he has to answer why and how POs came to be regarded as DOs. That is, what brought about the changes in the rules or their application posited as the cause of the appearance of the PreP? Without a suitable answer, any argument along these lines would be circular.

P-Stranding<sup>10</sup> has also been proposed as the cause or a main factor of the advent of the PreP. Thus, van der Gaaf (1930: 8) and Mustanoja (1960: 113, 441) claim that P-Stranding in V-P word order is significant for the origin of the PreP and its subsequent development. However, as Denison (1985: 197) notes, the PreP in early examples occurred also with P-V order.

On the other hand, Allen (1980a,b) connects the change in P-Stranding with the advent of the PreP as follows. First, OE had a constraint on movement out of PP, which made P-Stranding caused by processes other than deletion impossible.<sup>11</sup> Then, the 'superficial similarity' among relative clauses in the ME period prompted speakers to extend P-Stranding from *the*-relative clauses to other *wh*-relative clauses, and finally, the P-Stranding with *wh*-relatives spread to other prepositional constructions like the PreP.

Although the fact that *which* is virtually indeclinable may be in accord with the claim that *which*-relative clauses (and later *who*-relative clauses as well) acquired P-Stranding by analogy with *that*-relative clauses, the distinction between the two types of relatives has been very clear since OE, because "pied-piping"<sup>12</sup> has never been allowed for relative *that* (or its OE counterpart *the*) and also because *which* has never been used as a complementizer. Therefore, the basis for the proposed analogy is not solid. Furthermore, Allen's spreading scenario is not compatible with earlier examples: the PreP and new P-Stranding patterns began to appear almost at the same time and remained rare until the end of the 14th century (Denison 1993: 125, 132, Fischer 1992: 390, Mustanoja 1960: 440-1). At best, this means that P-Stranding began to spread to the PreP as soon as it began to be allowed in *wh*-relative clauses and long before it was fully established in any new P-Stranding constructions. In short, although changes in P-Stranding are clearly related to the advent of the PreP, how they are related is yet to be explained.

Many studies (e.g. Hornstein & Weinberg 1981, Fischer & van der Leek 1981, and Kemenade 1987) posit the mechanism of reanalysis, adopted from Chomsky (1965, 1974) by van Riemsdijk (1978: 218-26), to explain (the advent of) the PreP.<sup>13</sup> This

<sup>9</sup> Note that OE also had certain [V+P] collocations in which the [V+P] combination expresses a single predicate and governs a DO-like PO (Visser 1963-73: 391).

<sup>10</sup> Prepositional stranding refers to the phenomenon in which a preposition is not followed by its NP object. In general, the PO takes a non-canonical position (usually, the initial position of the clause), leaving the preposition deferred at the end of the clause.

<sup>11</sup> Allen assumes that OE had relative clauses via deletion (e.g. with *the*) or movement (e.g. with *se* (*the*)).

<sup>12</sup> Pied-piping is the phenomenon in which P is moved along with its complement (*wh*-)NP to the front of clauses. More generally, in pied-piping, the movement of  $\alpha$  in the structure  $\bar{p} \dots \alpha \dots$  causes the movement of  $\beta$  (Culicover 1997: 392).

<sup>13</sup> The proposal of reanalysis was envisioned much earlier by Jespersen (1909-49: part III, vol. II. 15.74) who said "... nothing hinders us from saying that *take notice of* is a verbal phrase governing an object (*me*), which can be made into the subject if the whole phrase is turned into the passive".

process of reanalysis makes a unit of the contiguous V and P, which are "listed in the

lexicon as semantic units', by optionally introducing an extra pair of V-brackets (Van Riemsdijk 1978: 222). Applying the insight concerning the reanalysis of V and P to an account of the rise of the PreP seems to be promising, and even necessary for later stages at least, since sufficient evidence for the unification of V and P is found in MnE PreP construction.

For this process to be fully justified as a major factor in the advent of the PreP, however, some significant aspects need to be better explained. Above all, we must answer what made such a reanalysis possible and why it happened in ME. Furthermore, we have to explain why OE didn't have a similar reanalysis since combining V and P was much easier in OE, in the sense that OE had so many morphosyntactically and semantically transparent P-V compound verbs (Kim 1997, Goh 1998b, in press).

The lexicalization of V and P collocations is also proposed as the cause of the PreP. For example, Fischer (1992: 386-7) says that after most OE prefixes disappeared, many OE P-V compounds with an inseparable prefix (e.g. *be-sprecan* 'to speak about') were simply replaced in ME by a new V-P combination, thereby making the semantic function of the new PO identical with that of a verbal DO and facilitating the lexicalization of the new V-P combinations.

Although the lexicalization of some V-P collocations may have been helpful for the reanalysis of V and P and the emergence of the PreP, invoking it also leaves some important questions untouched. First, OE also had V-P collocations whose prepositional objects are parallel to verbal DOs in semantic function (Visser 1963-73: 391, Denison 1985: 193). Why then is it that English didn't have the PreP at all, including the impersonal PreP, until ME? Moreover, not only is it the case that many unlexicalized combinations of V and P can be used in the PreP (e.g. *The bed was slept in*) but it is also true that some combinations of that kind (e.g. *run at, waded over, spat upon*, etc.), which are difficult to regard as lexicalized, were used even in early examples (cf. Denison 1985: 193). Finally, although it seems reasonable to argue that the semantic affinity of some V-P collocations helped the reanalysis of V and P and NP movement out of PP, why is the evidence of reanalysis found only in passive?<sup>14</sup> All this means that lexicalization may be necessary but not sufficient for the advent of the PreP.

The loss of case inflection and the subsequent reanalysis of dative object as passive subject have been proposed as a cause of the rise of the PreP as well as of the IP (van der Gaaf 1930, Lightfoot 1981, van Kemenade 1987, etc.). On the one hand, this position does not seem to be easy to maintain because, unlike the IP, the PreP had no impersonal counterpart like *\*Her was talked to* in OE (Denison 1985: 195-6). However, the loss of case inflection seems to have played an important role in the advent of the PreP by eliminating the formal distinction among NP objects of different cases, thereby motivating the change in the way of representing and maintaining relative obliqueness among NP arguments, as will be discussed later.

<sup>14</sup> V and P of prepositional verbs in the PreP form a single cohesive unit which cannot be broken up by other material, whereas they can be intervened by other material in the active form, as follows:

- (i) a. The committee agreed unanimously on the resolution.  
 b. \*The resolution was agreed unanimously on by the committee. (Radford 1988: 428)
- (ii) a. Napoleon slept fitfully in that bed.  
 b. \*That bed was slept fitfully in by Napoleon. (Riemsdijk 1978: 222)

Besides those we have considered so far, many other factors, e.g. semantic roles or types (Bolinger 1977, Vestergaard 1977, Couper-Kuhlen 1979, Thornburg 1985, etc.), social variations and borrowing (Wurff 1992) and fixing of SVO word order among others, have been proposed.<sup>15</sup> One common feature of most earlier studies is their emphasis on isolated aspects or factors of the given change. Despite the potentially significant role of each, however, none of the generally accepted main factors are sufficient on their own to explain the advent of the PreP. Thus, the problem is that few previous studies have shown how those main factors can be logically and coherently put together to bring about the new passive. Furthermore, even after we succeed in refining and integrating those main factors, we may need other complementary factors to fill some significant logical gaps in the resultant integrated account, which are difficult to identify until a well-woven overall picture of the change is drawn.

### 3. Prepositional Stranding in Old English and Its Syntactic Necessity

OE shows a very rigid contrast between two groups of prepositional constructions with regard to P-Stranding possibilities. Above all, P-Stranding was allowed and was obligatory in the following six prepositional constructions in OE: *the*-relative clauses, zero relative clauses, free relative clauses introduced by *swa hw- swa* 'whatever, whoever', infinitival relatives, comparative construction, and complement object deletion construction, as in (5)-(10), respectively:

- (5) ... nyhst thæm tune      ðæ se deada man on lið  
 ... next that homestead that the dead man in lies  
 '... next to the homestead that the dead man lies in' (Or 20.30)
- (6) ðonne is other stow elceordege men beoð on  
 then is other place barbarous men are in  
 'then, there is (an)other place barbarous men live in' (Maarv 18.1)
- (7) And heo gefret softnysse oððe samysse, swa hwæðer swa he on bið  
 and it feels softness or pain so which [acc] as it in is  
 'and it feels softness or pain, whichever it is in' (ÆHom 11.218)
- (8) ðeah he nu nanwuht elles næbbe ymbe to sorgienne  
 though he now nothing else not-have about to worry  
 'though he now has nothing else to worry about' (Bo 24.15)
- (9) he us ne mæge gescildan to beteran tidun thonne we nu on sint  
 he us not can shield for better times than we now in are  
 'he can protect us for better times than we are now in' (Or 86.4-5)
- (10) heowæs swiðe fæger an to locianne  
 it was very beautiful to look  
 'it was very beautiful to look at' (Or 74.12-3)

In each of the above OE constructions, P-Stranding was necessary in the sense that the alternative co-occurrence of P and its PO within the same PP (i.e. ... [pp P NP] ...) was not possible in the given sentence, whereas P-Stranding itself was not prohibited in OE. That is, the *the*-relative, which is often considered to correspond to the relative *that* in

<sup>15</sup> Denison (1985, 1993: ch.7) has a good summary along with some relevant criticism.



later English, has never been allowed to occur after P as a PO.<sup>16</sup> Thus, a construction

such as \*... *myd the we* ... was ungrammatical in OE.<sup>17</sup> In all the other constructions, neither pied piping nor having P and its object NP within the same PP, which can obviate P-Stranding in a given sentence, is allowed since there is no overt PO in the relative clause which can occur with P.<sup>18</sup> Note that none of these P-Stranding types involve (the displacement of a PO from PP and) the 'occurrence of an overt PO in a non-canonical position' (OPO, i.e., ... NP<sub>i</sub> ... [pp P t<sub>i</sub>] ...).<sup>19</sup> That is, the overt NP which should be the PO of the deferred P is missing in every case of OE P-Stranding.

Except for the above five cases, no other type of P-Stranding is attested in OE. In particular, none of Modern English P-Stranding types involving OPO (i.e. *wh*-questions, *who*- or *which*-relatives, the prepositional passive, topicalization, and exclamations) were possible. Therefore the alternative co-occurrence of P and its PO within the same PP through pied piping or impersonal constructions, which could avoid OPO, had to occur if it was not prohibited otherwise in OE. Thus, pied piping, as the only possible alternative in *se the*- or *se*-relative clauses, as in (11) and (12), in *wh*-questions, as in (13), and in topicalization, as in (14), was obligatory, whereas the prepositional passive and exclamations involving pied piping were not allowed, as in MnE.<sup>20</sup>

<sup>16</sup> There is no evidence that OE 'relativizer' *the*, which roughly corresponds to the 'relativizer' *that* in later English, is the PO of the deferred P. Above all, except for the dative and accusative forms of the second person, personal pronoun *thu* 'you', which has anything to do with relative pronouns, it is not identical in form with any pronoun. Furthermore, it was indeclinable, whereas virtually every pronoun (and noun) in OE was declinable. Note also that it was mainly used as a subordinating conjunction (in other places). Thus, the status of OE 'relativizer' *the* was clear, unlike its later English counterpart *that*, which is identical in form with the demonstrative pronoun *that* but is still analyzed as a complementizer rather than a relative pronoun in many theoretical frameworks including Government-Binding theory. Furthermore, there are some periods in which the 'relativizer' *that* could be used together with a relative pronoun. Thus, the co-occurrence of the 'relativizer' *that* and a relative pronoun was very common in late ME and is observed even in MnE, as follows:

- (i) ... the Minotaur, *which that* he slough in Crete.  
'... the Minotaur, which he had slain in Crete' (Chaucer, Knight Tale 122)

<sup>17</sup> At least in MnE, a resumptive pronoun as a PO can sometimes occur after P in *that*-relative clauses, as Jespersen (1939 III. 5.63) observes, as follows:

- (i) a. By force of argument, *that* you being licentiate should stand vpon<sup>1</sup>. (Marl F 206)  
b. They cure a lot of folks *that* you regular docs can't seem to find out  
what's the matter with em. (Tarkington MA 163)

Note, however, that the addition of a resumptive pronoun is not natural unless the clause is long. Furthermore, there is no clear evidence that this construction involving a resumptive pronoun after P in *the*-relative clauses was grammatical in OE. Thus, although it might be a conceivable alternative to the Stranding in *the*-relative clauses to consider, it is hypothetical at best.

<sup>18</sup> The free relative in (7) at least syntactically belongs to the main clause, not to the relative clause. This is clear from the fact that the *wh*-pronoun or demonstrative head of the free relative takes the case assigned by the verb (or preposition) of the main clause (i.e. accusative but not dative, which is normally expected to be assigned by the P *on* in the given relative clause) and that the PO is absent at surface structure. Thus, even if the free relative may be considered the PO (semantically), its co-occurrence with the deferred P within the same PP will be ungrammatical.

On the other hand, OE didn't allow a relative pronoun to be used with infinitival relatives. Thus, no examples such as *a book about which to talk* are found until Chaucer's period (Allen 1980b: 275). Except for this difference, English, since OE, has been the same in the necessity of Stranding in all the above prepositional constructions.

<sup>19</sup> Note that for the purpose of this study, it doesn't matter whether the non-canonical positioning of the PO is a result of movement or base-generation. What is relevant here is that it is there, not how it got there.

<sup>20</sup> Thus, the following types of sentences have always been ungrammatical in English.

- (11) Eala ðu wundorlicerod, *on ðære ðe crist wolde ðrowian.*  
 hailthou wonderful cross on which thatChrist would suffer  
 'hail, you wonderful cross, on which (that) Christ deigned to suffer'  
 (ÆIS 27.115)
- (12) Gehyr ðu arfæsta God mine stefne, *mid ðære ic earmto ðe cleopie.*  
 hear thou merciful God my voice with which I poor to thee cry  
 'hear you, merciful God, my voice, with which I, poor one, cry to you'  
 (BiHom 89.13)
- (13) a. *To hwæm locige icbutan to ðæm eaðmodum?*  
 to whom look I but to the humble  
 'to whom shall I look but to the humble?' (CP 299.19)
- b. *Ic nat ful geara ymb hwærthu giet tweost*  
 I not-know fullentirely aboutwhat you still doubt  
 'I do not fully understand what you still doubt about' (Bo 12.26)
- (14) a. *On thisne enne godwe sceolon geleafan*  
 in this one Godwe must believe  
 'in this one god, we must believe' (ÆIS i. 1.40)
- b. *For ðæs lichaman life, the langsum beon ne mæg,*  
 for the body's life, thatlong be not may,  
 swincað mennswide,  
 toil men greatly  
 'for the life of the body, that cannot last eternally, men toil greatly'  
 (ÆHom 6.145-6)

Note that none of the unattested types of P-Stranding were necessary in that the alternative co-occurrence of P and its PO within the same PP was available in the given sentences. Moreover, all the unattested (potential) types of P-Stranding must have OPO in OE, and no attested P-Stranding in OE involved OPO while P-Stranding was not prohibited otherwise in OE. All this strongly suggests that OPO was strictly prohibited in OE, allowing for the formation of an OE constraint against OPO: \*... NP<sub>i</sub>... [pp P t<sub>i</sub>] .... I refer to this constraint as \*OPO.<sup>21</sup>

One corollary of the above observation is that if some English construction requires P-Stranding involving OPO, it will not be allowed in OE, even if it is otherwise possible. One piece of positive evidence which demonstrates this corollary is the so-called *tough*-construction (e.g. *Bill is hard to convince; Mary is easy to talk with*). Although OE shows many examples of the *tough*-construction, as in (15), no such examples involve P-

- (i) (a) \**Into* the city's accounts were thoroughly looked (by a financial controller).  
 (b) \**About* the affair was talked (by all the people in the country).
- (ii) (a) \**In* what wonderful house you live!  
 (b) \*I can't believe *in* what a mess you've got!

<sup>21</sup> The impossibility of movement out of PP and the obligatoriness of pied piping in the above OE constructions have often been observed and PPs in OE have been suggested as an island (out of which no movement is allowed) in many previous studies within derivational frameworks (van Riemsdijk 1978, Allen 1980a,b, Hornstein & Weinberg 1981, Kayne 1981a,b, Lightfoot 1981, Bennis & Hockstra 1984, among others). Most previous studies, however, put their emphasis on the analysis of the constraint against movement out of PP in OE mainly from a theoretical standpoint and ignore why OE had such a constraint and what made it change in ME, aspects which are indispensable for a complete account of the changes in P-Stranding.

Stranding and thus no examples of the *tough*-construction with P-Stranding are attested in OE (Allen 1980a,b and van der Wurff 1990, 1992).<sup>22</sup>

- (15) a. se deada byðuneaðe ælcon men on neaweste to hæb benne  
 the dead is difficult for each man in neighborhood to have  
 'the dead man is difficult for everyone to have nearby' (BIHom 59.14)
- b. thæt him wære eaðelic se wifired to healdanne & to rihtanne  
 that him was easy the nunnery to hold and to rule  
 'that the nunnery was easy for him to lead and rule' (GD I. 27.4)

Since the *tough*-construction without P-Stranding was possible (as in (15)) and P-Stranding was allowed unless it violated \*OPO, the absence of the *tough*-construction involving P-Stranding can be best explained under the assumption that P-Stranding in the *tough*-construction would have had to involve the prohibited OPO.<sup>23</sup> This is supported by the fact that alternative, impersonal constructions with or without a dummy subject, which do not have to violate \*OPO, are well-attested in OE, as follows:

- (16) Hit bið swiðe unieðe ægðer to donne, ge wið ðone to cidanne  
 it is very difficult both to take and against him to contend  
 'it is very difficult both to take and to contend against him' (CP 355.41)
- (17) Eaðere ys olfende to faren thurh nædle thyrel,  
 easier is for a camel to go through needle's eye  
 '(it) is easier for a camel to go through a needle's eye' (Mk. Bos 10.25)

Finally, the non-existence of the PreP in OE can be explained in the same way. The personal PreP has to involve OPO because it requires the underlying PO to be in the

<sup>22</sup> Even though the *tough*-construction involving OPO has been said not to occur in OE in some studies including Allen (1980b: 283, fn.25) and van der Wurff (1990, 1992), its relevance in regard to the change of P-Stranding has not been sufficiently discussed in any previous studies.

On the other hand, although Kemenade (1987: 152) claims that P-Stranding in the *tough*-construction was obligatory in OE, this is simply not true. See (16) and (17) for some counterexamples. Furthermore, her single example, given in (i) below, is dubious: the adjectives *myrige* 'pleasant' and *smylte* 'serene' can hardly be considered *tough*-adjectives, since they often assign a theta-role to the subject of the sentence; rather, (i) would seem to be a case of the Complement Object Deletion construction.

(i) forðon the heo ish wilitidum *smylte* and *myrige* on to rowenne,  
 because that she is sometimes serene and pleasant on to live  
 'because it is sometimes serene and pleasant to live in' (Aeth. I. 182.31)

<sup>23</sup> This non-existence of the *tough*-construction involving P-Stranding in OE is expected if we accept the assumption that the subject of the main clause in the *tough*-construction is generated as a PO and moved to the subject position. Thus, the *tough*-construction with P-Stranding, for it to be possible, must involve the prohibited OPO, which makes the P-Stranding in the *tough*-construction ungrammatical in OE. See Rosenbaum (1967), Postal (1971), and Berman (1973) for this line of analyses based on object-raising.

There is considerable controversy about the *tough*-construction. One analysis within Government-Binding theory (e.g. Chomsky (1977b)), one of the earliest proposals along this line is to propose (the movement of) an empty operator that binds the trace in the object position and is coindexed with the subject, as in (i):

(i) Bill<sub>i</sub> is easy [<sub>CP</sub> O<sub>i</sub> [<sub>S</sub> PRO to convince t<sub>j</sub> ]].

In Head-Driven Phrase Structure Grammar, on the other hand, the *tough*-movement rule is analyzed as a lexical fact about some predicates such as *easy*, *take*, and *cost* which subcategorize for infinitive complements containing an accusative NP gap coindexed with the subject (Pollard & Sag: 1994: 167).

passive subject position.<sup>24</sup> Moreover, the personal PreP was not necessary because OE could have resorted to an alternative, impersonal PreP, which does not violate \*OPO since the impersonal construction allows P to co-occur with its PO within the same PP. In fact, an impersonal PreP in OE was not only theoretically possible but also very plausible in several respects. Above all, OE didn't always require a nominative subject and therefore had the impersonal passive as well as many other impersonal constructions. Furthermore, the impersonal PreP is actually found in some Germanic languages such as Old Norse, Icelandic and German, as follows:

- (18) a. Meine Mutter sorgt für die Kinder.  
'my mother is taking care of the children'  
b. Für die Kinder wird gesorgt.  
'the children are being taken care of'  
c. Es wird für die Kinder gesorgt.  
'the children are being taken care of'
- (19) a. Ich arbeite unter dieser Brücke.  
'I work under this bridge'  
b. Unter dieser Brücke wird gearbeitet.  
'this bridge is worked under'  
c. Es wird unter dieser Brücke gearbeitet.  
'this bridge is worked under'

However, if even this potential alternative was prohibited for some independent reason, it must be that OE could not have any form of the PreP. This line of reasoning will be developed later in this paper.

In short, the following generalization about OE P-Stranding can be made: first, OPO was strictly prohibited in OE; second, P-Stranding was allowed only when it was syntactically 'necessary' in that the alternative co-occurrence of P and its overt PO within the same PP in a given sentence was even potentially not available, on the one hand, and P-Stranding itself (without violating \*OPO) was not prohibited, on the other; finally, the co-occurrence of P and its overt PO within the same PP through pied piping or an impersonal construction, an alternative to the prohibited but potential P-Stranding involving OPO, was obligatory unless it was unavailable or prohibited otherwise.<sup>25</sup>

#### 4. What Was Behind the Constraint on the Separation of P and Its Object

The investigation of OE prepositional constructions has shown that OPO was strictly prohibited, and no matter how such a prohibition is theoretically analyzed, it seems clear that OE had some sort of constraint on OPO. Thus, I will now address the issue of what motivates the constraint by explaining what made OPO so difficult in OE. I will propose, in particular, that what was behind the constraint is a high degree of 'obliqueness' of OE prepositional arguments, which was rigidly marked and represented by P as an 'obliqueness marker', and the representation and maintenance of relative obliqueness among OE NP arguments.

<sup>24</sup> Even in the theoretical frameworks which do not assume movement or transformation (e.g. such lexicalist approaches as Head-Driven Phrase Structure Grammar by Pollard & Sag (1987, 1994)), the PO or verbal object in the active is considered to correspond to the subject in the passive.

<sup>25</sup> Note that the logic of my argument is more along lines of 'soft' or violable constraints, which can be well accommodated in Optimality theory.

#### 4.1. Relative Obliqueness Among NP Arguments in Old English <sup>26</sup>

Although grammatical roles or functions were so variably encoded in OE cases that the traditional notion of case government cannot be properly employed to systematically identify and generalize the grammatical contributions of OE cases,<sup>27</sup> there was a very rigid distinction among OE verbal arguments with regard to their morphological cases, especially between accusative NPs and NPs of other cases. This distinction is clearly seen in the behavior of verbal arguments in passivization: personal passive for accusative NPs versus impersonal passive for NPs of other object cases.

Now, let us assume that the rigid distinction between accusative NPs and other oblique NPs reflects the different degrees of obliqueness encoded in OE morphological cases.<sup>28</sup> Thus, an obliqueness hierarchy (OH) can be proposed as follows:

##### (20) Obliqueness Hierarchy for OE Verbal Arguments <sup>29</sup>

Accusative < Dative ≤ Genitive

Many OE preposition-verb compound verbs (P-V CVs) show that although the head V determines most of the morphosyntactic features of the whole CV, the valence of the CV is jointly determined by the head V and the nonhead P (Kim 1997).

- (21) *in leohte*him      tha word      *cwethað*  
 in light him [dat] those words [acc] speak  
 'they will speak those words to him in glory' (Christ 401)
- (22) *gif* inc      hwa      ðas      *withcwethe*  
 if you-two [dat] anyone that [gen] contradicts  
 'if anyone contradicts you about that' (BIHom 71.1)
- (23) *with* [acc/dat/gen] 'against' (Mitchell 1985: §1178)

*With-cweðan* in (22), as a ditransitive, takes dative and genitive, which come from the head V *cweðan* and the nonhead P *with*, respectively. This means that the nonhead (P) as well as the head (V) participates in the determination of the argument structures of OE P-V CVs.

On the other hand, the investigation of OE P-V CVs and their case government shows that relative obliqueness exists between verbal arguments and prepositional arguments and that it is systematically maintained in the subcategorization inheritance through OE P-V compounding (Goh 1998a,b, in press). That is, when a prepositional argument is

<sup>26</sup> Section 4.1 is mainly based on Goh (1998b, in press).

<sup>27</sup> See Plank (1983) and Goh (to appear) for such alternative object case markings and their discussions.

<sup>28</sup> The representation of grammatical relations by means of relative obliqueness can be found in many studies including Keenan & Comrie (1977, 1979), Comrie (1981: 148-55), and Pollard & Sag (1987: 67-72, 117-121, 1994). Note, however, that their hierarchies, being based mainly on grammatical functions, are difficult to properly apply to NP arguments which have the same grammatical function (i.e. the direct object) but alternative case markings. Thus, unlike most other scholars, I here define relative obliqueness with regard to the morphological cases of NP arguments rather than to their grammatical roles or functions.

<sup>29</sup> Case<sub>1</sub> < Case<sub>2</sub> means that Case<sub>1</sub> is less oblique than Case<sub>2</sub>. Unlike the impersonal passive for the dative object, on the other hand, clear examples of the impersonal passive for the genitive object seem to be rare (McLaughlin 1983: 62). Anyway, this rareness is compatible with the distinction between the dative case and the genitive case, which is reflected in the obliqueness hierarchy proposed in (24).

composed as a verbal argument, it always takes a case which is more oblique than the case of the original verbal argument.<sup>30</sup> This means that OE prepositional arguments are always more oblique than verbal arguments, regardless of the morphological cases involved. Thus, the following extended OH including prepositional arguments can be given:

(24) Obliqueness Hierarchy among Old English NP arguments

- a. Nom < Acc < Dat ≤ Gen  
 b. Verbal arguments < Prepositional arguments

In fact, the relative obliqueness between verbal and prepositional arguments is expected because the OH among verbal arguments is originally based on their potential for passivization and also because passivization in any form (i.e. personal or impersonal) was not allowed for OE prepositional arguments.<sup>31</sup>

The high degree of obliqueness of OE prepositional arguments is well supported by the productivity of OE P-V compounding. Unlike later English, OE had very productive compounding of P and V and thus most of the intransitive verbs commonly used in OE could combine with almost every frequently used P to form a P-V CV, as follows:

(25) *cuman* 'to come'<sup>32</sup>

a-, an-, be-, for-, fore-, forth-, ge-, in-, of-, ofer-, ofer-be-, on-be-, ongean-,  
 thurh-, to-, to-be-, under-, up-cuman

(26) *faran* 'to travel'

a-, be-, for-, forth-, ge-, geond-, in-, of-, ofer-, on-, oth-, thurh-, to-, ut-, with-,  
 ymb-faran

(27) *gangan* 'to go'

a-, æt-, be-, bi-, for-, fore-, forth-, ful-, ge-, in-, of-, ofer-, on-, ongean-,  
 thurh-, to-, under-, up-, ut-, with-, ymb-, ymbe-gangan

This unusual productivity of OE P-V compounding, together with the highly transparent argument structures of many OE P-V CVs, suggests that most selected prepositional arguments in later English are very likely to have occurred as a verbal argument of P-V CVs in OE. This claim is also supported by the fact that in the ME period English lost most OE P-V CVs: many of P-V CVs (and some simplex verbs) were replaced by (new) V-P phrases or prepositional verbs of new or same components (Fischer 1992: 386). Through this process, many (former) verbal arguments, which could be passivized (personally or impersonally), changed to prepositional arguments,

<sup>30</sup> This means that if a simplex verb subcategorizes for only an NP of dative or genitive, then it does not inherit accusative through compounding. Therefore, a P-V CV formed by that verb and a preposition must not take accusative either, because accusative is less oblique than either of dative and genitive. Goh (1998b) verifies this conclusion by examining the OE genitive- or dative-governing simplex verbs (cf. Mitchell 1985: § 1092) and by showing that none of them form a P-V CV which takes a less oblique case than the case specified for the simplex verb.

<sup>31</sup> Note that the argument so far is not circular, because the relative status of verbal and prepositional arguments in the OH was derived on the basis of empirical evidence. Furthermore, the relative obliqueness posited here between verbal arguments and prepositional arguments is not at all unusual, and this is suggested by the fact that although passive constructions have been reported in all the main language families, the PreP is found in only a very small number of languages of the world (Siewierska 1984: 23).

<sup>32</sup> The list of complex verbs is from BT(s).

maintaining the original (passivizable) degree of obliqueness. Thus, many early

examples of the PreP in ME have prepositional verbs or V-P phrases whose OE counterparts in the respect of form or meaning are P-V CVs, as follows:<sup>34</sup>

- (28) OE *be-licgan* 'to lie or sleep by/with/around' > ME *liggen bi*  
 this maiden ... feledalso bi her thi | thatsche was yleyen bi  
 this maiden ... felt also by her thigh thatshe had-been lain by  
 'this maiden felt by her thigh that she had been lain with'  
 (c1330 (?a1300) *Arth. & M.*(Auch) 849)
- (29) OE *ymb-sprecan* or *be-sprecan* 'speak of/about' > ME *speken of*  
 And the comaundment ys brokun, | And thehalyday, byfore of spokun.  
 and the commandment is broken and the holy day previously of spoken  
 'and the commandment was broken, and the holy day previously spoken of'  
 (a1400 (c1303) *Mannyng, HS* 1033)
- (30) OE *on-spætan* or *be-spætan* 'spit upon' > ME *spitten (up)on*  
 and aftyr he was turmentyd, and aftyr he was spyt vpon  
 and afterwardshe was tormented and afterwardshe was spat upon  
 (a1425 *Wycl.Serm.* I 39.26)

In conclusion, unlike the prepositional arguments of later English, OE prepositional arguments were absolutely as well as relatively more oblique than OE verbal arguments. Thus, 'true' prepositional arguments in OE were always too oblique to be subcategorized for by a verb and therefore to be passivized. Note that this high degree of obliqueness of prepositional arguments was systematically represented by prepositions. It is in this very sense that OE prepositions can be called 'obliqueness markers'.<sup>35</sup>

#### 4.2. Flexibility of Surface Word Order in Old English

Although many studies, including generative ones such as Koopman (1985, 1990a, 1990b, 1992) and Pintzuk & Kroch (1985, 1989), have tried to show that there is a general tendency, especially at a deep level, towards SXV or SOV in OE word order, the surface word order is very flexible and, in many cases, can hardly be conclusive for determining the grammatical relationships among NPs in an OE sentence.<sup>36</sup>

<sup>33</sup> The passive subject *that scyp* in (1) below is the former PO which has been inherited as a verbal object from the nonhead P *ofer*. Note that the P-V CV *ofer-geotan* has the corresponding V+P phrase, as in (ii):

- (i) swa thætthætscyp wearð ofer-goten mid ythum  
 so that the ship became over-poured with water  
 'so that the ship was being covered (< poured over) with water' (Mt 8.24)
- (ii) tha wæs wopes hring hat heafodwylm oferhleor goten  
 then was of weepingsound hot tears overcheek poured  
 'then there was the sound of weeping and hot tears poured over the cheek' (EI 1131-3)

<sup>34</sup> ME examples are from Denison (1993). By providing these examples, I don't mean to imply that prepositional verbs are functional replacements of OE P-V CVs, as in de la Cruz (1973).

<sup>35</sup> Both morphological case endings and prepositions can be called 'obliqueness markers' in the sense that the relative obliqueness among OE NP arguments is consistently represented and maintained by means of those two types of markers. In particular, OE prepositions can be called 'absolute obliqueness markers' since any NP arguments marked (i.e. governed) by them are always too oblique to be passivized.

<sup>36</sup> Most of the efforts to establish basic word orders in OE are mainly concerned with the relative order of subject and verb or with the position of the verb, often ignoring a large body of exceptions, many attested order possibilities, and some evidence of non-homogeneity within OE (Denison 1993: 27-9; Mitchell 1985:

In particular, an NP argument often doesn't seem to have any special restriction in its position with respect to other NP arguments in a sentence. Thus, an object NP could occur almost in any place in a sentence: it could follow the subject and V, precede the subject and V, or occur between the subject and V, as follows:

- (31) a. We willath secgan *eow sum byspel*  
 we wish to say you a parable  
 'we want to tell you a parable' (ÆCHom I. 212. 6)
- b. Geseahicwuldres *treow wædum geweorðod*  
 saw I of glory tree with garments adorned  
 'I saw the tree of glory adorned with garments' (Dream 14)
- c. for than the he tæhte *him tha gastlicanlare*  
 for he taughtthem the spiritual learning  
 'for he taught the spiritual learning' (ÆCHom I. 186.22)
- (32) a. *hine* geswencte seo wædlung  
 him afflicted the poverty  
 'poverty afflicted him' (ÆCHom I. 332.9)
- b. and *treowa* he deth færllice blowan  
 and trees [acc] he causes suddenly bloom [inf]  
 'and he causes trees to burst into bloom'  
 (HomU 34 (Nap 42) 196.1 [Denison 1993: 174])
- c. theah *hit him* man secge  
 though it him one says  
 'though people say it to him' (WHom 4.77)
- (33) a. Ic *the* gethyldlice geyrde  
 I you patiently hear  
 'I will hear you patiently' (ÆCHom I. 590.2)
- b. thaæt he mehte *his feorh* generian  
 that he could his life save  
 'so that he could save his life' (Or 48.18)
- c. and thaæt he *himand his geferan bigleofan* thenianwolde  
 and thathe him andhis companions food serve would  
 'and that he would serve him and his companions food' (ÆCHom II. 78.198)

ch. IX, 1992: 63). For example, most transformational generative studies of OE syntax assume that the verb is generated in final position, although verb-final is not the most common of attested word orders (Denison 1993: 35). Note that for determining the grammatical relationships involved in a sentence, the information about the relation between the object NP and other NPs (i.e. subject or other object NPs), which has hardly been dealt with in most studies of word order in OE, is indispensable. As Denison (1993: 28) said, the use of blanket labels like SVO or SOV, no matter how necessary for cross-linguistic comparison, is hardly practicable for OE. Thus, without the information encoded in the case endings, word order in OE still cannot do much in allowing for the determination of grammatical or semantic relationships in a sentence and this will be more than appropriate in the pre-OE or early OE period in which the main body of OE grammar must have been shaped.





clause could occur between V and its complement, especially an object, without causing any serious problems, as follows:

- (34) a. Geseah he *in recede* *rinca* manige,  
saw he in the building warriors many  
'he saw many warriors in the building' (Beo 728)
- b. Ond hiene tha Cynewulf *on Andred* adraefde,  
and him then Cynewulf in Andred drove out  
'and then Cynewulf drove him out from Andred' (Chron 755.4)

This flexibility in word order in OE can be considered possible because, like many other highly inflected languages, OE maintained the grammatical relationships among sentential elements mainly by encoding the various kinds of grammatical information in case endings and prepositions governing oblique prepositional arguments.

On the other hand, there was one conspicuous exception to such general flexibility in word order, that is, the relative word order of P and its object NP. P in OE could occur in one of the two positions with respect to its object NP: it could precede (as a pre-position) its (non-)pronominal object NP or follow (as a post-position) its pronominal object NP, as in (35) and (36), respectively (Mitchell 1985: §§1061-2):

- (35) a. se fæder *thurh hine* gesceop us  
the father through him created us  
'the father created us through him' (ÆCHom II. 3.11)
- b. Symon me *mid his englum* gethiwde  
Simon me with his angels threatened  
'Simon threatened me with his angels' (ÆCHom I. 378.1)
- c. Hu Moyses lædde Israhele folc *from Egyptum ofer thone Readan Sæ*,  
how Moses led Israelite people from Egypt over the Red Sea  
'how Moses led Israelite people from Egypt over the Red Sea' (Or 1. 16)
- (36) a. gehyrde mycclen menigo *him beforan* feran  
heard great multitude[dat.sg.fem] him before go  
'he heard a great multitude go before him' (BIHom 15.14)
- b. *Him to* genealæhton his discipuli  
him to approached his disciples  
'his disciples approached him' (ÆCHom I. 548.25)

In spite of the high degree of flexibility in OE word order, it was much more difficult in OE to separate P and its object NP from each other than in MnE, which has a fixed word order, to the extent that it constitutes a rare constraint on the otherwise flexible word order in OE. Note that such rigidity in the relative position of P and its governing NP is very commonly observed in most languages with a highly flexible word order such as Japanese and Korean, in which the representation of grammatical relationship among NP arguments heavily depends on the relevant case markers.<sup>37, 38</sup>

<sup>37</sup> 'Case markers' may be postpositions but nothing crucial hinges on this. Thus, 'case markers' here are used in a broad sense since in many languages such as Korean and Japanese they can encode almost any oblique relation and be attached even to a clausal argument, as in the following Japanese sentence:

### 4.3. Maintenance of Relative Obliqueness and Prepositional Stranding

In spite of the high degree of flexibility of surface word order in OE, one constraint on syntactic operations which seems to be generally but often implicitly assumed can be applied also to OE syntax: no matter what syntactic operation sentential elements undergo, it should not create any (serious) confusion in the grammatical relationships.

In particular, such a constraint seems to be most relevant in the case of OPO since OPO is very likely to cause a confusion in grammatical relationships, especially relative obliqueness, which was very rigidly maintained in relevant morphosyntactic operations such as passivization and subcategorization inheritance. Note that case endings cannot play a decisive role for the distinction in the relative obliqueness between verbal and prepositional arguments. This is because both verbal and prepositional arguments mostly take accusative or dative in OE and therefore P is the only distinctive obliqueness marker. In this situation, OPO in a language with a high degree of flexibility of surface word order will make it very difficult to distinguish between verbal and prepositional arguments.

Thus, if a PO is separated from its governor P and occurs in a non-canonical position, even general grammatical relationships as well as relative obliqueness will be very difficult to maintain, and therefore there wouldn't be any reasonable way to get the intended meaning of the relevant sentence. For example, in examples (35) and (36) above, it would be very difficult to decide whom the father created through whom in (35a), whom Simon threatened with whom (35b), who led whom and how (35c), and who went before whom in (36a). In spite of the general flexibility of word order in OE, therefore, the PO, with its governor P, should remain in its canonical position (i.e. within PP). There is therefore some functional motivation for why OPO was so difficult in OE.

Still, there may have been more to \*OPO than just functional concerns. For speakers of such languages as German, Japanese and Korean, which have prepositions or post-positions as periphrastic case markers, separating an obliqueness marker such as a case marker and a preposition or post-position from its host or argument is hardly tolerable and judged totally unacceptable, regardless of the comprehensibility of the given sentence. Thus, there seems to be a purely syntactic side to \*OPO. Moreover, OPO would entail the effacing of an inherent property (i.e. a certain degree of obliqueness) from the argument. Thus, \*OPO seems to reflect a tendency for speakers to reject the separation of an obliqueness marker from an oblique argument itself.

In short, the (relative and absolute) obliqueness of OE NP arguments was most systematically encoded through case endings and P as their obliqueness markers and could be best represented and maintained only when each obliqueness marker remained in its original form (for case endings) and canonical position (for P). In particular, OPO could bring about a serious problem in maintaining the grammatical and semantic relationships by altering or confusing relative obliqueness among NP arguments or at least by eliminating the absolute obliqueness of the prepositional argument. This is what

(i) Hanako-wa [s Taroo-ga zibun-o zibun-nohihan-kara mamorikir-e-nakatta] koto-o sittaite.

Hanako-TOP[ Taroo-NOM self-ACC self-GEN criticism-FROM defend-could-not COMP-ACC knew  
'Hanako knew that Taroo couldn't defend her/himself against her/his own criticism'

<sup>38</sup> Note that MnE allows parentheticals between P and PO, as in *John left his money to, for all intents and purposes, the whole family*. However, there is no evidence that such a parenthetical insertion is possible in OE as well as in Japanese or Korean.

motivated \*OPO, and the changes related to this factor can be seen to have played a significant role in the advent of new P-Stranding patterns and the PreP.

## 5. Why Old English Had No Prepositional Passive

### 5.1. What Questions to Ask and Why

Earlier studies essentially ask why OE had no PreP. It is fair to consider why we have to ask this question? First, most previous studies have put their main emphasis on an abstract analysis of the PreP and other (new) P-Stranding constructions mainly from a theoretical standpoint (e.g. generative studies such as Lightfoot 1979a,b, Allen 1980a,b, and Kemenade 1987) or on the account of some selected aspects involved (e.g. most earlier works such as van der Gaaf 1930, Jespersen 1909-49, Visser 1963-73, and de la Cruz 1973) from a traditional descriptive viewpoint. Second, unlike all the other P-Stranding constructions, the answer to the question "what was behind \*OPO or the loss of \*OPO?" doesn't directly explain the advent of the (P-Stranding in) PreP. This is because, unlike other P-Stranding constructions, the PreP requires some additional condition(s) besides "no \*OPO". To see why this is so, consider the following examples:

- (37) a. This is the long river which we slept beside last night.  
 b. Which river did you sleep beside last night?  
 c. This cold river is very difficult to sleep beside.  
 d. The long river is very beautiful to sleep beside.  
 e. \*This long river was slept beside last night (by us)

In the above examples, the sentence (37e), which has the preposition *beside* stranded in the PreP, sounds very odd at best, while the preposition can freely be stranded in other prepositional constructions, as shown in (37a-d). This difference between the PreP and other P-Stranding constructions, under the assumption that \*OPO prohibited the P-Stranding in the above constructions (37a-c) in OE, tells us that the simple loss of the constraint by itself is not sufficient for the advent of the PreP.

Few previous studies, except for Denison (1985), have tried to directly deal with the question of why OE had no PreP. Denison claims that OE differs from ME quantitatively rather than qualitatively in that OE also had the purely syntactic factor of P-Stranding but other factors (e.g. decay of OE case system, obsolescence of OE prefixal system, increased use of prepositions, lexicalization and semantic function, etc.) were "simply less strongly operative in OE" (p. 203).

However, it is not clear whether his account really addresses the issue of why OE had no PreP. Although OE might also have (the purely syntactic factor of) P-Stranding and while it might have been that the factors of the PreP were less strongly operative in OE, the kinds of P-Stranding allowed in OE are clearly distinguished from the new P-Stranding patterns in later English since P-Stranding involving OPO was never allowed in OE. Thus, the issues should be why OE had no P-Stranding with OPO, even in non-passive constructions which seldom require the additional factor(s) of the PreP, and how English came to have the (syntactic) factor(s) of the new P-Stranding patterns involving OPO, if the change in P-Stranding is significant for the advent of the PreP at all. Furthermore, although his claim that some factors promoting the PreP were "simply less strongly operative in OE than in ME" may be compatible with the general gradualness of language change, it does not sufficiently explain why OE had no PreP at all, including the potential impersonal PreP, which doesn't need OPO (see (18) and (19)).

Thus, what we really need for a complete account of why OE had no PreP and how the PreP came into being in ME is not just the lack of stronger operation of the factors promoting a PreP in OE. Rather, we need to bring other crucial factors into consideration. Above all, we must explain what, despite some already existing factors which could have facilitated a PreP but were never sufficiently developed so as to allow it, actively prevented the PreP from actually being allowed. Moreover, we have to answer what, under the ripened linguistic circumstances, actively triggered OPO in the passive construction.

## 5.2. Why Old English Had No Personal Prepositional Passive

Before the question of why OE had no PreP at all is answered, several assumptions about passivization need to be made clear. The first assumption is that an (NP) object should be 'not-too-oblique' in order to be passivized. Since the concept of 'obliqueness' doesn't seem to be clearly defined in any previous studies, let us say that an NP argument is too oblique if it is not subcategorized for by the given verb in the lexicon.<sup>39</sup> Thus, the difference in the acceptability between the following MnE examples of the PreP can be ascribed to whether the former PO (i.e. the passive subject) or the PP including the PO in each of the sentences is subcategorized for by the verb or not:

- (38) a. The document has been closely looked at.  
 b. Federal benefits have been desperately asked for by many poor people.  
 c. This conclusion was finally arrived at after a long discussion.  
 d. The boat was decided on.
- (39) a. \*Columbus was finally arrived at.  
 b. \*Columbus was died in by many people.  
 c. \*His mother was traveled with by John. (Riemsdijk 1978: 220)  
 d. \*Many hours were argued for. (Riemsdijk & Williams 1986: 147)

Second, I assume that the PO needs to be composed as a verbal argument in order to be passivized and that such argument composition is made possible by morphological incorporation (MI) or syntactic incorporation (SI), in which the verb incorporates the preposition to make a complex verb and the former prepositional argument becomes the composed argument of the complex verb [V-P].<sup>40</sup> The reason the PreP needs

<sup>39</sup> Among generally favored criteria for the passivization of a prepositional object (Radford 1988: 430) are: being a complement of a verb or being in a c-command relationship (Chomsky 1965: 105-6, Hornstein & Weinberg 1981: 58-59) with the verb, forming a 'semantic unit' (Chomsky 1977a: 87), and making a 'natural predicate' (Riemsdijk & Williams 1986: 188). Nonetheless, there are many grammatical examples of the PreP in MnE, in which the passive subject NP or the relevant PP including the NP is difficult to consider as subcategorized for by the verb, as follows:

- (i) a. The bed was slept in.  
 b. The bridge was walked under by many great people such as George Washington, Abraham Lincoln, Napoleon Bonaparte, Isaac Newton, Albert Einstein, and so on.

One crucial factor for the acceptability of the PreP is related to the 'affectedness' of the passive subject by the action expressed by the predicate (Hopper & Thompson 1980, Huddleston 1984: 441, Quirk et al 1985: 1164-5), which goes beyond the domain of syntax. However, this factor doesn't need to be considered in explaining the advent of the PreP since few early examples of the PreP concern such an aspect; still, it may reflect some significant on-going changes in the English passive and therefore should ultimately be considered in a complete diachronic and synchronic account of the passive construction.

<sup>40</sup> The reason I prefer to use the term 'incorporation' rather than 'reanalysis' is that OE also had a morphological mechanism, which is analogous to the syntactic 'reanalysis' of MnE in that the original prepositional argument can be composed as a verbal argument through the incorporation of the preposition

incorporation can be explained as follows: passivization must only be a change in the viewpoint for the semantic relationship expressed by the verb of the clause and, like most other syntactic operations, it must not involve a change in the complementhood of the involved arguments.<sup>41</sup> I propose that incorporation is responsible for a change in the grammatical function at a certain level.<sup>42</sup>

One logical conclusion from the discussion in section 3 is that if a type of P-Stranding violates \*OPO and is not necessary because some alternative construction is theoretically possible in the language system, then the given type of P-Stranding must be prohibited and the alternative must be obligatory. However, if even the theoretically possible alternative was not allowed for some independent reason, then it must be that OE couldn't have any such construction.

Thus, the answer to the question of why OE had no PreP at all can be given in two steps. The first step requires us to explain why OE had no personal PreP. First, POs in OE were 'too oblique' to be 'fully' passivized for the personal passive. That is, the personal PreP was impossible in OE because a PO in OE was always more oblique than a verbal argument, regardless of the morphological cases involved and because the rigid norm of the OE syntactic passive for any oblique object was the impersonal passive. Thus, if any form of syntactic passive were possible for a PO in OE, it would have to be the impersonal passive at best.

Second, the personal PreP had to require the P-Stranding involving OPO, which is clear since the passive subject (in the PreP) corresponds to the PO in the active in that they are assigned the same theta-role by the same head. Since no P-Stranding through OPO was allowed in OE, the personal PreP could not be allowed either.<sup>43</sup>

Finally, the personal PreP was not 'necessary' in OE because it had a potential alternative construction, that is, the (theoretically possible) impersonal PreP. In fact, no evidence has been found that OE had an impersonal PreP.<sup>44</sup> However, whether the impersonal PreP in OE existed or not doesn't make much difference. Note that the potential impersonal PreP does not violate \*OPO. Furthermore, OE had not only many examples of the impersonal passive (for oblique verbal objects) but also many prepositional constructions which have a dummy subject or no nominative subject, which made P-Stranding unnecessary. This means that in principle OE could have the impersonal PreP. Thus, if OE had no impersonal PreP at all, then there must have been some other factors which prohibited the construction. Thus, the more suitable question to ask should be why OE had no impersonal PreP or why, if it occurred, it was so rare.

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into the verb coupled with subcategorization inheritance (Kim 1997, Goh in press); also the existence of this MI is closely related to the absence of PreP in OE and to the advent of the PreP and SI.

<sup>41</sup> A similar but often more general assumption can be found in many syntactic frameworks. For example, the Projection Principle of Government-Binding theory requires lexical properties to be projected to all levels of syntactic representation (Horrocks 1987: 99).

<sup>42</sup> Radford (1988: 432) posits that the reanalysis of V and P must apply in the *Base*, after lexicalization and before transformations. I believe that the level at which such an incorporation occurs is somewhere in the lexicon rather than the syntax, which can be well accommodated in a lexicalist approach such as Head-Driven Phrase Structure Grammar (Pollard & Sag 1987, 1994).

<sup>43</sup> The correspondence between the relevant passive subject and active object is generally accepted in many syntactic frameworks, including those which don't assume movement. For example, Pollard & Sag (1994: 121) explains the passive by means of a lexical rule, in which SUBCAT lists of an active transitive verb form are permuted so that the passive subject and active object correspond to each other.

<sup>44</sup> This does not necessarily mean that OE had no impersonal PreP at all. Note that the impersonal PreP is not very common even in languages such as German in which the impersonal PreP is allowed. Thus, the unavailability of the evidence of the impersonal PreP in OE might be a gap in data.

### 5.3. Why Old English Had No Impersonal Prepositional Passive

Why then did OE essentially have no impersonal PreP? First, most true prepositional objects in OE were too oblique to be passivized. Note that OE had extremely productive P-V compounding. Thus, it is very likely that most not-too-oblique POs occurred as a composed verbal argument of P-V CVs through the MI of P by V and subcategorization inheritance.<sup>45</sup> Thus, we can find OE passive examples in which the original PO, composed as a verbal argument, becomes the passive subject. Interestingly but not very surprisingly, the meaning of the P-V CV *under-etan* 'to eat under, undermine' in (42) is quite different from that of its corresponding MnE CV *undereat*.

- (40) hie theah swa ondærendlice gebidon þæt se ege ofer-gongen wæs,  
they however fearfully awaited that the terror over-gone was  
'however, they fearfully awaited for the terror to be passed over' (Or 160.30-1)
- (41) ... þæt min freond siteð under stanhlithe storme behrimed,  
... that my lover sits under cliff by storm covered with hoar-frost  
wine werigmod, wætre be-flowen on dreorsele.  
lord disconsolate, by water around-flowed in the hall of sorrow.  
'... that my lover, my disconsolate lord, sits under a rocky cliff,  
covered with frost by the storm, surrounded by water' (Wife 47-50)
- (42) scearde scurbeorgescorene gedrorene ældo under-eotone  
cut down buildings torn collapsed by old age under-eaten  
'buildings (were) cut down, torn, collapsed, undermined by old age' (Ruin 5-6)

Supportive also is the fact that many early examples of the PreP in ME have prepositional verbs or V-P phrases whose OE counterparts in the respect of form or meaning are P-V CVs, as in the following examples (= (28)-(30)):

- (43) OE *be-licgan* 'to lie or sleep by/with/around' > ME *liggen bi*  
this maiden ... feled also bi her thi | that she was yleyen bi  
this maiden ... felt also by her thigh that she had been lain by  
'this maiden felt by her thigh that she had been lain with'  
(c1330 (?a1300) *Arth. & M.*(Auch) 849)
- (44) OE *ymb-sprecan* or *be-sprecan* 'speak of/about' > ME *speken of*  
And the comaundment ys brokun, | And the halyday, byfore of spokun,  
and the commandment is broken and the holy day previously of spoken  
'and the commandment was broken, and the holy day previously spoken of'  
(a1400 (c1303) Mannyng, *HS* 1033)
- (45) OE *on-spætan* or *be-spætan* 'spit upon' > ME *spitten (up)on*  
and aftyr he was turmentyd, and aftyr he was spyt vpon  
and afterwards he was tormented and afterwards he was spat upon  
(a1425 *Wycl. Serm.* I 39.26)

<sup>45</sup> Note that the incorporation of P by V and the composition of the prepositional argument as a verbal argument is very similar to the (syntactic) reanalysis proposed by Jespersen (1909-49: part III, vol. II, 15.74) and formulated by van Riemsdijk (1978), since through the reanalysis, P is incorporated with V to form a 'complex' or 'compound' verb and the former prepositional argument comes to have the status of a verbal argument.

Second, another reason, which is rather hypothetical since it is difficult to prove, is that OE only had MI but didn't have the mechanism of SI, which, under the assumptions made above, enables the prepositional argument to be composed as a verbal argument and then passivized, thus taking a detour around \*OPO. Regardless of the plausibility of the claim that the mechanism of SI came into being in ME and the general acceptance of the evidence of SI, the existence of the extremely productive MI of P and V in OE is very likely to have alleviated considerably or even obviated altogether the need for SI; such a situation will make the claim of no SI in OE much more plausible.

## 6. The Advent of the Prepositional Passive in Middle English

In section 3, we have observed that all cases of P-Stranding in OE had their own necessity and that such necessity was always compatible with the prohibition against OPO. What then would become of the grammar if, for some reason, the compatibility between the necessity and the prohibition collapses and they begin to conflict with each other? More specifically, if the previously unavailable P-Stranding in some prepositional construction becomes necessary due to some change(s) in the language system, with any alternative construction still remaining unavailable and \*OPO still strong, then what kind of result will the conflict bring to the relevant construction? Will one simply win over the other? Can the P-Stranding in that construction become acceptable unless it is prohibited for other reasons? In this section, I will argue that this is what really happened in the history of English and that such a conflict was resolved through an 'optimal' choice available which made it possible to detour around the apparent dilemma without immediately destroying the balance between the necessity and the constraint.<sup>46</sup>

How then did English come to have the new PreP in ME? Above all, the advent of the PreP in ME was a morphosyntactic change which was gradually nurtured by significant changes in other parts of the language system. In particular, almost every component of the grammar contributed to the advent of the PreP by jointly or independently enabling the English grammar to be equipped with the factors promoting the PreP, which were previously unavailable or insufficient.

### 6.1. Changes in Sound and Morphology

#### 6.1.1. Sound Change and Leveling of Inflectional Endings

As is well known, various sound changes around the ME period were simple but far-reaching. In particular, the weakening and reduction of (unstressed) final syllables reduced a number of previously distinct grammatical endings, including dative and accusative case endings, to a uniform *-e* [ə], which, along with the operation of analogy, brought about the leveling of inflectional endings. Thus, the OE case system was beginning to decay already in the OE period and by the early ME, English came to lose the accusative and dative distinction. Traces of this reduction of inflectional endings are found even in OE documents as early as the tenth century and by the end of the twelfth century, this change for the most part is completed (Baugh & Cable 1993: 155, Allen 1995).

In fact, the loss of inflectional endings in ME has often been linked to the advent of the PreP as well as the indirect passive (van der Gaaf 1930, de la Cruz 1973, Lightfoot 1981, etc.). We have already seen, in section 2, that the reanalysis of a prepositional

<sup>46</sup> By using the term 'optimal', I don't assume any particular theoretical framework, although this is in the spirit of Optimality theory, as noted in fn. 23.

dative object as passive subject is not tenable because of the nonexistence of the impersonal PreP in OE. Denison (1985: 193) argues that the loss of case distinctions increased the number of V and P collocations which, as a semantic unit, govern an NP as an affected DO, thereby making it eligible for passivization. However, note that, other things being equal, the loss of the formal distinctions in case itself cannot make the PO (semantically) less oblique and thereby increase the pool of eligible collocations. At best, it only means the increase of accusative POs, which already existed in OE (van der Gaaf 1930, Visser 1963-73: 391).

The loss of case distinctions, indeed, played a significant role in the advent of the PreP but its contribution lies in a different place. OE morphological cases, together with prepositions, were 'obliqueness markers', which systematically encoded and represented relative obliqueness among NP arguments. The loss of case distinctions brought about the loss of the morphological way of representing and maintaining relative obliqueness, making it necessary for English to have a different mechanism, since relative obliqueness of NP arguments has always been one of the most significant grammatical or semantic relationships, underlying both the rigid maintenance of the OH in the subcategorization inheritance of P-V CVs and the prohibition against OPO in OE.

Note that one main motivation for this prohibition was the maintenance of grammatical or semantic relationships, especially relative obliqueness. Now, due to the loss of the distinctions in case endings as obliqueness markers, English began to depend increasingly on fixed word order until it became the sole means of representing and maintaining relative obliqueness. This means that \*OPO weakened considerably, making OPO relatively easier. Thus, around the 13th century, the new types of P-Stranding, which involve OPO, began to appear. However, we should say \*OPO was still quite strong since the new P-Stranding types remained rare until the end of the 14th century.

### 6.1.2. Loss of Preposition-Verb Compound Verbs

Another important change in morphology which contributed to the advent of the PreP is the loss of OE P-V CVs. English lost most of P-V CVs along with the disappearance of many OE prefixes and appearance of a number of new prepositions (Fischer 1992: 386-7, Mustanoja 1960: 345-427). What is interesting here is that the loss of P-V CVs is not just due to the disappearance of many prefixes or prepositions or their replacement by new prepositions. This is clear since English lost even most of the P-V CVs whose components are both alive, as in (46)-(49), as well as the P-V CVs whose component P or V is lost or replaced, as in (50) and (51). Also note that almost every P-V CV which remains in MnE is semantically not very transparent, as in (52) and (53), which is generally expected from any result of compounding:<sup>47</sup>

#### (46) *ofer*-V

*ofer-beon* 'be over', *ofer-climban* 'climb over', *ofer-faran* 'go over',  
*ofer-gan* 'go over', *ofer-gestondan* 'stand over', *ofer-glidan* 'glide over',  
*ofer-hleapan* 'jump over', *ofer-lihtan* 'light upon', *ofer-ridan* 'ride across',  
*ofer-rowan* 'row over', *ofer-sawan* 'sow (over)', *ofer-settan* 'set over',  
*ofer-standan* 'stand over', *ofer-swimman* 'swim over', *ofer-þencan* 'think over',  
*ofer-wadan* 'wade over'.

<sup>47</sup> The OE words and definitions are based on Hall (1960). Note the difference between the CVs in (46) and (47) and those in (48) and (49): the preverbs *ofer-* and *under-* in (46) and (47) are still alive as both a prefix and a preposition, whereas *æt-* and *ðurh-* in (48) and (49) are alive only as prepositions.





*under-beran* 'support', *under-crammian* 'fill underneath', *under-delfan* 'dig under',  
*under-don* 'put under', *under-etan* 'eat underneath', *under-flowan* 'flow under',  
*under-gestandan* 'stand under', *under-gan* 'undermine', *under-secan* 'examine',  
*under-stredan* 'strew under'.

(48) *æt-V*

*æt-beran* 'carry to', *æt-clifian* 'cleave to', *æt-gangan* 'go to', *æt-sittan* 'sit by',  
*æt-slapan* 'sleep beside', *æt-standan* 'stand at'.

(49) *ðurh-V*

*ðurh-blawan* 'inspire', *ðurh-borian* 'bore through', *ðurh-brecan* 'break through',  
*ðurh-brengan* 'bring through', *ðurh-creopan* 'creep through', *ðurh-delfan* 'dig through',  
*ðurh-drifan* 'drive through', *ðurh-etan* 'eat through', *ðurh-faran* 'pass through',  
*ðurh-fleon* 'fly through', *ðurh-gan* 'go through', *ðurh-secan* 'search through',  
*ðurh-sceotan* 'shoot through', *ðurh-seon* 'look through', *ðurh-stingan* 'pierce through',  
*ðurh-swimman* 'swim through'.

(50) *ofer-V*

*ofer-geotan* 'pour upon', *ofer-leoran* 'pass over', *ofer-mæstan* 'over-fatten',  
*ofer-magan* 'prevail', *ofer-ricsian* 'rule over', *ofer-stigan* 'climb over',  
*ofer-swīðan* 'overpower', *ofer-teldan* 'cover over', *ofer-teon* 'draw over',  
*ofer-weorpan* 'throw over, overthrow', *ofer-wreon* 'cover over'.

(51) *under-V*

*under-bugan* 'submit to', *under-hnigan* 'submit to', *under-iernan* 'run under',  
*under-lutan* 'bend under', *under-smugan* 'creep under', *under-þenian* 'serve under'.

(52) *ofer-V*

*ofer-cuman* 'overcome', *ofer-don* 'overdo', *ofer-drincan* 'drink too much',  
*ofer-growan* 'overgrow', *ofer-libban* 'survive', *ofer-sprædan* 'overlay'.

(53) *under-V*

*under-lecgan* 'support', *under-licgan* 'be subject to, give way to',  
*under-standan* 'perceive', *under-writan* 'write at the foot of'.

Why then is it that English lost even most P-V CVs whose components are still alive? The answer to this question is closely related to the fact that those OE P-V CVs were not compounds in the MnE sense. In fact, the status of OE P-V CVs was very different from that of MnE P-V CVs: first, P-V compounding in OE was extremely productive; second, OE P-V CVs were semantically and morphosyntactically very transparent. Note that both of the features can be expected mainly for V and P phrases or prepositional verbs in MnE.

Furthermore, most of the transparent OE P-V CVs in (46)-(49), although they were lost, are still alive or have their counterparts in the form of V-P phrases or prepositional verbs in MnE. This strongly suggests that P-V compounding in OE was a morphosyntactic way of incorporating P by V as well as a part of ordinary compounding which is considered to be involved in such OE P-V CVs, as in (52) and (53), and also in most compounds in MnE. Thus, OE had at least two kinds of P-V CVs: one is the more

transparent P-V CVs which resulted from MI, and the other is the more opaque ones which were the result of ordinary compounding.<sup>48</sup>

Given this, the loss or change of OE P-V CVs can be explained as follows: first, most 'true' compounds (from ordinary compounding), if their components are alive, remain in later English, as in (52) and (53); second, English lost most of the P-V CVs whose components were lost, as in (49) and (50); and finally, English also lost most P-V CVs whose components are still alive and whose phrasal counterparts are found in MnE, as in (46)-(49). Note that the first and second cases are 'naturally' expected, whereas the third case is quite exceptional. Now if we assume that the third group was the result of the MI of P by V and that English lost the mechanism of that (transparent) MI, then the loss of the third group can also be well explained.

Although the loss of MI is related to the general trend of change in English, from synthetic to analytic, it also has much to do with the loss of formal distinctions in case as an 'obliqueness marker'. With a prohibition against OPO, flexible word order, and productive P-V compounding, one optimal choice in English for the composition of the prepositional argument as a verbal argument would be through MI by P-V compounding. In this case, the relative obliqueness between verbal and prepositional arguments can be maintained only by the morphological cases as obliqueness markers. Now, the loss of case inflections brought about the loss of the overt obliqueness markers, putting the representation and maintenance of (relative) obliqueness in danger. In this situation, an attempt to compose the prepositional argument as a verbal argument through MI will be fatal to the maintenance of relative obliqueness. Furthermore, along with the general trend of change in English grammar, relative obliqueness among NP arguments now came to be maintained mainly by fixed word order. Thus, MI came to be a very unfavorable option for the argument composition of the prepositional argument.

In short, the loss of OE P-V CVs means that English lost the productivity of P-V compounding, which was responsible for the MI of P and V as well as ordinary compounding. The loss of MI, in particular, is demonstrated by the loss of P-V CVs, which belong to the third group, exemplified in (46)-(49), and their survival as or replacement by prepositional verbs and V-P phrases in later English. This strongly suggests that the appearance of the so-called reanalysis of V and P or SI in ME is not new at all: English has had the same mechanism from the OE period, adjusting it to the change in the overall shape of the language system but maintaining its core properties.

## 6.2. Changes in Semantics

The loss of OE P-V CVs or complex verbs consisting of a (non-prepositional) prefix and a simplex verb and their replacement by corresponding V-P phrases or collocations affected the semantics of prepositional objects in general. As we have already seen, many OE P-V CVs and other complex verbs were replaced by corresponding V-P phrases or prepositional verbs (cf. de la Cruz 1973, Fischer 1992: 386-7). However, the V-P phrase replacements of P-V CVs are often semantic replacements rather than just functional replacements since many replacing V-P phrases or prepositional verbs have different P or V components from their corresponding OE P-V CVs, if they existed.

<sup>48</sup> Productivity and (semantic) compositionality are among the most commonly cited criteria for distinguishing syntactic rules and lexical rules (Wasow 1977). Considering that unlike the P-V CVs in (52) and (53), the P-V CVs in (46)-(49) are very productive *and* morphosyntactically and semantically transparent, their formations are more (morpho)syntactic rather than purely lexical.



that the transitivity function of those compound verbs, along with the loss of the Germanic prefixes as a productive system, came to be fulfilled by other means, especially by V and P collocations (Denison 1985: 193). This, above all, means that many concepts previously expressed by those P-V CVs came to be represented by V-P phrases of new or same components, in which process former verbal arguments, which could be passivized, became prepositional arguments, maintaining the passivable degree of obliqueness of the original verbal arguments. This can be considered one of the main sources which brought about not only Bennett's (1980) extension of the scope of the relation 'direct object' but also the lexicalization of some V and P sequences, as mentioned above (cf. Denison 1985: 193 and Fischer 1992: 386-7). Thus, we can find many OE P-V CVs which were replaced by V-P phrases with or without the change in their components and some are found even in early examples of the PreP.<sup>49</sup>

Most importantly, all this means that at least some English prepositional objects became less oblique or 'not-too-oblique' enough to be passivized and that the overall obliqueness of the English prepositional objects decreased. Thus, the relative obliqueness between verbal arguments and prepositional arguments now became valid only for the same verbal head in the same sentence. In OE, on the other hand, the relative obliqueness between the two was always maintained, as can be seen in the regularity of passive for verbal arguments versus no passive for prepositional arguments.

There was another semantic change which is significant for the advent of the PreP. In OE, prepositions were one of the two obliqueness markers and just like the other obliqueness marker (i.e. morphological case endings) they couldn't be overtly separated from their objects; as noted earlier, remaining in their canonical position was the best way to prevent any potential change or confusion in the absolute obliqueness of prepositional objects and the relative obliqueness between verbal and prepositional arguments.

On the other hand, along with the change in the means of maintaining relative obliqueness among NP arguments (i.e. from "obliqueness markers and the prohibition against their separation from their host NPs" to "rigid syntactic word order"),<sup>50</sup> the status of prepositions has been changed and their function as (absolute) obliqueness markers became trivial at best. Thus, prepositions in later English are now mainly used to express the relationship between the prepositional argument and other NPs (e.g. *put* NP<sub>1</sub> *on* NP<sub>2</sub>) or extra shades of meaning of the verb (e.g. *look at* vs. *look for*).

Although prepositions in later English often indicate a high degree of obliqueness (especially, with an adjunct PP), such is not always the case because at least some prepositional arguments are not very different from verbal arguments (e.g. *look into* NP vs. *investigate* NP, *look for* NP vs. *seek* NP). Furthermore, even the relative obliqueness between a prepositional argument and a verbal argument in the same clause can now be represented by the word order alone. Thus, now OPO no longer causes any serious problem in maintaining relative obliqueness or general grammatical relations, as long as the PO is located in a legitimate position after the separation.

<sup>49</sup> See the examples in (28)-(30).

<sup>50</sup> In general, relative obliqueness among NP (object) arguments in MnE can be expressed in terms of word order as follows: the nearer an (NP) argument is to the verb, the less oblique it is. This relationship between relative obliqueness and word order is generally accepted in many syntactic frameworks. For example, in Head-Driven Phrase Structure Grammar, the relative obliqueness of complements is modeled by position on the list for the SUBCATEGORIZATION value of the head (Pollard & Sag 1987: 70-1, 1994: 2-3), which generally represents the surface word order.

In short, the position of PP became much more restricted, whereas OPO came to be more permissible, opposite from the OE situation. This change in the semantics of prepositions, with the change in the obliqueness of prepositional arguments, is one of the main factors which contributed to the weakening of \*OPO.

### 6.3. Changes in Syntax

#### 6.3.1. The Establishment of Fixed Word Order

The most important syntactic change which contributed to the advent of the PreP is the establishment of fixed word order.<sup>51</sup> It is generally agreed that the loss of oblique case inflection and the fixing of SVO word order went together and that there is a correlation between these two, although the cause and effect relationship is not very clear. In addition, as we have considered above, the loss of case inflections as obliqueness markers and the replacement of their function of maintaining relative obliqueness by rigid word order are mutually supportive.

Most importantly, through the fixing of word order, English came to have a syntactic way of representing and maintaining relative obliqueness among NP arguments, which was morphosyntactically achieved by obliqueness markers (i.e. cases and Ps) and \*OPO in OE. Note that both OE morphosyntactic and later English syntactic ways of maintaining relative obliqueness are complemented by the incorporation of P by V: OE had MI by P-V compounding and later English came to have SI.

The core insight in this account of the change in the means by which relative obliqueness was maintained is that the establishment of fixed word order, along with the loss of case inflections, contributed to the weakening of the motivation behind \*OPO. That is, without the prohibition against OPO, it was almost impossible to maintain relative obliqueness between NP arguments in OE with a high degree of flexibility in word order. Furthermore, once the prepositional argument is composed into the argument structure of a verb, morphological cases were the only means to distinguish relative obliqueness. The SI of V and P or OPO with fixed word order, however, hardly causes any confusion in relative obliqueness as long as the PO is located in a legitimate position after the separation, since relative obliqueness is sufficiently represented and maintained by the relative word order and the well constrained reordering of relevant arguments.

#### 6.3.2. Syntactically Triggered Incorporation

Another important change for the advent of the English PreP is the rise of the reanalysis of V and P, the evidence for which seems to be quite generally accepted, no matter whether the mechanism itself is accepted or not.<sup>52</sup> As we have discussed in section 2, many studies posit this mechanism of reanalysis, which I call 'syntactic incorporation' (SI), to explain (the advent of) the PreP. Although several questions are still to be answered before we can accept the existence of SI, correlating SI with (the advent of) the PreP doesn't seem to encounter any serious logical problems. First, there is no clear evidence that OE had a similar type of SI. The presence of SI in OE is at best suspicious since OE clearly had functionally similar MI and this must have obviated the necessity of SI.<sup>53</sup> Second, there isn't any evidence that languages show similar evidence of SI without

<sup>51</sup> The fixing of English word order began around the beginning of the ME period and at least the prose of the ME period has much the same word order as MnE prose (Pyles & Algeo 1993: 162). On the other hand, Mitchell (1985: §3951) says that the SV(O) order was inevitable even before Norman influence began.

<sup>52</sup> For positions against the reanalysis proposal, see Postal (1986) and Koster (1987).

<sup>53</sup> Note that the impersonal PreP in German freely allows P to be separated from V, as in (18) and (19).

the PreP. Finally, the evidence for SI is found only in the PreP construction. Thus it

seems fair to assume that the PreP always involves SI and that whenever we have SI we also have the PreP; the priority relation between these two, however, is not easy to determine.

However, although applying the insight from SI to the account of the rise of the PreP seems to be essential, some serious questions need to be answered in order for this process to be fully justified as a major factor for the advent of the PreP. Why and how, above all, did English come to have SI in ME, if we assume that SI made the PreP possible or else always occurs with the PreP? Why didn't OE have a similar mechanism? It is crucial to answer such questions because otherwise a satisfactory diachronic explanation of the PreP will be lacking, even if synchronic accounts may be possible for the relevant construction at the beginning and endpoint of the change.

Furthermore, the evidence of SI seems to be found only with the personal PreP; there is no similar evidence from any impersonal PreP. Thus, even though German can have the impersonal PreP, as in (54), P-Stranding is not allowed at all and therefore the verb cannot occur with the preposition:

- (54) a. Meine Mutter sorgt für die Kinder.  
'my mother is taking care of the children'  
b. Für die Kinder wird gesorgt.  
'the children are being taken care of'  
c. Es wird für die Kinder gesorgt.  
'the children are being taken care of'  
d. \*Die Kinder werden gesorgt für / für gesorgt.

Thus, the question of why English came to have SI is closely related to the question of why the evidence for reanalysis/SI is found only in the (personal) passive and why English came to have the personal PreP only. Why then did English come to have SI and the personal PreP without having the impersonal PreP?

#### 6.4. Subject Requirement as a Trigger for the Change in the English Passive

The changes in sound, morphology, syntax and semantics considered so far influenced the English prepositional constructions and the potential for OPO in those constructions. The following table in (55) shows four main reasons why OE had no PreP at all, newly-developed, promoting factors in the advent of the PreP, and what still has to be explained:

##### (55) New Promoting Factors and What Yet To Be Explained

Reasons for no PreP	New promoting factors	What to be explained
Lack of necessity		<b>Necessity</b>
*OPO	Weakened *OPO	<b>Still strong *OPO</b>
Too oblique PO	New passivizable PO	
MI but no SI	(Loss of MI)	<b>Development of SI</b>

Above all, the various changes contributed considerably to the weakening of \*OPO. The main motivation for \*OPO was the high degree of obliqueness of the prepositional argument and the maintenance of relative obliqueness among NP arguments. Given this, the loss of P-V CVs and their replacement by V-P phrases, the development of less

oblique DO-like prepositional arguments, the loss of the case inflections, the fixing of word order, and the consequent change in the means of maintaining relative obliqueness must have decreased the possibility of confusion in relative obliqueness which can be caused by OPO.

Note that despite the significant contribution of those factors to the weakening of \*OPO, the constraint was still strong, since the new P-Stranding patterns involving OPO remained rare for a long period after the advent of the PreP. Moreover, although English lost MI along with the loss of OE P-V CVs, the passivization possibilities of previously verbal but now DO-like prepositional arguments must have been maintained without any significant change. In such a situation, one good option would be the impersonal PreP, which is quite plausible since it can satisfy the still strong \*OPO as well as the increased passivization possibility of the prepositional argument. However, there is no evidence to be found that English has had the impersonal PreP in any period.

What then made the advent of the personal PreP possible, leaving the impersonal PreP impossible? This, along with the question of why English came to have SI of V and P, suggests that despite all the major factors considered so far, there is still a logical gap which must be filled for a complete account of the advent of the PreP. More than anything else, we have to explain what, despite the still active \*OPO, actively triggered the advent of the previously unnecessary, personal PreP over the impersonal PreP, bringing about SI.

Along with other conspicuous changes in the language system, especially the establishment of the fixed SVO word order, English came to require an overt subject in virtually every sentence and construction, a constraint which can be called the 'subject requirement' (SubjR). I claim that this SubjR, which began to be dominant in English around the early ME period, triggered the long-prepared advent of the PreP, making the personal PreP virtually the only practical option. That is, the SubjR was what made OPO obligatory in the passive, changing P-Stranding in the PreP construction from unnecessary to necessary.<sup>54</sup>

Note that \*OPO was still very strong in the early ME period. Thus, the emerging SubjR must have brought about a serious conflict between the newly developed necessity of OPO and the still strong \*OPO. Because the developing SubjR required the (nominative) subject even in passive sentences and also because an indefinite dummy subject has not been allowed in English except for a limited number of impersonal verbs and in the expression of indefinite agency, passivization of the PO would have had to become obligatory, making the previously impossible OPO necessary and the potential impersonal PreP even theoretically implausible and thus unavailable.

In such a dilemma between the necessity of OPO and the still active \*OPO, SI must have been the 'optimal' choice. Above all, SI can break the apparently deadlocked situation with OPO, because it can nicely satisfy not only the obligatoriness of OPO but also the still strong \*OPO, by enabling the prepositional argument to detour around the

<sup>54</sup> Although the SubjR (cf. Perlmutter 1971: 100) is widely assumed or implied in most diachronic and synchronic studies of English, what brought about the SubjR into the grammar of English has hardly been made clear. See Kim (1996: 182-7, 234-55) for a useful discussion of the SubjR in English. Although Kim makes reference to such a constraint (i.e. "Prototypical Subject Requirement") and uses it in her account of the shift from pro-drop to non-pro-drop of the referential NP and of the changes with impersonal verbs, she does not explain what caused the SubjR to come into being in English, either. I suspect that the loss of case distinctions and (analogical) reanalysis of (dative) objects or complements as nominative subjects, which was influenced by the predominant personal constructions, played a significant role.

barrier PP. Under the assumptions we made in section 5.2, passivization requires the composition of the prepositional argument as a verbal argument, as a way of maintaining the complementhood of the prepositional argument in passivization. In fact, SI provides a good means for argument composition, enabling V to compose the original prepositional argument as its own argument so that the passivized argument in the PreP can not only maintain its complementhood in passivization but also avoid \*OPO (cf. Baker 1988, Sternefeld 1990).

Furthermore, SI of V and P is not completely new, because English already had the functionally similar MI in OE. OE had a morphological way of incorporating P into V and making the original prepositional argument inherited or composed by V through P-V compounding. English lost MI and many transparent P-V CVs came to be replaced by V-P phrases in ME, whereas a syntactic motivation for the incorporation of P by V was newly developed by the SubjR and supported by other relevant changes. In this situation, the rise of some compensatory means of SI in an analytic language would not be implausible, since loss in one component of the grammar is likely to be compensated for in another component (Hock & Joseph 1996: 211).

The SubjR as a trigger for the advent of the PreP can explain why the evidence for SI of P by V, in spite of no significant semantic difference between the two V-P phrases of the active and passive, is found only in the passive. This is because it is only in the passive that the SubjR requires the PO to become the nominative passive subject and motivates SI to make the consequent obligatory OPO possible. This also explains why English came to have only the personal PreP and also why similar evidence for SI is not found with the impersonal PreP that doesn't need a nominative subject.

In short, the advent of the English PreP was a morphosyntactic change, long in preparation and nurtured by the relevant changes in other parts of the language system. In particular, the change was triggered by the SubjR, which made OPO and the consequent P-Stranding in the PreP necessary and the impersonal PreP unavailable, and thus led to the deadlock between the necessary OPO and the still active \*OPO. This apparently contradictory situation could be saved by the help of SI of P by V, whose rise is not totally new but rather is very reminiscent, in almost every respect, of the MI of P by V through P-V compounding in OE.

## 7. Conclusion: How Syntactic Is the 'Syntactic' Change?

In this paper, we have discussed one good example of linguistic change which has generally been called 'syntactic'. Yet, it is fair to ask just how syntactic the change involved was in the advent and development of the PreP. While the change in the English passive can be considered syntactic in that it was triggered by the syntactic factor SubjR and in that SI played a significant role, even these aspects are not totally syntactic. Clearly, the triggering SubjR was developed under the influence of the changes in other parts of the grammar. As for SI, its development and status is closely related to OE MI through P-V compounding and thus there no good reason why 'syntactic' incorporation cannot be treated morphologically or morphosyntactically rather than purely syntactically. Therefore, the change was purely syntactic only at the beginning and endpoint of the change: "no PreP in OE" and "the advent of the new syntactic passive form in ME".

Our discussion so far, if it turns out to be successful, seems to argue for something interesting about language change. Linguistic change, especially syntactic change, is more likely to come about through change in the outputs which resulted from change in

the inputs (especially, in sounds and morphology) to a certain rule or principle rather than through the modification or addition of rules; at most such rule change could be a result of the change but not a cause.<sup>55</sup> This view of language change, especially regarding the passive, is also suggested by Hock (1991: 346-8) and Joseph (1992): a change in the passive, as Hock states, which is "notorious for its instability", does "not involve the syntax of the passive, but its morphological encoding".

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<sup>55</sup> See Joseph (1998) for this line of argument about linguistic change.



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## VARIATION IN VOICED STOP PRENASALIZATION IN GREEK

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### 1. ABSTRACT

Ancient Greek clusters of nasal (N) plus voiceless unaspirated (T) or voiced stop (D) merged to ND in Middle Greek, yielding mainly ND or D in the modern dialects. Impressionistic studies suggest that currently there is stylistic variation between D and ND in the dialects that have developed these reflexes, with ND as the formal variant. Our study reveals that age, not style, is the most important factor in ND/D variation, with speakers under 40 using dramatically fewer ND tokens than older speakers; at the same time NT, a variant which reflects spelling conventions and is possible only across word boundaries, emerges as a careful style marker. This abrupt change of pattern, which coincides with important sociopolitical changes in Greece and the official demise of Katharevousa, the H(igh) variety of Greek diglossia, suggests that a real sound change in progress away from the previous pattern of stable variation may be taking place in Greece.

### 1. INTRODUCTION

Ever since the pioneering work of Labov (1963), it has been recognized that the study of sound change cannot be divorced from a consideration of synchronic variation<sup>1</sup>. Similarly, the social context in which variation occurs must be taken into account, for

there is a crucial social dimension in the spread and generalization of sound change throughout a speech community.

In this paper, we examine variation in the realization of voiced stops<sup>2</sup> in Modern Greek, and arrive at the conclusion that the ways in which the phonetic variation correlates with various social factors indicate a sound change in progress. We further propose a possible reason for this change in a long established pattern of stable variation, namely that it has been induced by political and concomitant social changes that have taken place in Greece in the past 25 years.

We first present the historical background to the Modern Greek situation, which is important for the assessment of the nature of the variation reported on. We then turn to a sociophonetic study, followed by a discussion of our results.

## 2. HISTORICAL BACKGROUND

Three types of homorganic nasal+stop clusters occurred in Ancient Greek: nasal (N)+voiced stop (D), nasal+voiceless unaspirated stop (T), and nasal+voiceless aspirated stop (T<sup>h</sup>), as summarized in (1a-c):

### 1. Ancient Greek nasal+ stop clusters:

- a. ND: [mb, nd, ŋg], spelt <μβ, νδ, γγ> respectively
- b. NT: [mp, nt, ŋk], spelt <μπ, ντ, γκ> respectively
- c. NT<sup>h</sup>: [mp<sup>h</sup>, nt<sup>h</sup>, ŋk<sup>h</sup>], spelt <μφ, νθ, γχ> respectively.

Relatively early on in the development of Post-Classical Greek, during the Hellenistic period, the aspirated voiceless stops changed to voiceless fricatives even in the clusters with nasals (Browning, 1983: 26-7; Sturtevant, 1940: 83-5); thereafter the original NT<sup>h</sup> clusters followed their own path of development more akin to that of N+/s/ clusters (the other nasal+fricative cluster).

The ND and NT clusters, on the other hand, merged to ND (Tonnet, 1993: 40-46). The oral closure of the Ancient Greek voiced stops, which in other environments became voiced fricatives, was maintained after nasals, while the Ancient Greek voiceless unaspirated stops, which otherwise remained stable throughout the language's history, became voiced after nasals. The postnasal voicing of NT clusters was most likely a Byzantine/Middle Greek innovation, beginning around the 6<sup>th</sup> or 7<sup>th</sup> centuries and completed by roughly the 10<sup>th</sup> to 12<sup>th</sup> centuries AD<sup>3</sup>.

Evidence for the merger of NT and ND comes from several sources. Spellings like <πέμμπει> for Ancient Greek <πέμπει> ((pempē)) 'sends' in 7<sup>th</sup> century papyrus (Tonnet, 1993: 45-6) point to a merger: that is, on the assumption that the first <μ> indicates the nasal, the remaining letters, <μπ>, must represent something else, and that cannot be the voiceless stop [p], which would have been spelt simply with the letter <π>, rather than the digraph <μπ>; therefore <μπ> must stand for the voiced stop [b] here. Equally revealing are reverse spellings, understandable in the context of a merger of NT and ND; e.g., <πονδικόν> 'mouse' for etymological <ποντικόν>, or <τσιγκάνα> 'Gypsy-woman' from earlier <αθγγάνα> (*Tale of the Quadrupeds*, 150, 285 [14<sup>th</sup> c.]). Finally evidence comes from the use of <NT> spellings in loan words with ND in the source

language; e.g., <μαντάτο> 'news' from Latin (mandatum), <Λουμπαδοί> 'Lombards' (*Chronicle of Morea*, 1012 [13<sup>th</sup> c.]), or <εμπουκκόνεται> 'crams one's mouth' (Prodromos IV, 73 [12<sup>th</sup> c.]), a verb derived from the Latin *bucca* 'mouth'.

From this ND outcome in Middle Greek, two major developments are found in Modern Greek dialects (Mirambel, 1959; Newton, 1972): (i) preservation of ND word-internally and simplification to D word-initially, and (ii) simplification to D in all positions. Newton (1972: 94) observes that the former outcome is found "throughout the south east [...], most of northern Greece and much of the Peloponnese." The latter outcome is found in "all Cretan, Thracian and eastern Macedonian dialects, as well as those spoken in the islands which belong to the northern complex", and the Ionian islands of Kefhalonia, Ithaki and Zakynthos (Newton, 1972: 95)<sup>4</sup>. Thus the two main types of dialect differ according to the presence or absence of a nasal in the outcome of earlier ND in word-internal position. For example, from Ancient Greek <πέντε> ([pente]) 'five' and <άνδρας> ([andras]) 'man', representative modern dialects like Rhodian and Cretan show the outcomes in (2a-b):

2. a. Rhodian: [pente]           Cretan: [pede]  
b. Rhodian: [andras]       Cretan: [adras]

Foreign borrowings with sequences such as [b], [mp] and [mb] followed the same course, though the exact treatment of these sequences "seems to depend partly on the phonological rules operating for inherited words and partly on familiarity with the donor language" (Newton, 1972: 121).

Other developments also occurred but to a far lesser extent. For instance, Mirambel (1933) mentions some dialects of Asia Minor which, at least around the turn of the century, had nasals without stops as the outcome of ND: e.g., Cappadocian has [meno] 'enter' from earlier [embeno] (cf. Standard Greek [beno]). Also, in a few dialects, the nasal assimilated completely to the following stop, yielding DD, an outcome "found at least in the Dodecanesian islands of Simi and Kalimnos" (Newton, 1972: 95), and also in parts of Cyprus and Chios (Mirambel, 1933: 164). Despite this variety of reflexes, for the vast majority of dialects over a large area covering the central part of the Greek-speaking world the attested outcomes are either ND or D. Indeed, these two outcomes are the only ones present in the (primarily Peloponnesian and Ionian) dialects that provided the basis for the formation of the modern Athenian dialect on which, in turn, Standard Modern Greek is based (Browning, 1983: 100ff.).

The sound changes discussed so far concern the lexical level, i.e., applied within word boundaries. In addition, stop voicing now applies post-lexically<sup>5</sup>, i.e., across word boundaries, although the environments in which it takes place have not yet been given a full description<sup>6</sup>. It is not our intention here to give a full account of the rules of post-lexical stop voicing in Greek. Suffice it to say that it applies at least when certain function words—such as the negative markers /den/ and /min/ and all weak object pronouns and articles ending in /n/ (with the possible exception of /ton/ GEN, plural, which may trigger only nasal assimilation)—precede a host verb (or noun) beginning with a voiceless stop; e.g.,

3. a. <δεν καπνίζω> /den ka'pnizo/ → [ðe(ŋ)ga'pnizo] 'not I-smoke'  
b. <τον τουρίστα> /ton tu'rista/ → [to(n)d'u'rista] 'the tourist/ACC'



The post-lexical context presents an added problem, however. In most occurrences, a noun or verb with an initial voiceless stop is not preceded by a word-final nasal that would trigger voicing of the stop. As a result, the voicelessness of the stop is maintained underlyingly and frequently surfaces, e.g., in the nominative singular case (4a), when a verb is followed by a non-pronominal object (4b), and when it is preceded by a pronominal object that does not end in a nasal (4c):

4. a. <ο τουρίστας> [o tu'ristas] 'the tourist/NOM'  
 b. <πειράζω την Ελένη> [pi'razo tin e'leni] 'I-tease Helen'  
 c. <το καπνίζω> [to ka'pnizo] 'it I-smoke'

Therefore, at all stages of Greek in which post-lexical voicing occurred, there would be synchronic motivation for an underlying voiceless stop in all the words that have ND or D in the post-lexical context for NT developments, because of the morphophonemic alternations between T and ND or D. Thus, at each such stage, synchronic rules would be needed which mirror the sound changes: NT → ND or NT → D, depending on the dialect. By extension, it has been argued that all surface voiced stops can be treated as deriving from an underlying NT (among others, Kazazis, 1969; Malikouti-Drachman & Drachman, 1992; Newton, 1972; Warburton, 1970; but see also Joseph and Philippaki-Warburton, 1987: 230-231, for a discussion, and Arvaniti, in press, for a different perspective). Under such an analysis, there has been phonological stability with these developments for a long time in Greek: at any given stage since Middle Greek, there would be synchronic motivation for a nasal being involved in the derivation of voiced stops, whether or not the voiced stop occurring on the surface was preceded by an overt nasal.

### 3. SYNCHRONIC VARIATION

Although the gross division of Greek dialects into those that have a D and those that have an ND reflex appears to be largely correct, developments in the last few decades suggest that both ND and D dialects exhibit variation in the realization of voiced stops. The D dialects show ND pronunciations as formal style variants (Kazazis, 1968; Newton, 1972), while the ND dialects show a tendency to simplify ND to D word-internally in casual speech (Kazazis, 1976; Newton, 1972).

Indeed, the simplification of ND to D seems quite widespread nowadays in the ND dialects, including Standard Greek as spoken in Athens. As noted, this is not a new phenomenon; as early as 1972, Newton remarks that "in the Peloponnese there do seem to be speakers, particularly among the younger generation, whose speech would place them here [in the D dialects] rather than in group B [the ND dialects]; indeed in Athens itself the nasal is rarely perceptible at least as far as fairly rapid speech is concerned"; and further on, "many speakers in the Peloponnese and northern Greece have a very slight nasal onset [...] and indeed often seem to show fluctuation in the clarity with which the nasal element is articulated" (Newton, 1972: 95).

Earlier than Newton, Householder (1964) had attempted to account for this variation by suggesting that in Greek there are four categories of words: (i) those that fluctuate

between D and ND, (ii) those that are pronounced exclusively with ND, (iii) those pronounced exclusively with D, and (iv) those pronounced exclusively with NT. According to Householder the choice of variant depended on the etymological origin of the word; e.g., it seems that category (i) included mainly inherited words, although this is not explicitly mentioned. Householder's conclusion is highly doubtful—linguistically naïve native speakers do not usually know the etymology of words—and probably induced by the fact that his data included many recent loans and were elicited from just four native speakers, who were postgraduate students in the US and hence far from representative and linguistically naïve. Presenting a more balanced view, Mackridge (1990a: 71) remarks: "As the situation appears today, in Athens at least, the absence of the nasal in these cases [words spelt with a nasal+stop digraph] is generalized, even among people with higher education, though it is more widespread among the young, especially the males, and the less well educated [our translation]."

On the other hand, as we noted, ND pronunciations do appear in D dialects as formal variants. This is understandable given that "Standard Modern Greek" is described as one of the ND dialects, and ND has been the pronunciation prescribed by grammarians (see Mackridge, 1990a: 71 for a discussion). The higher prestige of ND is probably also related to the influence of spelling: in Modern Greek, voiced stops are written with a nasal element ([mb]/[b] are orthographically <μπ>, [nd]/[d] are <ντ>, and [ŋg]/[g] are <γγ>, or <γγ> word-internally). Furthermore, spelling reflects pronunciation much more in Greek than in other languages with historical orthography.

The influence of spelling is also due to the importance of the written language during over a century of official diglossia in Greece: the so-called "puristic" archaizing H(igh) variety of Greek, *Katharevousa*, was primarily a written language, the use of which was associated with education and power (on the importance of the written language and the prestige of *Katharevousa* see among others Browning, 1982; Frangoudaki, 1992; Mackridge, 1990b). Thus, the prestige of the written word may well have been reflected in pronouncing words as they are spelt, a trait obviously associated with literacy and education, hence with a formal style of speech. Kazazis (1968) for instance, mentions that a Greek first-year student visiting him in the US pronounced [koli(m)bo] 'I-swim' as [kolim'po], an utterly unacceptable pronunciation, which Kazazis interprets as the student's attempt to impress him (Kazazis) in his role as professor.

What emerges from the above impressionistic accounts of variation in the pronunciation of voiced stops is that in Standard Greek and many other dialects ND and D are perceived as being stylistically distinct: the observations of Kazazis (1968, 1969), Newton (1972) and Mackridge (1990a) suggest that prenasalized stops are perceived as reflecting a more formal style than oral voiced stops (see also Mikros, 1997, for the attitude of the media towards D and ND).

More recent quantitative studies (Charalambopoulos, Arapopoulou, Kokolakis & Kiradzis, 1992; Pagoni, 1989) have attempted to determine some of the social and linguistic correlates of the ND/D variation (henceforth (ND)). Pagoni (1989) recorded 22 middle class informants reading a word list (a mixture of words with voiced stops and distractors) and a short passage which imitated newspaper style. She found that the



realization of (ND) depends on age, with older speakers using more ND tokens than younger speakers, on education, with more educated speakers using more ND tokens than less educated ones, and on what she terms "beliefs and attitudes towards life and society" (p. 410), with more conservative speakers using, not surprisingly, more ND tokens. However, Pagoni's sample was, by her own account, rather limited in three ways. First, the data represent a formal style of speech. Second, the sample included only word-internal ND, and so provides no information on the realization of ND in word-initial and post-lexical position. Finally, her speakers formed a closely knit social network of conservative middle-class educated Athenians. Pagoni herself follows Milroy (1987) in accepting that "no claim can be made that the speech samples collected in this way are representative of the speech of a whole community" (Milroy, 1987: 38, quoted in Pagoni, 1989: 403).

Charalambopoulos *et al.* (1992), on the other hand, provide important information on the linguistic factors that influence (ND) realization, but have little to say on the social factors involved, as their sample of 20 speakers consisted of university students between the ages of 20 and 30, i.e., of educated speakers of the same generation. A second limitation of their study is that it included only casual speech, with all the data being elicited during an informal interview between two people who knew each other well, thereby eliminating the possibility of investigating a stylistic dimension to the variation. Third, the speakers were from Thessaloniki and their distinct accent may well have biased the results; our impression as speakers of Greek, as well as that of other Greek linguists<sup>8</sup>, is that D is far less prevalent in Thessaloniki than in Athens. Despite these limitations, certain of Charalambopoulos *et al.*'s observations are revealing. Particularly interesting is the comment that data from four older speakers, who were University lecturers, differed dramatically from those of the main body of the research: "The picture here is entirely different with a significantly higher tendency for prenasalization in all contexts, even in word-initial position" [our translation] (p. 296). In contrast, they observe that among the young speakers "the tendency not to prenasalize voiced stops is overwhelming, in contrast to the accepted norm that these sounds are pronounced oral in word-initial position but prenasalized word-internally" [our translation] (p. 295). Finally, they mention that "no important differences between men and women were observed relating to the question of voicing and prenasalization" [our translation] (p. 301).

The evidence from these two studies would suggest that the current situation is merely a continuation of a long period of stable variation (in the sense of Labov, 1981: 184). This view is further supported by (a) the fact that the ND/D variation has a history within Greek of at least several hundred years, perhaps even longer, and (b) the phonological stability of underlying NT, as noted above, resulting from the post-lexical application of the stop-voicing rule. However, there are several reasons why we would like to question this interpretation of the available data. First, we note that in both studies there were no significant differences between the speech of men and women; this lack of difference is thought to be an indication of a sound change that has been completed (Labov, 1990). Second, the age of the speakers emerges as a very important factor both in Pagoni (1989) and in Charalambopoulos *et al.* (1992), indicating that we may be dealing with change in apparent time. Thus, although the results of these two quantitative studies provide valuable insight into the ways the social factors affect the realization of voiced stops,

further study of the status of (ND) in Greek seemed necessary, in particular the investigation of whether in fact the current situation represents continued stable variation or a real change in progress altering the nature of the (now unstable) variable (ND).

#### 4. THE STUDY

##### 4.1. *The Sample*

Thirty native speakers of Greek, ranging in age from 18 to 71, were recorded in Athens, Greece. The speakers formed a *judgement sample* (see Chambers, 1995: 39ff.), in that they were not chosen randomly but on the basis of their age, gender, and occupation. Although a few of the subjects knew each other, they were not in any way part of the same social network(s), as they lived in different parts of the city, associated with different people and were employed in widely different professions.

The speakers fell into three age groups, from 18 to 30, from 31 to 45 and from 46 to 71<sup>9</sup>, each comprising ten speakers, five men and five women. The age groups were chosen so that the same number of years be included in each one of them as far as possible. At the same time, each group corresponded to a different stage in the life of the speakers (cf. Thibault & Vincent, 1990): most of the people in the first age group still lived at home or had just started their own family and career; those in the second group were largely established in their profession and had growing families, while most of those in the third age group had grown up children and the oldest among them were moving towards retirement.

The linguistic background of the subjects was not uniform. Although they all lived in Athens, only nineteen of them had been born and raised there. The rest had been born in other parts of Greece (e.g., Corfu, Thessaly, Siros and Mani) but had lived in Athens most of their lives. In addition, three speakers had studied in Britain, but they had all returned to Greece several years before the recording and had had little contact with English since their return. We believe that this lack of uniformity in the linguistic background of the subjects accurately reflects the reality of the situation in Athens: a large percentage of its inhabitants (especially the older ones) are not natives of the city, though they have lived there for decades, while knowledge of foreign languages (particularly English) is a widespread phenomenon.

The subjects were divided into three groups according to their education. Speakers were classed as having primary education if they had completed no more than the nine years of compulsory education; they were considered to have secondary education if they had graduated from secondary school or technical college; speakers who had continued their studies after secondary school (including University students) were considered to have higher education. Unfortunately, our sample was not as evenly divided in this respect as we would have wished; there were twelve subjects with higher education, sixteen with secondary education and two with only primary education (40%, 53.3%, and 6.7% of the sample respectively). The corresponding percentages in the literate subset of the Athenian population within the 20-69 age-span are 23.2%, 47.2% and 29.6% respectively (data derived from the 1991 census, Greek National Statistical Service).

Finally, our speakers were divided into three broad social classes, professionals, white-

collar workers and blue-collar workers, on the basis of occupation and income (see Thibault & Vincent, 1990, on the validity of a socio-economic classification of speakers on the basis of their profession). For the younger speakers who had just finished school or were university students, class was determined on the basis of their parents' occupation and income. For women who did not work outside the home, class was determined on the basis of their family background and situation at the time of the recording. To be sure, these class categories are not as fine grained as those used in some studies (e.g., Trudgill, 1974), but in the context of Greek society, which is not sharply socio-economically stratified and shows relatively high social mobility (see Lytras, 1993; Mouzelis, 1978; Tsoukalas, 1987), we believe that they are adequate for our purposes.

#### 4.2. Materials and procedure

The material used in this study included two speech styles, reading and conversation, and so it is intermediate between the very formal style elicited in Pagoni (1989) and the very informal one elicited in Charalambopoulos *et al.* (1992).

The specific question addressed by this study was not explained to the speakers: they were told that it related to the first author's research in linguistics, but no further details were given prior to the recording. The speakers were asked first to read a two-page narrative of childhood reminiscences, composed so as to include several instances of the (ND) variable (the original text and an English translation can be found in Appendices I and II respectively). The speakers were asked to read the text twice with a small break in between, a procedure none of them found particularly tiring. They were instructed to have a look at the text and read it as they would at school where it is standard practice to ask pupils to read literature passages aloud. The text was written in informal style in order to encourage the speakers to read in a natural way; most speakers in fact adopted a natural and lively style similar to that described by Laferriere (1979: 607) for her Irish speakers.

The text contained 18 instances of voiced stops in word-initial position, 28 instances of word-internal voiced stops, and 15 instances of post-lexical voiced stops<sup>10</sup> (see Appendices III, IV and V respectively). With the exception of word-internal stops among which alveolars predominated, the stops were roughly equally divided between the three places of articulation, including the two allophones of /g/ ([j] before the front vowels /i/ and /e/, and [g] elsewhere). As can be seen in Appendices III and IV the corpus included on the one hand both colloquial and learned words, and on the other both words of Greek origin and loans.

An extract from the text is given below in (broad) phonetic transcription (in which target sequences are underlined), and in translation:

[fisi'ka a'fti ðen 'itan i 'moni fo'ra pu 'vrika to be'la mu | 'imuna skada'ljariko pe'ði | ce si'xna me 'malonan | mja fo'ra ja pa'raðiyma | 'epeza stin pla'tia | 'otan 'epjase mja ðina'ti 'hora | a'ði na 'trekso sto 'spiti san 'tala pe'ðja | e'yo 'kaðisa capo'lamvana ta bubuni'ta ce ti vro'çi | me apo'telezma na 'jino mu'sciði | to 'ti 'ksilo 'efaya ja'fto | ðe 'lejete |]

'Of course this was not the only time I got into trouble. I was a naughty child and was often scolded. One time, for example, I was playing at the [village] square

when a heavy rain storm started. Instead of running home, like the other children, I stayed to enjoy the thunder and the rain, getting drenched as a result. I can't begin to describe the thrashing I got for this."

The reading of the text was followed by approximately 30 minutes of conversation with each speaker. The topic varied depending on their interests and background; e.g., the topics included the University entry examination some of the younger speakers had just taken, and the reasons for the telecommunications strike one of the speakers was taking part in. In general the speakers were relaxed and many chose to talk of personal matters (e.g., the recently broken engagement of a son, the illness of an aging father) although not acquainted with the interviewer. Most of the speakers soon forgot the tape recorder completely and some even expressed surprise when it was turned off at the end of the interview, as they had not noticed the point at which the recording had begun.

The recordings took place in reasonably quiet conditions, either in the speaker's or the first author's house. Although every possible precaution was taken to avoid noise, if prolonged noise (such as a telephone ringing or a dog barking) happened to occur during the reading session, the recording was stopped, and when the noise was over, the speaker was asked to repeat a paragraph or a few lines. No such interruption was deemed necessary during the recording of the conversation.

#### 4.3. Measurements and statistical analysis

The reading data were digitized at 16 kHz and wide-band spectrograms of the target sequences were obtained using a Digital Kay-Sonograph 5500. The data were classified (by the first author) into categories on the basis of the spectrograms and the auditory impression given by each token. In cases of doubt the spectrographic evidence prevailed. Initially, it was decided that seven categories should be used for the classification of the tokens: oral voiced stop (henceforth D), prenasalized voiced stop (ND), nasalized vowel+voiced stop ( $\check{V}D$ ), voiceless stop (T), nasal+voiceless stop (NT), nasalized vowel+voiceless stop ( $\check{V}T$ ), and voiced fricative (F). These categories were considered necessary in order to capture differences in the phonetic realization of the stops, which were discovered in the process of the acoustic analysis. For example, Charalambopoulos *et al.* (1992) and Pagoni (1989), who base their results solely on auditory transcription, do not make any mention of fricative pronunciations in place of stops (on the limitations of auditory transcription see Kerswill & Wright, 1990).

For the statistical analysis, however, some of the categories into which the tokens were originally classified were pooled. Thus, categories ND and  $\check{V}D$  were both classed as ND, categories NT and  $\check{V}T$  were both classed as NT, and categories D and F were both classed as D. The reason for pooling the realization categories with a nasal element on the basis of the voicing of the stop was that despite differences in phonetic realization, the presence or absence of nasality appears to be perceived categorically by the speakers. That is, naïve native speakers seem to classify voiced stops as either oral or prenasalized without making any further distinctions relating to the degree of nasality. A similar situation obtains in production: measurements of the nasal portion of the stop closure in part of the present data show that the length of the nasal closure varies widely from token to token even within the data of the same speaker, and does not seem to depend on any of the

parameters that affect the presence/absence of nasality itself. Thus, the prenasalized

tokens of older speakers (who in general used the ND variant more) do not show longer nasal stretches than those of younger speakers. It is also significant that in previous studies, in which auditory analysis only was used, there is no reference to degrees of nasality, although Pagoni (1989: 408) does have a category for tokens "with a very slight nasal onset i.e., cases that could be attributed to both categories [prenasalized and oral] due to a fluctuation in the clarity with which the nasal element was articulated." Finally, voiced fricatives, (F), were classed with D, because, without training, they were auditorily indistinguishable from oral voiced stops, (D), but auditorily and acoustically distinct from *underlying* voiced fricatives<sup>11</sup>.

The transcription of the conversation was done after the reading text had been transcribed and acoustically analyzed. Since the transcriber (the first author) had by then become familiar with the auditory and acoustic properties of the variants, it was possible to transcribe and classify the conversational data on the basis of auditory analysis alone. The relevant tokens from the conversational data were classified in the four main categories mentioned above, ND, D, NT and T. All together 1736 tokens of voiced stops in word-initial (181), word-medial (991) and post-lexical (564) position were recorded, i.e., the conversation with each speaker yielded on average 58 tokens.

The percentage of tokens in each category was calculated separately for each speaker and style, and these percentages, rather than the raw data, were used for the statistical analysis. (The data from the two readings of the text were pooled in each case, as initial tests did not show any differences between the two repetitions.) This procedure yielded twelve dependent variables, which represented the percentages of each of the variants of (ND), in word-initial, word-internal and post-lexical position (4 variants  $\times$  3 contexts).

The data were classified according to the following independent variables: gender (male, female); age (18-30, 31-45 and 46-71); education (primary, secondary, higher); class (professionals, white-collar workers, blue-collar workers); and style of speech (reading, conversation). Originally the data had also been coded for place of articulation, but as preliminary tests showed no effect of this factor on (ND), it was omitted from the main analysis of the data (Pagoni, pers.com., also found similar results for place of articulation). The same holds for the origin of the words (colloquial vs. learned, inherited words vs. loans) in the reading material. The data were analyzed by multivariate analyses of variance (MANOVAs); for significant interactions and factors with more than two levels, such as age, the tests were followed by planned comparisons.

##### 5. RESULTS

The realization of (ND) differed depending on whether (ND) was word-initial, word-internal or post-lexical. Figure 1 shows the percentage of the three main variants, ND, D and NT, in word-initial, word-internal and post-lexical position. (We will not be discussing the results for variant T, as it accounts for less than 1% of the data.) As can be seen in Figure 1, there were far fewer ND tokens in word-initial position than either word-internally or post-lexically, but only a small difference between the word-internal and post-lexical percentages of ND. In contrast, variant D shows considerable reduction from word-initial to word-internal to post-lexical context. This reduction in the use of D

is largely due to the fact that NT, which is virtually non-existent in the two lexical contexts, accounts for 10.75% of the tokens post-lexically. Because of these differences between the three contexts for (ND), and in order to make the results clearer, we present the effects of the various sociolinguistic factors separately for word-initial, word-internal and post-lexical (ND).

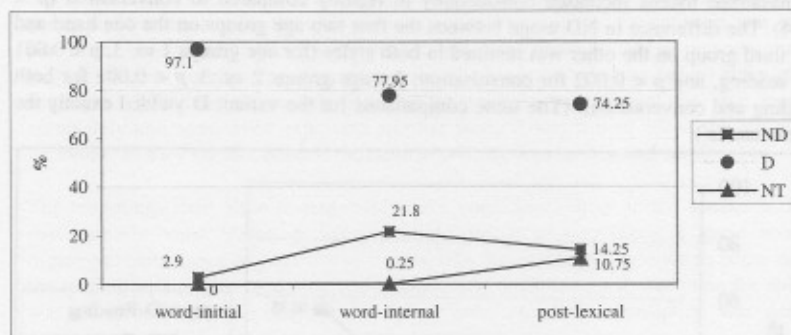


FIGURE 1: Mean percentages of the variants ND, D and NT in word-initial, word-internal and post-lexical position.

### 5.1. Word-initial (ND)

In 97.1% of the cases of (ND) in word-initial position the variable was realized as D, with the rest of the tokens being realized as ND (2.9%). Although the percentage of prenasalized tokens was very low, it is interesting to note that nearly 3% of the tokens did show prenasalization, contrary to impressionistic accounts claiming that word-initial stops are always oral (among others, Newton, 1972). These results are in agreement with those of Charalambopoulos *et al.* (1992) who also found prenasalized word-initial tokens. The results were not affected by age, class, or education, but were affected by gender (Wilks'  $\lambda(2, 55) = 0.88, p < 0.029$ ). Specifically, in word-initial position women used more ND tokens than men but only in conversation (the means were 5.25% and 0.55% for women and men respectively;  $p < 0.007$ ). In contrast, men's and women's reading percentages were the same (the means for men and women were 1.5% and 3.34% respectively). This difference is difficult to explain; however, prenasalized stops, especially in word-initial position, may sound somewhat emphatic and "involved", so their highest percentage in the data from female speakers could indicate higher involvement in the conversation; this interpretation is corroborated by the fact that many of these prenasalized word-initial tokens appeared in ejaculations, such as [ba] 'no (I don't think so)' and [bo'ri] 'may be'.

### 5.2. Word-internal (ND)

In the word-internal context the variants ND and D prevailed and together account for 99.85% of the data. The two interacting factors that affected (ND) realization word-internally were age and style.

As can be seen in Figure 2, the speakers in the two youngest age groups exhibited the

same pattern, namely a very low percentage of ND tokens and a very high percentage of D tokens in both styles of speech (no differences relating to either age or style were found between the first two groups). In contrast, the speakers in the 46-71 age group used a much higher percentage of ND tokens in both styles, but they also showed a significant difference between reading and conversation: in their data the percentage of the prenasalized tokens increased considerably in reading compared to conversation ( $p < 0.04$ ). The difference in ND usage between the first two age groups on the one hand and the third group on the other was retained in both styles (for age groups 1 vs. 3,  $p < 0.001$  for reading, and  $p < 0.002$  for conversation; for age groups 2 vs. 3,  $p < 0.001$  for both reading and conversation). (The same comparisons for the variant D yielded exactly the same results.)

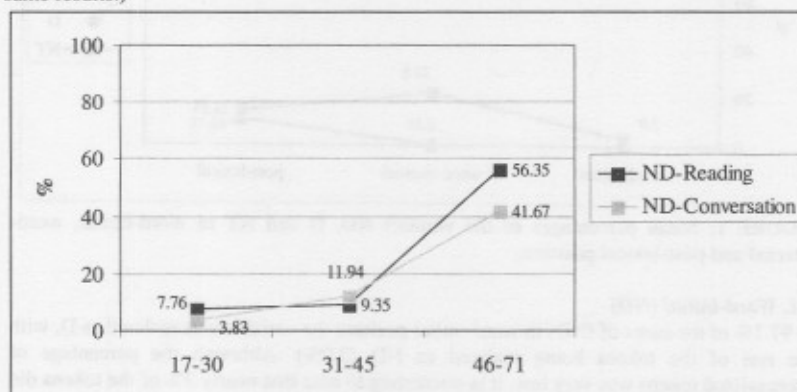


FIGURE 2: Mean percentages of the variant ND in word-internal position, by style and age.

In contrast to age and style, our results did not show any differences related to gender, class, or education (see Table 1 for a breakdown of the data according to these three factors). Note, however, that the statistical results on education concern only those speakers with secondary and higher education, because of the small number of speakers with only primary education in the sample.

### 5.3. Post-lexical (ND)

In contrast to the data from word-internal (ND) which showed little sociolinguistic variation (with the exception of the age and style effect), the realization of post-lexical (ND) was influenced by age, style and gender, though not by education or class (results broken down by education and class can be seen in Table II). The affecting factors interacted with one another and influenced each of the three variants, ND, D and NT, differently: while ND and D were affected by age and style, but not by gender, NT was affected primarily by gender and, to a lesser extent, by age and style.

**Table I:** Mean percentages and standard deviations of the ND and D variants word-internally, according to gender, class and education. (The sum of the ND and D percentages is on occasion slightly less than 100, due to the presence of some T tokens.)

			ND	D
GENDER	Women	Mean	23.71	76.27
		S.D.	22.61	22.62
	Men	Mean	19.92	79.82
		S.D.	27.65	27.56
CLASS	Blue-collar workers	Mean	15.60	84.39
		S.D.	18.62	18.62
	White-collar workers	Mean	21.82	78.02
		S.D.	23.63	23.60
	Professionals	Mean	24.40	75.43
		S.D.	29.18	29.08
EDUCATION	Primary education	Mean	30.48	69.51
		S.D.	22.02	22.02
	Secondary education	Mean	21.46	78.53
		S.D.	23.06	23.61
	Higher education	Mean	20.90	78.80
		S.D.	27.76	27.66

**Table II:** Mean percentages and standard deviations of the ND, D and NT variants in post-lexical context, according to class and education. (The sum of the ND, D and NT percentages is in some cases slightly more than 100 and in others, slightly less; the former result is due to rounding, the latter to the presence in these cases of some T tokens.)

			ND	D	NT
CLASS	Blue-collar workers	Mean	9.22	79.19	11.24
		S.D.	13.93	29.28	25.17
	White-collar workers	Mean	13.42	72.00	13.21
		S.D.	15.52	26.65	16.13
	Professionals	Mean	16.63	74.61	9.50
		S.D.	24.02	27.80	7.40
EDUCATION	Primary education	Mean	18.73	54.13	27.28
		S.D.	18.70	33.30	36.41
	Secondary education	Mean	15.16	75.15	10.55
		S.D.	13.23	25.66	15.95
	Higher education	Mean	14.19	76.29	9.10
		S.D.	23.35	27.90	8.00



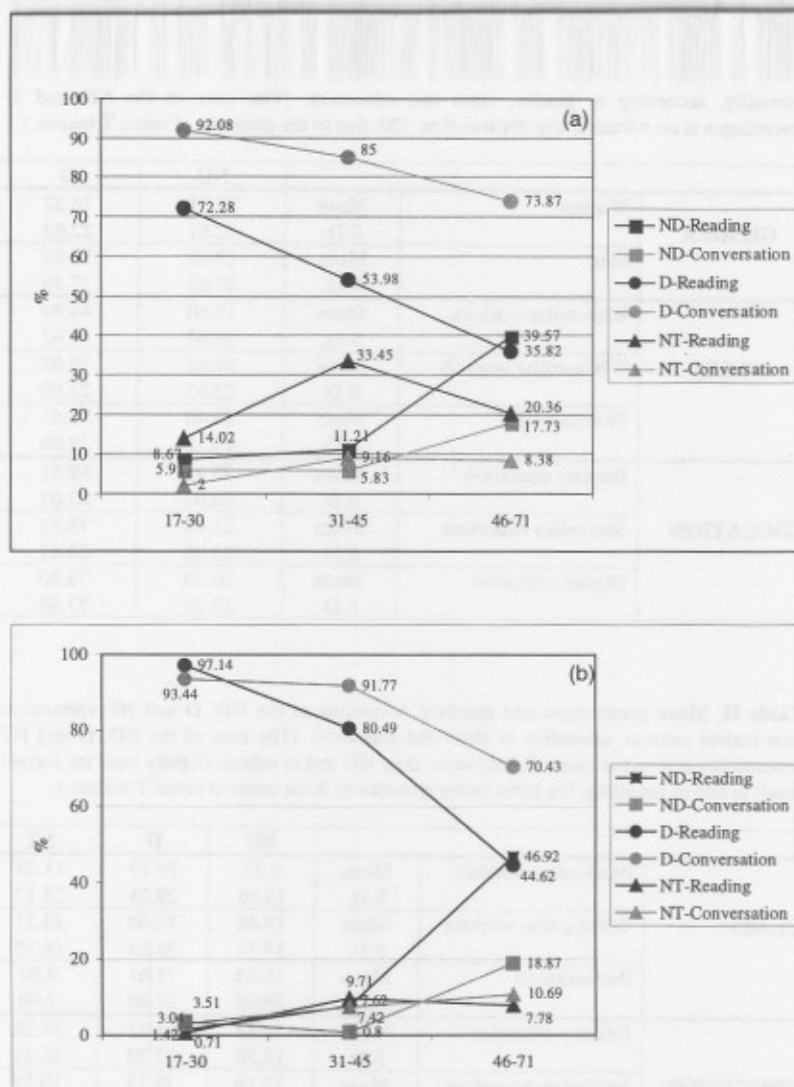


FIGURE 3: Mean percentages of the variants ND, D and NT in the post-lexical environment, by style and age, separately for female (a) and male speakers (b).

In particular, as shown in Figure 3(a & b), the ND variant exhibited exactly the same pattern post-lexically as it did word-internally: both men and women in the first two age-groups showed similarly low percentages of ND in both reading and conversational style. In contrast, for the older age group there was an increase in the use of ND in reading ( $p < 0.0001$ ) in the data of both the female and the male speakers.

Figure 3 also shows that in the data of the youngest and oldest age groups this similarity of pattern between word-internal and post-lexical (ND) was maintained for the D variant as well: post-lexically the young speakers used D almost exclusively, and the use of D was not affected by gender or style (although women did exhibit a statistically non-significant *trend* for more D in conversation than in reading); the older speakers, on the other hand, showed the expected decrease of D usage in reading ( $p < 0.001$ ), a pattern that was not affected by gender (i.e. both genders showed a D decrease). In the 31-45 year old group, however, the speech of men and women did not have the same pattern: while men's data did not show an effect of style, women's data showed a lower percentage of D tokens in reading than in conversation ( $p < 0.003$ ). Their D percentage in reading was also lower than that of the male speakers ( $p < 0.05$ ); on the other hand, the data of men and women showed no significant differences in D usage in conversation.

Interestingly, the decrease of D tokens observed in the reading of women in the 31-45 age group, did not affect their usage of the ND variant (which, as we saw, was used equally in both styles), but that of NT, which showed a much higher percentage of tokens among women than among men of this age group ( $p < 0.01$ ). This difference in the use of NT was not observed in the data of the other two age groups, where NT usage was not affected by either style or gender and was in general lower than that of the women in the middle age group. In short, it appears that women of the 31-45 age group are the most sensitive to the use of NT as a careful style marker, an effect not observed among the older or younger speakers irrespective of gender, or among the men of their age group.

#### 6. DISCUSSION AND CONCLUSION

In summary, we saw that the pronunciation of (ND) depended primarily on context, and age, and to a lesser extent on style and gender. Our results confirm traditional accounts that word-initial voiced stops are virtually always pronounced oral (but the occasional presence of nasality, also observed by Charalambopoulos *et al.*, 1992, is noteworthy). In addition, in word-internal (ND), variation in the use of the ND and D variants shows a strong correlation with age, with speakers below the age of 45 displaying a dramatic reduction in ND pronunciations when compared with older speakers, while other social factors, such as gender, education and class, did not affect the speakers' choice of variant. Significantly, style did not affect (ND) realization, except in the case of the older speakers, who showed an increase of ND usage in reading.

Finally, we found that the pronunciation of post-lexical (ND) was also affected by age, but that within each age group the variable was affected in different ways by gender and style of speech<sup>12</sup>. In the youngest age group these factors did not influence (ND) realization, and in the majority of cases the variant used was D. In the oldest age group, style affected the choice of variant, resulting in higher ND and lower D percentages in reading than in conversation for both men and women. In the middle age group, on the

other hand, women showed an increase of NT in reading compared to conversation; this increase was at the expense of the D variant, while women's percentage of ND pronunciations remained the same in the two styles. Unlike the youngest and oldest age groups, women in the middle group behaved differently from men, whose choice of variant was not influenced by style.

The overwhelming effect of the age factor compared to all other factors suggests that the pattern of stable variation depicted in most traditional grammars and descriptive works (e.g., Mackridge, 1990a, Newton, 1972), in which ND is the formal and D the informal variant, is changing. It appears from our data that ND is no longer a marker of careful speech, and D forms are no longer "stigmatized stereotypes" (Daltas, 1992: 21). On the contrary, our results suggest that prenasalized voiced stops may have actually begun to disappear from Greek, or more accurately, from the speech of the younger speakers of Standard (Athenian) Greek.

This change in the use of ND is apparent in the differing patterns of speakers below 45 and those above 45 years of age. Our results show that older speakers have two variants, ND and D, both word-internally and post-lexically. For these speakers, the prestige of ND is evident in their increased usage of it in reading, a result unique to this age group. In contrast, these older speakers do not use NT as a careful style marker. This should come as no surprise, since the older speakers can use ND, i.e., they can simultaneously apply the stop voicing rule *and* retain the nasal, nasality being for them the key element which conveys the impression of formality and carefulness.

In contrast, the almost complete replacement of ND by D in the speech of the two youngest age groups, (17-30, 31-45), is clear in their word-internal data. The constantly low percentage of ND word-internally, and the concomitant overwhelming use of D, suggests that for them D is no longer an "indication of careless pronunciation" (Mackridge, 1990a: 72) in this environment.

It could of course be argued that the reason why the younger speakers in our sample used D to such an extent is that they adopted a uniformly informal style in both reading and conversation, possibly out of a sense of solidarity with the interviewer, whose age fell at the time of the recording on the boundary of the two younger age groups. There are, however, two problems with this argument.

First, the post-lexical data show that these speakers do use a more formal style in reading. What is significant is that formality is expressed through the use of NT, so that the observed differences between word-internal and post-lexical (ND) relate to the D and NT variants, but leave ND largely unaffected. Our interpretation of this pattern is as follows. As expected, the effects of the sound change are more widespread in the lexical than in the post-lexical environment, and thus D is not fully accepted post-lexically by the younger speakers; since D retains some of its old connotations of carelessness and informality in the post-lexical context, it is to an extent avoided post-lexically. ND on the other hand is no longer a careful style marker and appears to be largely unavailable to these speakers as a possible realization. With ND unavailable, and D "stigmatized", speakers need another marker for careful style, so they opt for NT, that is they choose not

to apply the stop-voicing rule. For instance, one of the speakers, an 18 year old woman, originally read a sequence as [tide'tarti] 'on Wednesday', and after a short hesitation pause repeated it as [tin te'tarti]. It is, however, significant that there are no similar instances of correction of D to ND word-internally. This suggests that neither this speaker nor any other in the younger age groups felt that they were being careless when they were pronouncing D in the word-internal context; there is no evidence that they might have been monitoring that aspect of their linguistic behavior.

Secondly, even those speakers in the 17-45 span who showed preoccupation with sociolinguistic markers used D pronunciations in overwhelming numbers. A case in point is one of the men in the 30-45 year old group, who talked at great length about the unacceptable accents of newscasters. His concern was focused mainly, though not exclusively, on the use of the stigmatized palatals [ʎ] and [ɲ] before the high vowels /i/ and /e/, instead of the standard alveolars [l] and [n] (e.g., in [ʎitra] 'ransom' or [ɲisi] 'island'). He did not, however, mention D among the pronunciations he deemed unacceptable, and indeed his conversational data showed that he used D 98% of the time both word-internally and post-lexically, strongly suggesting that for him D does not belong among the stigmatized markers.

This change of attitude towards D and ND is supported by our further informal observations of D usage even in recent loans, such as [to 'barser] for <τον πάροερ> 'the parser/ACC' and [ko'bjuter] for <κομπιούτερ> 'computer', by young educated speakers even in the formal context of a linguistics conference presentation. In short, we concur with Mikros (1997) that "prenasalization is a social marker of prestige that is used and appreciated only by the older Greeks[.] in the younger generations it is not used as such"<sup>13</sup> [our translation]. This of course does not mean that some younger speakers are not aware of the prestige of the ND variant, even if they do not always use it in their own spontaneous speech. For instance, at a presentation of this study a graduate student expressed his surprise at the high D percentages of his generation, arguing that as an undergraduate at the University of Athens, he and his friends scorned the D variant as a marker of uneducated speech; he was unaware of the fact that he started his comment with [pados] 'however'.

It is also worth commenting on the gradation of the pattern we observed. The dilemma of which variant to use to express formality affects mostly the women in the 30-45 year old group, less so the women in the 17-30 year old group, and least of all the men of these groups. In other words, NT is more widespread among women, the group that has traditionally been described as being more conservative and sensitive to prestige norms (see among others, Labov, 1972, Trudgill, 1972, and the discussion in Chambers, 1995: 128ff., 221ff.). Men, on the other hand, appear to be in the vanguard of the innovation.

Yet, this alteration of pattern across generations would be relatively unremarkable if it were not for (a) the abruptness of the change, which seems to have taken place within one generation, and (b) the direction of the change, namely the fact that the variant which has emerged as the dominant one is D, the variant that is traditionally thought of as less prestigious. The direction of the change becomes all the more puzzling if one takes into account the high social mobility of Greece, which should normally have made speakers

adopt the more prestigious ND variant. It is well known that "the upwardly mobile

speakers not only use fewer non-standard variants than the people in the class in which they originated but also use fewer than the people in the class which they are emulating" (Chambers, 1995: 57). This finding is particularly true of working class speakers moving into the lower middle class, and this is precisely the kind of social mobility that characterizes Greek society in the post-Second World War era (see, e.g., Lytras, 1993, and references therein).

We propose that the dramatic age division and the puzzling direction of the sound change that we observed in our data are due to two related factors: (a) the long standing Greek diglossia, and (b) the overwhelming political changes which took place in Greece in the mid-seventies and led, on the one hand, to social changes, and on the other, to the official abolition of diglossia with the demise of Katharevousa in 1976.

First, it must be noted that diglossic communities appear to have certain peculiarities: specifically, sociolinguistic research in Arabic-speaking countries suggests that in cases of diglossia the prestige and standard varieties are not the same, as they are in other linguistic communities. Although the H variety is the prestige one, it is the L(ow) variety of urban centers that plays the part of the standard (Chambers, 1995). This distinction nicely explains the speech patterns of the upwardly mobile in diglossic Arabic-speaking communities: these speakers cannot master the features of the H variety (due to their lack of schooling), but they can and do master features of the urban vernacular in order to emulate the speech of the class they aspire to.

We would not wish to suggest that exactly the same analysis would apply to the Greek situation, but there are certainly similarities among diglossic communities. Thus, it is possible that the patterns described in older works on (ND), especially those based on samples elicited from highly educated people, such as Householder (1964), conceal part of the reality of the situation in Athens. It is possible that while ND was the prestigious variant linked to Katharevousa, an Athenian L standard with D as its reflex for older ND was emerging among those upwardly mobile strata of society—always considered innovators (Labov, 1980)—that after the war formed what Lytras (1993) terms the "new middle class" (roughly the equivalent of the white-collar workers in this study).

Obviously, the phonetic and phonological gap between Katharevousa and Dimotiki was not as great as that between Classical Arabic and the Arabic regional varieties. Moreover, the influence of Katharevousa was all-pervasive (Browning, 1982). In other words, the prestige of Katharevousa was not felt only by highly educated speakers who had to learn to use Katharevousa for their studies and work, but by all urban dwellers who read newspapers, listened to the radio, filled in forms, read notices in public places and felt uneasy about their mastery of the H variety (for a discussion see Browning, 1982, and 1983: 109ff.). This all-pervasive influence of Katharevousa probably accounts for the pattern we observe among the older speakers, irrespective of class or education, namely the roughly equal use of the D and ND variants. Eventually though, D prevailed for socio-political reasons, namely the end of the military government and the subsequent abolition of Katharevousa as Greece's official language.

The age division in our results roughly coincides with this socio-political landmark of Greek history and suggests that both the speakers in the youngest age group, who had little or no contact with Katharevousa, and those in the middle age group, who were educated in Katharevousa but, in their vast majority, rejected it because of its association with the dictatorship, are increasingly less sensitive to the waning prestige of the Katharevousa-linked ND variant. Understandably, the effect is less pronounced among the 31-45 year olds, who may well have rejected Katharevousa, but cannot be expected to be impervious to the prestige of the language in which they were educated<sup>4</sup>. As Browning (1983: 109) aptly noted: "On the linguistic level [diglossia] certainly contributes to [...] loading of emotional significance on to the linguistic form, a significance which may be a much more important part of the message than its overt content of information." Hence the speech of this age group is in a state of flux. In contrast, the speech of the youngest group presents a consolidated pattern. In turn, their pattern is clearly different from that of the oldest speakers whose norms, formed during the period of diglossia, cannot be expected to change.

This relationship between linguistic change and "catastrophic social events" is not uncommon, as Clermont & Cedergren (1978), Kemp (1981), Labov (1990), and Laferriere (1979) demonstrate. In the Greek case, after the 1974 fall of the seven-year military junta, a period in which the use of Katharevousa as the official language of administration and education had been reinforced, the newly elected democratic government abolished the official use of Katharevousa in all aspects of public life. This move was in part a reaction to the connection of Katharevousa with the junta, a link which had undermined its former status as the H variant of Greek diglossia. Frangoudaki (1992: 369) for instance, states that "since the 1950s, the use of K Greek [Katharevousa] connotated acceptance of established hierarchies, respect for traditional values, resistance to change, and support of the given order" and goes on to show how this power of Katharevousa was slowly eroded by its increasingly wide use, which was intensified even further during the junta. Through such extensive use, Katharevousa became increasingly understandable to a larger part of the Greek population, an outcome which was facilitated by more widespread access to education. Thus, Frangoudaki continues (1992:69 ff.), Katharevousa "gradually lost its legitimacy, thus losing its function as a high code", and "after the restoration of parliamentary government (1974), [...] served to identify the speaker with prodictatorship positions."

In short, Katharevousa related norms were rejected because of the connection of the H variant with the military government. The other side of the coin was of course the adoption of Dimotiki (or so-called Dimotiki) forms, a usage that automatically conferred progressive credentials on the speaker; the arguments over the form of the genitive singular of "third-declension" nouns (της πτώσης vs. της πτώσεως) are well known, as is the (thankfully short-lived) usage of phonological aberrations such as *σχολιό* (for *σχολείο*) in left-wing partisan literature. We contend that ND was among the rejected markers, though not one that attracted the kind of attention "third-declension" nouns did. This attitude towards ND, together with the former diglossic situation, which had possibly given rise to a D standard, and Greece's high social mobility, which brought D—the "new middle class" variant—to the fore, can explain the current minimal social stratification of the variable and the abrupt and unusual change of the observed pattern.

To conclude, in the case of Greek voiced stops, a changing social environment—i.e., political changes together with changes in the nature of Greek diglossia—seems to have given rise to linguistic change as opposed to merely adding to the already existing stylistically conditioned variation.

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#### NOTES

<sup>1</sup> The research reported here was carried out while the lead author held a Research Fellowship in Linguistics at Wolfson College, Oxford. We would like to thank Wolfson College and the Astor Travel Fund of the University of Oxford for providing the funds for her to visit The Ohio State University where most of the data analysis was carried out. Thanks are also due to Mary Beckman for advice and help in Columbus, Bruce Connell for allowing us to use equipment from the Oxford University Phonetics Laboratory for the recordings in Greece, and the audiences at the Annual Meeting of the LSA (Los Angeles, 1993), and at seminars at Oxford, Reading and Georgetown Universities, for their helpful comments on a preliminary version of this paper. This paper will appear more formally in an up-coming issue of the Greek journal *Glossologia*. We thank the editors for allowing the pre-publication appearance of this paper in this Working Papers in Linguistics issue. Correspondence regarding this paper should be addressed to the lead author, Dr. Amalia Arvaniti.

<sup>2</sup> A decision had to be made as to whether we should refer to "voiced stops" or to "nasal+stop clusters". Both terms are phonologically loaded, but we decided to use the term "voiced stop" as it is phonetically accurate, and we do not wish to make any claims in this paper about the phonological status of surface voiced stops in Greek (but see Arvaniti, in press).

<sup>3</sup> The relationship between this innovation and the tendency towards postnasal voicing of dental stops in Greek of the Hellenistic and Roman periods (see Dressler, 1966, and Bubeník, 1989: 239, for data and discussion) is unclear, and irrelevant in any case to the later developments under consideration here.

<sup>4</sup> Thus the ND/D division cuts across the traditional geographically based division of dialects into Peloponnesian-Ionian, Northern, Old Athenian, Cretan, and South-Eastern (see Newton, 1972: 13-15).

<sup>5</sup> Based on the Neo-Grammarian view of sound change, in which sound changes apply at first without regard for word boundaries, our expectation is that these rules applied post-lexically in Middle Greek too, but there is no firm evidence for this.

<sup>6</sup> For instance, Newton (1972: 97) talks about "close syntactic structures" which include, among others "the nasal-final forms of the article before a following noun." He adds, however, that the notion of "close syntactic structure" is not easy to define and gives as an example the fact that the word /an/ 'if' undergoes nasal assimilation in point of articulation to a following voiceless stop, but does not trigger voicing of the stop, as in /an pis/ → [am pis] 'if you-say'. Nespor & Vogel (1986) on the other hand, claim that nasal assimilation and stop voicing are two prosodic rules of Greek which operate

optionally (and together) in the Clitic Group prosodic domain, while Malikouti-Drachman & Drachman (1992) account for stop voicing by syllabification rules.

<sup>7</sup> In all cases, the nasal assimilates to the stop for place of articulation. Nasal assimilation is a more widespread phenomenon than stop voicing, and as it is not always connected with stop voicing (Newton, 1972), it will not concern us here.

<sup>8</sup> We would like to thank Evangelos Petrounias of the University of Thessaloniki for his observations on this point.

<sup>9</sup> This last age group appears to span a much wider age range. However, the age of nine of the speakers was between 46 and 60; there was only one speaker who was 71 years old. His speech was not different from that of the other speakers in this age group.

<sup>10</sup> There were in fact other post-lexical voiced stops, some of them across boundaries which, according to Newton (1972) and Nespor & Vogel (1986), should block stop voicing. For the purposes of the present study we included in our data only those clusters which according to all studies can surface as voiced stops, i.e., those that involve one of the following: a definite article followed by its host noun; a personal pronoun followed by its host verb; one of the negative markers, /ðen/ and /min/ followed by its host verb.

<sup>11</sup> The acoustic analysis of such tokens shows that the difference between the two types of voiced fricatives is probably due to the fact that underlying voiced fricatives have lower amplitude than voiced stops which were pronounced as fricatives. The latter appear to be a pronunciation variant favored by the younger male speakers.

<sup>12</sup> Broadly similar results are reported in a recent quantitative study of prenasalization and stop voicing in the post-lexical context (Mikros, 1995), which is based on data from five families, each of them being considered a minimal social network. Mikros' results, however, are difficult to interpret and compare to ours because he takes the presence of the nasal and the voicing of the stop as two independent markers, so that in his results our ND and D categories are classed together under "voicing", and our NT and ND categories are classed together under "nasalization".

<sup>13</sup> Although the norm seems to be moving in the direction of D, it is fair to say that, for at least a part of the population, foreign language learning *may* reintroduce ND and NT as possibilities, at least in relation to foreign words and recent loans. As Daltas (1992), in an insightful discussion of this phenomenon, remarks "this reversal is promoted [...] by young educated polyglots, and does not necessarily affect the rest of the population who may be quite content with stage 4 [our D] and quite unaware of snooty attitudes toward them on the part of the privileged youth—who, by the way, are far from exhibiting consistent adherence to their conscious linguistic norms with respect to the phenomenon under scrutiny" (p. 21-22). This is exactly what our own observations and examples suggest as well.

<sup>14</sup> Despite frequent changes in linguistic and educational policy in Greece, the use of the two diglossic varieties in school has remained relatively stable in the 20<sup>th</sup> c. From 1923 to 1967 (with the exception of the period 1935-36), Dimotiki was used as the language of instruction in the first four years of primary school only. Between 1964 and 1967 both Dimotiki and Katharevousa could be used in education (but obviously attitudes and textbooks did not change overnight). In 1967, and until 1974, the military junta imposed the use of Katharevousa at all grades. Again, the use of Dimotiki in education after 1976 was a slow process that took years to complete. Thus, despite the fluctuations, the



speakers who were in their mid-thirties or older at the time of the recording had had all or most of their schooling in Katharevousa.

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αλλά και πάλι καμιά φορά σκέφτομαι ότι θα 'πρεπε να τον θυμάμαι περισσότερο. Κι

όμως, το μόνο που μου 'χει μείνει απ' τον πατέρα μου είναι το ότι εμάς τα παιδιά δεν μας άφηνε να τον φιλήσουμε. Πάντα έπαιζε μαζί μας, αλλά μας απαγόρευε να τον φιλήσουμε γιατί θεωρούσε τις διασχύσεις (όπως τις έλεγε) κακό παράδειγμα για τα παιδιά. Ήταν λίγο περίεργος ο πατέρας μου σ' αυτό το θέμα και γι' αυτό κανένα απ' τ' αδέρφια δεν νοιώσαμε ποτέ πολύ κοντά του.

Παρά τα όποια μικροπροβλήματα όμως αυτά τα καλοκαίρια στο χωριό τα θυμάμαι και τα νοσταλγώ πολύ και θα 'θελα να μπορούσα να προσφέρω κάτι αντίστοιχο στα δικά μου παιδιά μια μέρα.

**APPENDIX II:** The translation of the reading text.

When I was a child we used to spend our summer vacation in my mum's village which is situated on Mt Olympus. We had a very good time, especially me, for I loved life close to nature. I remember, for example, that one of my greatest joys was to listen at night to the cries of the howler, and to be waken up in the morning by the cackle of my grandmother's hens, geese and guinea fowls.

My grandfather was not a farmer but a tradesman, so the family had no land, except for a vineyard; as for animals, they had only fowl and a horse, Wednesday, named after the day on which she was born. Sometimes my grandfather would let me ride her, but he always held the reins to make sure. What a joke! With one hand he would hold the reins and with the other his walking stick! But he thought I was too young and did not trust me.

The family was equally careful when I went out to play. There was a deep ravine close to our house and we were always scolded when we went near it. Nevertheless, I used to go there often to watch the gypsies who put up their tents in that area. I particularly liked watching the gypsy women dance, while their men, sitting under the shade of the surrounding trees, played their tambourines. But I was too shy to speak to them; I would just sit in a corner and watch them. One day my mum caught me looking at them and I got into trouble. "I won't have you dealing with gypsies. Do you hear?" she said again and again. I don't know what got into her, because in general she did not have racist ideas.

Of course this was not the only time I got into trouble. I was a naughty child and was often scolded. One time, for example, I was playing at the [village] square when a heavy rain storm started. Instead of running home, like the other children, I stayed to enjoy the thunder and the rain, getting drenched as a result. I can't begin to describe the thrashing I got for this.

The other thing that drove my mum crazy was that I did not eat salad. "We are in the countryside, we have local, fresh vegetables, and this monster does not want to touch the cucumbers and tomatoes! 'I don't like them' she says! How is this possible?" I would hear her complain to her friends when they were sitting in the garden having coffee. Once I got so upset because of these conversations with her friends that I hid myself in the bathroom closet and they took hours to find me.

In the end, because of the quarrels with my mum, I ended up loving my grandma more. She was a very sweet woman, small and gray-haired, who put up without complain with all the monkey tricks that her grandchildren, me especially, came up with. She always humored us and nothing could upset her. I always remember her with a smile on her face, unlike my grandfather who was rather sullen.

I also remember my uncle Sotiris, my mother's youngest brother, who lived in his parents' house before his marriage to aunt Fotini. He loved his nephews and nieces a great deal, bought us sweets and often played with us. Although he was tall and big with a thick moustache we were not afraid of him. After his marriage, when he moved with my aunt to a house nearby, we would see him less often, but later, when they got children, all of us cousins played together.

The funny thing is that when I think about the village, I never remember my father. It is true that he worked hard and came only on weekends, but even so, sometimes

I think that I should remember him better. Still, the only thing that I remember from my

father is that he did not let us, the children, kiss him. He always played with us, but he forbade us to kiss him, because he thought that such outpourings of feeling (as he put it) was a bad example for children. My father was rather funny in this respect and for this reason none of his children were ever very close to him.

Despite such little problems however I fondly remember those summers in the village and would like to offer something similar to my own children one day.

## APPENDIX III: The words with word-initial voiced stop found in the reading text.

<i>/b/</i>	<i>GLOSS</i>
/ba'stuni/	'walking stick'
/be'la/ (twice)	'trouble/ACC'
/bora/	'shower'
/bubuni'ta/	'thunder'
/bap'o/	'bathroom'
/bo'rusa/	'I could'
<i>/d/</i>	
/defja/	'tambourines'
/dre'pomun/	'I was shy'
/dara'verja/	'contact' (colloq.)
/dopja/	'native/PL'
/do'mata/	'tomato'
/du'lapa/	'wardrobe'
<i>/g/</i>	
/joni/	'howler/ACC'
/jemja/ (twice)	'reins'
/gre'mos/	'precipice'
/griza/	'gray/PL'



## APPENDIX IV: The words with word-internal voiced stop found in the reading text.

<i>/b/</i>	<i>GLOSS</i>
/olibos/	'[Mt] Olympus/ACC'
/eboros/	'merchant'
/a'beli/	'vineyard'
/ebisto'sini/	'trust'
/bubuni'ta/	'thunder/PL'
<i>/d/</i>	
/ko'da/ (four times)	'close'
/pada/ (four times)	'always'
/tsa'dirja/	'gypsy tents'
/adres/	'men'
/dedron/	'trees/GEN/PL'
/skada'ljariko/	'naughty'
/a'di/	'instead'
/kaθodan/	'they were sitting'
/skada'ljes/	'monkey tricks'
/skar'fizodan/	'they came up with'
/a'diθeta/	'in contrast'
/a'distixo/	'equivalent'
<i>/g/</i>	
/fra'gokotes/	'guinea fowls'
/a'jiksi/	'to touch/SUBJ'
/a'guri/	'cucumber'
/a'yojista/	'without complaining'
/e'gonja/	'grandchildren'

**APPENDIX V:** The (putative) post-lexical voiced stops found in the reading text. The relevant sequences are underlined.

<u>/b/</u>	<u>GLOSS</u>
/tin 'pirakse/	'it bothered her'
/stin pla'tia/	'at the square'
/ton pa'pu/	'the grandfather/ACC'
/ton pa'tera/ (twice)	'the father/ACC'
<hr/>	
<u>/d/</u>	<u>GLOSS</u>
/tin te'tarti/	'on Wednesday/ACC'
/ðen tus m'ɪlusa/	'I didn't talk to them'
/ðen ti stenaxo'ruse/	'it didn't use to upset her'
/ðen ton fo'vomastan/	'we were not afraid of him'
/ðen ton fi'lusame/	'we did not use to kiss him'
<hr/>	
<u>/g/</u>	<u>GLOSS</u>
/tin kra'vji/	'the cry/ACC'
/tin kavali'cevo/	'I mount it [the mare]'
/ðen 'ksero/	'I don't know'
/ston 'cipɔ/	'in the garden'
/ton kav'ɣaðon/	'the quarrels/GEN/PL'



### Proto-Mixe-Zoquean: A Case in Linguistic and Cultural Reconstruction\*

Craig Hilts

#### 1. Introduction

The Mixe-Zoquean (MZ)<sup>1</sup> family constitutes a group of Mesoamerican languages and dialects found in the isthmus of Mexico. Based on the reconstruction of the "ancestor" of the languages in this family, a protolanguage is hypothesized, from which a linguistic paleontological study can be made. In turn, hypotheses can be drawn about the culture of which the language was a part, including a possible dating of the language. Based on Kaufman's (1963) reconstruction of proto-Mixe-Zoquean (PMZ), Campbell and Kaufman (1976) had hypothesized that the Olmecs of southern Mexico were speakers of Mixe-Zoquean languages and dated PMZ to 1500 BCE through glottochronology, a method which is controversial (Bergsland, Vogt, and Akhmanova 1962). They based their hypothesis in part on the geographical congruence of known Olmec sites and current MZ languages. The stronger part of their evidence consists of approximately 50 borrowed apparently Mixe-Zoquean roots among other language families in the area, in conjunction with what they called "the rather sophisticated Mesoamerican culture" represented by the reconstructed etyma for PMZ.

If a culture must have such items to qualify as Mesoamerican and the terms for the items are borrowed from MZ, then it would seem reasonable

\* Earlier drafts of this paper were presented at the thirty-third annual Mid-America Linguistics Conference in October 1998, the seventh annual Workshop on Comparative Linguistics in November 1998 and the Linguistics Society of America annual meeting in January 1999. I would like to thank Lyle Campbell, Gwang-Yoon Goh, Hans Heinrich Hock, Richard Janda, Brian Joseph and John Justeson for their comments and suggestions. Any errors that remain are, of course, not theirs.

<sup>1</sup> Abbreviations for languages used in this paper are: PMZ – proto-Mixe-Zoquean, PM – proto-Mixean, PZ – proto-Zoquean, POM – proto-Oaxacan Mixe, PGZ – proto-Gulf Zoque.

to assume that speakers of the MZ languages possessed the uniquely Mesoamerican things early enough and had prestige enough that others borrowed from them. (Campbell & Kaufman p. 82)

Of that reconstruction, only the terms for 'write', 'divination', 'nagualism', and the vigesimal numbering system appear to be based purely on linguistic evidence.

In this paper, I examine Wichmann's (1995) reconstruction of PMZ and offspring languages to illustrate two general areas. First, I compare those etyma representing terms of material culture with available archaeological evidence and theory to determine on that basis the soundness of their inclusion in PMZ using the 1500 BCE date as a reference point, including relevant etyma from reconstructed offspring languages to account for the apparent lack of terms which might be expected to appear in a cultural reconstruction. I then consider three methodological aspects of culturally accurate reconstruction. I examine some meanings within the historical context of the area first to make the point that current ubiquity of an item or its meaning is not a sufficient condition for its inclusion in a reconstruction, and then to demonstrate the efficacy of historical knowledge of a culture by dating Proto-Oaxacan Mixe (POM) as a post-Conquest language stage based on the number of Spanish material items represented in the etyma. I present evidence for the necessity of a knowledge of the ethnology of the current cultures from which the synchronic data are drawn. Last, I offer data which point to the need for accurate translation from intermediary languages and examine possible sources of difficulty therein. I conclude that a linguistic reconstruction will ultimately be accurate only after the consideration of all of these factors.

## 2. The Archaeology

I begin the archaeological aspect with a discussion of general terms relating to subsistence in hunter/gatherer cultures, with emphasis on the ubiquity of the items in Mesoamerica at the time indicated, then move through items related to sedentary patterns, such as agriculture and more sophisticated technology.

The Olmec heartland is in the lowland areas of Veracruz and Tabasco, Mexico. With an ecology rich in plant and animal resources, this area has been shown to be one of the earliest sites for the existence of settlements, both seasonal and permanent, due in large part to the variety and availability of subsistence necessities.

In a study of animal protein sources by Wing (1978) of five sites in the Olmec heartland that date to the earliest Olmec-type settlements, skeletal remains found there correspond with the relevant PMZ etyma in Wichmann, shown in Table 1. Wing (1981) lists conch, snook (still a popular gamefish), turtle and iguana in the remains found at five Early Formative sites in the pre-Olmec region. She estimates that 58% of all animal protein was from snook and turtle (p. 25) and speculates on differences in fishing methods to explain the differences in the relative amounts of snook found in pre-Mayan vs. pre-Olmec areas. Wichmann has reconstructed two types of fishing in PMZ: 'fish

with a net' (also 'washnixtamal (leached corn)') and 'fish with a hook', which also means 'sew' not only in PMZ but in both current Jaltepec and San Juan Guichicovi Mixe.

## PMZ Reflexes

alligator	*ʔuspin	conch	*sa:ka
crab	*ʔe:si	crayfish	*cas(i)
iguana	*ti:ciC	iguana	*to:ki
shrimp	*ʔo:vo(?)	snook	*(h)a(?)y(aw)
to fish with a net	*ma:kʔ	to fish with hook	*suy
tortoise, turtle	*tuka		

Table 1: Aquatic Animal Protein Sources

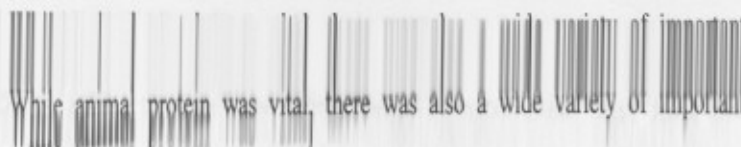
The heartland region was also rich in game as reflected in Table 2. The PMZ forms for 'shoot with a bow' and 'shoot with a slingshot' are indicative of hunting techniques, but Wing (1978) estimates that dogs provided 64% of the non-aquatic meat at San Lorenzo. Dogs were domesticated in Mesoamerica by around 3,000 BCE (Adams 1991: 37), and remained the only domesticated animal until around 300 CE, when turkeys were domesticated (Coe, in Campbell & Kaufman). Wing (1981) found evidence of socially stratified usage of turkey at San Lorenzo. Lyle Campbell (personal communication (p.c.)) notes that many Mesoamerican languages borrowed the word for turkey from a root something like *\*tul*, of indeterminate origin, which accounts for a lack of a PMZ reflex, since none of the MZ languages have /l/. Wichmann, however, offers two reflexes which are /l/ initial: PMZ *\*liklik* 'American kestrel' and POM *\*le(:)k(y)* 'baby'. Deer, for which Kaufman reconstructed PMZ *\*mɔʔa*, was quantitatively second to dog in the terrestrial animal protein hierarchy. The differences between PM and PZ reflexes for 'deer', 'opossum', and 'paca' must have some significance. This pattern of difference will be repeated throughout the data.

## PMZ Reflexes

agouti	*ʔuku	armadillo	*ni:c
black-bellied tree duck	*pi:ʔsisi	dog	*taka
iguana	*ticiC	iguana	*to:ki
monkey	*ca:wi	partridge	*wonon
rabbit	*koya	shoot with sling, to	*ti:ʔp
shoot with bow, to	*tip	tapir	*cu:ci(-ka:haw)
PM Reflexes		PZ Reflexes	
deer	*haycu	← <sup>2</sup>	*miʔah
mexican opossum	*po:	←	*cihi
paca (capybara)	*vukhoʔ	←	*huhnivi
turkey	*tu:tuk	←	*tuʔnuk, *kaʔncyi
agouti	*keki	dove	*kuʔkuʔ
brocket deer	*si:ti		
peccary	*ʔi:cimi		
set a trap, to	*nak		
slingshot	*tuh-an		

Table 2: Terrestrial Animal Protein Sources

<sup>2</sup> Same as PM gloss.



undomesticated plants for harvesting and use in Mesoamerica and the Olmec area. These are listed in Table 3. 'Sweetsop' is probably derived from a common PMZ root, but the differences in 'chicle', 'rubber (tree)' and 'century plant' certainly point to different origins.

<i>PMZ Reflexes</i>			
acorn	*soh-tim	bamboo	*kape
cedar	*ma:san-kuy	chili, type of	*kuy-ni:wi
cigarette (see below)	*huk?-i	fruit, plum-like	*tu:ni
cotton tree (see gourd)	*pokok	[tree cotton tree] <sup>3</sup>	[*pistin]
edible green	*camam	edible green (generic)	*cip?V
tree with edible	*?i:?(n)ki	guava	*pos, posos,
leguminous fruit			pohos
gourd tree	*cima-kuhy	honey	*ci:nu
hogplum tree	*ham(ay)-kuy	reed	*?o:kwin
palm (edible types)	*kuma		
<i>PM Reflexes</i>		<i>PZ Reflexes</i>	
century plant (agave type)	*ca:he	←	*?oho
chicle	*ci?m-pak	←	*hi?ya
rubber (tree)	*?o:me	←	*na?h
sweet apple {sweetsop}	*?a:ti	←	*yati
canna, {arrowroot}	*wayi-?ay		
edible piper	*wa:w	purslane	*pecV-pecV
grape (wild)	*cay-tim		
pickle tree	*tuš		
ramon breadnut tree	*moho		
soursop	*katuc-?a:ti		
sweet potato {palm with tuber}	*pa?ak-miny		
type of palm	*nuhn		
wild sugar cane	*pa:-wa:šuk		

Table 3: Wild Plant Resources

Many of the cultivars given in Table 4 are listed in MacNeish (1992: 87-8). His timeline has all of these domesticated by the time of the Olmec horizon. Lee (1989: 221-2) speculates that cacao may have been domesticated in the Olmec region, and that its trade was monopolized between 1200-900 BCE. MacNeish lists sapotes (a fruit) as domesticated in the highland area of Teotihuacan by 2300 BCE, and reports that chilies, gourds and pumpkins were already cultivated by seasonal foragers by 4000 BCE. By 1800 BCE, common beans, corn, squash, avocado, cotton, and sunflowers had been domesticated. Tobacco was not included in MacNeish's list, but Campbell (p.c.) points out that reconstructions for something like 'cigarette' or 'tobacco' are common in other Mesoamerican languages, such as Mayan, and Schoenhals (1988) lists species of wild

<sup>3</sup> Alternate or conflicting glosses are as follows: [ ] from Campbell (p.c.), <> glosses in Wichmann for synchronic forms, { } from Schoenhals (1988).

*Nicotiana* which may have been used. The existence of phonetically different reflexes in PZ and PM probably accounts for the lack of PMZ reflexes for 'pumpkin' and 'maguey' (century plant). The case with 'string bean' differs in that both reflexes share the common 'bean' root \*sik. On the bases of the importance of the maguey for both fiber and *pulque*, a fermented beverage, and the fact that it is easily transplanted or planted (judging from personal experience), I am assuming that it was cultivated at that time.

## PMZ Reflexes

avocado	*kúy-tim	bean	*sik
bean, white	*po:pʔoʔ- <i>sik</i>	cacao	*kakawa
chili pepper	*ni:wi	chili pepper, white	*po:pʔoʔ ni:wi
cigarette <thing smoked>	*hukʔ-i	to smoke	*hukʔ
gourd	*pok(ok)[pokok]	peanut	*nas-kakawa
type of sweet potato	*min(i)		
PM Reflexes		PZ Reflexes	
pumpkin, calabash	*ciʔwa	←	*pason
squash type	*ʔehkšah	←	*ʔapit-pason
string bean	*kuy-sihk	←	*yawa-sik
black sapote	*cu:ʔkV	cotton	*coha
sugar cane	*wa:šuk	tomato	*koya
roll of leaf or tobacco <or grass>	*ša:ʔc-e(k)		

Table 4: Cultivars

Table 5 lists terms used in the corn complex, another aspect of the sophistication of culture. Corn was domesticated by 3000 BCE and the fact that there are three terms for maize referring to different states (generic, shelled, and leached), and two kinds of grinding (dependent on the state of the corn, namely dry or leached), none of which are compounds, seems to point to a long pre-PMZ history. This may be contrasted with the POM term for 'metate roller', an obvious compound. The absence of a PMZ reflex for 'metate' must be due to linguistic difficulties, since Lowe (1989) dates footed metates to pre-Olmec times, and MacNeish (1981) dates less complex metates to 3000 BCE. The reflex for 'lime' \*ham is probably indicative of an ash-based leaching agent for maize. Campbell (p.c.) notes that 'ashes' \*kuy-ham combines that root with 'wood/stick'. This can be contrasted with a mineral source of lime, created by baking limestone, which may be the meaning of PM \*ʔakaš. 'Dough' and 'ear of corn' again point to different origins for the PM and PZ reflexes.



<i>PMZ Reflexes</i>			
corn cob	*hi:pak	cornfield	*kama, mo:k-kama
granary for maize	*ce?s	knead (dough)	*mi:k?, yo:ʔt
ashes	*kuy-ham	lime	*ham
leached cormeal (corn)	*pic-i	maize	*mo:k
shell corn, to	*ʔiks	shelled corn	*ʔiks-i
to grind	*way	to grind dough	*ho:ʔs
to grind pinol	*ki:ʔt	tortillas, to make	
tortilla, food	*ʔan-e	to work with	*ma:k
		sieve or net	
<i>PM Reflexes</i>		<i>PZ Reflexes</i>	
dough	*hic-i	←	*kiʔt-i
ear of corn	*mo:k-kohk	←	*cutu
corn drink	*ʃu:t(Vk)	corn leaf	*mok(o)-ʔay
eat bread, tortillas	*kay		
esale	*mu:š(ik)		
griddle	*wekši		
lime	*ʔakaš		
metate	*pa:w-an		
sowing stick	*ni:p-an		

Table 5: Corn Terminology

The rest of the agricultural complex is represented in Table 6. These PMZ reflexes are all to be expected for a culture characterized by Campbell & Kaufman as "slash and burn agriculturalists", except for the term for 'irrigate'. This appears to be a misglossing over-extended from current reflexes for 'pour liquid on, water something, turn upside down' and clashes with the archaeological evidence that dates the first Mesoamerican irrigation work starting around 800 BCE, according to Adams (1984: 110-11). This can be tied semantically to the term for 'stone railing', in Table 8, which is discussed later.

<i>PMZ Reflexes</i>			
to clear underbrush	*yu:h	field cleared of underbrush	*yuhʔ-i
bowl made from gourd	*cima	to harvest	*pi:ʔk
grind chili	*moʔc	seed	*puh
to sow	*ni:pʔ	sowing time; sown field	*ni:pʔ-i
to turn upside down; break; irrigate < sprinkle, spread out, water something >	*muc	to yield a crop	*ci:ʔ

Table 6: Agricultural Terms

A probable byproduct of extensive agriculture is permanent settlements. From the lack of relevant terms in PMZ in Table 7, it's possible that many terms were borrowed in later languages or that the process was just beginning. The making of adobe bricks is much more an act of planned permanence than the construction of wattle and daub houses, which were being built by 1500 BCE in the highlands near Teotihuacan by later developing agriculturalists, and the use of clay as a building material for floors dates back

to 2300 BCE (MacNeish 1992: 112). The earliest house floors preceded those by 600 years (Adams p. 37). "Sweep" is certainly a concept pertinent to settlement, and reflexes for 'broom' in PM and PZ differ significantly only in their suffixes, *\*-an* (deverbalizer) and *\*-kuy* ('stick') respectively. The reflexes for 'basket', 'door' and 'mat' in PZ and PM are sufficiently different to indicate different origins, although Kaufman reconstructs *\*pata* for PMZ. Basketmaking dates to at least 5000 BCE (MacNeish 1992:108). Reflexes for 'pitcher' in PZ and PM are literally "water-carrying instrument", with the same suffixal differences as 'broom'. Adams (p. 47) suggests that the Olmecs may have been responsible for the spread of Ocos type pottery (horizon 1500 BCE), which was the first to reach most of Mesoamerica. The fact that it was so specialized makes it possible that there were craftsmen who did nothing else, according to Coe and Lowe (Adams p. 48), a type of labor differentiation possible only with the surpluses available from settled agriculturalists. I have included it in this category also because the inherent fragility of ceramics makes safe transport difficult, making it an indicator of a sedentary lifestyle.

*PMZ Reflexes*

adobe (mud) bricks, make	*mu:ʔc	hearth	*ʔalwaʔn
house	*tik	house pole	*kom(om)
to carry water	*mah	to sweep	*pe:ʔʔ
<i>PM Reflexes</i>		<i>PZ Reflexes</i>	
basket	*kaʔka	←	*waka
broom	*pe:ht-an	←	*pet-kuy
door	*tik-ʔahw-kV	←	*ʔaŋ-kiy
mat	*toʔk-i	←	*pata
pitcher	*mah-an	←	*mah-kuy
attic	*kuy-win		
shelf	*moʔco-komom		
wall	*me:ʔʔ-i, poe-e		

**Table 7: Settlement Terms**

We can now consider terms of non-agricultural technology, listed in Table 8. This aspect of Olmec culture is in some ways the most difficult to adequately discuss in terms of the glosses presented by Wichmann. I begin with those reflexes which can be properly ascribed to Olmec culture of the time frame hypothesized by Campbell & Kaufman. Agave fiber was in use by 5000 BCE, and weaving was done by 3000 BCE, by which time cotton was in use in thread (MacNeish 1981). Canoes had been in use since 7500 BCE, according to MacNeish, Wilkerson, and Nelken-Turner (Adams, p. 39).

However, many of these terms are not synchronic with pre-Olmec and Early Olmec evidence. Although textiles were used, evidence from the earliest sculptural and figurine representations of 350 years later suggests that 'shirt' is an overgeneralization of upper body clothing, and that 'cape' would be more culturally accurate, according to Lowe (1989: 47), who also calls footwear "rare and late" in Olmec evidence, placing it around 300 BCE and later. It must be noted, however, that soil conditions in the Olmec heartland are such that only the most durable materials such as stone and bone survive the damp. The words for 'paper' in many of the current languages from which Wichmann's

data were drawn are glossed in Spanish as *amate*, a species of *Ficus*, the bark of inner bark of which may have been used as a surface for drawing. Wichmann doesn't reconstruct anything for 'draw', and the semantic overlap in PZ of 'work' as handicraft or drawing' makes it seem likely that PMZ 'write' is functionally (and anachronistically) related to 'paper'.

One item reflecting more durable evidence is 'stone railing'. According to the Spanish glosses given by Wichmann for 'stone railing' *\*me(?)ke*, one of which means 'low wall', another of which means 'dike', this could be reconstructed as 'dam', which would coincide with the previously mentioned 'irrigation'. Although these would not fit the time frame postulated by Campbell & Kaufman, if the time frame is later (the 800 BCE date given by Adams for irrigation), these two terms could fit the culture. The other term regarding what would be the most durable evidence is 'make a stone wall'. Campbell (p.c.) points out that a stone wall can be something as simple as piled stones around the edge of a field. The evidence suggests that, for the time period under consideration, this is all it may have been, since the earliest known stonework delineation of public space, consisting of unworked stone borders, date to 1350 BCE. Lowe (p. 47) puts the earliest date of dressed architectural stone at around 900 BCE, which applies to the apparently misglossed 'stone railing', as well. There is apparently some difference between PMZ 'rope' and the PZ and PM 'rope' reflexes, which afford such a close match.

<i>PMZ Reflexes</i>			
agave fiber	*nawin	canoe	*?aha
carpenter [woodpecker]	*cehe	coal	*hu?yi
cut with machete	*po:ʔt	paper	*noki
rope	*cay	sandal "limb leather"	*ki?-ʔak
shirt	*suy-i	stone railing < parapet, dam, dike >	*me(?)ke
stone wall, to make a	*ne?w	thread	*pi:ʔt-i
to chisel	*pa:hʔ	to cut with scissors	*me?ps
to spin thread	*pi:ʔt	to weave	*ta:kʔ
to write	*ha:yʔ	writing instrument	*ha:yʔkuy
<i>PM Reflexes</i>		<i>PZ Reflexes</i>	
rope	*tipš-i	←	*tiʔps-l
axe	*puš-an	blanket	*nuʔs-kuy
candle	*tikš-pa	drunk	*mu:k-(hu)-pa
cut with knife	*cuk	majagua (tree used for bark rope)	*poʔwah
knife	*cuk-an	native blouse	*?asa
saw	*hi;ʔt-an	pot	*suyu
to plane	*še:ʔw	skirt	*teʔksi
		white cotton trousers	*tukši
		work; handicraft, drawing	*cik-i

Table 8: Terms of material technology

Another aspect of specialization is the development of higher economic sophistication. Given the early trade mentioned in the corn complex, the possible monopolization of cacao trade, and the spread of Ocos type pottery, the PMZ reflexes in Table 9 seem justified.

<i>PMZ Reflexes</i>			
to buy	*huy	expensive < valuable >	*cow-ah
to pay, to owe	*yoh	road	*tu:ʔ-ʔaw
to sell	*maʔay		
<i>PZ Reflexes</i>			
to give credit	*haʔc-ci	village	*kumkuy

Table 9: Economic Terms

MacNeish (1981: 73) divides Mesoamerica into two distinct cultural developments, with the lowland Olmec heartland moving toward a theocracy, and the upland culture becoming a more secular culture. He gives the time frame for this split as occurring after 900 BCE. The reflexes in Table 10 are for PMZ and its daughters PM and PZ. None of the reflexes are synchronic mismatches to the Olmec era. MacNeish cites examples of apparent human sacrifice as early as 5000 BCE (p. 69) and postulates a "complex religious life" in the period of village agriculturalists beginning in 1500 BCE for upland Mesoamericans (p. 72). This is only slightly later than similar developments in the Olmec heartland. Thus we would expect to find the kinds of PMZ reflexes in Table 10 consisting of a complex involving incense, music, dance, and festival. Even the PM and PZ etyma are all reasonable within the framework of the Olmecs; conversely, all of the PMZ reflexes are reasonable for a much less sophisticated culture.

<i>PMZ Reflexes</i>			
copal incense	*po:m(o)	dance	*ʔec-e
dancer	*ʔec-pa	drum	*kowa
festival, name, sun	*siw	manna	*ma:san
necklace	*nam-cim(-i)	pray	*ko-nu:ks
remedy	*coy	ring	*kiʔ-kuma
shaman	*cokʔa	to dance	*ʔec
to play a wind instrument	*su:sʔ	to practice witchcraft	*pok
<i>PM Reflexes</i>		<i>PZ Reflexes</i>	
formed clay object	*mik-i	baptize	*niy-ʔiy
horn (musical)	*ʃu:ʃ-an	celebrate	*siʔpaʔ
sacred language	*ʔa-ma:san	flute	*sus-kuy
to divine	*ʔa-koc		

Table 10: Ritual Terminology

We can see from the archaeological evidence that, for the greatest part, the linguistic reconstruction matches the material culture reconstruction, with certain, perhaps inevitable, gaps from a multiplicity of causes. These include lack of evidence due to the nature of the environment, in this case damp acidic soil which destroys all but the most impervious materials; the time depth involved; and in terms of dating, the

crucial vocabulary that exemplifies the cusp of a major cultural change, namely that from village to urban society, which might be expected to be lost with the changes in culture in the language family since that time. The more that indicators of cultural sophistication are involved, the less surely the archaeological evidence matches the linguistic reconstruction. Those elements which are indicators of an urban culture have to do with the organization of the society and labor. Of the PMZ terms, only two imply the kind of community effort an urbanized society would exhibit: *\*muc* 'irrigate' and *\*me(?)ke* 'stone railing' per Wichmann; 'parapet, dam, dike' per his Spanish glosses. The discussion above for these terms shows that if they are to be included in PMZ, the date must be later than 1500 BCE.

### 3. Methodological Issues

I would now like to briefly examine three prerequisites of linguistic reconstruction that are also pertinent to cultural reconstruction.

#### 3.1 Knowledge of History

The first of these is a knowledge of the history of the area of the protolanguage (s) involved. Table 11 illustrates the necessity for this. Pigs and chickens are European imports to the Americas. The peccary is related to the pig, and the reconstruction of the PGZ reflex *\*mok-yo:ya* 'peccary' (literally "corn pig") and PZ *\*yo:ya* 'pig', is probably due to a reversal in post-Conquest cultural salience as the pig became the more familiar of the two and the peccary became more of a nocturnal cornfield predator than a meat source. Wichmann bases the PMZ form for 'chicken' on the root *\*ceweE* 'to prick'; however, he does call the set of proto-reflexes "speculative" (p. 276). PM *\*me:nyu* 'money' is based on the Spanish word *medio/media* 'half a real' which Campbell (p.c.) characterizes as "the almost ubiquitous Spanish loan in Latin American languages".

English Gloss	Spanish Gloss	Proto-Reflex	PLang
chicken, hen (m) <sup>4</sup>	pollo, gallina	*ce:wE(kV)	PMZ
money (l)	dinero	*me:nyu	PM
pig (m)	cerdo, marrano	*yo:yah	PZ
peccary, javelina	jabalí	*mok-yo:ya	PGZ

Table 11: \*Spanish material culture (m) and linguistic (l) loans

The strongest evidence for dating any of Wichmann's protolanguages is given for POM by the consideration of areal history. Table 12 lists those reflexes which strongly identify POM as post-Conquest, and two which indicate less surely that PGZ may have been. The presence of such items as 'sweat blanket for mount' *\*hipaʔan* and 'beast of burden' *\*hiyuk* presupposes either an unattested use of domesticated deer, or post-Conquest word formation. The semantics of PGZ 'peccary', previously discussed, point to a post-Conquest origin. There are no citrus species native to the Americas, and Wichmann notes that in 'lime'(the fruit) *\*cahp-pos*, *\*cahp* 'sky' is the usual first member

<sup>4</sup> The abbreviations here are (m) material culture, (l) linguistic loan, # included for the comparison of semantics only.

in nouns referring to objects associated with the Spanish. The other morpheme *\*pos* means 'guava'. 'Soap' is indeterminate in that it may refer to a kind of soaproot, rather than European soap. Mirrors were made by the Olmecs, but of hematite or magnetite, and apparently for religious or status purposes (Heizer & Gullberg, and Carlson 1981), and were not associated with glass, which was another import. 'Mescal', a distilled and therefore European product of *pulque*, unsurprisingly is literally "fire-water".

<i>English Gloss</i>	<i>Spanish Gloss</i>	<i>Proto-Reflex</i>	<i>PLang</i>
beast of burden	bestia de cargo	*hiyuk	POM
carpenter's plane	cepillo	*še:ʔw-n	POM
cock, rooster	gallo	*naʔaw-ce:wy	POM
cat	gato	*ci:ti	POM
fence	cercar	*kem	POM
glass, mirror	vidrio, espejo	*hiš-n	POM
glasses	anteojos	*win-ʔhis.-n	POM
lime (fruit)	limón	*cahp-pos	POM
key	llave	*ʔa-wa:hc-n	POM
match	cerilla, fosforo	*tack tiʔin-y	POM
mason	albañil	*pohc-pa	POM
mescal	mezcal	*hok-ni:ʔ	POM
peccary	jabalí	*mok-yo:ya	PGZ
saddle blanket	sudadero	*hipaʔan	POM
scissors	tijeras	*meʔpš-n	POM
small mill	trapiche	*wi:ʔt-n	POM
soap [soaproot?]	jabón	*sic	POM
sugar	azúcar	*po:ʔp paʔahk	POM

Table 12: POM and PGZ Post-Conquest forms

Thus we can see that for POM, the ethnohistory of the area firmly dates it as post-Conquest. However, it is also possible that these terms represent a natural dispersion, due to geographic proximity, of word formations that happened shortly after the arrival of the Spanish, and as such are a distinct development that occurred independently of the divergence of POM from the other Mixean language. It might be possible to fix the time period more closely by looking at loans from Spanish, but that would involve a different analysis beyond the scope of this paper.

### 3.2 Knowledge of Current Ethnology

The second methodological prerequisite is that of having an ethnological knowledge of the current cultures from which synchronic linguistic data are drawn. Wichmann's reconstruction of PMZ uses data from 39 languages collected from 18 sources. The large number of sources can be problematical in that the elicitor's knowledge of colloquial usage by informants in the eliciting language (Spanish) is not necessarily shared by the reconstructor. Table 12 lists glosses which are ethnologically inaccurate to varying degrees. Of them, the most misleading is 'carpenter'. If PMZ *\*cehe* were to be taken as 'carpenter' rather than 'woodpecker' (from the Spanish gloss *carpintero*), this would

suggest a labor specialization which may in fact have existed but cannot be reconstructed linguistically.

<i>Wichmann gloss</i>	<i>Alternate gloss<sup>5</sup></i>	<i>Proto-Reflex</i>	<i>PLang</i>
bamboo	[reeds]	*kape	PMZ
avocado	{laurel type tree, per <i>aguacatillo</i> }	*šihc	POM
carpenter	[woodpecker]	*cehe	PMZ
cotton tree	{ <i>ceiba</i> per <i>pongolote</i> }	*pokok	PMZ
grindstone	[gizzard (crop) stone]	*me:ʔc	POM
ground cherry	[miltomate, husked tomato]	*cap-koʔon	POM
iron, metal	[hard metal (not gold)]	*tiʔŋ-kuy	PZ
marmalade fruit	<mamey> {red sapote, Sp. gloss}	*kaʔwak	PM
pea	{pea-shaped seed} {tree of the "pea" family, per <i>chipilcoite</i> }	*cus-kuy	PMZ
sweet potato	{palm with edible tuber, per <i>camotillo</i> }	*paʔak-miny	PM
yucca	[sweet manioc]	*pisi	PMZ
yam type	[fish poison]	*naku	PZ

Table 12: Ethnologically inappropriate glosses

### 3.3 Accurate Translation

The third prerequisite is the necessity of using a fluent translation of glosses from intermediary sources (in this case Spanish) to avoid either a mistaken sense of the general aspect of the word or to avoid a possibly anachronistic interpretation to result. For instance, the PZ reflex \**tiʔŋ-kuy*, if glossed as 'iron' when what is meant is "hard metal", which Campbell cites as the rural meaning of Spanish *fierro*, would be anachronistic, since iron is another European import. John Justeson (p.c.) points out that *tiʔŋ* means 'to cut', making \**tiʔŋ-kuy* 'thing that cuts' in current Mixean languages, which is different from the 'bell; metal' meanings given for current Gulf Zoquean forms by Wichmann.

Table 13 lists other glosses that apply to this prerequisite. 'Bean plantation', 'pasture', 'irrigate', and possibly 'stone railing' seem to be inadvertent, possibly dictionary derived, mistranslations from Spanish to English, but each carries a connotation of a higher level of cultural sophistication than befits the early Olmecs. The use of 'to load a gun' might fit synchronic reflexes from which PZ \**maʔk* is derived, but there might well have been no Conquest had that gloss been accurate for PZ speakers. Spanish *armar* also means 'set a trap' which Wichmann includes, and more generally means 'assemble', according to Campbell. The others are reasonable glosses only in the broadest sense by equating modern usages as generic, as in 'cut (as one would) with scissors' or perhaps 'cut fiber/thread' since scissors were another European import, and 'instrument for writing' in the sense of 'stylus, or brush'. This broad sense can be misleading to those who are relying on the linguistic reconstruction as another basis for verification of hypotheses based on non-linguistic evidence.

<sup>5</sup> Alternate or conflicting glosses are as follows: [ ] from Campbell, <> glosses from synchronic languages, { } from Schoenhals.

<i>Wichmann gloss</i>	<i>Alternate gloss</i>	<i>Proto-Reflex</i>	<i>PLang</i>
bean plantation	[bean field]	*sihk-kama	PM
cigarette	(thing to be smoked: from deconstruction of proto-reflex)	*huk?-I	PMZ
to cut with scissors	<cut as with scissors / cut fibrous material>	*me?ps	PMZ
to hobble	<to button, to close: Sp. glosses>	*mo?ks	PMZ
instrument for writing	{instrument for drawing/painting}	*hay?kuy	PMZ
to load a gun, set a trap	[assemble, set a trap]	*ma?k	PZ
paper	(material for drawing upon) <amate bark>	*noki	PMZ
pasture, grass	grass, [long grass used in construction]	*mu?k	PZ
shirt	<cape: based on Lowe>	*suy-i	PMZ
to sprinkle, to spread out, to irrigate	<turn upside down, pour, to water: Spanish glosses>	*muc	PMZ
stone railing	<parapet, dam, dike: Spanish glosses per dictionary>	*me(?)ke	PMZ
iron, metal	[hard metal (not gold)]	*ti?η-kuy	PZ
to write	(draw)	*ha:y?	PMZ

Table 13: Semantic/anachronistic interpretations

#### 4. Conclusion.

Campbell & Kaufman wrote, "If the MZ-Olmec hypothesis is true, then this linguistic evidence confirms the archaeological evidence of these (reconstructed etyma)." The converse must also be considered in the reconstruction of a language. In this case, the great majority of the archaeological evidence, with the few aforementioned exceptions, supports both Wichmann's linguistic reconstruction and the hypothesis made by Campbell & Kaufman. We have seen how consideration of the history and the ethnology of a culture are important factors in the accuracy of a linguistic reconstruction, enabling the linguist not only to avoid anachronisms and semantic misinterpretations, but to provide evidence about the time frame in which the language stage existed. Further, the problem of possible mistranslation inherent in the use of a large collection of data from a number of sources has shown the necessity for independent review and verification of glosses by fluent speakers of intermediary eliciting languages. By making maximal use of these kinds of extralinguistic information sources, we can ensure the accuracy, efficacy, and extralinguistic value of linguistic reconstruction.





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**A Plain Difference:  
Variation in Case-Marking in a Pennsylvania German Speaking Community\***

Steve Hartman Keiser

**Abstract:** Previous studies (e.g., Huffines 1989, Loudon 1987) have demonstrated that Pennsylvania German (PG) is undergoing processes of convergence to English but that these processes differ between plain communities (i.e., Old Order Amish and Old Order Mennonite) and non-plain communities. I investigate a question that these studies have not addressed: what is the degree of variation *within* plain and nonplain communities? Data collected from 70 PG speakers from one traditional plain community (Old Order Amish) and two historically plain communities (Conservative Mennonite, and Mennonite) in Kalona, Iowa are analyzed for variation in dative case-marking. A comparison with Huffines's study shows that there are considerable differences in usage between these plain communities and those that she studied. In addition, quantitative analyses reveal that in Kalona, patterns of usage correlate most strongly with the speaker's age, reflecting an earlier period of relative social and linguistic homogeneity in the local religious communities. Quantitative analyses also provide an indication of which

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\* This paper would not have been possible without the generous help of many people in the Kalona community who took the time to serve as informants and to give me their perspective on language, life in Kalona, and, in many cases, how my life story fits in with theirs. Thanks to Paul Swartzentruber for spending an afternoon driving me around Kalona. Special thanks to Anne and Dan Miller and the Matt and Clara Schrock family who spent hours helping me translate and understand the interviews. And the biggest thanks goes to Alta Keiser for arranging countless interviews and above all for the love of words that she has passed on to her grandson.

Thanks to Brian D. Joseph for spurring me to take on this study and suggesting I explore language and ethnic identity as part of it. Thanks also to Don Winford and Norma Mendoza-Denton for kindly reviewing this paper and offering much-needed suggestions. All shortcomings remain mine alone.

functions of dative case-marking are undergoing the most rapid attrition. I discuss the implications that these findings have for the past and continuing development of PG varieties in the U.S.

## 1. Introduction

### 1.1 The question of variation within plain Pennsylvania German communities

There is a dearth of research on Pennsylvania German (PG) in Mennonite and Amish communities. This is true despite the fact that the only remaining locations where PG continues as a viable first language are found among the sectarian communities of the "plain people"—the Old Order Mennonites and Amish.

When research has included plain communities, it has often focused on the differences between plain PG (PPG) and non-plain PG (NPG)<sup>1</sup>. Huffines' 1989 study of case-marking and Loudén's 1987 investigation of convergence and innovation in PG tense systems fall into this category. Little work has been done to investigate variation in PG within plain communities<sup>2</sup>.

In this paper I report on a study of linguistic variation within and between PPG communities. This type of study can inform our understanding of the development of PG with regards to points of dialectal divergence and convergence between PPG communities, the relative homogeneity of earlier stages of PG in both plain and nonplain communities, and patterns of language change in situations of intense language contact and language death.

The aim of this paper then, is to demonstrate and account for the existence of variation within a PPG community as a first step toward understanding the nature of variation within and between PPG communities. Data on dative case usage in the PPG community of Kalona, Iowa will serve as the basis for the quantitative aspects of this study.

In the remainder of the first section of this paper I will review some previous research on variation in PPG and note its shortcomings. In the second section I will present the PPG community of Kalona, Iowa: its history and current sociolinguistic context. I describe the selection of dependent and independent variables and method of data collection for the study of case usage in section three. The data are presented in section four and factor weights and significance are calculated via binomial analyses and logistic regression using the GoldVarb 2.1 program. In section five I discuss a change in apparent time taking place in Kalona with respect to case usage and offer an account for these age-

<sup>1</sup> I adopt this terminology from Loudén 1987, p.1.

<sup>2</sup> Some exceptions include Enninger's (1979, 1988) study of the Old Order Amish community in Kent County, Delaware and Van Ness 1992 on loss of gender marking among Ohio New Order Amish. Enninger's research concentrates on lexical borrowing and the quantitative aspect includes data on only three speakers. Van Ness's study includes quantitative analyses, but neither Van Ness nor Enninger take the additional step of comparing the variation in PPG in these communities with other plain communities.

correlated patterns. Finally, I conclude in section six by noting implications that these findings have and suggesting next steps in research.

## 1.2 Previous Studies of Grammatical Change in Plain Communities

### 1.2.1 Louden 1987

Louden notes that although PG dialects were formerly distinguished by region, now the distinctions are social: contact with English has yielded different outcomes in the tense systems of PPG and NPG. In the former, change is occurring rapidly as PPG conforms to American English morphosyntactic patterns for the expression of tense. Although not explicitly stated, Louden's working assumption appears to be that PPG and NPG are monolithic varieties, with relatively little internal variation.

### 1.2.2 Huffines 1989 and 1992, Northumberland County, PA

Huffines (1989 and 1992) conducted a study among PG speakers in Northumberland County, Pennsylvania. The focus of her study was to identify differences in the PG of the non-sectarians of German Lutheran and Reformed background (i.e. NPG) and that of the sectarians, here Amish and some Old Order Mennonite (i.e. PPG). Among the non-sectarians the language is dying, while among the sectarians it remains the vital first language of the community. Comparisons were made of use of the dative case versus accusative. Huffines had her informants complete a translation task which consisted of a number of sentences which called for dative constructions according to PG grammars<sup>3</sup>. The results are given in Table 1. The Non-sectarian (N-s) group is divided into three sub-groups: Native speakers, speakers who were the first in their family to learn English as their first language, and speakers who were the second (or later) in their family to learn English as their first language. These last two groups are considered semi-speakers.

**Table 1 Huffines (1989) translation task.**

Group	Dative	Accusative	Other	Total
Non-sectarian	83	22	0	105
N-s 1st Eng. speaker in fam.	50	43	1	94
N-s 2nd Eng. speaker in fam.	30	39	8	77
Mennonite	1	86	0	87
Amish	2	90	0	92

Huffines arrives at the counter-intuitive conclusion that NPG is not maximally converging to English in the non-sectarian community in which it is dying. That is, it has not reduced its pronominal case-marking to match that of English<sup>4</sup>. Interestingly, PPG

<sup>3</sup> Huffines assumes a "standardized" form of PG at some previous time—at least with respect to case usage.

<sup>4</sup> English personal pronouns mark case distinctions only between nominative and common case in 1sg (I vs. me), 1pl (we vs. us), 3sg (he vs. him, she vs. her), and 3pl (they vs. them)

has nearly completely lost the accusative-dative distinction in the speech of the Amish and Mennonites of this community. Is this internal change or is it convergence to English? That is difficult to assess here, if only because case systems have been shown to have simplified via either/both external and internal influences in a number of languages.

Like Louden, Huffines focuses on the divergence between PPG and NPG. She does allow for some variation in the NPG speakers due to differing degrees of fluency, and she admits in a footnote to a certain amount (no figures are given) of age-correlated variation among the PPG speakers. However, on the whole it is clear that Huffines views the variation internal to the two groups as insignificant, commenting that among PPG speakers there is "remarkable uniformity" and that "lack of variation in this case merger bespeaks the cohesion of the plain community as a speech community" (1992, 171). It is not clear to what extent she perceives this lack of variation extending across all PPG communities.

There is, however, one scholar who has acknowledged the existence of significant variation between two varieties of PPG. Burrige 1992, notes in passing that the replacement of the dative with common/accusative forms in possessive constructions is "not nearly as advanced" among Old Order Mennonites in Ontario as it is among Huffines' Northumberland PPG speakers. Unfortunately, Burrige does not quantify her findings.

### 1.3 Implications for the history and development of PG

A detailed description of the nature and extent of variation that exists within PPG, that is between various regional PPG varieties and within a given PPG regional variety, is crucial to our understanding of the origins and ongoing development of PG.

First, the nature of variation within modern-day PPG varieties may suggest that earlier varieties of PPG were not nearly so homogenous as has been assumed. If instead we are to retain this assumption, then a satisfactory explanation for the subsequent divergence of PPG dialects must be proposed. The same holds for variation between PPG and NPG dialects, and a good deal of evidence pertaining to this problem has already been gathered (e.g., Louden 1987, Huffines 1989).

In any case, a study of variation within modern-day PPG dialects will help to highlight points at which the grammars of these dialects are diverging from each other. If divergence is taking place, is it impacting all subsystems of grammar equally and what social circumstances particular to the communities can be identified as influencing these developments?

Also, it is possible that a survey of the grammars of a number of PPG dialects will reveal points of convergence to English that all hold in common. Can these points of convergence be accounted for as a number of independent language contact or language

shift or even language death phenomena? If not, then what is the nature of the networks that link these noncontiguous language varieties?<sup>5</sup>

## 2. Sociohistorical context

### 2.1 A brief history of Pennsylvania German

Pennsylvania German, popularly known as "Pennsylvania Dutch," is the name given to the varieties of German that grew out of the leveling of mainly southwest German dialects in Pennsylvania in the years before the American Revolution. The dialects involved in this leveling process included as its main inputs Franconian, Palatinate, Alsatian, and Swiss varieties of German.

The term "varieties of German" is used intentionally here, because it is not clear that the leveling which took place in the early 1700's was so complete and widespread as to have resulted in a single, homogeneous PG dialect, though this is often assumed. Enninger 1988 suggests that a fairly thorough leveling process is entirely plausible for the plain communities and the related Anabaptist churches (the Mennonites); the nonplain PG are not mentioned. Enninger does note, however, the existence of at least one divergent PPG variety in Berne, Indiana (see also Thompson 1994).

In addition to the current linguistic heterogeneity of PG, which this paper demonstrates and which other research has amply confirmed, there are simple sociohistorical reasons to believe that PG was never monolithic. Firstly, there is the lack of contact between plain and nonplain speakers due in large part to the former's traditional separatism. Secondly, there is the continuing immigration of plain PG speakers in the 1800's which introduced a constant flow of continental German speakers to Mennonite and Amish communities in North America, both in Pennsylvania and elsewhere.

The speakers of the input dialects to PG represented many religious groups who came to the New World in response to William Penn's offer of religious freedom. The Mennonites were the first to arrive, settling in the Philadelphia area in 1683. They were soon followed by Amish, Dunkards, Schwenkfelders, and Moravians. In the 1720's a large influx of German Lutherans and Reformed began and these eventually outnumbered the earlier arrivals. It is not clear if nor when PG stabilized as a common code for these German communities in southeast Pennsylvania, but a cutoff in immigration during the Revolutionary War may have allowed the dialect leveling process to run to near completion (Van Ness 1989, 421). The extent to which varieties within PG evolved differently following the Revolutionary War is also unknown, although some researchers such as Huffines 1989 clearly assume a high degree of intradialectal homogeneity.

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<sup>5</sup> Answers to these questions would also further clarify the social embedding problem as noted in Weinreich, Labov, and Herzog 1968.

Today PG is spoken by 200,000 to 300,000 speakers in many of the contiguous United States and several Canadian provinces (Van Ness 1989, 420). Most of these speakers belong to the traditional plain communities and it is in their communities alone that PG is being acquired as a first language by children. In the other plain communities, that is, the historically or formerly plain communities (e.g., Mennonites) and the nonplain/nonsectarian communities, PG is a dying language.

## 2.2 The Amish-Mennonite settlement of Kalona, Iowa

As the 19th century began many Mennonites and Amish joined the pioneer movement westward from Pennsylvania in search of cheaper farmland. In the 1840's Amish and Mennonites were among the first Europeans to settle in the English River valley of southeastern Iowa in an area centered around what is now the town of Kalona. The community in its early years was essentially monolingual with all members speaking Pennsylvania German.

The Amish and Mennonites in Kalona (and elsewhere) were similar to each other in language and lifestyle until the turn of the twentieth century<sup>6</sup>, when numerical growth and increasingly rapid advances in technology forced the community to make decisions about such things as whether or not to construct church buildings and to accept restrictions on the use of telephones, electricity, and motor vehicles. Those in the community who accepted these changes eventually associated formally with the Mennonite (M) church. Those who didn't remained Amish, or Old Order Amish (OOA) as they came to be known. Ties between the two Anabaptist faith communities remained close over the course of the first half of the twentieth century, because many persons had family and friends in both church communities. Further, the Mennonite church continued to be a natural "home" for the significant numbers of OOA who chose to leave their own church. As the gap between M and OOA grew, some chose to affiliate with the Conservative Mennonite (CM) church which attempted to strike a lifestyle, theological, and—for a while—linguistic "middle ground" between the two poles.

A watershed event in the linguistic history of these communities is when English became the primary language for worship services. In the M church this occurred around World War I, in the CM churches the switch to English took place in the late 1930's. The OOA continue to use PG as the primary language in their worship services. High German, or "Bible German" as it is also called, is sometimes used in sermons. Scriptures in OOA worship are also in High German, generally read from the German Bible of Luther, though few understand it well.

<sup>6</sup> There is no small amount of confusion regarding the distinctions between Amish and Mennonites in the first seventy years of the Kalona settlement. This is perhaps best exemplified by a discussion between my grandmother and her brother (both now Mennonites aged 93 and 89 respectively at the time). She thought they were Amish growing up, but he maintained they were Mennonite—or at least "Amish-Mennonite." This label is used below in the table on demographic information.

### 2.3 The Community

This study concentrates on PG speakers living in the geographic area of Kalona town, population 2000, and the rural areas of high density Amish and Mennonite population which fall within an approximate 10-mile radius northeast and northwest of the town in northern Washington and southern Johnson counties. The nearest city is Iowa City, population 60,000, 20 miles to the north.

The total population of this area is estimated to be 5000-10,000, and members of various Anabaptist groups number approximately 3000. It is not known precisely how many of these are PG speakers, but a rough estimate would be fifty percent or 1500. This would include all OOA and many older M's and CM's. Most of the latter two are over 70 with the youngest non-OOA PG speakers being about 40.

The area around Kalona remains largely rural and the livelihood of many residents is closely tied to agriculture. Tourism is a growing industry—as it is in many areas with conservative Anabaptist settlements—parlaying the trappings of yesteryear into a successful business. Livestock auctions at the Sale Barn, the restored historical village and museum, “Amish” cooking, and a reputation as the “Quilt Capital of Iowa” are bringing more visitors from within and without the state.

As noted above, the major religious denominations in the community are the Old Order Amish, the Conservative Mennonites, and the Mennonites. There also exists a Beachy Amish (BA) church, a fellowship of New Order Amish (NOA), and some splinter groups of no official designation. Each of these communities is described below.

#### 2.3.1 Old Order Amish

The number of OOA in Kalona cannot be precisely determined. There are approximately 160 OOA families which comprise 8 church districts. Each church district has its own Sunday meeting which rotates between the houses of members. Based on the number in attendance at a Sunday meeting I attended and on the relatively large number of children in many OOA families, a conservative estimate would place the number of OOA in Kalona at 1000 individuals.

The lifestyle of the OOA is dictated by the church community code known as the Ordnung. The Ordnung can vary in its details from one OOA community to the next, but in general it stresses the virtue of humility, *demut*, and the sin of pride, *hochmut*. Adherence to the Ordnung of the Kalona OOA community manifests itself outwardly in a number of ways. I will mention only several of these. There is the characteristic appearance and clothing: men wear beards but no mustaches, women have long hair worn under a bonnet, men wear long pants and suspenders, women wear cape dresses, neither have buttons on their clothing. Electricity is not used in OOA homes, and members do not own telephones, televisions, radios, or cars. Local travel is done by horse and buggy,



and, for longer distances, by hired drivers or by bus. Decorations in homes are restricted to the utilitarian: clocks and calendars.

Nearly all OOA work at farm and farm-related occupations. Farming is very work-intensive given that much standard farming equipment is not used by the OOA including combines and trucks. Tractors are allowed, but are driven on their steel rims since rubber tires are not permitted.<sup>7</sup> Some young people, especially women, take jobs working in town at the Sale Barn or bakery and some are hired out to clean houses in Iowa City. This trend is likely to continue as the available farm land is not enough to meet the demand of OOA children as they grow up, marry, and seek to start their own farms. Even those who work in town are still most likely to live on a farm. A few OOA, mostly unmarried or widowed women of retirement age, live in town.

Outside of work, social life revolves around the home and the church. Sunday meetings consist of a two-hour long service followed by a simple noon meal and visiting. Socializing goes on before and after the service, generally in sex-segregated groups. Young people often go to "singings" Sunday evenings, events attended by unmarried people from several area districts. Socializing at singings also takes place in sex-segregated groups, although some cross-sex conversation can be assumed to take place on the buggy ride home—a common form of "dating." Socializing with non-OOA is discouraged, although non-members, which includes most young, unmarried OOA, are not subject to the same restrictions as members (e.g., some young men own cars).

OOA children attend the one room district school until the eighth grade. Teachers are selected from among the young, unmarried men and women of the community. Instruction and curriculum are all in English although there are classes in reading the German Bible for the older students. There is an unwritten, but widespread and strictly-observed rule that only English is spoken at school, ostensibly to help the younger children learn English.

Linguistic attitudes and practices of the OOA are examined in more detail in section 2.4.

### 2.3.2 Conservative Mennonites

There are three CM churches in the Kalona area. Precise membership figures are not available, but a total of 500 members is a conservative estimate. The lifestyle of the CM's is not so strictly structured as that of the OOA. Still, certain patterns of appearance prevail, in particular, the wearing of dresses and the prayer covering by women. In most other respects, CM's resemble outwardly the lifestyle of other rural Iowans. They own cars, televisions, radios, telephones, and other modern conveniences. CM children attend public schools, though there is a private school attended by some. Education for most

<sup>7</sup> From a theological viewpoint, rubber tires are considered "worldly"—that is, a fancy, unnecessary tool employed by the secular world. Using such a tool could make one proud. From a practical, rule-enforcement standpoint, steel rims may work to ensure that tractors are restricted to use in fields and not as a common transportation.

CM children ends after achieving a high school diploma. A few choose to go on to attend a two-year Bible college, usually at the CM school in Rosedale, Ohio. In part due to limited higher education, the occupations that CM's undertake tend to be in agriculture, in business, or in a skilled trade.

CM's hold Sunday meetings in church buildings and services are completely in English. Social activities with other church members are encouraged and may include activities in public places (e.g. roller-skating), though certain activities such as movies are discouraged.

Linguistic attitudes and practices of the CM's are examined in more detail in section 2.4.

### 2.3.3 Mennonites

There are a total of 10 M churches in the Kalona area with approximately 1700 members. In lifestyle M's are outwardly completely assimilated to the wider American society. In their dress and their homes, they are indistinguishable from neighboring non-M's. There is a Mennonite high school near Kalona with over 200 students, both M and non-M enrolled, though many M choose to attend public schools. Higher education has become increasingly acceptable since the first half of this century and is now widely encouraged. Many M students choose to attend college. Many M's continue to work in farm-related occupations, although many also operate businesses in town or work as professionals.

Church meetings are held in buildings and worship services are completely in English. Social activities differ little from other nonplain, non-Anabaptist members in the community.

Linguistic attitudes and practices of the M's are examined in more detail in section 2.4.

### 2.3.4 Other Anabaptist groups

The Beachy Amish (BA) and New Order Amish (NOA) are two relatively small groups in Kalona. Both resemble the OOA more than they do CM's or M's.

The differences are greater in the case of the BA who permit themselves to use the "big four" prohibitions of the OOA: electricity, cars, telephones, and rubber tires. In addition, the women's bonnet or "covering" is smaller and is worn untied, and most significantly for this study, the BA have not used PG in worship services since the 1970s. The Sharon Bethel Beachy Amish church currently has approximately 125 in regular attendance.

The NOA in Kalona formed in 1979 and resemble the OOA in almost every respect except for their use of tractors with rubber tires and a concomitant openness to other practices outside the OOA tradition.<sup>8</sup>

A third group, the Salem Church of South English 15 miles west of Kalona, falls into a niche somewhere between the CM and the BA. This particular group has also had a recent splinter group form which meets 5 miles north of Kalona. Both groups dress conservatively but allow modern conveniences and neither uses PG in worship services.

This study includes one BA member and two NOA members—an insufficient number to warrant their inclusion as separate groups. Instead these BA and NOA individuals are subsumed by the group in which they were raised, which is OOA in all three cases. No one from the Salem Church is included in the study.

### 2.3.5 Other non-Anabaptist groups

The rest of the Kalona community falls under the category "other non-Anabaptist." Although the geographical area in this study has a majority or plurality Anabaptist population, there are several thousand people living there who are not affiliated with OOA, CM's or M's. Non-Anabaptist residents in this area work primarily in occupations related to agriculture, although an increasing number live in the country and commute to jobs in Iowa City.

### 2.3.6 Relationship of Kalona PG speakers to noncontiguous German-speaking communities

Any investigation of variation in Kalona PG must consider also to what extent Kalona PG speakers might be influenced by interactions with speakers of PG or other German dialects from other communities. I will take this opportunity to note other German-speaking communities and their apparent relationship to Kalona PG speakers.

Other German-speaking communities exist in Iowa, most notably the Amana Colonies located 60 miles northwest of Kalona. To forestall any confusion, I will emphasize that the two communities are in no way related either historically or in any current interactions.<sup>9</sup>

<sup>8</sup> Both sides still harbor strong feelings about this relatively recent break from the OOA: an NOA member noted to my grandmother that passing OOA give them the "high hat," that is, the person turns away their face to make a show of ignoring the NOA.

<sup>9</sup> The Amana Colonies were founded in 1855 by 1200 members of a pietist group called "The Community of True Inspiration." This community originated in southwest Germany in the early 1700s, before travelling to America in 1842 in search of religious freedom, settling near Buffalo, NY before moving on to Iowa. Under separatist communal living arrangements the Amanas became a thriving community. Communal practices and German schooling were abandoned in the 1932 and English worship services were added in the 1950s. Although worship services in both English and German continue to be held today, the German language is dying in the Amanas in much the same way as it is among the M's and CM's of Kalona: it is spoken only by the older generations.

There are a number of other PG-speaking communities in Iowa and the midwest, all of them having a strong OOA presence and none of them geographically contiguous with Kalona. The nearest of these is in Davis County, 50 miles south of Kalona. The next closest is 100 miles north in Buchanan County, settled earlier this century by a group of disgruntled Kalona OOA seeking a stricter Ordnung.

Outside of Iowa, there are OOA settlements in Harmony, Minnesota, Missouri and then a string of communities spotting the states and provinces directly east: Arthur, Illinois; Elkhart and Lagrange Counties, Indiana; Holmes County, Ohio; Geauga County, Ohio; Lancaster, Pennsylvania; and Waterloo, Ontario. The latter five on this list happen to be the largest OOA communities in the world.

The role that these communities might play in setting or influencing linguistic norms for Kalona PG speakers remains an open question. It is clear that Kalona is geographically isolated from other PG-speaking communities, however we can not yet eliminate the possibility that regular and perhaps relatively intense interaction takes place between distant OOA settlements. This is an area for future research.

#### **2.4 Attitudes Toward and Usage of PG in Kalona**

In general, attitudes toward PG are overwhelmingly positive, although a sizable minority of M's and CM's do not like to speak it—often because they feel that their command of the language is no longer adequate for relaxed conversation.

All of the respondents are, or were at one time, bilingual in PG and English, though with varying degrees of fluency. All, except for one, acquired PG as their L1 in early childhood. For many, English was acquired at home alongside PG, often from older siblings. For some, mostly oldest siblings themselves, English acquisition began in earnest in grade school. For most respondents both home and school settings played a role in their acquisition of English.

Thus, by the early grade school years, nearly all of the speakers in this study had achieved a level of fluent bilingualism in PG and English. But this was not to last. For those in whose homes PG continues to be spoken today (i.e., all of the OOA), fluency in PG has been maintained. For many CM and M respondents, however, English rapidly and completely supplanted PG as an L1 once they married non-PG-speaking spouses or once their children were in school for a few years and had monolingual English-speaking friends.

Among the non-OOA population, then, there are many speakers who have not used PG as a primary means of communication in their homes for 20, 30, even 60 years. The effects of this language disuse are difficult to measure and appear to vary widely from one individual to the next, but certainly for some it has led to language attrition: rusty

speakers. The most obvious characteristic of the speech of rusty speakers is vocabulary loss, as one 78-year-old M woman admitted: "There's too many words I've forgotten...a woman talked German with me and I couldn't keep up."

Frequency of usage of PG ranges widely among the respondents from "every day" (all OOA and one CM) to several times a week to "only on certain occasions" (e.g. at the funeral of an OOA relative). When extended conversation in PG does occur, the context for all speakers is communication with OOA relatives or neighbors. The choice of English or PG in these situations is driven in part by the setting, e.g., English when meeting in public vs. PG when gathering at a OOA home for a reunion.

There is a strictly-observed convention that PG not be spoken when non-PG speakers are present. All respondents agreed that it is rude to speak PG when others in the conversation cannot understand. But many also affirmed that it is equally rude to speak PG even when others are not in the conversation but within earshot. Still, in town or in public, little PG is spoken. "Sometimes we're in a group and we speak English, even if all can speak Dutch," said a 50-year-old OOA woman. And English is used by OOA with customers in their places of business such as the harness shop, steel shed, upholstery business, print shop, etc...

In spite of the fact that they may have limited chances to use it, non-OOA Kalona PG speakers generally like speaking PG. Many cite its expressive power and some CM and M speakers use PG to add a dash of silliness to their English conversations and jokes.

Standard or literary German exerts only a small influence on the speech of Kalona PG speakers. Few have studied it in high school or college. Most have had exposure to the literary language in church where a rather archaic literary German is used in songs and in scripture reading. For the M's and CM's this exposure would have been in their early years; for the OOA it continues. Since a thorough reading knowledge of German is not always easily attained by the end of 8th grade, many OOA go to "Dutch College," an informal course which meets for approximately six weeks during the winter. The students, young adults now out of school (ages 15-21, approximately), focus on learning to spell and read high German.

English is used alongside PG among OOA. One 43-year-old OOA man noted that the domains in which PG and English are commonly used do not necessarily overlap, "Depending on what [the topic] is, I can think of the Pennsylvania German sooner than English."

Only one OOA revealed any openly hostile attitude towards PG, a 50-year-old woman (not the same woman as quoted in the previous paragraph) who made the startling statement: "I'd be glad if there was only one language: English." She offered no reason for this opinion.

<sup>10</sup> See Sasse 1992a, 23 on "rusty speakers."

In spite of the community norm which sanctions PG as the language for OOA in-group communication, some OOA prefer using English: school age children and young, unmarried adults, in particular. The influence of the OOA parochial schools, where only English may be spoken, is one cause for this preference. Thus, many OOA families with school-age children find themselves speaking English more than PG at home.

In addition, there are Sunday evening singings where unmarried OOA can gather to socialize. Unlike at church services, songs at singings are mostly in English and so is conversation—perhaps 70% English, according to one 23-year-old OOA woman's estimate. Her 30-year-old brother, now married, recalled speaking a lot more English when he went to singings.

At least some of the preference for English among young OOA appears to be due to its overt prestige. "Young folks like to speak English. It's a style. Dutch is too old-fashioned," commented a 50-year-old OOA woman. "It depends on where they work," qualified a 19-year-old OOA woman, explaining that young OOA who choose to work at less-traditional, non-agricultural jobs (e.g., at an OOA grocer or in the local bakery or restaurants) speak English more at the singings.

"I talk too much English," acknowledged Ruby, a 23-year-old OOA woman who has been employed at the Kalona bakery for over two years. She also claimed that this is a phenomenon peculiar to Kalona: "Our community young folk don't speak German like a lot [of OOA] in Indiana or Pennsylvania." Her language choices reflect her environment: English during the week, and PG on Sundays.

Over time, the influence of children speaking English can extend to the parents as well. When asked in what situations she speaks PG, an OOA mother of twelve mentioned everyday situations at home, but then acknowledged that even at home English is often used: "about fifty percent English."

Kalona PG speakers see several advantages in being able to speak PG. Bilingualism was cited by many. Many M's and CM's cite being able to talk with OOA relatives and neighbors as an advantage. A disadvantage some noted was the interference of PG in their English.

Even though M's or CM's generally value speaking PG themselves, they have not made it a priority to teach their children. Few expressed making a conscious decision for or against using PG in the home. A number noted the fact that their spouse could not speak PG, so English was the language used at home. All of the OOA respondents affirmed that they want their children to speak PG, and most saw it simply as a matter of course.

Most respondents said they do not avoid speaking PG—at least at this stage of their lives—unless it is to avoid excluding those who don't speak it. This was not always the case. The world wars were often tense times for pacifists who spoke German.

One OOA man affirmed that it is necessary to speak PG in order to be Pennsylvania Dutch—or rather, “in order to be OOA,” as the question was reinterpreted for him. He spoke of PG as crucial to the OOA religious identity: “That’s our standards we have that we use the German...if we’d use the English in the churches and stuff that wouldn’t be according to the rules and the regulations of the church.” Other responses to this question were not interpretable, because it was not clear if reference was being made to the OOA or not.

The degree of fluency of the interviewees could only be evaluated subjectively, and was generally based on their ability to complete the translation task.

The degree of fluency appears to vary quite widely—even between husband and wife. Factors that appear to impact fluency positively are: having PG spoken in the home throughout childhood, being the eldest sibling (i.e., having no older brothers and sisters serving as early models for learning English), having and visiting Amish relatives, having a spouse who speaks PG, being older in age, affiliation with OOA or CM churches, and, perhaps, simply an interest in the language.

To summarize, then, PG and English are both positively valued in Kalona, though it is only the traditional plain OOA of all ages who use both codes extensively on a regular basis. For the historically plain M’s and CM’s, the domains of usage for PG have become increasingly restricted, and English has been their primary and dominant code for most of their adult lives.

### 3. Methods

#### 3.1 Selection of dependent variable

In selecting a dependent variable for the study of variation in Kalona PG, I had in mind three criteria:

- the variable should be frequently occurring, and thus easy to elicit,
- the variable should have already been studied in other communities, thus allowing for comparison with previous research,
- the variable should be a clearly identifiable morphological or syntactic feature, thus avoiding any perceptual difficulties resulting from my unfamiliarity with PG phonology.

Morphological case marking in Pennsylvania German meets at least the first two of these criteria; the “clearly identifiable” criterion, proves difficult to satisfy (see section 3.1.4, p.265).

## 3.1.1 Case in PG

PG marks case in pronouns, determiners, and adjectives. In personal pronouns three cases are distinguished: nominative, accusative, and dative. The dative forms and the accusative forms with which they often vary in PG are shown in Table 2. The 1PL and 2PL use the same form for both accusative and dative and so are not included in the elicitation paradigms in this study (see Figure 1, p. 269).

Table 2 "Standard" PG dative and accusative pronoun forms (Van Ness, 1994).

	Accusative	Dative
1.sg.	/mɪx/	/mɪr/
2.sg.	/dɪx/	/dɪr/
3.sg.masc.	/ɪn/	/ɪm/
3.sg.fem.	/si/	/ɪrə/
3.sg.neut.	/əs/	/ɪm/
1.pl.	/ʊns/	/ʊns/
2.pl.	/aɪx/	/aɪx/
3.pl.	/si/	/ɪnə/

For possessive pronouns, demonstrative pronouns, interrogative pronouns, determiners and adjectives, two cases are distinguished: common and dative<sup>11</sup>. The interrogative pronoun, definite and indefinite determiner paradigms are noted in Table 3. The plural determiners use the same forms for both accusative and dative and so are not included in the elicitation questionnaire (see Figure 1, p. 269).

Table 3 "Standard" PG Determiners and Interrogative Pronoun Forms.

	Definite Common	Definite Dative	Indefinite Common	Indefinite Dative
sg. m. 'man'	/dər man/	/ən man/	/əm man/	/mə man/
sg. f. 'woman'	/di fra/	/ən fra/	/dər fra/	/rə fra/
sg. n. 'child'	/es kɪnd/	/ən kɪnd/	/əm kɪnd/	/mə kɪnd/
pl. 'children'	/di kɪnər/	/di kɪnər/	/də kɪnər/	/də kɪnər/
interrog. pn.	/wɛf/	/wɛm/	--	--

PG differs from Standard German in its pronominal morphology. Standard German does not collapse the nominative and accusative into common case and Standard German marks a fourth case—the genitive—as well.

<sup>11</sup> The common case in PG was formed by a collapsing together of the nominative and accusative cases.



### 3.1.2 The Functions of Dative Case in PG

It is the dative case in PG, and in certain other German dialects, that is often subject to variation as its functions are subsumed by the accusative or common cases. In order to understand this variation, we need to understand the functions that the dative case serves.<sup>12</sup>

The dative in PG serves four functions:

1. Dative marks the indirect object.
2. Dative is required for nouns governed by certain prepositions.
3. Dative is required for nouns governed by certain verbs.
4. Dative helps mark possession.

All of these functions, except for the last, are also present in Standard German.

An example of the first function in PG can be seen in:

- (1) /si gɛbt əm man ən bux/  
 She gives M-DAT man N-ACC book

Here /əm man/ is the indirect object and takes the dative definite article, whereas the direct object /ən bux/ is in the accusative.

An example of the second function in PG can be seen in:

- (2) /aus əm haus/  
 out of N-DAT house

Here /əm haus/ is in the dative because the preposition *aus* requires it.

An example of the third function in PG can be seen in:

- (3) /ɪx helfə dir/  
 I help 2SG-DAT

Here the second person singular pronoun is in the dative because the verb *helfe* requires it.

An example of the fourth function in PG (marking possession) can be seen in:

- (4) /əm man sai bu/  
 M-DAT man his boy ('The man's boy')

<sup>12</sup> Thanks to Ilse Lehiste for her detailed discussion on the function of the dative in standard German.

### 3.1.3 Selection of specific variables involving the dative case

This study focuses, then, on variation in dative case marking in PG personal and interrogative pronouns and determiners, omitting adjectives (with the exception of one sentence (#21) on the questionnaire which includes a possessive adjective token), in order to limit the scope of the research. The goal is to understand to what extent the dative case remains viable in PG and to what extent its functions are being subsumed by the accusative and common cases.

PG personal and interrogative pronouns and determiners and possessive adjectives fit the criteria noted in 3.1, above: they form a suitably restricted lexical set of high-frequency function words which bear case-marking; they have been the subject of at least one detailed study, Huffines 1989, while others, such as Anderson and Martin 1976, make note of them as well; and they are a relatively easily identifiable morphological feature. The dative and accusative or common case variants of these variables can be seen in Table 2 and Table 3, above.

### 3.1.4 Criteria for inclusion and exclusion of tokens of dependent variable

I encountered some difficulties in identifying variants which are phonetically similar, e.g. /mix/ (1 SG ACC) and /mɪr/ (1 SG DAT). In examples like this when the velar fricative lenites and the /r/ is vocalized, it is very difficult to distinguish dative variants from accusative variants. This pattern is true for both the 1 SG and 2 SG pronominal forms. In some instances tokens were recorded whose case are impossible to determine, e.g. /mi/. Compounding this difficulty is the fact that this indeterminate form coincides in phonological form with the English word of the same meaning.<sup>13</sup>

All tokens which were indeterminate with respect to case, e.g. /mi/, /di/, were excluded from the statistical analyses.

The 3 SG pronouns (/im/ DAT and /in/ ACC) also pose problems in that they are only differentiated by the place of articulation of the final nasal. I circumvented this problem in the majority of cases by looking for closure of the lips during the elicitations.

Further study should include more detailed phonetic analyses of these tokens.

If, in the elicitation/translation task portion of the interview, the informant self-corrected or offered two tokens, only the first one was included in the data unless it resulted from

<sup>13</sup> This fact suggests that contact with English is playing a role in the levelling of dative case with accusative and common cases. Language contact is not a necessary condition for case levelling, however, since some continental German dialects have eliminated the dative without contact with a non-case marking language. Still, I will take the position outlined by Sarah Thomason at the 5th Annual Comparative Linguistics Workshop, October 1996, in which she stated that language contact must be assumed to be a factor in language change unless it can be proven that it was not. I do not imply by this that PG speakers who use /mi/ have borrowed it from English.

interviewee's confusion—often understandable—over whether I was intending to elicit

1.sg. or 2.sg forms (e.g. informant responds with *zu mir* 'to me' when 'to you' was being elicited). In these cases only the form intended in the elicitation was included. Responses which substituted verbs other than those being elicited were also omitted.<sup>14</sup>

### 3.2 Selection of independent variables

Independent linguistic variable (factor group) selected for study:

1. Grammatical function: indirect object, object of dative-governing preposition, object of dative-governing verb<sup>15</sup>, possessive constructions.

Independent social variables (factor groups) selected for study were:

1. Religious affiliation: Old Order Amish, Conservative Mennonite, or Mennonite.
2. Age. A numeric variable assigned to a four-point ordinal scale: 0-40 years, 41-60 years, 61-80 years, 81+ years.
3. Sex: female and male.

#### 3.2.1 Rationale for the selection of the independent linguistic variable

The internal independent variable "grammatical function" was selected in order to test whether dative case-marking is undergoing change in every environment that calls for it, and to observe which functions may be leading the change.

#### 3.2.2 Rationale for the selection and scale design of the social variables<sup>16</sup>

The social variable "religious affiliation" was selected in order to observe to what extent the three major religious groups in Kalona pattern together in their use of dative case marking. As discussed in section 2.3 (p.255) the M, the CM, and the OOA differ considerably in their social practices with regards to education, occupations, style of worship, and past and current domains for usage of PG and English.

The social variable "age" was selected to help quantify any age-correlated effects such as those that Huffines 1989 observed. The four-point ordinal scale was developed on the basis of the data. Use of non-dative forms is nearly categorical for those under the age of 40 years-old. Those older than 40 were grouped into categories of roughly a generation time-span (20 years).

<sup>14</sup> The step of omitting unelicited verbs was not necessary, since tokens are coded according to the environment of the response.

<sup>15</sup> These verbs which govern the dative are somewhat idiosyncratic in that, unlike a number of ditransitive verbs which govern two objects—one accusative, one dative—these verbs govern objects which do not have thematic roles which typically receive dative marking.

<sup>16</sup> Other social variables which may merit some examination are speaker fluency and attitude toward PG. Neither of these was included in this study, primarily due to the difficulties in appropriately and objectively quantifying these variables.

The relatively rigid social roles and apparently different social networks in which women and men interact, particularly in the OOA community (see section 2.3.1), may provide ample opportunity for the development and maintenance of gender-differentiated speech. It was for this reason that the social variable "sex" was selected.

### 3.3 Subjects

I developed a subject pool through contacts of my grandmother, a 93-year-old Mennonite woman who accompanied me on many of the interviews. I was able to interview a total of 70 persons in an extensive but loose network of people who knew each other.

I attempted to interview a diverse sample of speakers, but some gaps remain, most notably in the young M and CM speakers. Among the M's, I had difficulty locating fluent or semi-fluent speakers younger than 60. There is only one CM younger than 40 in this sample. The result is that the sample from M and CM speakers is skewed since tokens elicited from older speakers form a greater proportion of the data set. This bias in the data is taken into account in my analyses (see section 4.2.1).

A couple interviews were conducted with Kalona natives now living elsewhere. One, is a 25-year-old male born and raised in Kalona and currently residing in a Mennonite community in Florida. An additional interview was conducted with a 58 year-old Kalona native currently living in Columbus, Ohio.

**Table 4. Distribution of interviewees and tokens across social variables.**

<i>Factor Groups</i>	<i># of interviewees</i>	<i># of tokens</i>
<i>Factors</i>		
<i>Religious Affiliation</i>		
Old Order Amish	38	722
Conservative Mennonite	17	303
Mennonite	15	239
<i>Age</i>		
0-39	16	297
40-60	18	330
61-80	28	490
81+	8	147
<i>Sex</i>		
female	35	642
male	35	622
<i>TOTAL (for each variable)</i>	70	1264

### 3.4 Data collection

I visited the Kalona community in August 1995, December 1995, and August 1996 to gather data. On the first two visits my stay was limited to one day. The bulk of the interviews were conducted during a five-day period in August 1996.

#### 3.4.1 Interviews

The interview consisted of two parts: 1) completion of a survey of personal linguistic background and attitudes toward the language, and 2) completion of a translation task of sentences which, in Standard German, call for dative constructions.

When possible, the entire interview was recorded using a Sony recorder and a Sony lavalier battery-powered microphone. In many instances, though, I took notes on the linguistic background and attitudes section of the interview, and tape recorded only the translation task.

Personal information and questions about language usage and attitudes were collected in an oral interview using a written questionnaire as a guide. The questionnaire was based in part on the work of Trudgill and Tzavaras 1977. The questionnaire used in the 1995 interviews included three questions designed to elucidate attitudes toward "Pennsylvania Dutch" ethnicity. These three questions proved confusing and generally unhelpful and were dropped from the 1996 interviews.

In addition, questionnaires were sent to eight individuals in 1995, and these then completed the questionnaires in written form and returned them to me. Six of these eight completed an abbreviated oral interview in 1996, including the translation task.

#### 3.4.2 Translation Task

For this part of the interview the subject was told that she or he would be read twenty-one sentences aloud in English, and she or he was instructed to "say it back to me in Pennsylvania German." While admittedly a less than natural speech task, this translation format was chosen because it allowed for focused elicitation of certain forms of interest in a short period of time, a factor that was crucial in order to include a sufficiently large number of subjects in my study.

Some sentences in the translation task were culled from Huffines' 1989 study, the rest I constructed based on the patterns of Huffines' sentences and forms found in other PG references. All were intended to elicit dative variants (see Figure 1 below).

Figure 1. The questionnaire.

<u>English translation</u>	<u>Standard German Orthography</u>
1. I helped <i>them</i> yesterday.	Ich habbe <i>ihnen</i> gestern geholfe.
2. It doesn't belong to <i>you</i> .	Das gehoert <i>dir</i> nicht.
3. His daddy often gives <i>me</i> money.	Sein fater gibt <i>mir</i> oft gelt.
4. The little girl throws the ball to <i>him</i> .	Das kleiner madchen wirft <i>im</i> den balle zu.
5. The teacher gave <i>her</i> a book.	Der lehrer gab <i>ihr</i> ein buch.
6. I will help <i>you</i> fix the door.	Ich helfe <i>dir</i> die teuer zu reparieren.
7. Grandfather told <i>them</i> a story.	Opa ertselter <i>ihnen</i> eine geschichte.
8. I still want to make <i>myself</i> a dress.	Ich will <i>mir</i> immer noch ein kleid machen.
9. It's hard for <i>her</i> to walk fast.	Es velt <i>ihr</i> schwer schnell zu gehen.
10. Who's sitting beside <i>him</i> ?	Ver zitst neben <i>ihm</i> ?
11. He gave <i>me</i> the man's hat.	Er hat <i>mir</i> den hut des Mannes gegeben.
12. I'll lend <i>you</i> my book.	Ich werde <i>Dir</i> mein Buch leihen.
13. We gave <i>him</i> the letter.	Wir habben <i>ihm</i> den Brief gegeben.
14. I give the wagon to <i>the</i> boy.	Ich gebe <i>dem</i> Jungen den Wagen. OR Ich gebe den Wagen an den Jungen.
15. The father gave <i>the</i> grandfather the key.	Der Vater gab <i>dem</i> Grossvater den Schlüssel.
16. To <i>whom</i> did you give the letter?	<i>Wem</i> hast Du den Brief gegeben?
17. <i>Whose</i> letter is that?	<i>Wessen</i> Brief ist das?
18. Mother helped <i>me</i> cook supper.	Die Mutter hat <i>mir</i> geholfen, Abendsessen zu kochen.
19. The boy gave <i>the</i> mother a shoe.	Der junge hat <i>der</i> Mutter einen Schuh gegeben.
20. The girl told <i>you</i> a story.	Das madchen hat <i>Dir</i> eine Geschichte erzahlt.
21. Let's go to <i>my</i> sister's house.	Lass uns zu <i>meiner</i> Schwester

Fifteen sentences in the questionnaire contain pronominal forms for which dative or *accusative* case are the variants: 1-13, 18, 20. See Table 2, above.

Seven sentences in the questionnaire contain determiners or interrogative pronouns for which dative and *common* cases are the variants: 11, 14-17, 19, 21.<sup>17</sup>

A caveat is needed here. What qualifies as a dative construction in standard German may not be so in PG—and may not have been in any of the dialects contributing to the

<sup>17</sup> Sentence 21, in fact, contains two instances of dative marking, one embedded within the other. First, since this is a possessive construction (*my sister's house* = *mainre schwester ire house* = *my sister her house*), the possessive adjective *mainre* should receive dative case. Secondly, the preposition *zu* subcategorizes for dative, thus the pronoun modifying *house* must receive dative case. I didn't recognize the nature of this construction until late in my research. Therefore, statistical runs include only the embedded dative resulting from the subcategorization of *zu*; I omit the possessive form (note that there are no instances of dative case for *either* construction). This is a regrettable error. I do make note of the embedded possessives in the discussion of the environments for dative usage (section 4.3, p.278).

formation of PG in colonial Pennsylvania. This valid criticism can be at least partially

corrected by making reference to PG grammars. I have consulted Frey's grammar of PG and it confirms the majority of these sentences as clearly requiring dative forms. There are, however, apparent discrepancies between Frey and two of Huffines's sentences which I employ:

- #8 can be interpreted as a reflexive, but Frey's reflexive pronouns have the same forms as the accusative personal pronouns<sup>18</sup> (30).
- In #9 *fur* ("for") is a preposition which supposedly subcategorizes for the dative. Frey, however, lists it as subcategorizing for the accusative (40).

An additional problem, however, arises when we consult a grammar such as Frey's. Inasmuch as these grammars often reflect non-plain (NPG) norms and usage that is perhaps archaic, even these sources may not be completely reliable for establishing norms for plain communities such as Kalona.

In two interviews conducted in August 1995 only 10 sentences were elicited. In the December 1995 interviews, 20 sentences containing 21 possible tokens of dative constructions were elicited.

### 3.5 Statistical Analyses

Tokens of dative case variants were taken from the responses to the translation task and were analyzed using the GoldVarb 2.1 statistical program<sup>19</sup>. The relative weight of each variant within each factor group was determined via a one-level binomial analysis. The significance of the contribution of each factor group to the variation in the data was calculated using logistic regression (step-up and step-down).

In addition, the degree of correlation of the factor group "age" with the amount of dative forms utilized by each individual was studied and graphed using logistic regression in the SigmaPlot 5.0 graphing system.

## 4. Results

### 4.1 Overall patterns of Dative Usage

There is a great deal of variation in morphological marking of dative case in Kalona PG, but non-dative forms are the most commonly selected variants (see Table 5). Out of 1442 tokens of potential occurrence, Kalona PG speakers mark less than half of them (27%) with the dative case, and a notable number of tokens (13%) are either indeterminate in form or are structured so as to eliminate the need for case marking (e.g. *es is net dains* for 'It doesn't belong to you'). These tokens—marked "Other"—are omitted from further statistical analyses.

<sup>18</sup> With the exception of the 3rd person forms which are *sich*.

<sup>19</sup> GoldVarb 2.1 is based on programs developed by David Sankoff, Pascal Rousseau, Don Hindle, and Susan Pintzuk. It is adapted for use on the Macintosh by David Rand.

**Table 5. Kalona translation task.**

	Dative	Non-Dative	Other	Total
TOTAL	27% (382)	61% (882)	13% (180)	1442

<sup>19</sup> "Non-Dative" refers to either accusative or common case.

The data in Table 5 clearly indicate that the dative vs. accusative and dative vs. common case distinctions are restricted in their use in Kalona PG.

#### 4.2 Significance and weighting of independent variables

All tokens, with the exception of those marked for exclusion (see section 3.1.4, p.265 and section 4.1) were fed into the GoldVarb statistical program with all four independent factor groups being included in the first run.

Step-wise logistic regression selected three of the four (independent variable) factor groups as significant:

1. grammatical function
2. religious affiliation
3. age

The factor group "sex" is eliminated; it is not a significant factor and will not be discussed further in this paper.<sup>20</sup>

A second run was then made using only the three factor groups identified as significant from the first run. The weighting of the variants within each of these three factor groups is displayed in Table 6. For "Weight," the closer the value is to 1, the greater the weight of the factor, i.e., the greater the probability for the application of dative case<sup>21</sup>. The "Application/Total" figure shows the percentage of dative tokens (applications) out of the total number of tokens of this variant (e.g., for "object of verb" this is 42/187 = 0.22). The "Input & Weight" figure combines the overall input<sup>22</sup> for application of dative case, here, 0.144, with the weight for that particular factor.

<sup>20</sup> There is a small but insignificant difference between female and male speakers' usage of the dative: Out of a total of 642 tokens from female speakers, 33% (212) were dative variants. This contrasts with the male speakers who yielded only 27% dative variants (170 out of a total of 622 tokens). However, this apparent difference between the sexes is, in fact, an artefact of the data, in which the informant pool contains a relatively high number of older females—and older speakers are more likely to use dative forms. The average age of the female informants is 59.5 years, while the average age of the male informants is only 55.5 years. Most important is the fact that over half (51%) of the female informants are 70 years or older, while only a third (34%) of the male informants are 70 years or older. So it is not the case that Kalona women use more dative forms than men, but that simply, there are more older women than older men in this study, which results in the women producing more dative forms overall.

<sup>21</sup> The "weight" for a given factor is roughly its contribution to the expected number of dative tokens for a particular cell in a contingency table (a cell might be: *tokens of indirect objects elicited from OOA 80+ yrs olds*). "Weight" can be used to figure this factor effect in ANOVA using the formula  $\ln(\text{weight}/1-\text{weight})$ .

<sup>22</sup> The "input" for a given run can be used to calculate  $m$  for the linear regression equation  $\ln(F_y) = m + A + B$  by employing the formula  $\ln(\text{input}/1-\text{input})$ .



Table 6. Factor weights.

Factor Group Factors	Weight	Application/ Total	Input & Weight
<i>Grammatical Function</i>			
object of verb	0.354	0.22	0.08
object of preposition	0.499	0.30	0.14
indirect object	0.466	0.29	0.13
possession	0.836	0.50	0.46
<i>Religious Affiliation</i>			
Old Order Amish	0.419	0.18	0.12
Mennonite	0.614	0.54	0.21
Conservative Menno.	0.601	0.41	0.20
<i>Age</i>			
80+ yrs. old	0.933	0.69	0.70
61-80 yrs. old	0.844	0.51	0.48
41-60 yrs. old	0.317	0.08	0.07
0-40 yrs. old	0.038	0.01	0.01
input <sup>23</sup> = 0.144			
Total chi-square = 51.6882			
Chi-square/cell = 1.0768			
Log likelihood = -502.194			

The low chi-square/cell—only 1.0768—demonstrates that the statistical model based on the frequency distribution of tokens with respect to these variables provides a very close fit with the data. In other words, this confirms that these three factor groups are significant in accounting for variation in dative case marking in these data. Also the total chi-square of 51.6882 is much greater than the corresponding critical value for  $p=.05$  (which is  $\chi^2=18.3$  for 10 df), and, in fact, indicates that the model is significant to  $p=.001$ .

The variants which are most heavily weighted in favor of dative use are: for grammatical function, the possessive construction (0.836); for religious affiliation, Mennonites (0.614); and for age, the oldest speakers, age 80 and over (0.933).

Among the three significant factor groups, "age" is the most significant followed by "grammatical function" and lastly "religious affiliation." This ranking of factor groups is based on the order in which each group was included in the step-up regression analysis, i.e., "age" was selected first and "religious affiliation" last.

In section 3.3 I note that within each religious affiliation group the distribution of speakers is not evenly spread across the four age groups. This can be seen in the cross-tabulation of "religious affiliation" against "age" in Table 7. The three figures in each

cell indicate the number of informants included in the cell, the total number of tokens in the cell, and the percentage of dative tokens. The shaded cells mark cells in which there is just one informant.

**Table 7. Crosstabulations of "religious affiliation" against "age".**

	OOA			M			CM			total <sup>d</sup>		
	p <sup>a</sup>	t <sup>b</sup>	% <sup>c</sup>	p	t	%	p	t	%	p	t	%
80+ yrs	4	74	72%	3	54	72%	1	19	53%	8	147	69%
61-80 yrs	9	173	35%	9	143	59%	10	174	60%	28	490	51%
41-60 yrs	11	202	7%	2	29	14%	5	99	10%	18	330	8%
0-40 yrs	14	273	1%	1	13	0%	1	11	0%	16	297	1%
total	38	722	18%	15	239	54%	17	303	41%	70	1264	30%

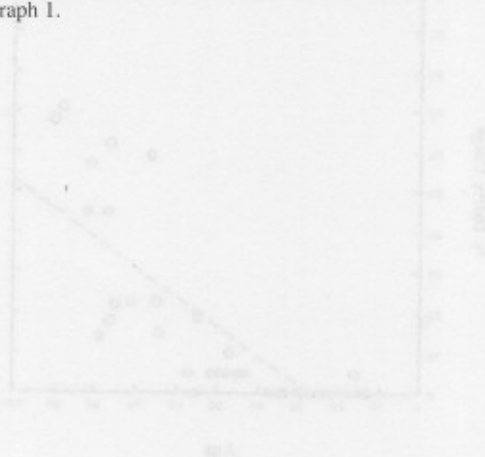
<sup>a</sup>p = # persons. <sup>b</sup>t = total # tokens. <sup>c</sup>% = percent dative tokens

<sup>d</sup>"total" here and elsewhere in statistical analyses omit the approximately 180 tokens which were not marked with dative, accusative, or common case.

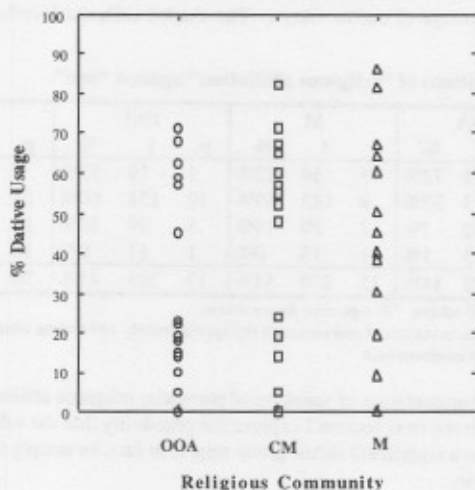
Given this unequal representation of speakers of particular religious affiliations within certain age cohorts, in the next section I explore the possibility that the selection of religious affiliation as a significant factor group might, in fact, be simply the result of this bias in the data sample.

#### 4.2.1 The significance of factor group "religious affiliation" as an artefact of unequal representation of factor group "age" in the informant pool

In order to observe the patterning of dative usage within each of the three religious communities in Kalona, I graphed individuals against the percent of dative forms that they employed. The patterning of individuals within the OOA, the CM's, and the M's can be seen in Graph 1.

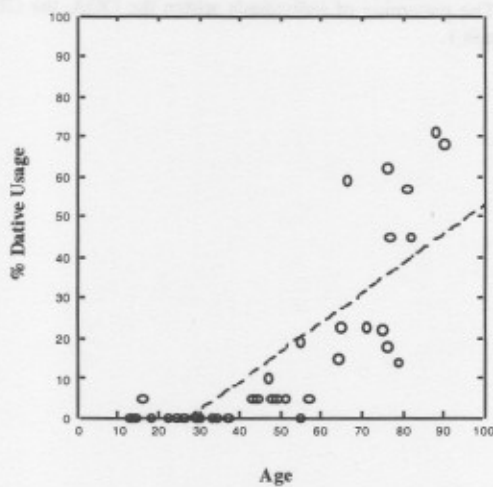


Graph 1. Religious Community vs. % Dative



Graph 1 reveals that there are widely varying individual practices (0% to over 70% percent dative usage) within each religious affiliation group. The next three graphs plot age vs. % dative usage for OOA (Graph 2), CM (Graph 3), and M (Graph 4).

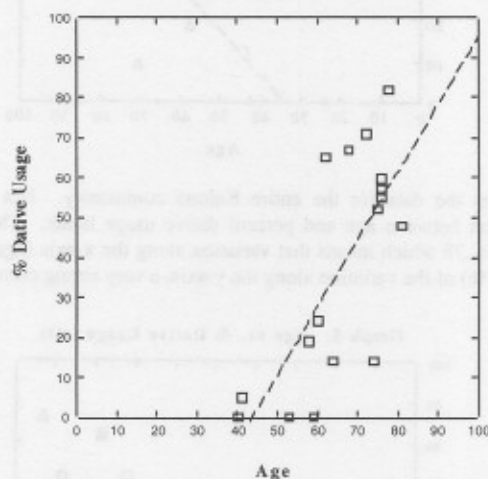
Graph 2. Age vs. % Dative Usage (OOA)



Note in Graph 2 how a pattern immediately emerges. The older the speaker, the more dative forms she or he uses. Of the dozen or so younger OOA speakers below the age of 40 only one individual employs even a single dative form. However, in spite of this obvious pattern, there is one group of OOA who exhibit great variation of percent dative usage: those in their 70's.

The dashed line in Graph 2 is the regression curve. The  $r$ -value for the regression curve is .79, which means that variation along the  $x$ -axis (age) can account for well over half (62%) of the variation along the  $y$ -axis.<sup>23</sup> This is a strong correlation.

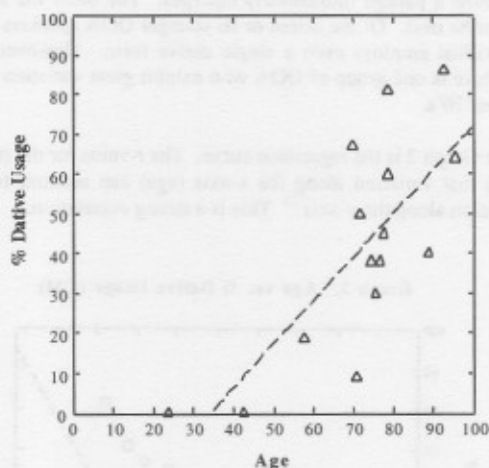
Graph 3. Age vs. % Dative Usage (CM)



Graphs 3 and 4 illustrate a similar pattern among the CM's and M's respectively: the older the speaker, the more dative forms used. The pattern is not as obvious in either of these groups as it is in the OOA due in large part to an absence of younger speakers. Still, the  $r$ -values for the regression curves for both Graphs 3 and 4 are high: .73 for Graph 3 and .75 for Graph 4. This means that variation in age can account for 53% of the variation in percent dative usage in the CM data and 56% of the variation in percent dative usage in the M data.

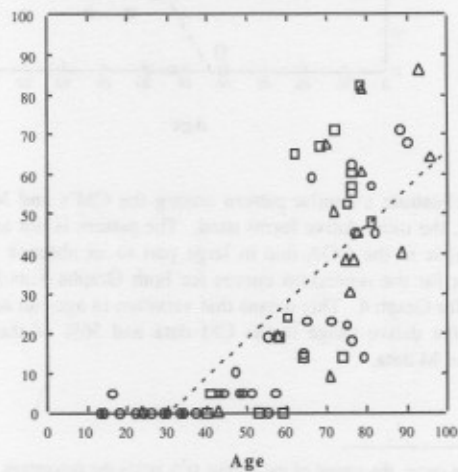
<sup>23</sup> For any regression curve, the square of the  $r$ -value ( $r^2$ ) yields the percentage of variation in the  $y$ -axis which can be accounted for by variation in the  $x$ -axis.

Graph 4. Age vs. % Dative Usage (M)



Graph 5 combines the data for the entire Kalona community. Not surprisingly, the positive correlation between age and percent dative usage holds. The  $r$ -value for the regression curve is .78 which means that variation along the  $x$ -axis (age) can account for well over half (61%) of the variation along the  $y$ -axis: a very strong correlation.

Graph 5. Age vs. % Dative Usage (all)



So the overlapping pattern of usage between individuals of different religious affiliations in Graph 1 hides the fact that the majority of CM and M speakers have a relatively high-frequency use of the dative, which in turn is a result of the fact that the majority of these speakers are relatively old.

These findings are supported by another run of the data through logistic regression, this time identifying as input only the independent variables grammatical function and age.

**Table 8. Factor weights: age and grammatical function.**

<i>Factor Group</i> Variants	Weight	Application/ Total	Input & Weight
<i>Grammatical Function</i>			
object of verb	0.360	0.22	0.09
object of preposition	0.494	0.30	0.14
indirect object	0.465	0.29	0.13
possession	0.830	0.50	0.46
<i>Age</i>			
80+ yrs. old	0.933	0.69	0.71
61-80 yrs. old	0.862	0.51	0.52
41-60 yrs. old	0.317	0.08	0.07
0-40 yrs. old	0.030	0.01	0.01
input = 0.147			
Total chi-square = 15.1236			
Chi-square/cell = 0.9452			
Log likelihood = -513.560			

The result is an even better fit with the chi-square/cell being only 0.9452.<sup>24</sup> So variation in the data is best accounted for simply by variation in age and in the grammatical function of the case marking.

<sup>24</sup> Of course, since GoldVarb does not take into account interaction effects, a comparison of the log likelihood of this run with that in Table 6 suggests that the model which includes religious affiliation is a better one. Using the formula  $\chi^2 = -2L_{\text{simpler model}} + 2L_{\text{more complex model}}$  we calculate  $\chi^2 = 22.732$ , which, for  $df = 12$  is significant at  $p=.05$ , though *not* at  $p=.025$  (In fact,  $df = 11$ , but I am using an abridged table of  $\chi^2$  distributions). If we are to have GoldVarb take into account the interaction effect between age and religious affiliation, we would need to recode the data so that various combinations of age and religious affiliation would be the factors in a separate factor group, e.g., 80+ yrs old and Mennonite would constitute one factor, 80+ and Conservative Mennonite another, etc. If this new factor group then is statistically significant and if it is included in the step-up regression *before* religious affiliation, then we have a clear measure of interaction.

#### 4.3 Linguistic independent variables: the environments of dative usage in Kalona PG

Of crucial importance is the identification of constructions in which the dative is best conserved and those in which it has virtually disappeared in the speech of all of the interviewees. By identifying contexts in which it is used and not used, we can identify which functions the dative continues to fill and which functions speakers have relinquished to the accusative or common cases. The data gathered in this study, while limited in the variety of contexts in which potential dative tokens are elicited, can reveal some tendencies.

The grammatical function of possession (weight = 0.830) is easily the most likely to elicit dative forms from all but the youngest group of informants, who have categorically neutralized dative case distinctions. Less heavily weighted are the functions object of preposition (.494) and indirect object (.465). The function least likely to receive dative case is object of verb (.360).

The heavy weight for the possessive function can be attributed to a single form: the interrogative possessive pronoun /vem sai/, "whose." The interrogative possessive pronoun is by far the environment which most favors use of the dative case in Kalona PG. It is the only context for which a majority of informants (65%) provided a dative form. It is also a frozen construction, as evidenced by two facts. First, the function of marking possession does not, in other contexts, favor the use of the dative to nearly the same extent. This can be seen in sentences such as "He gave me *the* man's hat," (#11a), and "Let's go to *my* sister's house," (#21), which yielded only 28% and 0%, dative forms respectively<sup>25</sup>. Secondly, the same pronoun serving a different function, that of interrogative personal pronoun, /vem/, "to whom," elicited much fewer dative forms.

Even so, the interrogative personal pronoun, /vem/, "to whom," ranks high as a dative-eliciting construction with 39% of the informants utilizing a dative form. We can generalize, then, and note that the high-frequency interrogative pronouns are two contexts in which many speakers have preserved the dative case.

Since use of dative case is an age-correlated phenomenon, I also compared age groups in order to identify differences in the patterns of case marking for the various functions of dative case (see Table 9).

<sup>25</sup> Note that the construction in #21 was inadvertently left out of the statistical analyses. See footnote 17, p.269.

Table 9 Crosstabulation of "grammatical function" vs. "age".

Age	Object of Verb			Object of Prep			Indirect Obj			Possess			Ttl% %
	d <sup>a</sup>	t <sup>b</sup>	% <sup>c</sup>	d	t	%	d	t	%	d	t	%	
80+	15	20	75	26	43	60	49	70	70	12	14	86	69
61-80	27	75	36	75	140	54	109	227	48	39	48	81	51
41-60	0	48	0	8	91	9	8	159	5	12	32	38	8
0-40	0	44	0	0	91	0	2	131	2	0	31	0	1
Total	42	187	22	109	365	30	168	587	29	63	125	50	30

<sup>a</sup>d = # dative tokens. <sup>b</sup>t = total # tokens. <sup>c</sup>% = percent dative tokens

Again, the possessive function is clearly the most frequently marked with dative for the three age groups that still retain the dative case, as the shaded areas in the fourth column indicate. But the function which ranks second for application of the dative is not the same for these age groups. For those 80 years-old and older it is "object of verb" with 75% application. In the 61-80 and 41-60 years-old groups, this function ranked last. This pattern is difficult to explain, though it should be noted that the data set includes only one verb subcategorizing for dative, so this is perhaps not probative.

A clearer pattern emerges if we look at the function "object of preposition" which ranked second in both the 61-80 years-old and the 41-60 years-old groups. Though it trails possessive function by a large margin, this function ranks well ahead of the other two functions in the percentage of dative-marking, particularly among the 41-60 year-olds. This suggests that prepositions which subcategorize for dative case are also particularly resistant to the intrusion of accusative and common case marking. This may be due, in part, to the prevalence of contracted forms occurring with the preposition /tsu/ "to," e.g., /tsu/ + /əm/ (3SG DAT def. det.) which results in /tsum/. This contracted form then is also frozen; it appears it may be acquired as simply a morphological variant of /tsu/.

Certain forms of the dative were not produced by any of the speakers in this study: 3 SG feminine determiner /dɛr/ (in sentence #19 "The boy gave *the* mother a shoe") and the 1 SG possessive adjective /mamrə/ (in sentence #21 "Let's go to *my* sister's house"). Both have simply been collapsed together with the accusative or common case. The 3 SG feminine determiner /dɛr/ has collapsed with the common case /di/, and the 1 SG feminine possessive adjective, /mamrə/, has collapsed with the common case and converged to English as /maɪ/.

In both of these cases, however, the functions which the dative serves—marking the indirect object and marking possession—are maintained by a significant minority of speakers in their use of the pronominal and/or determiner paradigms. For example see the 3 SG masculine determiner in sentence #15, "The father gave *the* grandfather the key," (indirect object) and in sentence #11, "He gave me *the* man's hat," (possession).



So we note here the irregular and gradual nature of the attrition of dative usage in the Kalona community. As the dative case collapses with the accusative or common cases, it does so not in a general way affecting all members of a given paradigm in the same way at the same time. Rather, certain forms are lost first.

It is not clear why the dative form of the 3 SG masculine determiner is retained while that of the 3 SG feminine determiner is not, although the existence of contracted forms with certain prepositions may play a role. There is no evidence from other possessive adjectives which could help us to assess which forms, if any, from that paradigm might retain the dative, and, more importantly, why they would do so.

This pattern, however, was also observed by Dorian (1989b) in the NPG of Hamburg, Pennsylvania. For the three functions indirect object, object of preposition, and possession<sup>26</sup>, the masculine singular and neuter singular pronouns and determiners were near-categorically marked with dative case. The plural and feminine determiners, on the other hand, were much less likely to receive dative case: plural objects 87%, feminine objects 91% (vs. 100% for non-masculine), plural possessives 36%, feminine possessives 27% (vs. 87% for non-masculine). No explanation is offered, but we might note that masculine and neuter dative forms are identical, so that their occurrence is statistically more frequent than the feminine and the plural forms.

Dorian also found that embedded NPs or NPs which were the second NP in a compound only rarely received dative case. She notes that in these cases where there are two consecutive NPs, deviance from dative norms increases with the distance from the element which subcategorizes for dative case.

Further analysis in this area, along with elicitation of a wider range of dative-requiring verbs and phrases is of great interest as it may show just what environments are most saliently dative environments to Kalona PG speakers and perhaps also yield some clues as to the nature of the process of attrition in the rusty speaker.

## 5. Discussion

In all instances dative variants are considered to be evidence of conforming to a more conservative norm and accusative or common case tokens are evidence of innovation away from this (perhaps now quite dated) norm<sup>27</sup>.

<sup>26</sup> The function object of verb, was not tested in Dorian's study.

<sup>27</sup> Here we are faced again with the difficulty of defining the conservative norm. In the absence of documentation of the speech of the Kalona PG community in its early years, I assume that its norms of usage were similar to those noted in standard German and PG grammars (but this may be problematic, too, see section 3.4.2, p.268).

### 5.1 Change in apparent time

Graph 5 clearly displays a change in apparent time in the Kalona speech community. On the basis of current age-differentiated usage of dative forms, we, then, can hypothesize a change in real time, that is, that the dative case has undergone considerable reduction in use in Kalona in this century.

The relationship between change in apparent time and change in real time rests on certain assumptions related to language acquisition and language attrition. If the process of language acquisition does indeed lead to a gelling of the speaker's grammar in a relatively immutable state during or shortly following adolescence, then we should expect that a given speaker's usage will reflect the language use of the entire speech community at that point in time.<sup>28</sup> In addition, language attrition, due either to the effects of aging or to disuse of the language, must be assumed to be minimal.

Thus, the usage of the 19-year-old reflects current norms of usage in the Kalona community, while the usage of the 79-year-old would reflect the norms of usage of the speech community some sixty years ago, in the 1930's.

What this tells us is that the reduction of use of the dative case began before the beginning of the twentieth century (since no speaker has 100% usage of dative tokens) and has occurred largely during the lifetimes of the oldest of the informants in this study.<sup>29</sup> Given that speakers forty years old and younger use no dative forms (with the exception of one), we can say that this change appears to have reached it's apex approximately thirty years ago.

In order to be sure of this proposal for apparent time change, however, we need to eliminate the possibility of age-graded change, that is, a stable situation in the speech community in which young persons' usage differs from that of older persons', and the individual's usage, then, changes over time to match those of her age-mates. If this were the case in Kalona, then the data could be accounted for by assuming that individuals increase the frequency with which they employ the dative as they grow older. Eliminating the possibility of age-graded change requires at least one piece of data showing a real-time change. Recordings and writings of Kalona PG from thirty or more years ago providing evidence of a relatively more dative-rich usage by the entire speech community would confirm real time change.

<sup>28</sup> Labov (1994, 98-112) has mustered some evidence to show the stability of individual phonological systems over time. The stability of individual morphological and syntactic systems, central to this paper, has not been studied in a similar way.

<sup>29</sup> The origins of the loss of the dative are not fully explored in this paper. Certainly, reduction in the use of the dative might have begun much earlier--perhaps even several centuries previous to the lifespans of the informants in this research. All we can say with assurance is that the process began before the critical gelling period in the acquisition of these informants.

Such data are not currently available. However, as Labov notes: "Age-erasing is most typical of the more conscious types of style shifting and correction," (1994, 101) and "...variables operating at high levels of social awareness are modified throughout a speaker's lifetime, with consistent age-grading in the community" (1994, 111). There is no evidence that dative usage is a variable of which Kalona PG speakers are highly aware to the extent that they employ it in style shifting<sup>30</sup>. It is thus not likely that it is a variable undergoing age-graded change.

## 5.2 Accounting for age-correlated differences in dative usage in Kalona PG

### 5.2.1 Religious heterogeneity; linguistic homogeneity: a single speech community

Underneath its disguise of remarkable heterogeneity in religious and lifestyle practices, Kalona retains, in the speech of its people, evidence of the days when the community was much more homogeneous. At the turn of the twentieth century, the norms for the use of PG, as well as English, which had a more limited role at the time, were shared by the Anabaptists in Kalona even as their religious beliefs began to diverge.

Age reflects the sociological changes which took place during the fracturing of the erstwhile religiously homogenous Amish-Mennonite community of Kalona. Dative usage reflects the speaker's age of acquisition in relation to changes in education, interaction with English-speakers, and church community rifts.

Hinskens (p.c.) and Arvaniti and Joseph (1999, this volume) have noted that age in itself is not explanatory. We must identify ways in which particular historical events or trends have impacted speakers' usage in specific ways. This is the focus of the next section.

### 5.2.2 Historical events influencing loss of dative marking in Kalona

The first events which indicate linguistic change was afoot in Kalona took place only thirty years after the community was founded.

In the 1870's the first Sunday schools were established in Kalona. However, the motivation for these was not entirely for spiritual formation. As Gingerich notes, these first Sunday Schools were "in reality German schools in which that language was taught to the children. With the growing importance of the public school system, the Amish felt that it was necessary to take more active steps in retaining the German language." (Gingerich, 132). So apparently already contact with English, in particular via the public schools, that is, via their *children*, spurred the community to try to shore up its linguistic defenses.

<sup>30</sup> There were a couple instances of self-correction in which older speakers first produced non-dative forms and then changed to dative forms. However, this is at least partially balanced out by instances in which speakers produced a dative form in the wrong person (i.e. 1SG instead of 2SG) and when making the correction also changed to a nondative form (e.g., *mir* changed to *dix*) with no apparent attention paid to the difference in case marking.

At this same time there occurred the first splits in the religious community due to a clash in leadership styles. This first split would be followed by others, and eventually language, more precisely the role of PG, would play a role in church divisions.

More divisions occurred in the 1890's when the first meeting houses (church buildings) were constructed by some Amish-Mennonites as a means of dealing with burgeoning growth and overcrowded worship services in homes. Other Amish-Mennonite meetings saw this as a move toward worldly fashion and continued to meet in homes.

The next significant period in Kalona history was the 1910's. A rapid series of decisions by certain churches during this period provided the impetus for the evolution/realignment of the community into the three major Anabaptist groups there today.

The first decision was the telephone question of 1912. There were long, vociferous debates among the Amish-Mennonites as to the worldliness of the telephone and whether members should own or use them. Eventually some chose to adopt the phones, a move which placed them in closer contact with their "Englisch" neighbors.

At this same time high school education was gaining grudging approval in some corners. By the next decade many Mennonites (as they were by then known, see below) were attending public schools all the way through to the achievement of the diploma. It should not be a surprise that these students gained a toe-hold in an English-speaking world, and that English was making greater inroads into the home lives of these PG speakers.

The extent to which English was replacing PG as the language of common currency among these more liberal Mennonite groups was clearly evidenced when many churches began singing in English and then allowing for sermons to be in English. For these speakers the acceptable domains of usage for PG were increasingly being restricted to occasional use in the home, often with older relatives.

It was during this time, too, in the 20's and 30's, that certain churches made the decision to completely break with the OOA and to affiliate with the Mennonite or Conservative Mennonite conferences separate from the OOA. This resulted in the religious groups as they exist today: the OOA continuing as the traditional "plain people" and the M's and CM's creating a new identity as historically plain communities assimilating, to varying degrees, to the dominant culture.

During this time of communal and linguistic upheaval we might wonder whether or not linguistic norms—such as maintaining distinct dative forms in PG—might be in flux. If we perceive of linguistic change as graphing over time in the shape of an S-curve, that is, beginning slowly, continuing rapidly in the middle, and slowing again as it nears completion, then perhaps it is the 1920's and 1930's in Kalona which map onto the steep slope of the S-curve of change in dative usage. It is, then, no surprise that the children of

this time period, the 70-year-olds in my study, should end up with such widely varying usage of dative variants. In acquiring PG case, they were taking aim at a shifting target.

In this section I have presented a chain of events in the history of the Kalona PG community, culminating in a series of divisions over the adoption of practices which encouraged the use of English in more and more domains of life (e.g., the telephone, the automobile, high school education). These events coincide with changes in the linguistic norms of the community, i.e., changes in case usage which reflect convergence to English.

### 5.2.3 Comparison of Northumberland County, PA with Kalona

There are two ways in which we might compare Huffines's "sectarians" with our Kalona community. If we think of "sectarian" strictly in terms of historical religious affiliation traditions, then the entire Kalona community is sectarian. If we define "sectarian" instead as a separatist "traditional plain" community in which PG is being maintained, then only the OOA of Kalona would qualify as sectarian while the "historically plain" M and CM communities of Kalona would be equated with Northumberland nonsectarians (see Table 10).

**Table 10. Comparison of Northumberland Co. with Kalona subjects.**

	Northumberland Co., PA	Kalona, IA
Anabaptist Faith Tradition	non-sectarian (other)	none
	sectarian (Amish and Menno.)	OOA, CM, M
Language Maintenance	non-sectarian (lg death)	CM, M
	sectarian (lg vitality)	OOA

A quick comparison of Northumberland and Kalona data does reveal some similarities, as Table 11 (reproduced from Table 1 for ease of reference) and Table 12 show. The Kalona data include percentages as well as raw token counts since the size of the token pool differs considerably between each group.

**Table 11 Huffines (1989) translation task.**

Group	Dative	Accusative	Other	Total
Non-sectarian	83	22	0	105
N-s 1st Eng. speaker in fam.	50	43	1	94
N-s 2nd Eng. speaker in fam.	30	39	8	77
Mennonite	1	86	0	87
Amish	2	90	0	92

**Table 12. Kalona translation task.**

Group (# subjects)	Dative	Non-Dative <sup>1</sup>	Other	Total
Mennonite (15)	131 (42%)	112 (36%)	66 (21%)	309
Conservative Mennonite (17)	126 (36%)	181 (52%)	39 (11%)	346
Old Order Amish (38)	126 (16%)	586 (74%)	75 (10%)	787
<b>TOTAL</b>	<b>383 (27%)</b>	<b>879 (61%)</b>	<b>180 (13%)</b>	<b>1442</b>

<sup>1</sup> "Non-Dative" refers to either accusative or common case.

The most significant similarity is that the plain communities in both Northumberland County and Kalona are the ones who are maintaining PG and the ones who appear to be most advanced in the collapsing of the dative case into accusative (or accusative/common in the Kalona data).

Still, whether we compare the two data sets with reference to the language maintaining communities in each or to the Anabaptist faith traditions in each, there are clear differences. Kalona OOA do not pattern with Huffines' Northumberland County Amish. In Northumberland the sectarians have virtually no dative variants in their speech. In Kalona, among the OOA and every group, the number of dative variants used correlates with age. Clearly the "remarkable homogeneity" in case usage among the Northumberland sectarians is not true of Kalona as a whole or even among the traditional plain OOA of Kalona.

What is more, there appears to be no obvious reason for us to posit an earlier stage in PG for which the norms for case usage (and, of course, for all other grammatical subsystems) were shared between Kalona and Northumberland County. It seem, rather, that every community may have had significantly different norms of usage, dependent in large part upon the origins of its founders. In the case of Kalona, for example, there is evidence that a considerable number of the earliest settlers were first-generation German immigrants. The dialects spoken by these settlers may have differed considerably from that of the newly emergent Pennsylvania German. The settlement patterns of each PPG community must, therefore, be carefully examined to determine what significant input dialects may have existed to modify PG.

## 6. Conclusion

In this paper I have described variation in case marking in a plain Pennsylvania German speaking community. Dative case marking is an age-correlated phenomenon which is also sensitive to particular functions the dative can serve in PG. I have offered a sociohistorical account for the spread of accusative and common case marking into functions formerly reserved for dative case. This account demonstrates that the traditional plain OOA and historically plain CM and M religious communities must be considered a single speech community with respect to this development. Finally, I offer clear quantitative evidence that this linguistic change is not proceeding at the same rate in at least one other PPG community (Northumberland County, PA).

These findings corroborate the observation by Burridge 1992 that there is significant variation between PPG communities. These findings lead us to question the notion of the homogeneity of PG at earlier stages—even homogeneity across various OOA communities.

In addition, there is the continuing evolution of PPG dialects as they exist in noncontiguous regions across North America. These sprachinsel dialects offer us the opportunity to observe simultaneously processes of divergence and parallel processes of convergence to the dominant language of the matrix culture (English).

Future research must be conducted across a much broader bundle of phonological and morphological features in several regional plain Pennsylvania German dialects.

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