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# **Working Papers of the Linguistics Circle of** the University of Victoria

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The Linguistics Circle of Victoria is pleased to present the first issue of an on-going series of *Working Papers* on current linguistic research in the community. The Circle, under the sponsorship of the Department of Linguistics, includes among its membership interested scholars from other departments at the University and other institutions in Victoria. The *Working Papers of the Linguistics Circle* will be published semi-annually, normally one issue in fall and one in spring. The next issue, which together with the present one will constitute the 1981 Volume, will be published in June. Publication of this first volume has been made possible by equal grants from the Graduate Student Society and the office of the Dean of Graduate Studies at the University of Victoria.

The reader is advised that the articles appearing in the *Working Papers* vary in the degree to which they represent the authors' final views on the matters under discussion. With this in mind, comments and suggestions on ideas set forth in these papers would be welcomed.

While this first volume will be distributed free, it is our intention in future years to charge a modest subscription rate to try to recover some of the costs of publication. We would be pleased to send *Working Papers* on an exchange basis at no cost to any institution which publishes similar volumes. We wish to acknowledge the work of Ms. Bev Wagner in typing these papers and to thank her especially for her patience.

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# Dedication

The first volume of *Working Papers of the Linguistics Circle* of Victoria is dedicated to Professor M. H. Scargill, B.A., Ph.D., F.R.S.C. Dr. Scargill, the founder of the Department of Linguistics at the University of Victoria and for many years Head of the Department, has decided to retire from active service at the University at the end of this academic year.

Dr. Scargill's retirement marks the end of an era in the development of the Department. Under his leadership the Department has grown from a complement of two professors in 1965 to its present strength of nine full-time faculty members. Directly affecting the Department of Linguistics, but extending to the whole University, was Dr. Scargill's role in the establishment of the Faculty of Graduate Studies. He was Chairman of the committee charged with the responsibility of designing the Faculty, and later became its first Dean.

As a scholar and teacher, Dr. Scargill's influence will be felt for many years to come. His contributions to Canadian English lexicography and lexicology will remain as permanent monuments in that field. Through his teaching Dr. Scargill has significantly touched the lives of many. A number of his former students are now well-established scholars in linguistics. His colleagues and students express their gratitude to Dr. Scargill and wish him well for the future. These *Working Papers* are a modest indication that Dr. Scargill's pioneering work will continue to bear fruit.

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An Assimilation Process in Altamurano and Other Apulian Dialects: an Argument for Labio-velars.

> Terry B. Cox University of Victoria

### 1.0 INTRODUCTION AND PLAN

In a cursory examination of data gathered during a recent field trip to the central and southern regions of Apulia in southeast Italy, I was struck by a seeming complementarity of contexts for two superficially distinct phonological processes: a ue diphthong<sup>1</sup> reduces to e after certain consonants; an insertion of u takes place after certain other consonants. On closer scrutiny, it was ascertained that the complementarity of contexts for the processes in the dialects taken as a whole was more illusory than real: some dialects had ue reduction and a restricted type of u insertion; others had ue reduction and no u insertion; still others had sporadic instances of u insertion and no ue reduction. In Altamurano, however, the contexts for the two processes were fully complementary.

In this paper I shall attempt to demonstrate that these two processes can best be understood when seen as two parts of a single diachronic process of consonant labialization, subject to a single surface phonetic condition that permitted *u* insertion after certain consonants and not only blocked it after certain other consonants, but also eliminated the glide of *ue* after these same consonants. I shall also show that Altamurano alone holds the key to this solution as it is the only dialect where both processes reached

<sup>&</sup>lt;sup>1</sup> For expositional purposes I use ue as a general transcription for a diphthong which in some dialects is realised phonetically as [we]and in others has the allophones [we] and [we], as seen below.

their maximal extension.

Earlier studies will first be reviewed in order to pinpoint the areal and temporal distribution of the two phenomena, held previously to be unconnected. Prose accounts enumerating centres or areas affected will be checked against the data contained in the Italo-Swiss (linguistic) atlas (henceforth AIS), Melillo's Apulian phonetic atlas (AFP), using Bari province data only, and his new Apulian phonetic atlas (NAFP). Barese and Leccese diphthong reduction will next be examined, followed by a presentation of the data from Altamurano. Finally, after accounting for some pseudoexceptions in Barese, evidence will be submitted from both Barese and neighbouring Molese to corroborate my labialized consonant hypothesis - evidence which necessitates a slight revision of the initial formulation of the labialization rules, 2.0 THE DIPHTHONG ue

In the South Italian dialects that have the  $\mu e$  diphthong, it is the result of the anticipatory assimilation process known as umlaut. In a proto-stage of the dialect group(s), a stressed \*J was diphthongised when the final syllable of the word contained \*u or \*i. The change thus affected primarily masculine singular nouns and adjectives (u-forms) and plural nouns and adjectives together with 2nd singular present tense verbal forms (i-forms), as in Altamurano:

(1) \*bonu > \*buonu > buenə 'good msg.' (cf. bonə 'good fsg.')
(2) \*koši > \*kuoši > kuešə 'you cook sg.' (cf. košə I(he)cook(s))

Gerhard Rohlfs notes, in his monumental *Grammatica storica della lingua italiana e dei suoi dialetti* (1966:153-4), that this diphthong appeared in three separate centres in old texts in South Italy: in 14th and 15th century writings of Rome; in those from Naples

of the 17th adigue; and in a 15th century document from the Today, he area, the old 3 \$ 0 tree to fouth Chattems () ien onti sliespread**o** hnew Se U D 3 we are ditte CON DALES N al on world aso: RINDI N JC () Jr de per Ne 21; Par angell A PINONS When days MAPP. ara combined, as shown in the 1alta out to be two separate -asitos ignor betweet 11.900 **0**1788 taining 16 centres; and muse 19 a 52£ 2 NAI AIS Paci 3 3 n s.r. He han bested 1 shoiwon Lectree by stating hat (9) (=) dialatis ison do/ B consecution of The evilonments for the H. Ded (eté and after palatal or dental 9 2 in Deress, Valence (1975-17) 3 ଭ AL 701 105025 Plasnoenco OF F the may, whow 109.9° within the t ALC MALLERY lettik-0 1 0 inab basaloa ums d I A 🛛 a 😽 10H he vouthwest corner. 34 (2) V 2115106 Under A elas to obr Hanna ella 🗋 v [ 141 xd-(1966-19-20), that wide former Central and South eolaslb. "@

of the 17th century; and in a 15th century document from the Salentine peninsula (the heel of Italy). Today, he says, the old Roman diphthong is conserved only in two centres in South Latium; it is absent in modern Napoletano; it is quite widespread, however, in Apulia in a zone stretching southeastwards from Bari to Lecce, a distance of 150 kilometres. As atlas sources show no points in South Latium having  $\mu e$ , attention will be focussed on the Apulian zone. When data from the AIS, AFP, NAFP and Parlangeli (1960:48-9) are combined, as shown in the map on page 3, Rohlfs' zone turns out to be two separate zones: a Bari zone, designated zone A, containing 16 centres; and zone B, a Lecce zone, numbering 28 centres.

2.1 The Change ue > e

Rohlfs (1966:154) acknowledges this change for Barese and Leccese by stating that 'ue tends to reduce to e when near certain consonants...' The environments for the change in Leccese, says Parlangeli (1960:48-9), are: in absolute word-initial position, and after palatal or dental consonants. Describing the process in Barese, Valente (1975:17) indicates it occurs after all consonants except k, p, b, f, m. Atlas sources, summarized in the map, show, of course, that the centres where ue > e fall within the two ue zones, and are marked with vertical lines. In the Lecce zone they form a compact group comprising Lecce itself and eight centres that surround this provincial capital. The centres affected in the Bari zone do not cluster in one group. There is a Bari enclave comprising Bari and three nearby centres, one isolated centre, and two centres, one of which is Altamura, in the southwest corner of the zone.

3.0 THE INSERTION OF u

Under the name of velarization, Rohlfs mentions briefly (1966:419-20) that wide zones of Central and South Italy display

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u insertion before the stressed vowel under the influence of a preceding u. These zones include, he says, the Abruzzi, South Latium, North Campania, North Apulia, Basilicata and Central Sicily. I was able to verify most of these from my one source of primary data outside Apulia, the AIS, but the South Latium and Central Abruzzi areas he mentioned slipped through its mesh, which at times has rather large holes. Other than specifying that in Sicily u insertion occurs only before a and at Verbicaro in Calabria it takes place before any vowel, Rohlfs does not indicate whether all or only some consonants permit this change. No mention is made of it occurring in old texts, so if it had been a speech habit when they were written it was probably subphonemic.

Valente (1975:36), reporting on the dialects of North and Central Apulia, states that u (or o) from a pretonic syllable is often 'propagated' after a following k in Barese but makes no mention of it occurring outside that dialect. In a study involving three Basilicata speech communities, Leonard (1969:450) refers to what he calls a morphological innovation in two of the dialects, Matera (just south of Altamura; see map) being one of them. Masculine singular nouns and lst conjugation infinitives show an intrusive labial element after k before a. Finally Merlo (1925:94) shows u propagation in two lst conjugation infinitives in a dialect neighbouring Barese.

Verification of these reports through primary sources was undertaken for the central and south Apulian areas, given the interest in determining, in this study, the exact relationship between u insertion and ue reduction. The AIS shows u insertion only in 1st conjugation infinitives in just two centres, Bari and

Ruvo, out of a total of eleven surveyed. The AFP gives a total of 44 out of 50 points in Bari province having u insertion with 1st conjugation infinitives, and in six out of 50 centres it occurs in masculine nouns after k and before a, if the pattern of *il cane* (60-62) may be taken as general.<sup>2</sup> Three of these centres are in the ue zone A, one being Altamurano. In the NAFP there is no trace of the phenomenon, either for the Bari or for the Lecce ue zones. Thus, to summarize, u insertion appears to have occurred throughout most of Bari province, but only after k and before a. In all lst conjugation infinitives having u insertion, the change can be linked to an earlier u in a preceding syllable. In just four centres does, or did, it occur in nouns (cf. statement above on Matera). It therefore seems impossible, from the data examined or from the accounts reviewed, to link directly the processes of ue reduction and u insertion. I shall next delineate the phonetic environments for ue reduction.

### 4.0 BARESE AND LECCESE

The following examples illustrate the process in Barese and Leccese, and show that it is identical in both:

	_		Barese	Leccese	Italian	
(3) t	JF*၁[ <sup>3</sup> >	ue	buenə	byεnu	buono	'good msg.'
(4)			kyeČə	kyɛči	cuoci	'you cook sg.'
(5) l	JF*3[ >	е	senə	senu	suono	'sound'
(6)			<b>š</b> ek <b>ə</b>	šεku	giuoco	'game'

<sup>2</sup> Colasuonno (1976:73) indicates that it occurred in Old Grumese, too (point 17 on map).

<sup>3</sup> UF is the underlying or base form, which is arrived at by examining the non-umlauted alternation or by reconstruction. The sign [ after a vowel indicates a free syllable, ] a checked one.

(7)	UF*ɔ] > ue	kyɛddə	kueddu	collo	'neck'
(8)		muɛ¢¢əkə⁴	mųε¢¢iku	morso	'bite'
(9)	UF <b>*</b> 3] > e	nestə	neššu	nostro	'our msg.'
(10)		testə	testi	tost i	'hard mpl.'

It can be observed that the diphthong *ue* appears after labials and velars, elsewhere it is reduced to *e*. All other centres in the two *ue* zones, apart from those in the shaded areas on the map, preserve *ue* in all environments. It is probable that in the past more centres had the diphthong reducing rule and that its sphere of influence has receded, but it is hazardous to conclude that such was the chain of events unless evidence from the speech of old people were to bear it out (cf. remarks below on Molese). 5.0 THE SITUATION IN ALTAMURANO

5.1 Altamurano ue>e

In Altamurano, as may be expected, the conditions for the change parallel those for Barese and Leccese:

	Altamurano	Italian	
(11)	buenə	buono	'good msg.'
(12)	kuešə	cuoci	'you cook sg.'
(13)	senə	suono	'sound'
(14)	Šek <del>a</del>	giuoco	'game'
(15)	kueddə	collo	'neck'
(16)	mue¢¢əkə	morso	'bite'
(17)	ðermə	dormi	'you sleep sg.'
(18)	testə	tosti	'hard mpl.'

This diphthong reduction also takes place in absolute word-initial position, but I consider this environment the same as in (11), (15)

4 /¢/ represents [ts].

or (16), that is, after a dental, as such words usually appear with the elided form of the definite or indefinite articles, with which they are in close juncture:

(19)	l∽evə	l'uovo	'the egg'
(20)	n-ertə	un orto	'a garden'
(21)	l-eggjə	l'olio	'the (olive) oil'

The same situation holds true for Barese and Leccese, although it is masked somewhat for the former in the case of the definite article, which is u. At this point it may be said that Altamurano is no different from the other dialects discussed so far.

# 5.2 Altamurano *u* Insertion

Where Altamurano innovates is in the form of its u insertion rule. As implied in section 0 above, it does not have the restricted application found in the other dialects examined. It is pervasive, though it involves some restrictions which are predictable. Cirrottola (1977:32) states that 'harmonization' occurs in nouns which are preceded by the articles or the (msg.) demonstrative adjectives  $kuss \partial$  'this' and  $kudd \partial$  'that'. They harmonize by adding a 'weakened' u before the vowel of the first syllable, or of the second syllable if the first vowel is  $\partial$ :

- (22) benə 'well'; u buenə 'good (vs. evil)'
- (23) u/nu/kussə/kuddə furkuaunə
  - 'the/a/this/that pitchfork'

He notes further that harmonization does not take place after dentals or palatals.

Cirrottola's rule implies that this harmonization is a restricted morphological process with certain phonological constraints. His characterization of the rule proves, however,

to be incomplete, as I discovered when I made a thorough examination of his data. The following examples are offered as evidence for my revision of his rule:

(24)	u pyalumyìddə	'the small pigeon'
(25)	la skupyéttə	'the brush'
(26)	kumuannè	'to command'
(27)	n-u fuačitə	'will you do it for me?'
(28)	u mənùnnə	'the child'
(29)	u kulə kakètə	'the shitty behind'
(30)	u mueggjə wutiddə	'the best calf'
(31)	u surəky <b>i</b> kkjə	'the small mouse'
(32)	u selə	'the salt'
(33)	u čendrə	'the centre'

The restriction of the domain of the rule to nouns is shown to be inaccurate by (26), (27) and (30); the restriction to the deictics as triggers for its application is counter-evidenced by (25) to (27); (24) to (26) and (31) show that it is triggered also by a preceding u within the same word. Thus it is an iterative process that applies syllabically. It cannot apply before a schwa as shown in (28), and (31) indicates that it can jump the schwa-syllable and apply to the next eligible syllable. Examples (32) and (33) are included to show that it is blocked by dentals and palatals. I have attempted to capture formally the revised generalization in the following rule:

(34)  $\begin{bmatrix} C \\ +peripheral \end{bmatrix}^5 \rightarrow [+labialized]/u((CCa)#)(C) \__V$ Condition: V ≠ u or a

<sup>5</sup> Peripheral consonants in Altamurano are labials and velars. It

The data available to me indicate that there are at least two syntactically definable restrictions on the domain of (34). Example (29) above was included to illustrate one of these. It operates on NPs as far as the end of the head noun, and in VPs from object pronouns as far as the main verb, as in (27).

To account for the interaction of labialization and the *ue* diphthong I posit the following deletion rule:

These rules are ordered as they appear serially, for by deleting the labial glide after all consonants, the correct surface forms are obtained. The formalism hides a change that may rather have involved the coalescence of the glide with the labialized consonants and its deletion after all others. 6.0 PSEUDO-EXCEPTIONS IN BARESE

There are forms in Barese which would appear to violate rule (34), as non-peripherals should not be labialized:

(36) sfuà 'to flow: give vent to: a ruaññe 'the chamber pot'

(37) tə¢¢uà 'to knock (door); kalduà 'to keep warm'

The apparent labial glides are not, however, the result of labialization; in fact,  $t\partial\phi\phi u\hat{a}$  of (37) is the only item that is a potential candidate for the assimilation, since the unstressed schwa is a reduction of a former u, as evidenced by neighbouring dialects. There is an orthographic convention in Barese which

is interesting to note that the single Jakobson-Fant-Halle feature grave is sufficient to label this natural class, but that the Chomsky-Halle system requires several features to do so.

does not distinguish between the glide u and the vowel u. I contend that the  $u\alpha$  of the above forms is bisyllabic. In some other dialects the same lexical item has uga and the g has lenited to  $\emptyset$  in Barese<sup>6</sup> (a change common to many dialects of South Italy, cf. Cox 1977). The actual pronunciation shows that the hiatus so produced between the two full vowels is filled with a transitional u, so that it is more accurate to write sfuud or staud. In the forms in (37) I derive the ua from ula, which is still seen in the cognate forms in many Bari province dialects. The l first velarized and then vocalized, so again the orthography should be revised to  $t \partial d d u u \hat{a}$  or  $t \partial d d \partial u \hat{a}$ , and so on. Whilst the lenition of g is an older change and appears more general in Apulian dialects, that of l, although appearing at other isolated spots in South Italy, seems to be present in Apulia only in Barese and a few surrounding centres. It is thus likely that Bari has been the centre of diffusion for this more recent change, destined as it is to further proliferate the labial glide.

6. Corroboration from Barese

It was shown in (1) and (2) that presence or absence of umlaut produced the alternation  $o \sim ue$  in a given verbal or adjectival paradigm having an underlying stressed \*J. This pattern extends to nominal paradigms too:

(38) monəkə 'monk' ; muɛnəčə 'monks'

The expected alternation is not to be found in the following paradigms, however:

(39) soffra 'he suffers'; siaffra 'you suffer sg.'

<sup>6</sup> Scorcia (1972:117) notes that in a 15th century Barese text the form *rugagne* is recorded.

(40)	dərmə	'he sleeps	;	diərmə	'you sleep sg.'
(41)	pyerkə	'pig'	;	puiərčə	'pigs'
(42)	senə	'sound'	;	siənə	'sounds'

The verbal paradigms in (39) and (40) have the expected segment on the left side of the alternation but differ on the right:  $\Im \sim i\partial_{\sigma}$ , but it is by looking at the nominal paradigms in (41) and (42) and the alternation  $\mu \varepsilon \sim \mu i\partial$  that the clue to the path of evolution may be found. Stripped of its labial glide in (42), the alternation parallels that found in nominals having an underlying stressed \* $\mathcal{E}$ :

(43) vermə 'worm', cf. viərmə 'worms'

Thus the base vowel in (41) and (42) has been interpreted by the speakers as  $\boldsymbol{\varepsilon}$  which becomes  $i\partial$  when umlauted. It is irrelevant that the singulars in (41) and (42) are already umlauted and the stressed vowel in the plural should therefore be the same as that of the singular; morphological pull has overcome phonological pull because of the need to mark number by the prevailing internal inflection pattern. The verbs in (39) and (40) must have undergone a similar re-interpretation of the vowel of the 2nd singular forms. For such an interpretation to have taken place, I suggest that the labial glide must have separated from the  $\mathcal{E}$  in some sense for the  $\boldsymbol{\xi}$  to have followed the evolution of primary  $\boldsymbol{\xi}$ . This separation could most easily have occurred if u had ceased to be a discrete segment and instead become secondary articulation on the preceding consonant, that is, labialization. I would rewrite (41) as  $p^{w} \mathbf{\epsilon} \mathbf{r} k \partial$  and  $p^{w} i \partial \mathbf{r} \dot{\epsilon} \partial$ .

According to Valente (1975:17), this rule affected only part of the lexicon before dying out, and there is a modern tendency that is destroying even the input forms for the rule such that  $buen \partial$ 

is being pronounced  $bu\partial n\partial$ , and  $k\mu ek\partial$  'cook' as  $ku\partial k\partial$ . 8.0 CORROBORATION FROM MOLESE

In Molese<sup>7</sup> I have discovered 23 relic forms containing  $\mu \epsilon$ (or  $\epsilon$ ), indicating that the  $\mu e$  zone has receded in recent times. Less that half of the forms are known to speakers under 25 years of age and the rest are known to those usually over 35 and more often than not as words used by their grandparents. In the dialect of today umlauted \*2 appears as  $\mu$ :

- (44) monaka 'monk'; munača 'monks', cf.
- (45) kueddə 'neck', and
- (46) u ESSƏ 'the bone', but n-ussə 'a bone'

Apart from the vowel initial forms such as (46), the remaining forms all appear with k before u, as in (45). No forms remain (or existed?) where  $u\varepsilon$  has become  $\varepsilon$  before a non-peripheral. Like *n*-usso in (46) they all have u. Some forms which elderly people pronounce with  $u\varepsilon$  are pronounced with u by the younger generation of adults.

The forms that are especially interesting and relevant for my labialization hypothesis are given in (47) to (50). They have something in common with the plural form in (41) above:

- (47) u əvə 'the egg'; I-ovə 'the eggs'; cf. Barese u evə
- (48) prekueke 'type of peach'
- (49) kuairə 'leather'; cf. Barese kutrə
- (50) čəkuairə 'chicory, endive'; cf. Barese čəkuarə

Interdialectal comparison shows that Molese  $\bar{\partial}$  normally appears

' The dialect of Mola di Bari, 22 kilometres southeast of Bari (see map). Data from the writer's fieldnotes. where other dialects have  $\boldsymbol{\varepsilon}$  and only appears in open syllables. Thus the path of evolution in (48) must have been something like:

(51)  $*k + *_{2}$  >  $k + u\epsilon$  >  $k^{W} + \epsilon$  >  $k^{W} + \overline{a}$ In the singular form of (47), the glide of the diphthong appears to have been absorbed by the definite article before  $\epsilon > \overline{a}$ .

The ai diphthong in (49) and (50) is common to other Central Apulian dialects, excluding Barese, but dialects outside this area have e:

Molese Ruttésə<sup>8</sup> Napolet. Italian (52) majsə mesə mesə mese 'month'

The reason for the closed e here, instead of the open  $\epsilon$  as in (51), is not clear to me at present, but does not detract from the value of these forms in my argumentation. I take it that the evolution was as follows:

(53) \*k + \*2 >  $k + u\varepsilon$  >  $k + u\varepsilon$  >  $k^{W} + e$  >  $k^{W} + e$ 

As *ue* underlies the stressed vowels in these words, the subsequent restructuring and further evolution lend additional support to the labialization hypothesis outlined above.

9.0 A RE-APPRAISAL

In the foregoing account I originally attempted to show a link between labialization of the Altamura type, that is, lag assimilation, and the delabialization, or reduction, of the diphthong *ue*. It has become clear, however, that an interaction of these two processes cannot be demonstrated for Barese, where labialization

<sup>&</sup>lt;sup>o</sup> The dialect of Grottaminarda, east of Naples in Avellino province; see Cox 1977.

of the type found in Altamurano is present in a very restricted form. It is especially not the case in Leccese, where it is, in fact, completely absent. How then can the Barese and Leccese diphthong reductions be accounted for? The answer seems to lie in examples (41), (42) and (47) to (50), and in the discussion of them. What seems to have occurred is labialization by anticipatory assimilation, which is blocked, as in Altamurano labialization, by non-peripherals, which instead lose the glide. This generalization may be formalized as:

(54)  $\begin{bmatrix} C \\ + \text{ peripheral} \end{bmatrix} \rightarrow [ + \text{ labialized } ] / \___ u$ 

Rule (54) and rule (35), which ordered after it, will then handle the coalescence of the glide with the labialized consonants and its deletion elsewhere. Furthermore, as it has been shown that the diphthongization of \*0 to ue was most certainly a much earlier change than Altamurano type labialization, rules (54) and (55) should be ordered before (34). Indeed, it seems reasonable to assume that the Barese type labialization (of rule (54)) paved the way, so to speak, for the Altamurano type (rule (34)) by establishing the phonetic parameters for its application. The discussion in section 2 implies that a more restricted form of rule (34), something like (56), was operative in Central Apulia, which later generalized to the environments of (34) in Altamurano:

(56) 
$$k \rightarrow [ + labialized ] / u \left\{ \frac{a}{\#} \right\}$$

10.0 SUMMARY

The labialization hypothesis is, of course, plausible only to the degree that labiovelars were compatible with the phonological systems of the dialects under consideration. I maintain that at

a certain period in the past conditions were most favourable for the integration of labio-velars into the systems of the Apulian dialects in the ue zones on the map, and probably many more as well. Historically, Latin is posited as having inherited labio-velars from Indo-European, and writings in early Romance show that  $k^{\omega}$  or  $k\mu$  alone survived, and only before back vowels, with  $k\mu a$  having a high functional load. When the ue evolved in the South Italian region, it favoured the spread of this labialization, at least phonetically, to other consonants and before a front vowel. The lag assimilation typified by (56) acted to spread further the incidence of these labialized segments. I am not proposing that the phonemic inventories of present-day Apulian dialects include labio-velars, but that present conditions strongly suggest their inclusion in a diachronic account of the dialects of the region.

In accounting in detail for only three out of fifty dialects in Bari province, I can make no claim that the three are representative enough to preclude possible future refinements to my analysis. My conclusions are therefore necessarily somewhat tentative, and much remains to be done. I hope to have demonstrated in this study, however, that u insertion and diphthong reduction were indeed the results of a general, albeit incomplete, labialization process, and further, that the technique of interdialectal comparison, essential to linguistic geography, still may provide valuable insights concerning the paths language change has followed.

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Passive Use in Modern Sinhala Hemamali Gunasinghe University of Victoria

Considered as part of performance, language use has generally been regarded as a factor outside the domain of theoretical linguistics (within the Chomskyan paradigm), whose goal is the description of linguistic competence. This paper looks at the Passive Voice and its use in modern Sinhala and presents some evidence from passive use that may be of heuristic significance to linguistic theory. Two assumptions, by no means novel, underlie the observations made in this paper. One is that cross-linguistic evidence has a bearing on theoretical issues. The other is that evidence from linguistic performance contributes to the formulation of linguistic descriptions of greater adequacy.

Modern Sinhala<sup>1</sup> is clearly diglossic, revealing considerable syntactic, morphophonemic, and lexical distinctions between the colloquial and literary levels.

At the literary level, the language preserves, though not intact, the older, more elaborate, syntactic and phonological systems. Contrastively, colloquial Sinhala reflects the drift of the language, generally towards simplification, in its current syntax and phonology.

One significant syntactic distinction between the two levels is the absence of the Passive construction and, consequently, the structural relationship between Active and Passive Voice at the

<sup>1</sup> Sinhala is an Indic language spoken in the Republic of Sri Lanka.

colloquial level.

The literary Passive can be adequately described in transformational terms. Syntactically and semantically there is an intuitively clear relationship between these and their Active counterparts; e.g.  $^2$ 

(1)	a.	sebəlu	saturan	wænðsu:hð
		soldiers- PL+Nom	enemy- PL-Acc	destroy-pt- 3p-p1-Active

'The soldiers destroyed the enemy'

Ъ.	sebəl-un	wisin	saturo	wanəsənu	læbu:hə
	soldiers- Pl-Acc	by	enemy-PL	destroy-Pcpl	get-pt- 3p-p1
				Passive	2

'The enemy was destroyed by the soldiers'

These structures can be generated and related to each other through a transformational rule such as (2).

ົ		
2	Pl = Plural	Nom = Nominative
	Acc = Accusative	Pt = Past tense
	3p = Third person	Pcpl = Participle
	tns = Tense	p = Person
	n = Number	g = Gender
	loc = Locative	pres = Present tense
	sg = Singular	Indef = Indefinite
	Int = Interrogative	voc = Vocative
	ger = Gerund	Dat = Dative
	Msc = Masculine	Ins = Instrumental

<sup>3</sup> Since many morphemes are portmanteau in native and numerous morphophonemic changes underlie the overt forms no attempt has been made to indicate morpheme boundaries.

(2) SD 
$$\begin{bmatrix} NP_{1Nom} \end{bmatrix} \begin{bmatrix} NP_{2Acc} \end{bmatrix} \begin{bmatrix} V_{Active} \end{bmatrix} 1 \end{bmatrix} \implies$$
  
SC  $\begin{bmatrix} NP \ wisin \\ 1Acc \end{bmatrix} \begin{bmatrix} NP_{2Nom} \end{bmatrix} \begin{bmatrix} V_{Passive} \end{bmatrix} 1 \end{bmatrix}$ 

This transformation applied to a phrase marker that is isomorphic with those that underlie Active structures produces a systematic change of voice. It alters the deep structure grammatical relations that existed between subject, verb and other NPs. The change of voice is morphologically realized in the passivized verbal construction (Pcpl + get + tns-p-n-g), and the altered case relations reflect the switch in grammatical relations. The Agentive postposition *wisin* 'by' marks Agent.

Although no NP reordering is involved in the Passive Transformation in Sinhala, this rule is comparable with (3) below, which is the standard TG Passive Transformation.

(3) SD 
$$\begin{bmatrix} NP_1 \end{bmatrix} \begin{bmatrix} V \\ NP_2 \end{bmatrix} \begin{bmatrix} NP_2 \end{bmatrix} \end{bmatrix}$$
   
SC  $\begin{bmatrix} NP_2 \end{bmatrix} \begin{bmatrix} V+be+en \end{bmatrix} \begin{bmatrix} by NP_1 \end{bmatrix}$    
S VP VP S

The basic similarity of the two transformation rules, (2) and (3), for Sinhala and English respectively, appears to support the cross-linguistic applicability of a passive transformation.

Yet the Passive rule (2), and the structures it generates, such as (1b), are totally absent at the colloquial level in Sinhala.

At this level, Active structures alone suffice in the communicative task. Allowing for surface structure distinctions, the structure (1a) would be realized on colloquial Sinhala as the

### structure in (4) below:

(4)	sebəlu	haturan-(wə) <sup>4</sup>	wænəsuwa
	soldiers- pl- Nom	enemy-p1-Acc	destroy-pt- Active

'The soldiers destroyed the enemy.'

Word order is relatively free in both colloquial and literary Sinhala and passivizing is not necessary for reasons of focus, emphasis or topicalization as has been claimed for English Passive use.

It must be noted that even in literary use the Passive structures are extremely low in frequency of occurrence. Passive use becomes obligatory at the literary level only in the pragmatic context when the agent is unknown or obvious from the context. In such a context, literary Sinhala uses a truncated Passive structure.

(5)	dæn	kadukərəye:	boho:	te:	wa	wən	u	læb	e:		
	now	hillside- loc	much	tea- grow- Nom pcpl			get-pres 3p-sg				
				Р	a	s	s	i	v	е	

'Nowadays much tea is grown in the hills.'

In such contexts colloquial Sinhala uses a truncated Active form:

(6) dæn kadukəre: huğak te: wawənəwa now hillside- much- tea- grow-pres loc indef Nom

Active

'Nowadays much tea is grown in the hills.'

4 /wə/ = Accusative (Patient) marker in colloquial Sinhala.

In the structures (5) and (6) above, though superficially deleted, an Agent—indefinite, unknown, or understood—is perceived as being actively involved in the action specified by the verb. This accounts for the synonymy between these two structures despite the fact that syntactically they are different.

Truncated Actives and Passives are synonymous and form paraphrase sets. However, they are mutually exclusive at the two levels. Surface structure constraints, such as verbal concord requiring overt subjects, rule out truncated Actives at the literary level. This constraint is absent at the colloquial level, where the availability of the truncated Active structures makes the equivalent Passives redundant.

It is interesting to note that even in English, the Passive structures have been regarded as 'optional stylistic variants or embellishments that are a linguistic luxury' (Green 1966). Sledd (1959) regards them as 'devices that mark one as educated' and Evans and Evans (1957) consider the choice of the Passive voice to be a 'stylistic determination to be made for the sake of effective prose.'

These and many similar statements are based on the view that the Passive structures are redundant in use, and that they are more complex stylistic variants of Active structures in sophisticated use. Evidence from Passive use in Sinhala clearly supports this view.

The tenability of such a view is further enhanced by the results of an investigation by Goldman-Eisler (1968) into the spontaneous use of the Passive voice. Their study of the occurrence of the Passive voice in the speech of a cross-section of English speakers, from academics to schizophrenics, has shown that the Passive voice occurred in spontaneous speech only about 7 - 10% of

the time (compared to 80% for the Active voice). They also found that the use of the Passive voice decreased relative to diminishing intellectual and education levels. The Passive voice is much higher in frequency of occurrence in writing and seems to increase in frequency relative to the degree of formality of the text (Prideaux, 1980).

Viewed as optional, more complex variants of the Active structures, the low frequency of occurrence (or the total absence, as in colloquial Sinhala) of Passive structures is not surprising. The full and truncated Passive structures discussed so far clearly parallel Active structures in being Agentive, i.e. they entail an Agent, understood or unknown or overtly expressed, DOING the action specified by the verb.

Note, however, the structures in (7) a, b, and c, below. Despite the surface similarity, only (7al) is unambiguously agentive and relatable to a structure like (7a2). (7bl) is ambiguous as to Agency---were they trapped accidently or by someone as in (7b2)? The structure (7cl) implies no agency and has no relatable Active counterpart.

- (7) a 1. Ted was mugged in the street.
  - a 2. Somebody mugged Ted in the street.
  - b 1. They were trapped in the quagmire.
  - b 2. Somebody trapped them in the quagmire.
  - c 1. I was caught in the turmoil.
  - c 2. ?Somebody caught me in the turmoil.

It seems that a distinction between Agentive Passives, full and truncated, which entail a relationship with Active structures, and Agentless Impersonal Passives, which entail no such relationship, is relevant to the question of Passive use. Speakers appear to make the distinction systematically, for a significant disclosure of the Goldman-Eisler report was the fact that the Passive, when it did occur in spontaneous speech, was generally the Agentless Impersonal subset. It is also interesting that many languages of the world have only Impersonal Passives.

The indications are that it is the Agentive Passives which are truly redundant in spontaneous use. This is definitely the case in Sinhala. Agentless Impersonal structures relating to events are obligatory in the communicative task.

The semantic distinction between *DOING* (ACTION) and *HAPPENING* (PROCESS)<sup>5</sup> can be used as a valid criterion for distinguishing Agentive and Agentless Passives. This distinction is not overtly manifest in English, where the passivized verbal form and the 'by-Agentive' phrase are both ambiguous as to Agency. Choices made in language use, however, indicate the validity of this criterion.

In Sinhala the distinction between ACTIONS and PROCESS is pervasive and is morphologically marked by stem alternation in the verbs, e.g.

(8)	ACTION	PROCESS	1
	kapa	kæpe	'cut'
	mara	mære	'kill'
	Wawa	Wæwe	'grow'
	ari	ære	'open'
	adi	æde	'pu11'

ACTION verbs entail the semantic notion of DO while PROCESS verbs entail that of HAPPEN. In certain verbal derivatives, which are functionally similar to ACTION and PROCESS verbs, the verb stems

<sup>&</sup>lt;sup>5</sup> The terms ACTION and PROCESS are borrowed from Chafe (1970). I have, however, disregarded, in the limited use in this paper, the distinction in terminology that Chafe makes between ACTION— Intransitive Active, and ACTION PROCESS— Transitive Active classes.

/kərə/ 'do' and /we/ 'happen' are overtly evident as the second member of the compound.

(9)	ACTION	PROCESS	
	wina:səkərə	wina:səwe	'destroy'
	kadəkərə	kadəwə	'break'
	poliŠkərə	poliŠwe	'polish'
	wiwurtəkərə	wiwurtəwe	'open'

Verb stems that form paraphrase sets with the compound derivatives are also not uncommom in the language. Note the synonymy between the examples in (9) and (10).

(10)	ACTION	PROCESS	
	wanəsa	wænəse	'destroy'
	kada	kæde	'break'
	madi	mæde	'polish'
	ari	ære	'open'

The psychological reality of the DO/HAPPEN dichotomy and the pivotal nature of the (two classes of) verbs exemplifying this dichotomy is evident in the following dialogue:

(11) mokə-də laməy-o me: gaha-ø kæpuwe what-Int child-voc this tree-Acc cut-pt-ger 'Why did you cut this tree, child?' ane: no:na kæpuwa nemei, kæpuna lady-voc cut-ACTION not cut-PROCESS oh 'Oh lady, it was not cut, it got cut.' Literally, 'lady, cutting was not done, cutting happened'

In Sinhala the verb is selectionally dominant and is central to syntax. The syntactic and semantic structures specified by the two classes of verbs, ACTION and PROCESS, may be schematized as in

NPs in non-nuclear relationships with the verb, such as those of location, instrument, etc., are ignored in the highly simplified schema in (11a) and (b).

It seems relevant to point out that the structure (11a) specified by ACTION verbs is what undergoes the Passive Transformation (2).

- (12)a. kolla gasə kapuwe:yə
  boy-NP<sub>1</sub> tree-NP<sub>2</sub> cut-pt-3p-sg-Msc-ACTION
  'The boy cut (down) the tree.'
  - b. kolla wisin gasə kapənə ladi boy-NP<sub>1</sub> by tree-NP<sub>2</sub>  $\frac{\text{cut-pcp1 get-pres-3p-sg}}{P A S S I V E}$

'The tree was cut (down) by the boy.'

The Impersonal Agentless structures specified by PROCESS verbs, (11b), do not passivize since they do not meet the structural description.

### (13) a. gaha kæpuna

tree-NP<sub>1</sub> cut-pt-PROCESS

'The tree was cut (down)'

There are, however, structures like (13b) below which appear to be passivized.

(13) b. kolla atin gaha kæpuna
 boy hand-Ins tree-NP<sub>1</sub> cut-pt-PROCESS

'The tree was cut (down) by the boy (accidently).'

But this is not the case. No change of grammatical relations has taken place in this structure relative to the PROCESS structure (13a). The NP *atin* phrase, when used, indicates an animate NP causally but unwittingly involved in an event or happening. In this sense it is not Agentive but instrumental. The postposition *atin* clearly indicates lack of volition or intent on the part of the the participant whereas the postposition *wisin* in the literary Passive structures indicate volitive active participation in an ACTION. The literary Passive is merely a syntactic variation of the Active structures.

The description in this paper is of necessity incomplete and ignores many sub-classifications and irregularities within the two verb classes. Its main purpose is to demonstrate the fact that Sinhala exemplifies the existence of two types of Passive structures which may be called syntactic or Agentive and semantic or Agentless. The former are totally absent at the colloquial level and occur rather infrequently in the literary language.

The Agentive Passives appear to be adequately generated and described transformationally, Rule (2). The Agentless Passives on the other hand are more basic and are best accounted for lexically as in (11b).

Some grammarians like Kumaranatunga do not recognize the literary Passives as true Passive structures since they are based on Active participles and relate to ACTIONS. The verbs called PROCESS in this paper are considered to be the true Passive forms.

The recent origin<sup>6</sup> of the literary Passives, their marginal nature and their limited, sophisticated use, reveal them to be truly 'optional stylistic variants or a linguistic luxury.' The Impersonal Agentless Structures called PROCESS in this paper are more basic and widespread in both literary and colloquial use. We saw a similar division of labour, so to speak, between the Agentive and Agentless Passives in English too, though not to the same extent as in Sinhala.

The evidence from Sinhala supports separate analyses for these two types of 'Passives'. The semantic distinction between ACTIONS and PROCESSES, i.e. between DOING and HAPPENING, was seen as a useful criterion for determining the two 'Passive' types. Is this criterion language-specific, or can it be extended to cross-linguistic analyses? This seems to me to be an interesting question and worthy of further investigation.

<sup>6</sup> The Passive construction in literary use seems to be an 11th or 12th century innovation (T. Somananda 1962).

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Distinctive Feature Matching as a Basis for Finding Cognates T. R. Hendrie University of Victoria

The purpose of this paper is to describe a preliminary project to search for cognates in dictionary lists using a computer. Of equal interest is the use of a systematic examination to test the hypothesis that diachronic sound shift proceeds by the very gradual process of changing only one or two distinctive phonological features at a time. By defining the phonemes of each language under consideration in terms of distinctive features which are proper in the context of the individual inventories, it is possible to compare the segments of possible cognate pairs and measure the similarity of the forms. Once some level of phonetic similarity is established, it is then worthwhile to examine the meanings of the forms for semantic correspondence. The computer program makes possible the rapid selection of only those pairs of words which are considered close enough phonetically to warrant further inspection.

Phonological comparison of languages which are the result of historical drift from a common proto language typically reveals differences between corresponding phonemes of only one or two distinctive feature values. To illustrate the validity of the premise to this project, a cursory look at the Germanic Consonant Shift will serve as an example. Table I shows the Proto-Indo-European voiceless stops \*p, \*t, \*k together with their respective Germanic reflexes f,  $\theta$ , h, in a matrix showing some of the distinctive features that might be used to differentiate them. Members of each pair made up of the ancestral consonant and its reflex differ by a single feature value, that is, seven out of eight feature specifications are the same, while pairs not supposed to reflect

systematic sound shift, for example /p/, /t/, share six or less feature values.

	CONS	SON	ANT	COR	CONT	NAS	LAB	VOICE
р	+	-	+	_	-	-	+	-
f	+	-	+	_	+	-	+	-
t	+	-	+	+	-	-	-	-
θ	+		+	+	+	-	-	-
k	+	-	-	-	-		-	-
h	+	-	-	-	+		-	-
	Grimm's Law: P.I.E. ptk					Germanic f 0 h		

## Table I

To take an Australian example (O'Grady n.d.:4) there is an  $/1/^1$ -/1/ correspondence between Wadjuk (WJK) and Nyungar (NYU) where, measured in shared distinctive features, the sounds are very close.

WJK	'KY-LI'	'BILO'
NYU	kerl	pirl
	'boomerang'	'creek'

Of course, none of this is very surprising, but it does demonstrate that sounds change little by little and, more importantly for the argument at hand, similarity between phonemes which are reflexes of a proto-form can be measured in a useful way. On the other hand, there is no assurance that all sounds in a phonological system change equally gradually: in a given time period one set of phones may change by a single feature while another set may change by two or more features. Consider the hypothetical cognate

<sup>&</sup>lt;sup>1</sup> This /1/ stems from the 'L' used in the analysis of Wadjuk by Moore in 1884. Because such 'pre-scientific' analyses cannot always be counted on to show all the phonemic contrasts of a language, one must be careful when ascribing phonemic and phonetic values to the symbols found in such works.

pair \*paka in Australian language A and \*waka in Australian language B which manifests the p - w correspondence attested between some Australian languages. Using the same features as those of Table I, a comparison of the second consonant of each word would show all feature values to be shared, whereas a comparison of the initials would reveal that they share only three out of eight feature values. Consequently, if the program is set so that only words with consonants differing by a single feature or less are singled out, significant correlations will be overlooked. Conversely, setting the number of shared features too low will produce such a mass of 'possible' shared cognates that anything significant will be obscured.

The 'value' level of the computer program is the parameter defining the number of shared features chosen as a minimum for consideration as cognates. This number is very important to the usefulness of the paired forms in the printout. The level has to be set low enough so that nothing significant is missed and high enough to avoid so many paired forms that no advantage is gained over manually comparing the dictionaries. The previous example from Nyangamarda and Gupapuyngu shows how the most appropriate 'level' can vary even within a single pair of cognates.

The choice of distinctive features to be used in the program obviously depends on the languages being compared and varies with the number of phonemes in each language and the similarity of their inventories. As this is a preliminary working program, only consonants are considered in this project. The oral stop series in Walbiri are not really voiced but typically devoiced, and since there is only the one series it is of little importance that they are written as voiced or voiceless. Hale writes them as voiceless in his dictionary. Gupapuyngu does have a voicing distinction, or rather a fortis-lenis distinction as several linguists claim, but

since it is one of the few Pama-Nyungan languages that has it, it is presumed for the simplication of this project that the lenis series corresponds in a systematic way with the single obstruent series of the proto Gupapuyngu - Walbiri languages and that the omission of the feature [tense] is therefore justified.

Gupapuyngu

b	d	d	đ	dy	g		
р	t	t	ţ	ty	k	?	
m	n	n	ņ	ny	Ŋ		
		1	1				
		ř	r	У	W		
Ъ	d	dy	đ	g			
m	n	ny	ņ	ŋ			
	1	<b>1</b> y	1				
	ř						

Walbiri

y r w Phoneme Inventories for Gupapuyngu<sup>2</sup> and Walbiri<sup>3</sup>

## Table II

Since most Australian languages possess alveolar, alveopalatal palatalized and retroflexed stops there is no reason to expect that the proto language did not or that the distinctions should be conflated. According to Capell (1962:3), the glottal stop, present in Gupapuyngu, is rare in Australian languages and restricted in its geographical distribution, but since there are several possible sources for this stop it cannot be subsumed under the k for example.

This program is not, strictly speaking, dependent on distinc-

<sup>2</sup> Adapted from S. A. Wurm, Languages of Australia and Tasmania, p. 51.
<sup>3</sup> Adapted from A. Capell, Some linguistic types in Australia, p. 17.

tive feature analysis since the distinctive codes of presence or absence of each feature could be replaced by a purely arbitrary system. Such a system would eliminate redundancy and make possible minimal number of features - a definite advantage in some respects. On the other hand, the code would have to be modified when working on different languages or introducing phonetic variants. In spite of the preference for proper and accurate distinctive features some of the value assignments may be decided arbitrarily in favour of simplicity.

The method for searching for possible cognates by comparing dictionary entries makes assumptions not only about phonological structure but also about word shape. Stress generally falls as close as possible to the front of the word (Capell 1962:4). If stress falls on different syllables during the development of the two languages, that is, on syllable x in language A and on syllable y in language B, different phonological processes, particularly a variation of lenition processes, must be anticipated. Fortunately, because so much is already established about Gupapuyngu and Walbiri it can be assumed that stress has always been on the first syllable. Tone, too, would have different effects on the phonological development but Capell states (1962:4) that aside from a few languages with 'ornamental' tone, it does not occur in Australian languages.

Another major complicating factor is affixing. Since the program assumes the first syllable (at least) to be the root, the existence of prefixes would pose grave problems. Although Wurm (1972) says that Pama-Nyungan languages are not normally prefixing, it is still possible that some rare forms do have a frozen prefix and this program would not be able to identify the cognate pair if the prefix were only in one of the languages. Nevertheless, any language sufficiently analysed for a dictionary to be produced

would probably have most of the prefixes noted.

There are a number of problems arising from the features used to describe the phonemes of the languages to be compared. When 11 features are used, the shared feature scores for nasals shows  $/n^{y}$ , n/-7;  $/\eta$ , n/-8;  $/\eta$ ,  $n^{y}/-8$ . Wurm points out that there is a frequent interchange of /n/, /ny/, and  $/\eta/$  among related Australian languages. Without modification this program will not select cognate words differing solely by  $/n^{y}$ , n/, without the 'value' level of shared features being so low as to be useless. For example, /L/ and  $/\theta/$  also share 7 out of 11 as well as many other unlikely pairs. The correspondence of /g/ and /w/ is also noted by Wurm (1972) but in this analysis they share only 6 out of 11 features.

In order for this program to examine the dictionary entries they must be filed on a computer disc or tape storage device. The format of the entry does not matter as long as it is consistent, although both dictionaries would not necessarily have to be in the same format. The format used here is the same as that of O'Grady's Australian files. Each entry is limited to a single line of length 80 characters, the last 52 of which are reserved for the gloss sometimes not enough to duplicate the full entry. Where some of the entry is omitted, this is indicated by suspension points. A line looks like this:

Language C<sub>1</sub> V<sub>1</sub> C<sub>2a</sub> C<sub>2b</sub> C<sub>2c</sub> Etcetera Gloss

D O P

GUP

The first three characters name the language. Each segment position of the initial grouping is reserved two spaces. This is because of the impracticality of using diacritics and special symbols with the computer. Even when there is no consonant at  $C_{2b}$  or  $C_{2c}$ , the space is left empty so as to separate the supposed root from other morphological material. Because of the usual lack of a reconstructible

ULU

third syllable in Pama-Nyungan roots, all but the initial vowel of the 'Etcetera' group is normally irrelevant for the reconstruction of the root; hence the original spelling found in the dictionary was kept rather than modifying it as was done for the initial grouping.

The orthographic representations of each segment are chosen so as to simplify working with a computer. It is possible to use all the normal phonetic symbols but because of the increased complexity of the programming involved in doing so, it is better to be content with the much simpler system of allowing two character spaces per phoneme. In the orthography used for this computer program, alveopalatals  $/t^y n^y 1^y$  are written TY, NY, LY;  $/\eta$  as NG; dentals /t d n/ as TH, DH, NH; retroflexed consonants /t d n 1/ as RT, RD, RD, RL, /r/ as RR; and all others as a single capital letter plus an empty space. Some sounds may have varying spellings in the different writing systems of different languages. This approach is a great advantage as it allows dictionaries using any system of spelling to be used. Each spelling is defined in the array in terms of distinctive feature values such that b and p have the same specifications if they represent the same phoneme and different specifications if they represent different phonemes.

This program for extracting possible cognates considers only the phonetic form (actually the spelling), and ignores the meaning. After the pairing process the glosses of the two entries must be compared. If the semantic connection seems plausible, then the pair may tentatively be considered cognate. When a number of putative cognates have been found exhibiting the same systematic sound correspondence, then the weight of numbers may be taken as support for cognation and for the idea that the sound correspondences are systematic. Added to the relatively concrete and technical problem

of quantifying the relation between phonemes is the abstract and more difficult task of deciding what is a plausible semantic connection. The pair

	WAL	R AMPAKU	light in w	reight		
	GUP	RDAMBA	light in w	veight		e
are	eminently	acceptable as	cognates.	On the other	hand,	the pair

inside (hold of ship)

WAL

RDANDJA

GUP

RRANJARR-KA to carry a full load

are similar enough to arouse interest yet far from convincing. O'Grady (n.d.) gives a putative cognate set in which the Gupapuyngu and Walbiri forms mean 'dry, dried up, burnt, stale, overcooked' and 1. NEGATIVE 2. 'absent', respectively while the form in Pintupi means 'continually, still'. Were it not for the close similarity of the phonetic form (/rawak/, /lawa/, /rawa/, respectively), and without independent semantic corroboration from other roots, it would be extremely difficult to establish cognation with such divergent meanings. It is only by combining the phonological and semantic evidence that relationships can be stated with a reasonable degree of confidence. When several clear cases have been found, the systematic sound correspondences exhibited can add credence to other pairs with less obvious semantic connection. Without an efficient way of codifying the semantics that includes all possible types of shift a search of this type must begin with the phonological shape.

The problems of writing this program are greatly simplified in a number of ways. First, the dictionary sections used contain words whose initial consonants are already known to correspond in a systematic way between the two languages. Consequently, the compounding of problems involved in comparing more than one thing at a time is avoided. Similarly, only the first consonant of the cluster following the first vowel is examined. In the plausible case of a  $/\eta k/ - /g/$  correspondence only the  $/\eta/$  of the cluster is compared with the /g/ where both segments ought to be taken into consideration. Vowels are ignored. This is perhaps not too serious an omission since most Australian languages have basically only /i a u/ (Capell 1962) and with primary stress on the first syllable the vowel would normally be expected to be relatively stable. The program is already confronted with many problems in a pair of Australian languages where initial dropping has led to the development of monosyllabic roots in which initial clusters are found and it can only be expected that comparing languages with more complicated word shapes would present even more difficulties.

As this program considers only the first segment of the rootmedial consonantism it cannot be very selective. In this preliminary trial the compared dictionary lists have as initials phonemes that are known to correspond so that a necessary step would be to compare the initials in case of unknown correspondence. The vowels, too, need to be considered so that in the case of the present printout many of the improbably cognate pairings such as

WAL RRAKU hole in the ground

GUP RDIKADIKA curly

would be avoided. Another less easily solvable problem is that of the proto form having as reflexes a single segment in one language and a cluster in the other. When more than one position in the word is considered, some kind of averaging or dependency must be worked out.

While this preliminary program is limited in scope and is subsequently not as selective as one would like, it does show potential for being a useful tool. There are possibilities for improvement by choosing different distinctive features or by weighting

those considered more relevant. Given the typical complexity of language it is probably impossible to refine such a program to the point of its selecting all and only those pairs which are cognate. Nevertheless, the results of this first attempt indicate that a system based on the principles used here could be useful as an initial step in isolating pairs of words which have high potential as cognates from the thousands of words which are unrelated.

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# LES PROCEDES LINGUISTIQUES DE LA POLITESSE: ANALYSE D'UN CORPUS

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# ABSTRACT

Politeness proposes rather than imposes. It deals with ideas, hints or impressions rather than with facts. Thus the interlocutor has some freedom of choice to actualize the locutor's proposition the way he wants, or not to actualize it at all, since the locutor remains at the level of virtualities, or notions.

In linguistics this means that politeness is an open door to abstract semantics and vice versa; and finally all expressions of politeness can be analysed as the virtual used to avoid the actual.

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#### 1.0 INTRODUCTION

## 1.1 Politesse et écart

La politesse est une loi de la linguistique. Elle s'observe dans toutes les langues, à toutes les époques et dans le parler de toutes les couches sociales (on peut être impoli en tenant un langage soutenu tout comme on peut exprimer la politesse en langage vulgaire).

C'est la nature même de l'expression polie qui en fait un des phénomènes universellement observés dans le langage. Pour comprendre, le linguiste doit passer du concret d'un énoncé à des abstractions. L'étude des phénomènes de politesse nous fait saisir directement cette bipolarité abstrait / concret dans laquelle oscille tout énoncé: les phénomènes de politesse en effet peuvent tous se définir par une fuite de l'aspect concret, brut, actuel, "grossier" du procès en cours.

La politesse tourne donc le locuteur vers l'aspect abstrait, plus fin, virtuel, plus subtil du procès. Cette préférence du non actuel, ce passage par le virtuel se retrouve à un degré plus ou moins grand dans toute production langagière. C'est cela qui en fait un énoncé plus ou moins "poli".

Tout procédé de politesse se résume donc à un écart. Ecart entre le procès dont on parle et la façon dont on parle du procès; écart entre l'actualité objective du procès et la virtualité subjective préférée par la politesse.

A partir de là, la langue dispose de toute une série de plans sur lesquels peut se jouer l'écart de politesse. Par convention, nous opposerons cet écart vertical unique qui existe entre l'actuel et le virtuel aux différents moyens que se donne le système pour l'exprimer dans l'horizontalité du discours. Que l'écart soit temporel, modal ou autre, il n'est qu'une projection sur le plan horizontal de l'écart acte / puissance inscrit sur l'axe vertical. Selon l'éclairage choisi par le locuteur l'écart peut être projeté dans différentes directions sur le plan du discours.

La direction choisie par le locuteur pour exprimer l'écart est même tout à fait secondaire par rapport à la position initiale de l'écart. Concrètement cela se traduit par une équivalence sensible entre les phrases suivantes :

> Je voudrais une paire de chaussures. Je voulais une paire de chaussures. Avez-vous tel type de chaussures ?

**1.**2 Les principaux types d'écart

Un écart peut être posé sur n'importe quel plan sémantique. Nous recensons ici les écarts les plus courants, qui serviront de repères pour analyser le corpus.

### 1.2.1 L'écart temporel

C'est le plus éclairant des phénomènes d'écart. Il consiste à transposer sur l'échelle du temps une différence d'ordre verticale : le locuteur parle au passé ou au futur pour éviter la contrainte d'un présent.

Le non-présent est moins contraignant que le présent car son actualisation peut être discutée. Le présent, lui, équivaut à de l'actuel, ce pour quoi il choque : *Je veux savoir* deviendra donc *Je voulais savoir*.

1.2.2 L'écart de mode

La modalité est ce qui définit l'adéquation d'une proposition au réel. Elle sert à exprimer l'écart entre le réel d'une proposition et la réalisation qui en est décrite. Il s'agit du même écart qui caractérise les phénomènes de politesse. Nous pouvons donc affirmer que toute opération visant à modifier le degré d'adéquation d'une proposition au réel (modalité) modifie en même temps—et par définition—la présentation qui est faite du réel en question. Nous détectons donc là un procédé de politesse.

1.2.3 L'écart de personne

Il modifie en apparence les rapports allocutoires. Le tu est un espace du je qui engendre le rapport allocutoire; le il est un tu réfléchi par un autre tu. Il est donc plus distant du je. C'est cette distance qui est utilisée par l'écart de personne quand le rapport allocutoire se fait à la troisième personne: *Monsieur est-il satisfait* ? Parfois c'est le *je* qui devient *il*, une façon pudique de parler de soi-même.

1.2.4 L'écart de nombre

Il consiste à exprimer la qualité par la quantité. C'est le cas du vouvoiement par exemple: le je, aussi petit soitil, sera toujours un; c'est donc du côté du tu qu'il faut chercher pour établir la supériorité du tu sur le je. Or le tuest aussi un; l'addition de plusieurs tu est alors le seul moyen d'exprimer la différence d'importance.

Le je peut aussi devenir pluriel, dans le cas d'un chef d'état grandi par ses fonctions.

1.2.5 l'écart de genre

L'abstrait étant du un par rapport au deux du concret, il est moins différencié mais plus plein de virtualités. Ainsi le neutre servira souvent à contourner une actualisation en masculin ou féminin puisqu'il est la virtualité des deux. D'où l'emploi fréquent de mots tels que *on*, *ça* ou *quelqu'un* qui incluent les deux genres :

> On va bientôt passer à table. Alors, ça n'a pas été ? Quelqu'un pourrait m'aider ?

## 1.2.6 L'écart lexical

Il consiste à préférer un mot à un autre jugé trop direct. C' est le terme *euphémisme* qui décrit le mieux l'écart lexical. Le sens exact du mot *euphémisme* est *emploi d'un mot favorable*. C'est bien de cela qu'il s'agit ici. On préfère un terme qui décrit la notion plutôt que la chose elle-même.

## 1.3 Le corpus

Le répertoire ci-dessus n'est qu'un outil de base. Il ne rend nullement compte de toutes les nuances de politesse que peut contenir la moindre phrase.

Il y a des phénomènes moins pondérables tels que l'intonation, le déplacement des àccents les silences et prolongement de syllabes, il y a toute la gestuelle, le jeu entre l'affirmation et la question, sans oublier l'utilisation de majuscules pour l'écrit.

Les phrases ou expressions qui vont maintenant nous permettre de voir ces catégories en action ont toutes été relevées en contexte spontané. En vue d'une synthèse nonnormative nous nous sommes détournés des manuels de savoirvivre ou de bien-parler. Nous nous sommes intéressés au français courant actuel: celui entendu ou lu dans les media, en famille, dans des discussions entre amis, ou en ville dans les magasins ou services publics.

## 2.0 ANALYSE DU CORPUS

## 2.1 Enoncé 1

- a) Qu'est-ce qu'elle voulait, la petite dame ?
- b) Qu'est-ce qu'il voulait, le petit gamin-là?

Ces deux énoncés ont été relevés dans des petits commerces, épiceries et boucheries, et à plusieurs reprises. Il s'agit du commerçant qui, après avoir servi plusieurs clients, s'adresse à un autre client qui ne s'avance pas de lui-même.

Pour certaines personnes une telle manière d'aborder un client est plus inélégante que polie. Pourtant elle fait partie d'un univers linguistique à part entière et exprime, dans un langage populaire et "de quartier", un certain écart de politesse de la part du locuteur.

Assez curieusement nous trouvons ici un rapport allocutoire à la troisième personne à un niveau de langue très simple, et sans mépris ni ironie. Pour tenter d'expliquer cette troisième personne, revenons sur les rapports qui définissent le je,le tu et le il. Le il est un tu qui a été exclu du rapport allocutoire; il est un tu potentiel que le jemaintient hors de son espace allocutoire. le il est donc un tu absent.

Cette sémantique du il lui procure des affinités avec certains temps verbaux: il, étant exclu du présent de parole, ne correspond pas au présent, mais à l'absent. L'absent égale le passé ou le futur. Le il présent est donc un tu passé ou futur. Ici le client ne peut être dans le futur puisque, logiquement et chronologiquement, le vouloir du client est antérieur à son achat. Le commerçant s'adresse donc à un client qui est passé; ceci est corroboré par la mise au passé du procès concernant le client: *Qu'est-ce qu'elle voulait*...

Il s'agit maintenant de savoir pourquoi le passé de politesse, qui est courant, se double d'une exclusion du rapport allocutoire, même d'un rapport allocutoire dans le passé.

La manière particulière dont le commerçant traite le client est révélatrice. Le gamin - mot révélateur à lui seul et la dame sont dits petits. Ce petit exprime une infériorité, une faiblesse, un manque de savoir-faire par rapport aux autres clients. De fait le commerçant ne s'adresserait jamais en ces termes à une maîtresse femme ou à un homme par exemple. Les expressions a) et b) ont souvent été relevées dans le cas de personnes laissant passer leur tour et peu susceptibles de s'imposer: un enfant timide, une très vieille femme.

Le commerçant perçoit donc - ou parfois imagine une infériorité chez un tel client. D'une part il se permet de l'exprimer, et cette familiarité paternaliste a l'air de mauvais goût pour des gens plus réservés. D'autre part il maintient la politesse en plaçant l'interlocuteur dans un ailleurs (troisième personne) et dans un passé (*voulait*).

Il s'agit d'un procédé de politesse assez rugueux, dirons-nous, puisque le locuteur a la délicatesse d'écarter l'interlocuteur pour parler de sa faiblesse, mais il profite de cette même faiblesse pour se permettre de l'écarter de

l'actuel et ainsi se permettre des familiarités.

Des expressions telles que ma pauvre dame, mon pauvre monsieur ou le jeune homme, la demoiselle en rapport allocutoire relèvent du même type d'impérialisme poli.

L'analyse du *là* dans *le petit gamin-là* confirme le procédé. Notons d'abord qu'on n'entend jamais dire:

Qu'est-ce qu'elle voulait, la petite dame-<u>là</u>? ni Qu'est-ce qu'il voulait, le petit gamin \_\_\_? Le mot là a valeur de fléchage; il extrait un élément d'un ensemble uniforme et le désigne en particulier.

L'emploi exclusif de  $l\dot{a}$  au lieu de ci dans ce cas est également révélateur.  $L\dot{a}$  est plus loin dans le temps et dans l'espace que ci. Or le gamin en question n'est pas ici, un tu au présent, mais là-bas, un il au passé; c'est donc  $l\dot{a}$  qui convient.

Pour ce qui est de l'extraction et du fléchage, ils ne peuvent s'appliquer à une dame agée qui a une individualité plus marquée qu'un gamin. C'est une marque de respect pour la dame de ne pas l'extraire d'un groupe uniforme de personnes toutes semblables et donc de ne pas l'y mettre a priori. A l'inverse le gamin a une personnalité plus dérisoire aux yeux du locuteur ( d'où l'emploi de ce terme ); il est un gamin du quartier comme tant d'autres. De fait le commerçant ne dira jamais a) à une dame qu'il n'a jamais vue, tandis que b) peutêtre adressée à un gamin inconnu du locuteur.

L'énoncé a) marque donc plus de respect que b). Là encore la politesse est rugueuse parce qu'incomplète. Le locuteur accuse inconsciemment une différence entre la dame et le gamin, mais d'un autre côté il pousse la familiarité au maximum dans chacun des cas.

2.2 Enoncé 2

a) Alors, on ne t'a pas accepté?

- b) Alors, ils t'ont refusé, aux Beaux-Arts ?
- c) Ça n'a pas marché ?

Ces trois énoncés sont ceux d'étudiants essayant d'engager la conversation avec un ami au sujet d'un récent échec connu du locuteur.

La forme interrogative ne porte donc qu'en apparence sur le succès ou l'échec du candidat. La demande d'information porte en fait sur le comment, pas sur le quoi. En effet, si l'interlocuteur ne se lance pas dans des commentaires sur son échec, le locuteur le poussera en général à le faire, par des questions plus précises. Il y a donc un procédé illocutoire exprimant la politesse dû à un écart entre l'information connue et l'information demandée. L'effet de politesse est dû à la liberté qui est laissée à l'interlocuteur de donner une réponse littérale ou de faire un commentaire plus élaboré.

Le terme *alors* souvent employé pour entamer ce genre de discussion nous met sur la piste de la vraie question posée: le locuteur ouvre un chapitre, il marque le début d'un temps d'explication : a-lors = en ce temps-là (logique ou chronologique)

Les trois phrases présentent des exemples d'écart de genre (voir I.2.5) avec les mots *on*, *ils* et *ga*. Les énoncés a) et b) partent des examinateurs, et c) de l'examen. La formulation a) diffère de b) en ce qu'elle rapproche moins le locuteur de l'interlocuteur que b).

Le *on* de a) est la formulation la plus neutre pour parler des examinateurs; elle contient tous les genres et tous les nombres tout en insistant sur leur aspect abstrait de un. Le *ils* de b) contient également tous les genres et tous les nombres, mais en insistant sur leur aspect multiple.

L'étudiant malchanceux, lui, est un, au même titre que les uns qui composent le *ils*. A l'inverse, les uns qui composent le *on* forment une entité plus abstraite et de ce fait supérieure. La formulation a) écrasera donc l'interlocuteur tandis que la formulation b) fait du locuteur un allié; le jury est plus une somme d'individus comme eux. Ces individus sont bien concrets, et situés dans un cadre concret: *aux Beaux-Arts*, précision moins courante dans une formulation en a).

Le locuteur se range donc du côté du camarade malchanceux en b). Une telle formulation s'accompagne d'ailleurs souvent d'une formule dépréciative à l'égard des examinateurs, chose bien plus rare dans une formulation en a).

La formule c) joue non plus sur l'écart examinateurs / examiné mais sur l'écart examen / examiné. Le ga générique évite déjà de préciser la nature de l'examen; l'examen une fois réduit

au minimum est ensuite écarté le plus possible de l'examiné: c'est le terme *marcher* qui sert à cela. On dit d'une machine qu'elle marche bien, mais d'un individu qu'il va bien. Il y a donc écart lexical entre :

> ça n'a pas marché. et ça n'a pas été.

Cette dernière formulation mettrait trop en cause l'individu et ses capacités intellectuelles, voire physiques.

2.3 Enoncé 3

- a) Tu as tes résultats, d'examen ?
- b) Qu'est-ce que ça a donné, ta demande de poste?

Le contexte de ces phrases est le même qu'en 2.2, sauf que l'écart illocutoire entre question et demande d'information n'existe plus. Il s'agit donc de demander une information réellement inconnue. La politesse, qui consiste déjà à s'informer sur autrui, consiste également à ne présumer en rien de la réponse.

C'est laisser au tu les IOO % de ses possibilités d'actualisation. La question a) porte ici avant l'actualisation, sur l'existence même d'une réponse. En fait c'est pourtant la nature de la réponse qui est demandée :



figure 1

Le procédé est courant. Voici d'autres exemples en situation :



Les deux phrases de 2.3 repoussent à la fin l'objet de la question. En a) c'est par une pause entre *résultats* et d'examens. En b) c'est par l'utilisation du cataphorique générique *ça*. L'écart se retrouve sur l'axe syntagmatique où le champ du concret est repoussé vers la droite :



figure 2

2.4 Enoncé 4

Bien, (euh...) je disais tout à l'heure, mais malheureusement vous n'étiez pas euh... tout à fait en ligne (...)

Cet énoncé a été relevé lors d'une émission radiodiffusée au cours de laquelle les auditeurs pouvaient appeler et intervenir en direct. C'est le speaker qui parle, après avoir eu quelques difficultés à entrer en contact avec l'auditeur.

Pour entamer la conversation le speaker commence par tirer un trait sur ce qui vient d'être dit ou fait. Cette technique est employée des dizaines, voire des centaines de fois par jour par tout locuteur. Elle est analysable comme fait de politesse. D'autres mots que le mot *bien* servent à amorcer un virage dans une conversation :

<u>Bien</u>... Qu'est-ce qu'on fait en définitive ?
<u>Bon</u>, euh... je ne vais pas rester très longtemps...

<u>Parfait</u>; il va falloir s'en occuper alors.
<u>Impeccable</u>. On y va ?

Tous ces mots ont la même sémantique. Ils reprennent l'action ou l'idée en cours et affirment son adéquation au réel. De ce fait ils signalent que le procès en cours est arrivé à son degré ultime de réalisation. Une fois ancré dans le bon, dans le bien, ce procès n'a donc littéralement plus cours puisqu'il est parvenu à son entière réalisation. La conséquence est que le locuteur a le champ libre pour amorcer tout autre procès de son choix.

Le locuteur peut soit constater, soit décréter que le procès passé est parvenu à son terme. Ainsi dans la phrase Bien... qu'est-ce qu'on fait en définitive ? le locuteur peut :

- <u>Constater</u> que la discussion à propos de ce qu'ils veulent faire est arrivée à maturité suffisante.
- 2) <u>Décider</u> que ce que le locuteur est en train de faire, et qui n'a aucun rapport avec ce qu'ils veulent faire, est arrivé à terme, mené à *bien*.

Le deuxième cas fait moins poli que le premier car il brusque l'interlocuteur. Dans le premier cas le locuteur se place dans le cours des choses voulu par l'interlocuteur. Dans le deuxième cas il interfère sur ce cours des choses pour brancher la discussion sur son propre cours :



figure 3

Dans la phrase 2.4 que nous étudions la démarche du speaker s'inscrit dans le premier cas:

bien = le contact téléphonique est bien établi.

Le temps de silence qui suit ici l'affirmation *bien* s'observe dans la majorité de ce type d'énoncé. Il correspond au silence dans lequel le premier procès est plongé et duquel



Le silence établi est maintenu quelques instants par politesse, pour bien laisser le temps à la pensée de l'interlocuteur d'être effectivement *menée à bien*, réduite au silence.

Une analyse plus fine révèle la manière polie dont le speaker rompt ce silence: le *euh*, qui transcrit en fait très mal une série d'attaques vocaliques progressivement vocalisées, marque une hésitation à rompre le silence trop brutalement.

Le silence qui est progressivement rompu au niveau de la pensée correspond à un silence progressivement rompu au niveau de la parole. De fait c'est le coup de glotte qui est le phonème primordial, celui qui est le plus proche du silence: il s'opère au point d'articulation le plus profond; il est l'expression primitive du nourisson qui sort du silence prénatal, la tentative de parler d'un être privé de cette fonction. Ici le speaker passe par des étapes de moins en moins silencieuses de sa parole, image des étapes de plus en plus exprimées de la pensée qu'il veut introduire. Le second élément intéressant de cet énoncé est l'expression *tout à fait* dans:

Vous n'étiez pas <u>tout à fait</u> en ligne. Elle exprime l'adéquation d'une chose à une autre. L'expression pas tout à fait exclut donc ce pôle de perfection, ce bien du être en ligne. Ce procédé, qui est le meme que dans ça n'est pas mal, est donc un euphémisme (voir I.2.6). Le procédé est classique, il n'exclut que le pire pour ouvrir l'éventail des possibilités jusqu'au mieux :



figure 5

Ce qui est bien plus curieux est l'emploi de cette échelle graduée pour juger d'une chose qui ne peut qu'être ou ne pas être. En effet, comment peut on être en ligne à IO %, à 40 % ou à 50 % ? Dans ce cas soit la communication est suffisament claire pour que l'auditeur puisse être en ligne, soit elle ne l'est pas. Il y a donc impertinence dans l'incidence du décalage.

On remarque beaucoup de telles impertinences dans les énoncés de personnes peu assurées, des jeunes gens interviewés ou des journalistes qui "tâtent le terrain" d'un interlocuteur qu'ils redoutent. Il y a alors une prolifération de *peut-être*, de *éventuellement* ou de *pas exactement* à tous propos, sans

souci de leur inadéquation sémantique. Par exemple :

impertinence
Je crois <u>peut-être</u> qu'il y a une erreur.
Moi je ne sais pas mais il m'a aidé ce type-là.
Je me demande éventuellement si on peut dire ça.

Nous retrouvons là une des conclusions de notre étude : c'est l'écart au niveau de la forme qui compte le plus. Ici il suffit que se trouvent exprimés des écarts en soi tous les deux ou trois mots dans un énoncé pour que le locuteur soit couvert, même si les écarts ne se rapportent pas à la lettre de ce qui est dit.

2.5 Enoncé 5

Monsieur Marchais, vous vous méprenez.

Cette phrase est le début d'une réponse faite par le premier ministre M. Barre à une attaque formulée par M. Marchais au cours d'un débat télévisé.

Nous avons ici un cas type d'écart lexical: M. Barre veut dire à M. Marchais qu'il se trompe, mais il lui dit simplement qu'il se *méprend*. Ce qui donne un caractère de distance, de réserve qui est d'ailleurs de règle chez M. Barre dans ce genre de situation.

En quoi *se méprendre* est-il moins grave, moins total que *se tromper* ? Soit les énoncés :

a) Vous vous méprenez.

b) Vous vous trompez.

Dans chaque cas nous avons un verbe réfléchi. Le procès part donc d'un *vous* pour aboutir à un autre *vous* qui peut être le même. Ainsi décomposés les énoncés donnent :

a) Vous méprenez vous.

b) Vous trompez vous.

Méprendre signifie littéralement mal prendre. On retrouve cette forme décomposée dans l'expression mal s'y prendre. Cette expression est très éclairante car elle 'ex-plique' le sémantisme de se méprendre:

vous vous méprenez = vous (dans cela) mal prendre vous.

Le premier vous est le je de départ de l'interlocuteur. Le y se rapporte au lieu dans lequel l'interlocuteur est dit se méprendre. C'est un lieu abstrait, en l'occurence un domaine de la politique. Le deuxième vous -deuxième je de l'interlocuteurapparaît comme passif.

Ainsi en a) l'interlocuteur a deux *je* : Le premier est actif; c'est celui qui *prend mal*, et il n'y a aucun sème dans cette expression qui indique une erreur volontaire, donc culpabilité. Le second *je* est celui qui est *pris mal*. Il est passif, et donc non-coupable, non-source d'erreur. Il n'y a donc attaque dans aucun cas.

A l'inverse le *vous* de la formulation b) se décompose en deux parties chacune coupable. Le sémantisme de *tromper* implique plus de gravité; il y a calcul, recherche de l'erreur. Ainsi le *je* de départ est coupable, mais le *je* d'arrivée est

également perverti par cette erreur. C'est ce qui explique l'impression de plus grande politesse en a) qu'en b) :



figure 6

L'écart lexical consiste donc à dire qu'il y a erreur mais à ne pas l'imputer directement et intégralement à l'interlocuteur. C'est suggérer qu'il n'y est pour rien, alors que soi-même on a assez de recul pour percevoir un tel aveuglement.

Ce procédé paternaliste pour contredire quelqu'un ressort également dans l'intonation, qui est toujours très marquée chez M. Barre :



Mon-sieur Mar-chais vous vous mé-pre-nez

figure 7

L'intonation se caractérise surtout par une grande descente en fin de groupe, notament sur le mot porteur du sens principal; mé-pre-nez.

De façon générale une montée marque une ouverture (une question par exemple), et une descente une fermeture, un point final. Imaginons les énoncés :

a) Vous vous méprenez. (pause)
b) Vous vous trompez. (pause)

L'énoncé b) a été relevé dans le cadre de semblables débats. Il combine une affirmation marquée (se tromper) et une ouverture dans l'intonation. Cette ouverture attendrait réponse mais la sémantique de l'énoncé l'interdit. C'est donc un énoncé de défi, qui se glose de la manière suivante : Je déclare que vous vous trompez. Avez-vous quelque chose à me répondre?

C'est pour cela que a), sur le même schéma intonatoire, et toujours suivi d'un silence comme en 2.5, est impossible: on ne peut contredire quelqu'un avec la politesse d'un tel écart lexical tout en le mettant au défi de répliquer quoi que ce soit.

A l'inverse considérons l'intonation descendante :

a) Vous vous méprenez.b) Vous vous trompez.

L'énoncé a) peut trés bien se prononcer ainsi, et c'est ce qui se passe ici, avec cependant une chute plus accentuée que dans un énoncé normal (accentuation de l'effet docte). L'énoncé

b) pose plus de problèmes: la chute indique une fermeture, une conclusion. Or le locuteur ne peut contredire totalement son interlocuteur ( *se tromper* ) sur le ton d'une conclusion, d'une chose acquise; b) n'est possible ainsi que dans le cas d'une certitude appuyée sur des éléments posés antérieurement: répétition de l'affirmation; phrase concluant une démonstration; appui syntaxique tel que :

Je vous dis que vous vous trompez.

En résumé la politesse vient ici d'une présentation paternaliste de la contradiction. Un d**e**rnier élément corrobore cette analyse: le terme *Monsieur* n'est suivi d'un nom qu'en cas d'adresse à un subalterne :

Monsieur Martin, vous passerez dans mon bureau. Autrement il fait très impoli, en régime soutenu. Ici c'est donc une manière supplémentaire qui est utilisée pour présenter l'interlocuteur comme étant subalterne a priori, moins expert à juger des choses politiques.

L'attitude est celle du père de famille grondant amicalement son grand fils, ou du directeur faisant des remontrances bienveillantes à un employé fort gentil mais qui n'y connaîtrait rien.

2.6 Enoncé 6

Je vous demande pardon, monsieur, mais j'étais là avant vous. Cet énoncé est celui d'un automobiliste à qui on

voulait prendre sa place de parking. La phrase est banale mais il est intéressant de remonter à certaines étymologies pour voir l'effet de sens qui en est tiré ici. Nous avons en effet un énoncé assez cassant et impératif avec une série de termes propres aux formules de politesse.

Demander pardon, c'est littéralement demander *don total*, demander toutes possibilités a priori ou a posteriori à quelqu'un. Accorder ce pardon, c'est lui laisser *carte blanche*. Cette expression est un peu familière mais elle traduit le mieux le sens de *pardonner* : c'est donner un accord total sur une chose, quelle que soit cette chose. En d'autres termes pardonner quelqu'un c'est être d'accord sur un faisceau de virtualités :



figure 8

Demander pardon c'est donc demander d'ignorer une faute si totalement qu'on en oublie la nature. C'est demander d'être magnanime, littéralement d'être grand, supérieur à

soi-même. S'il y a faute commise, impolitesse, le locuteur en fait un moyen pour l'autre de se montrer poli, et cette proposition même est une politesse, la seule possible ici à vrai dire. Ceci explique la façon autoritaire dont est utilisée la formule ici et dans la plupart des situations semblables : l'intrusion que se permet le locuteur est accomplie au moment où il s'en excuse.

L'emploi du terme *monsieur* relève du même procédé: la politesse est là en formule et comme le locuteur est persuadé d'avoir raison il ne se prive pas d'être poli. Mais c'est en fait un défi qui est lancé car l'attitude et l'intonation sont en contradiction flagrante avec ce qui est dit.

Etymologiquement *monsieur* reconnait la supériorité, l'antériorité. En ancien français *sieur* est une forme de *seigneur* issu du comparatif de supériorité latin *senior*, *plus ancien*. Ce qui donne :

## monsieur = mon aîné

Nous avons là au niveau lexical un exemple type d'écart temporel (voir 1.2.1 ) exprimant une différence de valeur. La même chose se retrouve d'ailleurs dans le mot

madame, dame venant du féminin du dominus latin: domina.<sup>1</sup>

Ainsi *monsieur* indique que le locuteur reconnaît un certain statut chez son interlocuteur. Une fois ce statut posé et reconnu, le locuteur affirme sa position. D'où l'impression de mépris qui ressort de cette phrase.

C'est la technique du *oui mais* qui est utilisée ici, mais avec un tel décalage entre le *oui* et le *mais* que l'énoncé est plus impoli que poli :

oui = je vous demande pardon.

*vous* = Monsieur *mais* = j'ai raison.

Le mais qui apparaît en surface de notre énoncé ne

<sup>1</sup> Le procédé se retrouve dans des civilisations totalement différentes. L'arabe utilise le mot sayyîdî (sidi) pour dire monsieur. Or sayyîdî vient du verbe sâda qui signifie dominer. Le î final traduit le mon. Nous retrouvons donc le dominus latin.

Le chinois associe également vieillesse et supériorité. Si l'on emploie toujours des formules déprécatoires en parlant de soi, on a toujours des termes de respect pour parler de ses ancêtres. Quant à *monsieur*, il se traduit et se décompose de la manière suivante:

Monsieur = Xiānsheng = Xiān (avant) - shēng (engendrer, vivre) = né avant (Yanru 1966:50) La filiation sémantique avec les termes issus du latin (.sieur, seigneur, sire, signor, señor, sir...) est donc frappante.

peut s'expliquer qu'ainsi. On ne peut l'opposer terme à terme dans la lettre de la phrase : *Certes je vous demande pardon*, *cependant j'étais là avant vous*. En réalité il oppose le statut reconnu de l'interlocuteur au *j'ai raison* du locuteur. L'impolitesse vient de ce que *j'ai raison* apparaît trop en surface.



figure 9

En définitive, l'énoncé 2.6 est donc plutôt une formule d'impolitesse.

## 2.7 Enoncé 7

On imagine qu'un expert comptable est bien payé.

Cette phrase a été relevée à la radio, dans la bouche d'un speaker interviewant un expert comptable sur sa profession. Sans insinuations malveillantes, il lui demande simplement de parler de son travail et de son salaire.

La question passe dans cette affirmation du fait de l'emploi du terme *imaginer*. Il y a écart lexical entre *imaginer* et s'*imaginer*, ainsi qu'entre *imaginer* et savoir.
Le se de s'imaginer réfléchit le procès en direction du *on*. Ce dernier n'est alors plus tourné vers la réalité extérieure mais vers sa propre création. Si le *on* décide donc qu'un expert comptable est bien payé, c'est sans appel; le comptable ne peut faire de rectification éventuelle, le reste de l'énoncé ne le lui proposant pas. Sinon il aurait l'air de se défendre.

Or ici le speaker demande une information sans arrière pensée. Il suggère qu'un comptable est bien payé, mais est prêt à changer d'avis. De même il préfère donc dire que l'on imagine la chose plutôt qu'on la sait car le savoir juge de l'adéquation d'une chose au réel, et pas l'imagination. En figure :



figure 10

Le speaker montre également beaucoup d'honnêteté dans sa manière de formuler la fin de la phrase. Soit les deux formulations :

- a) On imagine qu'un expert comptable est bien payé.
- b) On imagine qu'un expert comptable n'est pas mal payé.

Curieusement ici ce n'est pas b) qui serait le plus poli, bien qu'il ouvre l'éventail des possibilités et que a) le ferme (voir 2.4, notament la figure 5). En effet, ouvrir l'éventail reviendrait à suggérer que les salaires des comptables sont scandaleusement trop élevés, par un effet euphémique. La politesse consiste donc à limiter l'imagination du on, à la fixer sur un degré précis de l'échelle des salaires.

### 2.8 Enoncé 8

- De qui voulez-vous parler ?
- De certaines personnes, dans certains couloirs...
- Soyez précis.
- Je... Je vous pose la question.

A l'inverse de la phrase 7 cet énoncé est plein d'insinuations. Relevé lors d'une interview d'homme politique à la radio, il met en scène deux personnages : l'homme politique, qui demande des précisions sur l'insinuation, et le speaker qui ne les donne qu'en surface pour finalement s'en tirer par la formule : *Je vous pose la question*. Cette formule est intéressante car son sémantisme ne résout rien, pas même en surface. Pourtant elle est souvent utilisée pour mettre un terme à ce genre de situations.

La première réponse est déjà un modèle d'imprécision. Le speaker ne peut viser explicitement les personnes dont il veut parler. Quant on lui demande de qui il veut parler, la fuite consiste à donner l'illusion de préciser l'identité des personnes alors que la précision ne porte en fait que sur leur existence : elles sont ancrées dans le *certain*.

L'interlocuteur qui demandait une précision en a bien une, et donc la politesse est sauvegardée. Mais la précision est de pure forme puisque dans le fond elle reprend même l'insinuation : les personnes dont il parle existent bien.

Le principe est le même pour la dernière réplique : Loin de préciser, le speaker se détourne encore de l'actuel pour se plonger de plus belle au coeur de sa question chargée de virtualités :



figure 11

La technique consiste donc à forcer l'interlocuteur à répondre par politesse face à "l'ignorance" pesante et avouée du locuteur. C'est une façon polie de mettre l'interlocuteur au défi entre la lettre et l'esprit de la réplique.

D'autres énoncés similaires attestent ce sens :

- Je ne sais pas, je pose simplement la question.

- Je n'en sais rien, je posais la question comme ça.

Ici le locuteur avoue clairement son "ignorance" et il prend ensuite appui sur elle pour tout justifier. En 2.7 il y a effectivement un accent fort sur le mot *pose* pour insister sur la factualité de la question.

2.9 Enoncé 9

Il convient de rectifier une erreur trop souvent commise.

Nous avons ici un autre exemple de contradiction polie, relevée sur les ondes; c'est un directeur d'entreprise qui . reprend une analyse sur laquelle il n'est pas d'accord.

C'est la manière dont la réplique est amorcée qui est intéressante ici : *Il convient de* ... Le locuteur tire sa phrase de l'impersonnel, de ce *il* imprécis qui est posé comme point de départ. Le verbe lui même sert à ancrer ce qui suit dans le *ce qui se fait* - ce qui est déjà et qui n'est plus à démontrer. La technique est donc simple, c'est toujours celle du choix maximum autorisé par le notionnel.

Ici l'écart est cependant plus recherché encore. Non seulement le locuteur fait partir son énoncé du notionnel, mais il s'en écarte en l'inscrivant dans un cours des choses pré-établi et inéluctable. Le procédé est fréquent, bien qu'assez formel :

- Il faut insister sur l'importance de...

- Il n'en est rien.
- Je me dois de rectifier une erreur.

Dans ce dernier cas le *je* réapparaît, mais l'impersonnel est rétabli lexicalement ( *se devoir de* ). C'est un devoir du *je* envers un deuxième *je*, artifice oratoire, que de rectifier l'erreur.

La seconde moitié de la phrase en 2.9 complète le procédé: le locuteur parle d'erreur, mais il revient aussitôt à l'impersonnel de départ pour compenser la franchise lexicale. L'erreur est dite *trop souvent commise*. Elle est ainsi raccrochée au *on* qui, bien qu'incluant le locuteur, déplace le centre de gravité de l'erreur vers d'autres.



figure 12

2.10 Enoncé 10

Ce n'est peut-être pas tout à fait ce qu'il a voulu dire.

Cette phrase est celle, tirée d'une discussion entre amis, où le locuteur prend la défense d'un tiers vivement contredit. La défense est présentée avec beaucoup de précautions. C'est surtout cet enchevêtrement d'écarts qui est intéressant, les écarts en eux-mêmes étant facilement identifiables.

Le premier consiste à parler de *ce qu'il a voulu dire* au lieu de *ce qu'il a dit*. L'écart lexical s'explique par ce que le vouloir dire est au dire ce que la **virtualité** est à l'actualité.

Le second écart consiste à limiter le vouloir dire à la zone très restreinte du tout à fait. Plus cette zone est étroite, moins ce qu'en a dit l'interlocuteur a de chances de s'y trouver.

Cet écart est complété par le troisième, qui vient du peut-être pas. La négation indique la tendance du locuteur, mais elle est soumise au peut-être. Il y a une complémentarité subtile entre l'ouverture de ce peut-être et la fermeture du tout à fait. Le tout à fait restreint la négation par politesse, et le peut-être admet une remise en cause du tout.

Voici représentée ci-après cette cascade d'ouvertures qui se multiplient chaque fois l'une par l'autre. La base de départ ( *ce qu'il a dit* ) est augmentée d'espaces qui la multiplient. Ceci donne en définitive un *ce qu'il a dit*  au cube, et l'espace ainsi créé engendre un plus grand nombre d'espaces potentiels :





## 3. CONCLUSION

L'étude des phénomènes de politesse est un domaine très vaste. Les possibilités de position d'un écart existent à différents niveaux et leurs multiplications par combinaison sont très riches.

Un seul principe cependant a guidé cette étude: tout procédé de politesse peut se résumer à un écart entre les aspects actuel et virtuel du procès en cause. En règle générale l'aspect actuel d'un procès est contourné et cet écart ne peut se faire qu'au profit du notionnel, plus imprécis quant aux niveaux de surface.

C'est également une conclusion de cette étude que la politesse consiste à changer la présentation des choses et . nullement les choses elles-mêmes. C'est leur surface qui est "polie". Ceci nous pousse à aller au-delà de la constatation et à essayer de voir quelle peut être la nature véritable de la politesse. En effet le sens commun voudrait que politesse égale respect du tu ou du il par le je. Or si ces rapports n'existent que dans la surface des énoncés, comment peut-on encore parler de politesse ?

Il faut donc voir si la politesse qui considère l'actuel pour s'en détourner n'est pas une imitation de la seule politesse théoriquement acceptable, celle qui pose un écart à partir du virtuel. En effet, dans les deux cas la politesse est un je qui pose un écart à partir d'un non-je. Mais dans le premier cas le non-je est identifié à de l'actuel, du multiple, de

l'objectif; dans le second cas il est identifié à du virtuel, du un, du subjectif.

Or le je ne peut être que cela: au centre de tous les énoncés émis ou potentiels, il est le un face au multiple; il est chargé de toutes les virtualités énonciatrices; il est sujet, surtout, le maître de son espace référenciel. La politesse permet donc au tu d'être ce que le je est par essence.

Ainsi si la politesse est le respect du non-je par le je, elle exige que tous les non-je possibles soient contenus à l'état virtuel dans le locuteur. Ceci est réalisé totalement quand le je laisse au non-je son statut de je au-delà même de toute actualisation. La politesse est donc un je dans le je.

Dans les exemples étudiés au cours de ce travail nous avons souvent remarqué que la politesse servait à faire passer un choix prédéterminé et non à donner le choix. Si donc le *je* dicte au non-*je* une actualisation, ce dernier perd son statut de sujet. Pour cela, la plupart des procédés, bien que marquant un écart de politesse, se sont révélés être assez 'rugueux'.

Ainsi tous les procédés visent bien à rétablir ce 'je dans le je'. bien qu'imparfaitement réalisé dans la plupart des énoncés. Tous en effet utilisent la technique du *oui mais* dans laquelle le locuteur se départ de l'actuel. En cela elle est une imitation inversée de la technique d'acquiescement total; appelonsla la technique du *mais oui* qui n'est en fait pas une technique puisqu'elle est politesse par excellence: c'est le fameux *don total* (voir 2.6) qui élargit l'éventail des possibilités au point de trouver le *je* dans le non *je*.

### Appendice : Phrases du corpus

- 2.1 a) Qu'est-ce qu'elle voulait, la petite dame ?b) Qu'est-ce qu'il voulait, le petit gamin-là ?
- 2.2 a) Alors, on ne t'a pas accepté ?
  b) Alors, ils t'ont refusé, aux Beaux-Arts ?
  c) Ca n'a pas marché ?
- 2.3 a) Tu as tes résultats, d'examens ?b) Qu'est-ce que ça a donné, ta demande de poste ?
- 2.4 Bien, (euh...) je disais tout à l'heure, mais malheureusement vous n'étiez pas euh... tout à fait en ligne (...)
- 2.5 Monsieur Marchais, vous vous méprenez.
- 2.6 Je vous demande pardon, monsieur, mais j'étais là avant vous !
- 2.7 On imagine qu'un expert comptable est bien payé.
- 2.8 .....
  - De qui voulez-vous parler ?
  - De certaines personnes, dans certains couloirs...
  - Soyez précis.
  - Je... Je vous pose la question.
- 2.9 Il convient de rectifier une erreur trop souvent commise.
- 2.10 Ce n'est peut-être pas tour à fait ce qu'il a voulu dire.

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# Cowichan Relative Clauses<sup>1</sup> Thomas E. Hukari University of Victoria

### 1. INTRODUCTION

Cowichan relative clauses are not introduced by relative pronouns or by subordinating particles. Let us call the argument in a relative clause which is bound to the head the *pivot* (cf Dixon 1979). The relative clause pivot is marked by its absence in Cowichan, as in (2) and (3) where e marks the site of the missing pivot.

- (1) ni? Kwicet--es kwee swey?qe? kwee smeyee. AUX butcher-3SUBJ ART man ART deer The man butchered the deer.
- (2) k<sup>w</sup>0ə swəy<sup>?</sup>qe<sup>?</sup> ni<sup>?</sup> k<sup>w</sup>icət--e e k<sup>w</sup>0ə sməyə0 ART man AUX butcher-Ø Ø ART deer the man who butchered the deer
- (3) k<sup>w</sup>Oə sməyəO ni? K<sup>w</sup>icət--əs k<sup>w</sup>Oə swəy?qe? e ART deer AUX butcher-3SUBJ ART man Ø the deer which the man butchered

Note that both the subject NP and the transitive subject agreement marker  $^2/-\partial s/$  are missing in (2) where the relative clause has a subject pivot. Let us call this absence of an argument *deletion*, using the term without prejudice, since a number of formal analyses are possible and not all involve deletion.

<sup>1</sup>I wish to thank Ms. Ruby Peter of the Cowichan Band, Duncan, B.C., for her advice and her grammaticality judgements.

<sup>2</sup>See Hukari (1977) for details on the Cowichan person system. We can analyze the missing subject marker in (2) as the dependent subject marker, rather than the transitive subject marker. The two forms are homophonous and no more than one can appear on a word. As any dependent subject marker appears on the verb of the relative clause, this may cause the deletion of the transitive subject marker, accounting for the fact that (3) does not have two such suffixes. The generalization then is that the dependent subject marker is deleted in a relative clause with a subject pivot. The relativization of syntactic roles other than subject or object appears to involve other strategies<sup>3</sup>. In section three I show that other arguments are promoted to the pivotal role of subject by nominalization, which can be viewed as a lexical process, and therefore all relative clauses involve the apparent deletion of the subject or the object.

While deleting the relative pivot (or not generating one in the first place) seems highly plausible from a cursory examination of the data, it is possible that other considerations, such as properties of universal grammar, lead to another analysis. In section two I explore major analyses of English relative clauses with a view toward the possibility that they describe universal principles which are operative in Cowichan, despite formal dissimilarities between the two languages. In section three the Cowichan data are subjected to closer scrutiny. In section four properties of Cowichan relative clauses and related constructions are examined in light of the analyses of section two. I conclude that relative clause formation in Cowichan does not involve a movement rule, so Cowichan relative clauses are either base generated with missing pivots or they are derived by the deletion of a pro-form. 2. RELATIVIZATION IN ENGLISH

In this section several major analyses of English relative clauses are examined in light of the possibility that they describe universal principles which are operative in Cowichan, despite formal dissimilarities between the languages.

2.1 Wh-movement and Deletion

Discontinuities in English relative clause constructions are

<sup>3</sup>See Keenan and Comrie (1977) and Comrie and Keenan (1979) for discussions of relative clause formation strategies across languages. A re-examination of their data might be interesting in light of my claim that nominalization is a lexical process in Cowichan relatives.

often taken as evidence for a grammar with transformations. Consider the following examples where the relative pronoun may be a considerable distance from the clause in which it seems to play a role.

- (4) the cupboard in which [Mary believes [that John keeps his books e, ]]
- (5) the crime which, [Marsha thinks [that Felix said [that he was charged with e ]]]

It would appear that a simple phrase structure analysis would lead to overgeneration, assuming that the relative pronoun (or prepositional phrase) is generated in situ.

(6) \*the cupboard in which Mary believes that John keeps his books there

A movement analysis, on the other hand, will account for such discontinuities. There are however essentially two surface relative constructions in English, as exemplified in (7) and (8).

- (7) a. the person who e gave the book to Maryb. the person to whom Felix gave the book ec. the person who Felix gave the book to ed. the book which Felix gave e to Mary
- (8) a. the person \*(that) e gave the book to Mary b.\*the person to (that) Felix gave the book e c. the person (that) Felix gave the book to e d. the book (that) Felix gave e to Mary

A movement rule is generally accepted for (8) in the transformationalist literature, as in Chomsky (1965), Ross (1967), Edmonds (1976) and Bresnan (1979). Following along the lines of Emonds, let us say that relative clause structures contain a complement node COMP which attracts the pivot NP (which has the feature WH). That is, (9) is transformed into (10) by wh-movement.

(9) [the	man, [	[ e	WH] [John	saw [ WH ]	11	]	
NP	1 S	' COMP	S	NP	S S'	NP	
(10) [the	man [	[	[ WH ] WH ]	[John saw	[ e]	]]	]
NP	1 S	COMP	NP.	S	NP,	SS'	NP

While some controversy surrounds data such as (8), Bresnan (1977) posits a deletion rule where wh-fronting does not apply in the derivation, as does Emonds. Following along the line of Emonds, the COMP node of this construction does not attract the pivot (as his wh-movement rule operates only if the COMP node contains the feature WH, as in (9) above).

(11)	[the	man,	[	[ e]	[Jo	hn	saw	]	WH	]	]	]	]
	NP	T	S'	COMP	S			NP			S	S '	NP
(12)	[the	<sup>man</sup> i	]	[that	:]	[Jo	hn	saw	[ e	2]	]	]	]
	NP		5'	COMP		5			NP 4	•	S	51	NΡ

The pivot is erased and that is inserted into the empty COMP (and may be optionally deleted subsequently).

Chomsky (1977) derives that-relatives through wh-movement with the deletion of the relative pronoun once it is in the COMP. His analysis is based on the assumption that cyclic transformations are governed by various constraints which generally limit their domain to one clause. For example the subjacency condition stipulates that no cyclic transformation may move an element (X or Y below) up or down more than one cyclic node:

(13) ...X...[...[...Y...]...]...X..., where  $\alpha$  and  $\beta$  are cyclic nodes.  $\alpha$   $\beta$ 

Chomsky  $(1980)^4$  takes S', S and NP to be the cyclic nodes in English. In cases where subjacency seems to be violated, he claims that movement takes place in successive cycles, raising an element from COMP to COMP, as in (14), where *e* marks the trace of the wh-element which has been moved on successive cycles.

<sup>4</sup>I take here a more restricted version of subjacency, following Chomsky (1980). In Chomsky (1977) only S' and NP are cyclic nodes in English and other constraints cause wh-movement to be bounded. For the the discussion at hand the two formulations seem to be equivalent.

This apparent violation of subjacency, then, involves a special COMP-to-COMP movement which is lexically restricted in English and is not permitted at all in German, for example. Consider the following examples where the verb complain does not form a bridge over which COMP-to-COMP wh-movement can apply.

(15) What did John {\*complain} that he forgot?
(16) the book {which} John {\*complained} that he forgot

As that-relatives also appear to accord with the subjacency condition, Chomsky argues that they too are the product of a movement rule--wh-movement being the likely candidate. That is, a deletion rule could not operate in a successive-cyclic fashion.

The case for positing a movement rule in the derivation of that-relatives (despite the absence of a wh-word) is the fact that a simple deletion rule would apparently not account for the lexically governed violations of subjacency (assuming we extend the definition of subjacency to cover deletions), since there would be no COMP-to-COMP rule to function as an escape hatch. Bresnan and Grimshaw (1978) counter Chomsky's successive cyclic analysis by proposing essentially the inverse operation: a syntactic interpretation rule which operates on the output of both wh-fronting and relative deletion, binding the head, any intervening COMP nodes, and the empty pivot, as in the following analysis of (4).

The rule will bind the relative head with the  $\omega h$ -phrase (or COMP), working down through any intervening COMP nodes (which are indexed in the process) until it reaches the pivot. As in Chomsky's analysis, a verb such as *believe* may function as a bridge for COMP-to-COMP binding, whereas the binding operation would otherwise not violate subjacency. An interpretive analysis has the advantage of not requiring a movement rule in the derivation of constructions in which no surface  $\omega h$ -words ever occur, as in topicalizations (which are derived by  $\omega h$ -movement in Chomsky (1977)).

(18) The books (\*which) Mary  $\begin{cases} *complained \\ says \end{cases}$  that John keeps e in the cupboard.

The credibility (such as it is) of a movement rule in the derivation of constructions such as (18), where a wh-pronoun never surfaces, hinges on the fact that wh-movement is a plausible analysis of wh-relatives in English. One wonders if the existence of constraints on known movement rules constitutes sufficient evidence for positing the analogue of wh-movement in a language for which subjacency holds butwhere there is no surface evidence of a displaced element such as a relative pronoun. I return to this question in section four.

## 2.2 Head Raising

Vergnaud (1974), in his analysis of French relative clauses, generates an empty NP head, filling it with the pivot NP from the relative clause after first moving the pivot into the COMP by whmovement. His motivation for raising involves verb-noun idioms for which I consider here English counterparts discussed by Bresnan and Grimshaw (1978). Compare the following examples.

(19) The headway that we made was insufficient.(20)\*The headway that we enjoyed was insufficient.

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The noun *headway* generally occurs only as an idiom chunk along with *make*, yet here it occurs as the relative head and not in construction with *make*. A raising analysis would account for the distribution of *headway* in such examples, but the weakness of this argument becomes apparent when we consider the following.

- (21) We didn't make the amount of headway that was expected of us.

Clearly a raising analysis will not account for both (19) and (21). Evidently such idiom chunks need not be adjacent in base structures and hence (19) and (20) do not provide evidence for the raising analysis. Nevertheless, I will consider below the plausibility of deriving relative heads in Cowichan through raising from the relative clause (without  $\omega h$ -movement), since this seems no less plausible than a Cowichan version of  $\omega h$ -fronting.

2.3 Base Generated Relative Clauses

Brame (1978) presents an interpretive analysis of English relative clauses. As he operates within a nontransformational model, his analysis is substantially different from those discussed above, although Bresnan's and Grimshaw's binding rule is analogous in some respects and their binding rule is offered in the matrix of a transformational model.

Brame presents a system for assigning functional (semantic) interpretations to syntactic and lexical categories. The lexical items hit and put, for example, are assigned the following specifications.

(23) hit;  $F^{v}$ , \_\_\_( $A^{n(s)}$ ,  $A^{n(o)}$ ) (24) put;  $F^{v}$ , \_\_\_( $A^{n(s)}$ ,  $A^{n(o)}$ ,  $A^{p(dir)}$ )

That is, they are both verbal functions  $(F^{v})$ , both taking subject and object arguments  $(A^{n(s)}$  and  $A^{n(o)})$ , but *put* is a three-place

predicate which requires a directional PP argument  $(A^{p(dir)})$ . Suppose *put* occurs in the wrong syntactic context, as in (25).

(25) [ [Marsha] [ [put] [the car] ] ] S NP NP VP V V NP NP VP S

The assignment of functional structure will not associate the argument  $A^{p(dir)}$ , the directional PP, with any syntactic material, so the functional interpretation of the sentence will be incomplete, as in (26) where the various functions are defined to operate over phrase markers in the obvious way (e.g.,  $A^{n(s)}$  is the NP daughter of S).

(26) F<sup>v</sup>(A<sup>n(s)</sup>, A<sup>n(o)</sup>, A<sup>p(dir)</sup>)
F<sup>v</sup>: put = f (assign put a verbal function, f)
A<sup>n(s)</sup>: Marsha = i (assign Marsha the subject function)
A<sup>n(o)</sup>: the car = j (assign the car the object function)
A<sup>p(dir)</sup>: (no assignment)
Interpretation: F(i,j,A<sup>p(dir)</sup>), incomplete

That is, the functional interpretation of (25) is incomplete since there is no syntactic material to associate with the directional PP function and, unless additional rules interpret this argument, the construction is ill-formed.

If (25) is a relative clause of the appropriate type, the directional PP argument will receive an interpretation by rules which interpret the relative function and bind a relative clause argument to the relative pronoun. That is, rules will identify where and  $A^{p(dir)}$  to be of the same functional type and the relative pronoun can be assigned to the argument, completing the interpretation of the clause in (27).

(27) the place where Marsha put the car In fact, if there is not an uninterpreted argument in the subordinate clause, the relative operator cannot make an assignment and the construction is deviant, as in (28), where too many arguments are generated for the construction.

(28) the place where Marsha put the car there

Since neither movement rules nor deletions are part of Brame's model, both wh-relatives and that-relatives must be generated directly with missing pivots. Optional base rules generate a variety of syntactic structures, some of which will be filtered out by the rules of functional interpretation. Provided an uninterpreted argument is available in the functional structure of the relative clause, it can be bound to the head noun.

### 2.4 CONCLUSIONS

A number of points emerge from the discussion of English relative clauses which may have some bearing on the analysis of the Cowichan construction.

2.4.1 COMP

A leftward movement rule has some plausibility in the analysis of English relative clauses--perhaps even for *that*-clauses, given the facts concerning wh-relatives and their virtual identity to interrogatives (cf, Chomsky, 1977).

(29) Where did Martha  $\begin{cases} *complain \\ say \end{cases}$  that John keeps his books?

Bresnan (1979) makes the conjecture that there is a correlation in languages between the position of wh-type words and that of clausal complementizers (e.g., that). She points out that if interrogative and relative pronouns move only into a COMP node, this correlation is no mere fortuitous coincidence. A possible issue in the analysis of Cowichan, then, is the existence of a COMP node. That is, if COMP is not evident in relative clauses, does it surface elsewhere?

### 2.4.2 Raising

Given the formal possibility of raising the relative head from its pivotal point in the relative clause (Vergnaud 1974),

this might offer an alternative to wh-movement in Cowichan, if wh-movement lacks motivation.

# 2.4.3 Deletion

If neither wh-movement nor raising are defensible analyses of Cowichan relative constructions, then deletion in situ would seem to be an alternative. However the possibility of a strictly interpretive approach along the lines of Brame (1978) may be at least equally plausible.

3. COWICHAN RELATIVES AND RELATED CONSTRUCTIONS

In this section relative clauses and related constructions (interrogatives and clefts) are examined. I show that all relatives involve a subject or an object pivot and that putative counter-examples which appear to contain oblique or adverbial pivots are actually subject-pivoted. Further, we shall see that interrogatives and related constructions do not appear to involve a movement rule analogous to wh-movement.

3.1.1 Subjects and Objects

Subject and object pivots are phonologically absent from relative clauses, as shown in (2) and (3) in section one above. However only in the case of subject pivots is it clear that the surface form involves the conspicuous absence of an element (i.e., deletion), since third person objects are never marked in the person system. Further, noun phrases are optional in Cowichan and hence it is difficult to say that a pivotal NP has been deleted. This much seems to be true: a subject or object pivot is not overtly marked in Cowichan.

3.1.2 Obliques and Adverbials

Oblique objects and adverbials (instrumentals or locatives) appear to function as pivots, as in (31) and (33) below.

- (30) ni? cən q<sup>w</sup>ələm ?ə t<sup> $\theta$ </sup>ə sceeltən. ART I barbecue PREP ART salmon I barbecued the salmon.
- (31) k<sup>w</sup>Oə sceeltən ni<sup>?</sup> nə--s--q<sup>w</sup>ələm ART salmon AUX my S barbecue the salmon which I barbecued
- (32) ni? cən pasa?q<sup>w</sup> ?ə tən?a šləmelə. AUX I head-hit PREP this bottle I got hit on the head with this bottle.
- (33) k\*0ə \$ləmelə ni? nə--\$(x\*)--pasa?q\*
  ART bottle AUX my INSTR head-hit
  the bottle which I got hit on the head with

The argument introduced by a preposition in (30) is an oblique object of a syntactically intransitive verb and the corresponding relative involves the prefix /s-/(S-NOM). Similarly, an instrumental appears to be the pivot in (33) and this role is signalled by the presence of the instrumental-locative prefix / $sx^{w}$ -/ (INSTR).

If we examine nominalization independently of relativization a natural explanation for the nominal forms emerges. Nominalization may change the thematic (case) role of the syntactic subject of a lexical item. So we can compare *employ*, *employer*, and *employee*.

- (34) John employs Felix.
- (35) John is an employer.
- (36) Felix is an employee.

The nominal *employer* takes agent subjects when it happens to function as a predicate nominal, while *employee* takes patient subjects. In Cowichan S-NOM and INSTR are highly productive, regular processes. An S-NOM, when functioning as a predicate, takes subjects which correspond to oblique objects of the corresponding unnominalized predicate. Similarly, an instrumental nominal predicates on semantic instruments. These facts are born out by the following examples which correspond to the relative clauses of (31) and (33).

- (37) ni? nə--s--qwələm tə?i sceeltən. AUX my S barbecue this salmon This salmon is what I barbecued.
- (38) ni? nə--š(x<sup>w</sup>)--pasa?q<sup>w</sup> tən?a šləmelə. AUX my INSTR head-hit this bottle This bottle is what I got hit on the head with.

Here the nominals function as main clause predicates. While such sentences are highly restricted in discourse function (and hence exceedingly difficult to elicit) they are fully grammatical and illustrate the theme changing role of nominalization. Clearly there is no need to posit additional relativization processes, since constructions such as (31) and (33) follow respectively from (37) and (38) via whatever analysis we accept for the relativization of subject pivots.

3.3 Interrogatives

Cowichan interrogatives show no apparent displacement of an interrogative pronoun. The interrogative pronouns function as main predicates and may take simple or complex NP subjects.

- (39) stem tə<sup>?</sup>i. What is this? what this
- (40) stem  $K^{w} \Rightarrow ni^{2} K^{w}ic \Rightarrow t^{\theta} \Rightarrow sw \Rightarrow qe^{2} e$ . what ART AUX butcher-3SUBJ ART man  $\emptyset$ What did the man butcher?
- (41)  $\frac{1}{2}$  where  $t^{\theta} ey^{\theta}$ . Where  $t^{\theta} ey^{\theta}$  is that?
- (42)  $\frac{1}{2}$  wet  $t^{\theta} = ni^{\gamma} K^{\omega} ic_{\vartheta} t e = t^{\theta} = sm_{\vartheta} = sm_{\vartheta} = 0$ who ART AUX butcher- $\emptyset$   $\emptyset$  ART deer Who butchered the deer?

Sentences (40) and (42) are predicate plus complex NP constructions, where the complex NPs are headless relative clauses, which are common constructions in Cowichan, as in (43).

(43) ni? cən ləmnəx<sup>w</sup> k<sup>w</sup> $\Theta$ ə ni? K<sup>w</sup>icət t<sup> $\Theta$ </sup>ə sməyə $\Theta$ . AUX I see ART AUX butcher ART deer I saw the one who butchered the deer.

Further, the complex NP may have a lexical head, as in (44), bearing out this analysis.

(44)  $\frac{1}{2}$  wet  $t^{\theta} = \frac{1}{2} \frac$ 

The initial position of interrogative pronouns follows from their predicate function, as in (39), where the subject is a simple NP. In fact, a movement analysis of interrogatives such as (40) or (42) would cause the grammar to generate them doubly, since the interrogative predicates in (39) and (41) are presumably base generated and headless complex NPs are independently attested, so (40) and (42) follow from existing constructions.

3.4 Cleft Sentences

Interrogatives are a special case of a general construction type, the Cowichan cleft construction, which is comprised of a predicate nominal plus a complex NP subject. Compare the following cleft sentences to the basic pattern of (45)

- (45) ni? Kwićət--əs t<sup>θ</sup>ə swəy?qe? t<sup>θ</sup>ə sceełtən ?ə AUX butcher-3SUB ART man ART salmon PREP t<sup>θ</sup>ə šəptən. ART knife The man butchered the salmon with a knife.
- (46) sway?qe  $t^{\theta}a$  ni? Kwicat  $t^{\theta}a$  sceeltan ?a  $t^{\theta}a$  saptan. It was a man that butchered the salmon with a knife.
- (47) sceeltan  $t^{\theta}$  ni? kwicatas  $t^{\theta}$  sway?qe? ?a  $t^{\theta}$  saptan. It was a salmon that the man butchered with the knife.
- (48) šəptən t<sup> $\theta$ </sup>ə ni<sup>?</sup> š(x<sup>w</sup>)-K<sup>w</sup>ic'əts t<sup> $\theta$ </sup>ə swəy<sup>?</sup>qe t<sup> $\theta$ </sup>ə sceeltən. It was a knife that the man butchered the salmon with.

Again a movement rule is not only unnecessary but redundant, since both predicative nominals (cf, (49) below) and headless complex NP are independently attested.

(49) swey?qe?  $t^{\theta}ey$ ?. That one is a man. man that

### 3.5 Conclusions

An examination of Cowichan relatives and related constructions has not revealed apparent evidence of a movement rule. Relativization in Cowichan reduces to two cases: subject pivots and object pivots, since putative oblique and adverbial pivots were shown to be subjects of nominal predicates. This reduces considerably the complexity of relativization and, as argued in section four, makes a movement rule far less attractive. Further, no evidence of a movement rule was found in our examination of related constructions such as interrogatives and clefts.

4. MOVEMENT RULES AND COWICHAN RELATIVE CLAUSES

# 4.1 Wh-movement

No case for wh-movement emerged from an examination of relative, interrogative and cleft constructions in section three. This finding does not preclude a wh-movement rule in Cowichan, but it shows that there is a remarkable lack of language-internal evidence for such a rule.

An argument against wh-movement may be possible, that moving a pro-form to a COMP node is not tenable because there is no COMP in Cowichan. The lack of a COMP would not eliminate the alternative of a movement rule which does not involve COMP, although such a rule would be a counterexample to Bresnan's (1979) generalization that the position of relative-interrogative pronouns and complementizers correlate cross-linguistically. It is difficult however to argue that Cowichan does or does not have complementizers (and hence the node COMP), given the lack of cross-linguistic criteria for complementizers. Perhaps the most revealing observation is that relative clauses are not introduced by particles or words which might be candidates for the category of complementizer.

Since there are elements which introduce subordinate clauses,

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the possibility that Cowichan has a COMP cannot be absolutely dismissed, although the case for one is quite slim. Perhaps the most likely candidate as a complementizer is a particle which is variously realized as  $/k^w \partial/$ ,  $/k^w \partial/$  or  $/k^w S(\partial)/$ , with a complex and somewhat idiolectal distribution of the alternate forms. However its resemblance to the articles  $/k^w/$  masculine, hypothetical and  $/k^w S/$ feminine, hypothetical is striking, despite the fact it does not seem to carry the gender distinction (as far as I know). Further, it is confined to nominalized clauses, as in (50) and (51).

- (50) ?i cən ?u?--statəl?stəx<sup>w</sup> k<sup>w</sup>(ə) ən?--s-ni? ce? AUX I IYP--know ART? your--S--AUX FUT cala?i@am?š ?ə k<sup>w</sup>s ?əpanus. lend-me PREP ART ten-dollars I know that you will lend me ten dollars.
- (51) <sup>?</sup>i cən @ay<sup>?</sup>@ət kwə nə--s--ni<sup>?</sup> ce<sup>?</sup> tey. AUX I practicing ART? my--S--AUX FUT race I am practicing to canoe-race.

The subordinate clause in (50) is interpreted as an object, while the one in (51) seems to be an oblique object, despite the absence of a preposition (as the verb is reflexive and hence syntactically intransitive). The fact that this particle introduces nominalizations but not other subordinate clauses suggests that it is an article rather than a complementizer.

4.2 Boundedness

Relativization is clause-bounded in Cowichan. This fact is not evidence for a COMP node. If, however, there were an escape hatch (COMP-to-COMP movement) this would provide indirect evidence for COMP, assuming we accept Chomsky's (1977) analysis. As it turns out, there are apparent exceptions to boundedness but on closer examination the pivot is actually in the higher clause of the relative, not the more deeply embedded one.

- (52) stem k<sup>w</sup>Oə ni? s--cəsəOe?əlt ?u?--K<sup>w</sup>icət--ən?. what ART AUX S--telling-me HYP--butcher--I PASSIVE What was I being told to butcher?
- (53) <sup>?</sup>i cəsəO--əs <sup>?</sup>u<sup>?</sup>--nem<sup>?</sup>--ən<sup>?</sup> K<sup>w</sup>icət k<sup>w</sup>Oə sceeltən. AUX telling-me HYP--go--I butcher ART salmon 3SUBJ

She is telling me to go butcher the salmon.

The relative clause verb in (52) has the S-NOM prefix, which should indicate that the pivot has been promoted from oblique object to subject. But a corresponding affirmative sentence (53) shows that argument to function as the object of the lowest clause. This would seem to indicate that relativization can go down into lower clauses and that arguments besides oblique objects function as pivots of S-NOM predicates. If this is so, it is a serious counterexample to my claim that S-NOM is a lexical process. But (53) is a spurious comparison, as shown by (54), where the pivot of (52) functions as the oblique object in the higher clause.

(54) ?i cəsə0--əs ?ə kw0ə sceeltən ?u?--nem?--ən? kwicət. AUX telling-me PREP ART salmon HYP--go--I butcher 3SUBJ She is telling me, of the salmon, to go butcher it.

Whatever the explanation for the raising phenomenon of (54) may be, the generalization that S-NOM predicates promote oblique arguments to subjects is still valid and relativization is bounded. Hence there is no necessity for proposing COMP as an escape hatch in such constructions.

# 4.3 Raising

While the primary argument for a raising analysis of lexical heads in English relatives was shown to be invalid above, let us consider raising as an alternative to wh-fronting in Cowichan. Assuming that Cowichan has no COMP, consider the following derivation for (2), the man who butchered the deer.

The relative head (possibly NP or perhaps a lower category) is empty in the base structure (55) and the lexical head appears in the relative clause. By raising, the lexical item (and the NP) is moved into the empty head position, as in (56).

There are a number of formal problems with this analysis which, taken along with the dubious status of relative raising in general, point toward its implausibility.

4.3.1 Empty NP

Since noun phrases are optional in Cowichan and since a relative need not have a lexical head, it appears that a raising rule would have to move a lexically empty NP into a lexically empty head, as in the derivation of the relative clause in (43) *the one* who butchered the deer.

The sole purpose of this exercise would presumably be to bind the pivot to the head, an operation which can be handled by other formal devices such as Bresnan's and Grimshaw's (1978) binding rule.

A formal problem arises in the derivation of relatives with no lexical head: the deletability of lexically unfilled nodes. Recent work by Chomsky (1980 and 1981) suggests that empty nodes should be deleted at some point in the derivation, but only under specific conditions. For example, languages with subject and/or object agreement markers may permit the deletion of lexically unfilled subject or object noun phrases. Let us say that a lexically

empty NP must be deleted or bound (in the sense of relative clause binding), otherwise the derived structure is filtered out as illformed. Since Cowichan has subject and object markers (hereafter, an *agreement system*, AG) subject and object NPs are deletable. With a slight revision, this will also account for the fact that only subject and object pivots occur in Cowichan. Let us say that only NPs which are cross-referenced with AG may be deleted and that all empty NPs are ill-formed at some point in the derivation otherwise. It then follows that only subjects and objects can be pivots, since these are the only NPs which are cross-referenced with AG and hence they are the only deletable NPs.

Returning to the problem of lexically empty relative heads, the relative pivot is deletable in (43) since it is a subject and is therefore cross-referenced with AG in the relative clause. But the lexically empty head is not cross-referenced unless it appears as a subject or an object in the higher clause. A headless relative NP may appear as an oblique object, where it is not deletable in our theory and hence it should be ill-formed, yet (59) is grammatical.

(59) ni? cən q<sup>w</sup>ələm <sup>?</sup>ə k<sup>w</sup>Oə ni? K<sup>w</sup>icət--əx<sup>w</sup>. AUX I barbecue PREP ART AUX butcher--you I barbecued what you butchered.

I see no way around this formal inconsistency without an ad hoc condition on relative constructions. This formal problem, along with the implausibility of moving empty nodes for the purpose of binding, seems sufficient grounds for rejecting the raising analysis. There is however an argument against raising based on lexically filled relative heads, which I consider next. 4.3.2 Raising and the Structure-Preserving Constraint

The structure-preserving constraint (Emonds 1976) states roughly that no cyclic transformation can move a category into a position

where that category is not generated by the base rules. That is, a rule moves a category C into C' where C and C'are the same syntactic category. For the structure-preserving constraint to have empirical substance (cf, Hooper (1973: 41-42) it is necessary to assume that no category can be introduced by the base rules in a position where it is never lexically filled except by a movement rule. This excludes the possibility of circumventing the structurepreserving constraint by generating categories in positions where they are merely place holders.

Given this version of the structure-preserving constraint (which is accepted by Emonds (1976)) R. Levine has pointed out to me that one should expect to find noun (or NP) plus S constructions where the noun node is lexically filled in the base for any language where the raising analysis is proposed for relative clauses. In English, for example, there are noun-complement constructions such as the following.

- (60) the fact that Felix is a genius
- (61) the claim that there are cyclic transformations

If such constructions do not occur in a language, then generating base structures with empty relative heads may constitute a violation of this stronger version of the structure-preserving constraint, since the only time a N or NP node in this position is lexically filled is when relative raising applies. As Cowichan has no N plus S constructions, aside from relative clauses, it would appear that raising is incompatible with the structrue-preserving constraint. 4.4 Conclusions

The initial observation about Cowichan relative clauses seems to be the correct one: that the relative clause forming strategy is the deletion (or nongeneration) of the pivot, which may be either a subject or an object. The remaining alternatives, then, are a transformational deletion rule or a strictly interpretive approach

within the matrix of a nontransformational model. While these two approaches may ultimately turn out to be notational variants in some sense, an interpretive analysis appears to provide an interesting direction for further research given the highly restricted nature of Cowichan relative clause formation. 98

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# Suprasegmental Effects Upon Segment Duration John C. L. Ingram University of Victoria

# 1.0 INTRODUCTION

It is well known that the duration of segments in running speech is substantially affected by a variety of suprasegmental phenomena that encode higher order syntactic and discourse features of the message.

Contrastive or emphatic stress is one way a speaker may direct the listener's attention to crucial items in the message, for such purposes as establishing a topic, to countermand a false inference he feels the listener may be inclined to draw on the basis of previous information given.

A possibly related but distinct phenomenon is the tendency for first mentioned or unfamiliar items to be spoken with deliberate stress. A speaker will momentarily retard his rate of speech and perhaps control articulation more carefully on bringing a novel lexical item into discourse. Upon subsequent mention, the speaker will return to normal rate of articulation (Coker 1973).

Prepausal lengthening occurs on words immediately preceding an actual or junctural pause. Actual pauses do not invariably occur at phrase, clause, or sentence boundaries to mark major syntactic constituents. Where such pauses may occur but do not, there is nevertheless often marking of the boundary by prepausal lengthening, i.e., lengthening of the syllables in the word just prior to the boundary. Prepausal lengthening is invariably accompanied by pitch inflection, to which it is probably mechanically linked (Lyberg 1979).

The constituents marked off by prepausal lengthening are variously referred to in the literature as 'phonemic clauses' (Boomer 1962), 'tone groups' (Crystal 1969) 'breath groups' (Lieberman 1967), or 'syntagma' (Kozhevnikov and Chistovich 1965). The diversity of terminology reflects diversity of conception as to the functional significance of units marked off by prepausal lengthening. Phoneticians like Lieberman have tended to regard them as natural units of production on an egressive airstream of finite capacity. Linguists tend to see the units marked by prepausal lengthening as there for the benefit of the listener, to aid in decoding syntactic structure. Kozhevnicov and Chistovich see them as the highest units of motor programming. There is possible truth in all these views.

In addition to the aforementioned systematic suprasegmental influence on segment duration, there are obviously paralinguistic features of individual speech style, fluency, and rate that may exercise an habitual or variable influence on speech production.

Word level or lexical stress pattern is another important influence on speech segment duration. The lexical stress pattern of a word is likely to be an important perceptual cue for its recognition. The term 'lexical' stress pattern may be a misnomer. Certainly, some portion of the internal stress pattern of words is predictable by rule, though this is a controversial matter.

Finally, there are well known phonological effects of segment combination, such as the lengthening of a vowel in a closed syllable before a voiced obstruent in English, the shortening of individual consonants in a consonant cluster, etc.

Given all these extrinsic influences on segment duration, it may sound unreasonable to assert that every phone has its own inherent duration. But experimental evidence clearly supports such a notion. Temporal filtering or gating of individual speech segments can alter the phonemic identity of a sound. A labial glide may be converted into voiced bilabial stop by temporal compression of the formant transitions. Similarily, it is possible to convert [1a] into [da], [sa] into [ta], etc. It is the temporal factor of abruptness of syllable onset that is being manipulated; a factor that has been found to be highly potent in the perceptual scaling of consonants (Ingram 1980). Duration is also an important perceptual cue for English vowel recognition (Klatt 1975).

In short, every phoneme (or, more accurately, small phonemic subclass) has an inherent duration which is important for distinguishing it from other targets. But in connected speech, there are a number of suprasegmental features which must be taken into account or 'normalized out' of the signal if phonemic recognition is to be achieved. From the restricted standpoint of phonemic recognition, these effects upon segment duration are simply sources of error. From the broader standpoint of extracting meanings from utterances however, they are vital independent sources of information.

The phonetic feature of segment duration would appear to suffer from a severe case of 'information overload'. Klatt (1976) has characterised the problem as a perceptual chicken and egg paradox. In order to detect the presence of suprasegmental factors affecting segment duration, the perceptual analyser would appear to need information on the phonemic identity of the segments involved. But in order to recognize the phonemic identity of a segment, the signal would appear to require normalization on the basis of suprasegmental information. Catch 22.

From the viewpoint of production, the central problem of accounting for segment duration would appear to lie in understanding the control mechanisms operating at different levels of

linguistic organization and how they mutually influence one another in running speech. The neuromuscular control systems are not of course amenable to direct investigation, but through the careful study of output characteristics, under controlled conditions, where the relevant linguistic sources of variance are systematically manipulated in the task set for the speaker, it should be possible to gain information on the separate and joint influence of the various factors affecting segment duration mentioned above.

Quantitative information on the temporal patterning of speech, and how the various factors known to affect segment duration interact, would seem to be a logical basis from which to begin to construct theoretical models of the speech production process. Interaction effects are of particular interest because they may place conditions of competing demand on the speech mechanism and its behaviour under such conditions may be particularly revealing of underlying control mechanisms.

In the following preliminary study, subjects were presented with short passages to be read aloud. The passages were constructed so as to control for the segmental composition of the spoken material while attempting to obtain systematic variation of three suprasegmental features:

- 1. Prepausal lengthening
- 2. Contrastive stress
- 3. Lexical stress pattern

Only performances which met criteria of prosodic adequacy were analyzed for segment duration.

# 2.0 METHOD

### 2.1 Materials

The test passages constructed for the study are given below.
The familiar noun - verb paradigm (*pérmit - permit*) was used to generate the lexical stress pattern and provide a constant segmental environment against which to evaluate the durational effects of the other two suprasegmental factors, contrastive stress and prepausal lengthening. The word *permit* was chosen from among other candidates for its relative ease of segmentation with the sonograph.

#### Test Passages

- 1) John has lost his visa. He knows that the border officials will not listen to arguments. Nor will bribery get him across that border, or personal connections. Only the permit permits him to go. That's all that will do.
- 2) The house needs painting and John's wife does not want him to go fishing. He argues that his fishing licence expires next week. But she argues that the licence does not require him to go fishing. It does not oblige him to leave. The permit permits him to go. That's all it says.
- 3) If he refuses this person a licence there will be a noisy appeal. But to allow the licence will create an exception that is unfair to the rest who applied. Should he refuse or allow it? He permits the permit. That's what he does.
- 4) Our popular building inspector encourages every applicant, no matter how crazy the scheme. But by careful use of the by-laws, he can often reject their plans. He says his job is not to approve the person but the plan. He permits the permit. That's all he does.

The strategy used for invoking contrastive stress ('accent' may be a more accurate term, but has less general currency) is aptly described by Bolinger (1961):

...two or more items are counterbalanced and a preference is indicated for some member or members of the group. The test passages possess a relatively uniform discourse structure. The first two sentences establish a context and set up a contrast which is explicitly stated in the sentence preceding the target, invoking focus on either the noun or verb. An attempt was made to build variety and plausability into the 'stories', to distract attention from the odd and repetitious collocation of items in the target sentence.

#### 2.2 Subjects

Subjects for the experiment were 6 male, native born speakers of Australian English, one of whom was the author. All were college instructors, between 25 and 45 years of age. One subject, RM, had professional broadcasting experience, a factor which turned out to be quite significant.

# 2.3 Procedure

Recordings were made in a quiet but not sound-proofed room Subjects were instructed to:

'Read the following passage through silently and decide how it should best be read to bring out its meaning clearly. Then read the passage aloud in your natural speaking voice. Try to read the passage as you might say it in casual speech. Do not try to read it in an exagerated or overly formal manner.'

The passages were typed on cards and presented to the subjects in partially randomized order such that no two successive passages contained the same basic target sentence. The set of passages was read through 4 times to provide more stable performance measures and permit statistical analysis of individual subject performances.

## 2.4 Analysis

Broad band sonagrams were made of the 96 (6x4x4) target sen-

tences using a Kay 6061B Sonograph with standard 300 Hz bandpass filter setting. The key words (*pérmit - permit*) were identified in the sonagrams of target sentences and segmented according to established acoustic features: noise bursts, silent periods, voicing striations, formant structure. A sample segmentation of the target sentence:

# The permit permits him to go.

is given in Figure 2.1. Segment durations were measured by eye with a special ruler for sonagraph displays calibrated in 10 msec. intervals, permitting a reliable resolution of approximately 3 - 5 msec.



Figure 2.1 Measurement of Segment Duration. The target sentence is The permit permits him to go. Subject RK.

Measurements of the segment durations in the key words were stored on disk file for statistical analysis. The variable transformation facility provided by the *Statistical Package for the Social Sciences* (*SPSS*) permitted easy recombination of segments into syllabic or whole word units.

#### 3.0 RESULTS

3.1 Individual differences in subjects' performance

Cooper (1976) has described the sentence reading procedure as a '...controlled yet relatively natural speaking situation'. The evidence of this experiment suggests that the fluent reading of sentences in context, requiring the correct placement of a marked prosodic feature evoked by discourse cues, requires considerable skill that only a minority of people may possess. Both impressionistically, and on the basis of a crude objective index of fluency (Table 3.1), it was found that subjects varied substantially in their performance.

'Fluency' is a complex attribute, composed in unknown ratio of such factors as 'evenness of tempo', frequency of false starts, filled pauses, the 'normal' realization of syntactically or semantically anticipated (not necessarily required) pitch inflections, etc. As a practical expedient to the problem of assessing the fluency of the subjects' performance, a simple index based on the consistency of reading rate was used. It was reasoned that fluent readers would have smaller differences in total reading time over the same passages on successive readings than less fluent readers. Hesitations, changes in speech rate, false starts, would be expected to increase the variability as well as the overall reading time.

The standard deviation of the total reading time was calculated for each passage over the four readings. This was then divided by the mean reading time for a given passage in order to take into account base differences between subjects in reading rate. The rank ordering of subjects on this measure agreed with the author's impressionistic ratings of the subjects' reading fluency.

Subject		Р	assage			
	1	2	3	4	Х	
RM	05	03	02	04	3.50	Most Fluent
JI	03	04	05	03	3.75	
CL	02	06	07	07	5.50	
RK	05	07	09	06	6.75	
BM	17	08	07	07	9.75	
GT	02	03	17	20	10.50	Least Fluent

Fluency Index =  $\frac{\text{Standard Deviation Reading Time}}{\text{Mean Reading Time}} \times \frac{100}{1}$ 

Table 3.1Fluency index of subjects'<br/>reading of the four passages

The fluency index also correlated with the author's judgements as to the number of errors of contrastive stress placement (including failures to perceptibly highlight the target word) that subjects made (Table 3.2). One subject (CL) was in fact quite consistent in his <u>misplacement</u> of contrastive stress on passages 2 and 4.

Only two subjects consistently read the passages 'correctly' - their author (JI), and RM, who was regular professional broadcasting experience. Consequently, only the data from these two subjects has been used for the subsequent analysis of durational effects, reported below.

Subject					
	1	2	3	4	TOTAL
JI	0	0	0	0	0
RM	0	0	0	1	1
RK	0	3	0	2	5
GT	0	2	1	3	6
BM	1	2	2	1	6
CL	0	4	1	4	9

Table 3.2 Errors in placement of emphatic stress

## 3.2 Suprasegmental effects on word duration

The effect on word duration of the presence or absence of contrastive stress (STRESS), of the grammatical class membership of the key word (WORD CLASS), and the position of occurrence of the word in the target sentence (POSITION), was assessed by a 3-way analysis of variance (ANOVA) (2x2x2 design). The factor of 'sentence position' refers to the ordering of the noun and verb in the target sentence:

(i)	The	permit	permits	him	to	go
(ii)	He	permits	the permit.			
		POS.1	POS.2			

Note that the key lexical item *permit*, may function (depending on lexical stress) as a noun or a verb, may occur in the first or second position in the target sentence, and take or not take contrastive stress. However, in interpreting the results of the ANOVA in a linguistically meaningful way, it is necessary to step outside the factorial framework of the ANOVA design. In particular it is obvious that POSITION has quite different linguistic signifi-

cance in the target sentences (i) and (ii) above.

Durational effects were analysed separately and jointly for the two subjects RM and JI. Apart from a POSITION x STRESS interaction which was significant for JI, but merely a non-significant trend in the same direction in the case of RM (see Appendix A), almost identical results were obtained for both subjects, allowing for base differences in reading rate. Consequently the data from both both subjects has been combined for the following presentation of results.

n Sum of squares	DF	mean square	F	sig. of F
53883.797	3	17961.266	10.394	0.000
534.766	1	534.766	0.309	0.580
276.391	1	276.391	0.160	0.691
53072.641	1	53072.641	30.713	0.000
56434.266	3	18811.422	10.886	0.000
41158.266	1	41158.266	23.818	0.000
3585.016	1	3858.016	2.075	0.155
11691.016	1	11691.016	6.765	0.012
6420.000	1	6420.000	3.715	0.059
6420.000	1	6420.000	3.715	0.059
	Sum of squares    53883.797   53883.797   534.766   276.391   53072.641   56434.266   41158.266   3585.016   11691.016   6420.000   6420.000	Sum of   squares DF   53883.797 3   534.766 1   276.391 1   53072.641 1   56434.266 3   41158.266 1   3585.016 1   11691.016 1   6420.000 1   6420.000 1	Sum of squares mean DF square   53883.797 3 17961.266   534.766 1 534.766   276.391 1 276.391   53072.641 1 53072.641   56434.266 3 18811.422   41158.266 1 41158.266   3585.016 1 3858.016   11691.016 1 11691.016   6420.000 1 6420.000   6420.000 1 6420.000	Sum of squares mean DF square F   53883.797 3 17961.266 10.394   534.766 1 534.766 0.309   276.391 1 276.391 0.160   53072.641 1 53072.641 30.713   56434.266 3 18811.422 10.886   41158.266 1 41158.266 23.818   3585.016 1 3858.016 2.075   11691.016 1 11691.016 6.765   6420.000 1 6420.000 3.715   6420.000 1 6420.000 3.715

Table 3.3 Contrastive Stress, Sentence Position, and Word Class, effects on Word Duration

	Posi	tion 1	Position 2		
	Stressed	Unstressed	Stressed	Unstressed	
Noun	381	261	403	353	
Verb	380	355	341	305	

3 1

Table 3.4 Mean duration of key words in msecs.

The only significant main effect was for the presence or absence of contrastive stress. There was an average 57 msec. (18%) increase in duration on the stressed word, but this effect varies substantially with Word Class and Position, as the higher order interactions on Table 3.3 indicate.

The absence of a significant main effect for word class, despite the fact that the verb contains an extra phonological segment, the subject-agreement marker, /-s/, which was counted in the total word duration, is attributable to the phonetic (and arguably phonological) fact that the noun [p**3**mit] in Australian English contains a long vowel, whereas the verb [pəmit] contains two short vowels.

Sentence Position affects the duration of the verb form in the same way, regardless of Contrastive Stress. But this appears not to be the case for the noun form, where there is a much stronger positional effect on duration for the unstressed than the stressed condition. Hence the significant 3-way interaction. Closer inspection of the data base however shows that this effect is an artificity of measurement difficulties.

The release of the final stop for the noun [p3mit] in subject position was only observable under contrastive stress. This problem did not arise in sentence final position where both subjects employed



e - 1



The permit permits him to go. He permits the permit. POS. 1 POS. 2

Fig. 3.1

1 Combined data for subjects RM and JI showing the effects on word duration of the 3 ANOVA factors. See text for interpretation. a clearly released [t] regardless of contrastive stress. Correcting for the truncation of the silent period and (absent) release of the [t], by comparing the duration of only the first 4 segments (i.e. pimit' vs. pimit ), indicates an actual increase for contrastive stress on the noun in subject position of 14% for JI and 11% for RM; figures in accord with the increase observed for sentence final position.

It still remains however to account for the opposite effect of Position on Duration in the case of the noun and the verb forms. Unlike the verb form, the noun in Position 2 becomes the object of prepausal lengthening:

The permit permits him to go. He permits the permit.

#### PREPAUSAL LENGTHENING

We may attempt to estimate the effect of prepausal lengthening upon word duration by comparing the mean durations for the contrastively stressed nouns in Position 1 and Position 2: 22 msec, or, approximately 6% increase. This result is at variance with other studies (Klatt and Cooper, 1975; Klatt, 1975). The effects of prepausal lengthening are usually much larger. Discussion of this problem is taken up in section 4.1.

The durational effect of Position on the verb form is interesting and unanticipated. The verb with the complex complement (i) is consistently shorter than the one with the simple object (ii), regardless of contrastive stress:

(i) The permit permits him to go.

(ii) He permits the permit.

Kozhevnikov and Chistovich (1965) noted an inverse relationship between segment duration and phrasal complexity. However, in the present case, the effect cannot be unambiguously attributed to syntactic factors. There is a difference in the metric structure of the two sentences. The primary stressed syllable in the verb in (i) is separated by two weak syllables from the next primary stressed syllable, but in the case of (ii) only a single weak syllable intervenes. The tendency towards isochronous units of rhythm in English may be responsible, at least in part, for the extra 43 msec observed duration of the verb in (ii)<sup>1</sup>.

4.0 DISCUSSION

## 4.1 Durational control mechanisms

Different control mechanisms most likely underlie the durational effects of contrastive stress and prepausal lengthening. These mechanisms may be revealed by an examination of the temporal patterning of articulatory events at the syllabic and segmental levels.

A reasonable model for describing prepausal lengthening would appear to be a deceleration curve, in which there is progressive lengthening of segments in the final word before a major constituent boundary, as the articulatory gestures are slowed down in preparation for the pause. This deceleration curve may be diagramatically represented:



A potential test of the model may be provided by a comparison

1 The author is grateful to H. J. Warkentyne for bringing this point to his attention.

of the relative duration of the first and second syllables of the noun in the target sentences:

He permits the <u>permit</u>.

The permit permits him to go.

However, it is not possible to make this comparison satisfactorily in the critical case of the non-contrastively stressed noun because of the (forementioned) problem of determining the point of closure for the unreleased  $[t^7]$ . A partial comparison based on the first and truncated second syllable provides weak support for the model in the case of RM but none whatsoever in the case of JI. For RM, the initial syllable of the non-final noun occupies 57% of the total word duration, but only 48% in clause final position. However, for JI, the relative duration of the initial syllable is actually slightly greater in clause final position.

Subject:	RM		J	Ĩ		
Syllable:	p :	3 \$mi_	p	з <sup>\$</sup>	m 1	
non-final	133 (	57%) 99	148	(53%)	131	msec.
clause fin	al   117 (	48%) 125	176	(58%)	127	msec.

Table 4.1 Duration of initial and final syllables of unstressed nouns in non-final & clause-final position

The same comparison for the contrastively stressed noun is apparently non-supportive of the model also. Neither subject shows the expected decrease in the relative duration of the initial syllable in clause final position.

Subject:	RM		JI	
Syllable:	р 3	\$ m	р <b>З</b>	m'
non-final	143 (41%)	205	187 (45%)	226 msec.
clause final	139 (38%)	221	200 <b>(</b> 45%)	246 msec.

Table 4.2 Duration of initial and final syllables of contrastively stressed nouns in nonfinal and clause final position.

However, the contrastively stressed nouns are a dubious test case for the model of prepausal lengthening, because it is not unlikely that when contrastive stress is required in clause final position, the normal mechanism of prepausal lengthening is overridden. It may be speculatively suggested that English syntax provides the speaker with a way of avoiding this 'conflict of interest' between syntactic boundary marking and semantic highlighting (both of which involve suprasegmental lengthening), by use of the cleft construction, which is a more natural way of focusing the object:

He permits the <u>permit</u>. (non preferred) It's the permit he permits. (preferred)

Turning to the question of a durational mechanism for contrastive stress, two competing hypotheses may be offered for highlighting a lexical item in running speech. One model, tentatively labeled 'uniform expansion', posits the whole word or morpheme undergoing contrastive stress as the domain of lengthening. Within limitations imposed by different types of phonetic segment, the

uniform expansion model predicts that all segments in the word will undergo lengthening, as diagramatically illustrated:



Such a temporal expansion could be simply achieved by retarding the rate of articulation on the contrastively stressed word.

An alternative strategy for contrastive stress, takes as its domain the most prominent syllable in the focused word and selectively expands it, thus changing the internal temporal patterning of gestures within the word, but with less or minimal impact on overall word duration. Thus, diagramatically:



A test of these alternative models is provided by comparisons among the contrastively stressed and unstressed verbs in the two target sentences:

(i) The permit permits him to go.

(ii) He permits the permit.

The relevant observations are summarized in Table 4.3.

Subject:		JI	RM					
	Stressed		Unstressed		Stressed		Unstressed	
Sentence	рə Ş	mits	pə 🗄	\$ mits	рə	\$ mits	pə S	mits
(i)	78	287 (21)	74	<b>2</b> 65 (22)	38	261 (18)	70	228 (23)
(ii)	84	341 (20)	75	318 (19)	59	280 (17)	57	265 (17)

Values in brackets ( ) indicate duration of the primary stressed syllable as a percentage of total word duration.

# Table 4.3 Relative syllable duration in contrastively stressed and unstressed verbs in msecs.

Table 4.3 indicates no proportional increase in the duration of the primary stressed syllable of the word undergoing contrastive stress. Hence, the relatively simple 'uniform expansion' model of contrastive stress is supported by data, over one which would imply some reorganization of the temporal pattern of articulatory gestures within the word.

## 5.0 CONCLUSION

On the basis of the very limited data reported in this pilot study it would be quite inappropriate to take too seriously the strong inferences that the author has attempted to draw about mechanisms underlying the suprasegmental control of segment duration in running speech. On the other hand, the systematic study of segment duration by the controlled manipulation of linguistically contrastive suprasegmental features appears to be a promising avenue of research. It does seem possible to ask and gain answers to important questions regarding the process of speech production and strategies for encoding linguistic information that are expressed, at least in part, by the temporal patterning of segments in speech. In particular, the following areas would appear to warrant further investigation based on the initial findings of this study:

1. Further clarification of the mechanism of contrastive stress and its interaction with lexical stress.

2. Further study of how the durational effects that serve the function of semantic highlighting (such as contrastive stress) interact with those that serve to carry information on (superficial) grammatical constituent structure (such as prepausal lengthening).

# APPENDIX A

INTERACTIVE EFFECTS OF STRESS, WORD CLASS, & SENTENCE POSITION ON WORD DURATION SUBJECT: JI



SUBJECT: RM



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Lyberg, B. 1979. Final lengthening - partly a consequence of restrictions on the speed of fundamental frequency change? *Journal of Phonetics.* 7. 187-196.

# Wundt and Bloomfield: The Leipzig Connection\* Joseph F. Kess University of Victoria

Leonard Bloomfield (1887-1949) was responsible for two classic textbooks in the field of linguistics. The second of these, Language (1933) is familiar to most, but the first, An Introduction to the Study of Language (1914), is not. It is primarily this earlier book that is of interest here, and this short essay will attempt to bring into focus some of the Zeitgeist factors, particularly the psychology of Wilhelm Wundt, which influence the substance and direction of the early 1914 book. This introduction will have little to say about the more recent history of linguistics since 1933, except insofar as it contrasts with the 1914 stage of Bloomfield's development. This has been done elsewhere (Haas 1978; Hall 1969) and is not taken up here.

Bloomfield's early book fits into the English-speaking gap left unfilled since Whitney's Language and the Study of Language (1867) and The Life and Growth of Language (1875). Bolling, in reviewing it, compared it to Hermann Paul's Prinzipien der Sprachgeschichte (1880; 5th and last edition in 1920), noting that 'there is no work in the English language with which it should be compared' (Bolling 1917; reprinted in Hockett 1970:50). Possibly the most respected and influential codification of linguistics, that is, comparative linguistics, up to Bloomfield's time had been Paul's Prinzipien. While comparative linguistics was the initial arena for a highly structured orientation towards language, the notion of the study of language as a natural science was also present, developing into what would become descriptive or structural linguistics. It was not as if linguistics had no place in the human sciences; in fact, comparative linguistics had blossomed earlier and more rapidly than did psychology.

As a textbook, Bloomfield's 1914 book (henceforth B-1914) had considerable influence. One cannot underestimate the value of an integrated model presented by a popular textbook. As Hall (1979: 192) notes, the influence that Bloomfield had 'was due primarily to the thorough organization of Bloomfield's book and the guidance in scientific method that many younger linguists found in his work and no-one else's.' Though Hall obviously had Bloomfield's 1933 book (henceforth B-1933) in mind, one can also voice the same sentiment about the 1914 version as well. Even so, not all were happy about B-1914 in all respects. For example, Bloomfield's use of innovative terminology was a feature neither familiar to nor welcomed by all. Diekhoff's review (1915; reprinted in Hockett 1970) criticizes him for it, as does Aron (1918; reprinted in Hockett 1970). His (1917) work on Tagalog, appearing about the same time, also shows the same characteristics, and he was accused by the Philippinist Blake (1919) of having changed the face of Philippine structure by having used innovative terminology to describe the language (see also Kess 1979). This was a pattern which was not to change between 1914 and 1933. Looking back in retrospect, a later structuralist generation of course found this entirely laudatory; for example, Bloch's (1949:9) obituary observes that 'to some readers, unaware of the danger that lies in the common sense view of the world, Bloomfield's avoidance of everyday expressions may have sounded like pedantry, his rigourous definitions like jargon.'

The notion of Kuhnian paradigms (Kuhn 1970) in linguistics has attracted a good deal of attention. Some, like Koerner (1972, 1976) have attempted 'to outline paradigmatic stages in the development of linguistics in a rough and ready sort of fashion. Others, like

Percival (1976) suggest that we simply abandon it in whole or in part. Psychology has also gone through its discussion of whether the field has undergone Kuhnian stages in its evolutionary development of the past century and a half (see Weimer and Palermo 1973, and Weimer 1974 a, b). Regardless of the degree to which Kuhnian notions can be applied to the history of linguistics or psycholinguistics, it remains the case that there are interesting differences in the early B-1914 and the later B-1933, particularly with respect to Bloomfield's psychology of language.

Psychology has tried itself in a number of different directions, and has failed, to come up with a dominant systematic paradigm for more than several decades at a time. Some find this discouraging (see Hockett's introduction to Esper, in Esper 1973) while others (Mueller 1979) simply take this as a matter of course. The systematic overview to which B-1914 owed intellectual allegiance was the one fashioned by Wundt. In the first thirty years of the century, we then see a period during which the goals and methods of psychology are open to question, characterized by the pursuit of a unified view of the discipline of psychology. It was during this period that Bloomfield's later views must have been formed regarding the lack of consensus in psychology, coupled with his associations with A. P. Weiss, leading to the conclusion that linguistics could ultimately do without allegiance to a given system of psychology in its formation as an independent science.

While one may not agree with Kuhn's attempts to relativize the practice of science, one certainly can appreciate in Kuhn's arguments the notion that a given stage may be the product of the ideas of a given period. So one may say that Bloomfield was a product of the intellectual tenor of his times and his training. His early work very much reflects, as did much of American academe in philology

and psychology, the prominence of German intellectual institutions. Many of the early linguists and philologists were trained in part or in whole at German universities or received their training under American scholars who had undergone such training. Bloomfield himself took further studies at Leipzig and Göttingen with scholars like Leskien, Brugmann, and Oldenberg in 1913 and 1914, several years after his Chicago doctorate (1909). Bloomfield's roots, like those of his contemporaries Boas and Sapir, thus were also in the Neogrammarian tradition, having been trained in comparative linguistics. His work not only reflects this training in the rigors of Junggrammatiker traditions in historical linguistics, but also the specific influence of Leipzig's Wundt on the psychology of the period. It is more than likely that he listened to Wilhelm Wundt's (1832-1920) lectures more than once while at Leipzig, and certainly Wundt's psychology would have been not only fresh in his mind from the experience, but also prominent. Bloomfield was only 26 at this time and 27 when his 1914 book was published, a formative stage in his intellectual career. Wundt's influential Die Sprache was already in its third edition by this time, having first appeared more than a dozen years previously.

Psychology, in the period between 1870 and World War I was largely mentalistic, due largely to the influence of Wundt; its method was largely that of introspection. Wundt is seen by many as the 'father of experimental psychology'; testimony to this is seen in the centennial of the original opening of the Leipzig laboratory recently celebrated by the World Congress of Psychology convening at the same site in 1980. Behaviorist objections to introspection, as personal, unreliable, and trivial, had not yet arisen in any concerted sense. His concern with regularities of overt behavior in an 'experimental' setting, however, was not quite the

the same as later interpretations of what such inquiry might mean, despite the appellation 'experimental'. Psychologists prior to this time found themselves logically within departments of philosophy, just as Wundt himself had; in fact, Wundt first shared the chair of philosophy with a philologist. It was after all only in 1888 that the first professorship of psychology in the world was filled by Cattell (also a student of Wundt's at Leipzig between 1883-1886) at the University of Pennsylvania.

Wundt has been seen of late by many as a kind of early cognitive psycholinguist, very much akin to the modern sense. Some of his ideas have found a renewed respect among many generativistoriented psycholinguists--indeed, Blumenthal (1970) even characterizes him as the 'master psycholinguist'. Wundt was interested in the experimental control of introspection; his functional mentalism allowed experimental approaches to the conditions of a stimulus situation and observation of reported changes in the experience of the observer. In being an advocate of experimental control of any condition in a scientific sense, dealing with introspection or otherwise, Wundt changed the orientation of the field, in fact, allowed the field to become what it had. In admitting both the notions of experiment and mathematical evaluation to psychology, Wundt was following interests that European scholars had in extending notions of the highly successful natural sciences like biology and organic chemistry to the study of human behavior. It was no wonder that his methods were so attractive and that his school at Leipzig attracted younger scholars of the time. It was more than just the intellectual climate that contributed to Wundt's stature; he had a great deal to say and it was worth paying heed to.

His laboratory and lectures were frequented by students and scholars from abroad. Many of the early prominent American psycholo-

gists were one-time students under Wundt, though some later were unhappy about Wundt's influence on American psychology (see Rieber 1980); and the transplanted Titchener and Munsterberg, as well as Angell, Scripture, Pace, Witmer, Judd, and Warren. Although the American version of Wundtian psychology somewhat modified his views (see Blumenthal 1980) it is obvious that he was influential in the early development of American psychological practices through his returning students. One recalls, moreover, that from about 1850 to 1914 much of American academe, particularly the heavily Germansettled Middle West, looked to Central Europe for its model. Leipzig in fact remained a flourishing center of activity until the war and its aftermath disrupted much of European academe, eventually breaking its overwhelming influence in many American circles. (Hall (1969:211) provides some further, more personal, reasons for the decline of European influence in the post-war depression years, when resentment against European scholars in America was also linked to mundane pragmatic forces like job placement, as well as the intellectual ones.)

Language had an important place in Wundt's psychology. His major psycholinguistic work, *Die Sprache* (1900; revised 1904 and 1911-1912), introduced the ten-volume *Völkerpsychologie* series (1900-1920) as the first book of the series. The original 644page volume was even revised to 1378 pages in the 1912 edition (Blumenthal 1979). Wundt had not only a profound impact on the psychology of the time, but he also exerted a certain degree of influence on linguistics at the turn of the century. Wundt was true to his philosophical origins, and everything in its turn was linked, deriving from a set of primary principles, but it was from psychological principles that all others were to be derived, including linguistic principles. Clearly, Wundt was very much aware of

what was transpiring in the adjacent field of philology. Wundt had maintained a running dispute with Paul and in 1901 had answered Delbrück's (1901) critique of his two-volume (1900) *Die Sprache* with a complete statement of what was for him the relationship between psychology and historical and descriptive linguistics. As Baker and Mos note, (1979:3) 'an analysis of the exchanges which took place between Wundt and the linguist, Delbrück, reveals Wundt's extensive command of and respect for the linguistics literature, and his acceptance of the philologist's premise that he could achieve an understanding "of each social group through the analysis of its language, believing that the very vocabulary and grammar of a people reveal its psychic constitution"... The writings over the last 30 years of Wundt's life (1890-1920) clearly reflect his pursuit of this area, an area he had labelled *Völkerpsychologie*.

Bloomfield was very much aware of not only Wundt, but also the specifics of his work. For example, his (1913) review of Wundt's *Elements der Völkerpsychologie*, appearing in the American Journal of Psychology, has nothing but the highest praise for the scope and content of the work. Bloomfield writes that 'the monumental volumes of Wundt's Völkerpsychologie find not only a summary but also a crowning supplement in the *Elemente der Völkerpsychologie*. Here the entire mental history of man is outlined in a continuous narrative ... it is safe to say that no other man could have told the story as Wundt has; his vast learning, powerful psychologic insight, powerful insight, vivid sense of history, and, not least, his stylistic ability to present states of flow and change have produced a work of tremendous and awing effect (1913; Reprinted in Hockett 1970:39)'

In his preface (p. vi), Bloomfield also outlines his intellectual allegiance to Wundt, saying 'it will be apparent, especially, that I depend for my psychology, general and linguistic, entirely

on Wundt; I can only hope that I have not misrepresented his doctrine. The day is past when students of mental sciences could draw on their own fancy or on 'popular psychology' for their views of mental occurrence (1914:vi).'

Bloomfield seems to have acquiesced completely to Wundt's establishment of psychology as the propedeutic science. He observes (1914:322-323) that 'the relation of linguistics to psychology is, on the one hand, implied in the basic position of the latter among the mental sciences. These sciences, studying the various activities of man, demand in differing degrees but nonetheless universally, a constant psychologic interpretation . . . As language is in its forms the least deliberate of human activities, the one in which rationalizing explanations are most grossly out of place, linguistics is, of all the mental sciences, most in need of guidance at every step by the best psychologic insight available.' Wundt's influence is also clearly seen in several early chapters in the book. By far the most interesting chapters in this regard are Chapters I on the nature and origin of language and 3 on the mental basis of language (neatly contrasting with Chapter 2 on the physical basis of language). B-1914 would also have generally subscribed to psychological interpretations of language as an outgrowth of emotional expression, and certainly Bloomfield's discussion of gesture language and the origins of language mirror Wundt fairly closely.

Whether or not one agreed with Wundt, one would certainly have to cope with his ideas on the psychology of language in setting out a detailed explication of what it was that linguistic science was dealing with. One could not ignore him, and in dealing with the topic of language, the options would have been either to agree with him or present compelling arguments why not. There was nothing that would have replaced Wundtian psychology on the same grand scale,

had one the temerity to reject it wholesale. And indeed, there was so much of Wundt, considering the voluminous output which characterized his professional life; Boring (1950) has estimated that Wundt must have produced at a rate averaging 2.2 pages every day between 1853 and 1920 to turn out an astounding 53, 735 pages that one could find argument and counter-argument for most fresh approaches if one took the time to look.

As Lane (1945) observes, Wundt's influence was a force to be reckoned with, and even Paul's fourth (1909) edition of the Prinzipien der Sprachgeschichte continued their dispute by putting its work in perspective as regards Wundt's notions. But by the fifth (1920) edition, 'his quarrel is not with any "system" of psychology, but ... Wundt's operation with a Völkerpsychologie instead of an individual psychology--the same objection he had earlier made to Steinthal and Lazarus' work (Lane 1945:472).' It is unlikely that the early Bloomfield would have been in a position to have challenged or ignored the powerful Wundtian system in his 1914 work. But by 1933 he could and did, to the degree that Wundt was even around to be criticized or ignored. By the time of B-1933 Anglo-American empiricism and pragmatism had already challenged and largely replaced Wundtian psychology. The later Bloomfield, of course, was by then highly formalistic, concentrating on a mechanistic view of language and laying the foundations for a highly operationalized methodology.

But, in all fairness, it should also be pointed out that the early Bloomfield saw the value of language data for psychology as well. Thus, note the following quote where Bloomfield observes that 'psychology makes a wide use of the results of linguistics ... such mental processes, then, as those involved in the utterance of speech cannot find their explanation in the individual,--he receives his speech habits from others, --but must be traced for explanation form individual to individual ad infinitum. They are products of the mental action not of a single person, but of a community of individuals. These products, --not only language but also myth, art, and custom, -- are the data which make possible the second phase of psychology, social psychology (German *Völkerpsychologie*). As language, moreover, is less subject than these other activities to individual deliberate actions which interfere with the communal nexus, it is the most important domain in the study of social psychology.' (1914:323-324)

In looking for beginnings of psychology as a science, Meuller (1979) finds Wundt a more convincing candidate for 'founding father' than other contemporaries like Fechner, Helmholtz, and James for several reasons. There is his publication of the first psychological journal as well as his Leipzig experimental laboratory. But there is particularly the matter of perceived intent--Wundt claimed that he was setting out to establish experimental psychology as a new science, and this he did in the first edition (1873) of Principles of Physiological Psychology ('physiological' having come to mean 'experimental' by this time.) One should still admit, as Mueller (1979) has done, that there were many paths, representing different names, ideas, and events that contribute to the evolutionary development of psychology around the turn of the century. In this respect, the Wundt of 1879 is best seen as a symbol of those converging ideas and events; Wundt is convenient to focus upon, with the highly systematized psychology that he offered. It was perhaps not that Wundt was really the great innovator as much as he was a great synthesizer. His presentation of psychology as a science, and systematic organization of the field must have been attractive to younger scholars like Bloomfield, who in his turn provided the same kind of

systematic synthesis for the study of language. Bloomfield was to do the same for his own field both in 1914 and in 1933. Simply stated, it is not always easy to demonstrate where paradigms leave off and begin, for scholars who are depicted as revolutionizing a field in the Kuhnian sense are usually found to have historical antecedents whose thoughts they carry on (see Percival, 1976, for an example of this in linguistics). Moreover, one must also give some credence to the intellectual climate of the times, such that figures must also be seen in the light of their times and their discipline. Whether our picture of Wundt is an accurate one or not has been questioned by some (Blumenthal, 1979, characterizes him as the'founding father we never knew'). But whether or not Wundt was quietly reinterpreted, by Titchener, and by historian Boring after him, as Blumenthal (1979, 1980) suggests, is a difficulty one faces with most earlier scholars. They are often interpreted through one's own particular training and set of theoretical and methodological positions. For example, Wundt's experimental journal, Philosophische Studien (begun in 1881), is a good indication of Wundt and his contemporaries' feeling that philosophy and psychology were one. Indeed, Wundt himself authored four texts in philosophy between 1880 and the turn of the century. Despite the title and its containing reports of experimental studies from his laboratory, the journal was devoted as much to philosophy as psychology. Wundt did think that philosophy should be more psychological, and in this, he was innovative.

The latter-day student of historical antecedents is often left with a disciplinary history which is written from the bias of the reviewer. Witness, for example, Weimer's (1974a) contrasting of Blumenthal vs. Boring's presentation of Wundt to us or Marshall's (1970) review of Blumenthal vs. Esper's account of the substance and relevance of Wundt to current psycholinguistics. History is not only written by the victors; it happens to be rewritten by every scholar who looks back, and in so doing to some degree squares history with his own theoretical and methodological bias, setting it either at odds or in concert with it.

There is also some questions as to just how 'experimental' Wundt was experimental psychology. According to Blumenthal (1979: 550), 'for Wundt, "experiment" meant the study of processes by means of publicly observable and measurable events.' Introspective reports, in anything like Titchener's style, are indeed very rare in the experiments that come out of the early Leipzig laboratory.' According to Esper (1971), the interpretation is incorrect, and Esper neither sees Wundt as the important psycholinguistic figure that Blumenthal does nor the experimental value of introspection as a psychological method. Even Blumenthal (1979) notes that the massive *Völkerpsychologie* involves no experimentation, despite the attention paid to psycholinguistic matters. As an exceptionally lucid piece of deductive speculation, it is quite compatible, though, with the modern generativist frame of reference.

To some like Blumenthal (1970, 1973, 1975), Wundt's theories seem especially modern. Wundt's goal as a psychologist was 'to give an explicit characterization of the principles that govern the functioning of cognition in humans, and it was his belief that the study of human langauge would provide one of the best means of knowledge about the human mind (Blumenthal 1973:11).' Chomsky's (1968, 1972) injunction about linguistics being a sub-branch of cognitive psychology sounds neither innovative nor startling in this epistemological frame of reference. Moreover, Wundt had similar notions about generating an infinite array of sentences from finite means, a precursory notion of deep and surface structure ('inner and outer forms'), and the idea of the sentence as the basic unit. It is no

wonder that generativists have rediscovered him with such delight.

Thus, for Blumenthal (1970:242) and others, if we now ask 'what is the historical relevance of the new American psycholinguistics as a discipline today?', assuming it to be heavily under the influence of the developments in generative grammar. It is, in fact, in an analogous position to that of Wundt and his followers who in the 1880's opposed the Junagrammatiker (or narrow empiricist) tradition in linguistics because of its strict limitation of linguistic study to descriptions of utterances. The Junggrammatiker had studied only the physical shape of inventories. Wundt then revived the Humboldtian notions about language, essentially the same notions that were recently revitalized in overcoming the limitations of American behavioral linguistics.' Chomsky is pictured as the one who succeeded the preceding paradigm, and in this respect, leapfrogs back as a parallel to Wundtian psychology in both ideas and achievements. (Not everyone, of course, agrees with the thesis that Wundt should be honored as Chomsky's ancestor nor that there is even any honor in such a claim; see Esper 1971). Chomsky claims that linguistics and psychology both had failed to reach their full and proper potential by concentrating instead on taxonomy and empirical studies. They are from the Chomskian point of view trivial in informing us of anything vital about the essential nature of language. Chomsky's traditions instead are to be traced, like the early 1914 Bloomfield, from a tradition more akin to one in which Wundt's interests would have been compatible. For Chomsky, the Descartesian philosophy of language the Port Royal grammarians, and finally Humboldt himself, are more fitting prototype figures in terms of their interests in language.

Linguistics of the last century were more like philologists in our terms. Given the Humboldtian view of things which so permeated

much of the contemporary work on language, one of their chief preoccupations was to determine how ethnic character and culture were reflected in language, how the mind might be differently expressed in different languages. But true to the philosophical proclivities of philologists of the period, there was more sepculation about the nature of such considerations in language than there was actual research into the formal mechanics of given languages. The practical details were often left to missionaries, teachers, and others who had a need for such things, while the underlying human essence of language was considered more properly approached by deductive The influence of ideas about language are seen in Wundt's means. Volkerpsychologie treatment of national psychology and the psychology of language. According to Blumenthal (1973:15), 'this is again the Humboldtian influence . . . the spirit of a society may largely be influenced by the structure and the nature of the language that binds it together.' This tradition, though not necessarily through Wundt, is also found in the work of Boas, Sapir and their followers on this continent, but does not figure as prominently in Bloomfield's followers.

The psychologic interpretation of language was not unique to Wundt. One also sees it in his predecessor Steinthal (1823-1899), as well as in the American philologist Whitney (1827-1894). Bloomfield himself pays homage to this tradition in noting (1914:312) that 'both of these men have been followed by numerous investigators who have contributed to our understanding of the mental processes of speech and of its change and development in time; the great advance of psychology in recent decades and the rise of social and ethnologic studies have been, of course, of the highest benefit to this phase of the science of language.'

Diekhoff's review of B-1914 (1951; reprinted in Hockett 1970)

in fact criticizes Bloomfield for just such notions stemming from following earlier philologists and Wundt a little too closely in their Humboldtian notions of the relationship between language and ethnic character. Diekhoff (1915; reprinted in Hockett 1970:47) observes that 'it is quite true, as our author means to illustrate, "that the categoric and other distinctions of one's own language are not universal forms of expression or of experience"; yet the conclusion ought not to be pressed too hard that the idiomatic differences between various languages indicate a corresponding difference in the mental make-up of the peoples concerned . . . I cannot convince myself that in this outward remedy of a growing indistinctness any corresponding psychological change should have been involved . . . Modes of utterance, or idiomatic turns are very often the result of the most curious historical development, and they no more adequately express psychological analyses, or complex psychological operations, than the sound of the individual word can be said to cover a single psychological concept. Both become conventional . . . ' Diekhoff further criticizes Bloomfield for his treatment of the nature and origin of language, particularly the notion that gesture language was the result of earlier purposeful movements, and that these accompanied by vocal utterances, ultimately form the original basis of language. Diekhoff characterizes this as a matter of faith more than a demonstrable fact, and the faith is obviously one placed in Wundt's notions about gesture language. Bloomfield (1913) himself had noted of Wundt that his discussion of 'the origin of language is splendidly treated . . . toward this we find in the Elemente only a sketch of the origin of vocal language in the light of gesture (Wundt's greatest single linguistic contribution lies here) . . . (Bloomfield 1913; reprinted in Hocket 1970:40).' It

is obvious that B-1914 notions about gesture language and language origins must have been directly distilled from Wundt.

Wundt's work fits between the two positivist cycles in European thought, the first during the mid-nineteenth century, the second around World War I. The second period sees positivism coupled with behaviorism as a popular philosophy of science. Academe witnesses the rise of Anglo-American empiricism and pragmatism, and the strong turn toward a positivist philosophy of science makes for a final undermining of Wundt's place of prominence. With psychology considered a natural science, enhanced by the physiological interests which become so much a part of the discipline, Wundt's concerns are seen as strictly metaphysical and at odds with psychology as a natural science. Eventually, this positivistic view wins out almost completely and Wundt is largely replaced in the discipline's development. (See Danziger 1979, and Blumenthal 1975, for a fuller account of the positivist replacement of Wundt).

Whether Wundt himself presaged the move to radical behaviorism by his pushing psychology out of philosophy into the natural sciences is questionable, but is interesting to note that his desire for a new and independent science using experimental methods in the analysis of mental events does give rise to the next logical step, American positivism taking it to radical behaviorism with its concentration on experimental methods with observable features (see Baker and Mos 1979). Wundt perhaps provided the catalyst by which this turn of events essentially materialized.

The intellectual climate thus changes, and Wundtian mentalistic psychology gives way to behaviorism. By the time B-1933 appears, the decline was largely complete, and German intellectual hegemony was also considerably weakened. Others had also changed their appreciative impressions of Wundtian notions, and Bloomfield was

not alone in having coming full circle in abandoning Wundt's notions on the psychology of language (see Blumenthal 1973:16-17). Behaviorism in psychology had largely captured the American academic imagination, and by the 1920's behaviorism was fashionable not only amongst professional academics, but also in the popular sense. Watson's (1919) Psychology from the Standpoint of a Behaviorist and Weiss' (1924; 1929) A Theoretical Basis of Human Behavior were very much in evidence and were largely to replace what vestiges of Central European functional mentalism remained on this continent. One can see B-1914's adherence to Wundtian dualistic psychology as being quite out of place, had he continued with such loyalties in B-1933. In Mueller's words, (1979:28), 'Wundt's way of thinking about psychology, and the thinking of those that followed in his tradition, did not contain the essential ingredients that could have generated twentieth century psychology . . . one is struck by the fact that all [but Wundt] are characterized by experimental or observational procedures that are still acceptable as bona fide scientific procedure. The one line of inquiry that specified, and was based on, an "experimental" procedure that is not judged acceptable as a method of scientific investigation at the present time is the line established by Wundt. The paradox is that psychology has selected as the founder of its science a man whose line of inquiry brought with it no acceptable experimental method.' Wundtian psychology was simply not compatible with the turn of events in psychology, nor the kind of behavioristic positivism that Bloomfield and others in the 1930's subscribed to. While such Wundtian belief systems were perfectly legitimate in their 1914 context, in 1930 they would have been both outmoded and incompatible with the developments that were taking place in the behavioral sciences in America. In contrasting the two intellectual climates and noting the pendulum swings
between objectivism and mentalism in psychology and other social sciences over the last two centuries, Esper (1978) even suggests that we would have been better off staying where the Bloomfieldian swing took us rather than having continued on into another mentalism, that of Chomskian mentalism. This, however, may be far from a majority opinion in the discipline.

Just as the sociology of Durkheim had an influence on Saussure, so also did the behaviorist psychology of A. P. Weiss (1879-1931) on the later Bloomfield. How highly Bloomfield thought of Weiss is captured in Bloomfield's obituary of Weiss: 'Weiss was not a student of language, but he probably was the first man to see its significance' (Bloomfield 1931; reprinted in Hockett 1970:237). Bloomfield moved from a Wundtian view that language could be accounted for only in terms of human psychology to a more Weissian view that human psychology can only be accounted for in human language terms. According to Hockett (see Esper 1973:xiv), 'Weiss helped Bloomfield to realize that the traditional psychological "explanations" of this or that feature of language were nothing more than paraphrases, in mentalistic terms, of what could be (and often enough already had been) perfectly well described in purely linguistic terms.' Although this particular form of behaviorism did not hold center-stage in either psychology or linguistics, it did have sufficient impact on linguistics to endow the discipline with the particular complexion it had from 1930-1956. Bloomfield had already swing over to the position that it makes little difference which psychology the linguist accepts by the time his (1926) article on 'A Set of Postulates for a Science of Language' appeared, and the period of psychology-independent structuralism probably finds its origins here as well as anywhere else.

Weiss' influence is particularly obvious in Boomfield's (1930)

presentation of 'linguistics as a science.' His beliefs as to linguistics being a science are still firm, but what this entails is somewhat modified from B-1914. No longer does one find explanations of the why of human linguistic behavior; description thereof is sufficient. Bloomfield observes, 'linguists do not pretend explain conditions or changes by saying that the speakers strove toward such an end, such as euphony or clearness, and when linguists speak of a soul or mind, the term is otiose . . . it is true that in the last years some students of language have tried to galvanize the finalistic and animistic factors into some effect upon linguistic forms, but these scholars have in this way produced nothing but less useful restatements of results that were gained by the ordinary methods of linguistic study' (Bloomfield 1930; reprinted in Hockett 1970:229). These Weissian notions are also evident in Bloomfield's criticism (1933:17) of Paul's psychological interpretations of language characteristics, noting that Paul was given to 'statements about language with a paraphrase in terms of mental processes which the speakers are supposed to have undergone. The only evidence for these mental processes is the linguistic process; they add nothing to the discussion, but only obscure it.'

Whether Bloomfield recognized that in the Kuhnian sense psychology is also given to cycles of thought and paradigms of activity is difficult to discern, but like his predecessor Delbrück, he came to eschew such choices between theories in what had become a separate and distinct field. Delbrück's advice (1901), of course, was to simply ignore developments in psychology and proceed on with the linguistic business at hand. In Delbrück's time, the choice was between Herbart and Wundt; in Bloomfield's time the choices were different, but the principle of disciplinary independence remained for Bloomfield to enforce. Wundt had attempted to replace

Herbart's mechanistic and associationistic psychology with a new experimental psychology, seeing psychology as the propaedeutic core science, not one as subordinate to or even partner with other sciences of human behavior. Language is the result of psychological processes, and one extrapolates from this that the study of philology and psychology must be linked. And of course looking at his volkerpsychologie series one can easily see how for Wundt the entire complex of human organizational phenomena is ultimately psychological. Delbrück's conclusion (1901) that it makes little or no difference which system of psychology, Wundt or Herbart, is chosen was for Wundt a rejection of psychology altogether, since Wundt was himself so convinced of the superiority of his own system (see Kantor 1936). Delbrück simply saw no particular advantage in understanding or explaining language by choosing one psychological system over the other, and in so doing, simply rejects them both as interesting, but not germane. In so doing, Delbrück sets the stage for Bloomfield's similar rejection of psychology. While Delbrück might have been some superiority in Wundtian psychology, Bloomfield would have seen the same compatibility in behaviorist psychology; however, a choice is not required, and linguistic science can proceed without being wedded to either.

Thinking probably of Delbrück, Bloomfield (1933:vii) writes, 'in 1914 I based this phase of the exposition on the psychologic system of Wilhelm Wundt, which was then widely accepted, ' but 'since that time there has been much upheaval in psychology; we have learned, at any rate, what one of our masters suspected thirty years ago, namely, that we can pursue the study of language without reference to any one psychological doctrine, and that to do so safeguards our results and makes them more significant to workers in related fields.' That the relationship could even be reversed, with

linguistics having a good deal to offer psychology, is obvious in Bloomfield's (1933:32) suggestion that 'the findings of the linguist, who studies the speech signal, will be all the more valuable for the psychologist if they are not distorted by any prepositions about psychology. We have been that many of the older linguists ignored this [perhaps having Hermann Paul in mind]; they vitiated or skimped their reports by trying to state everything interms of some psychological theory.' Although the B-1933 carefully makes explicit its intention to set aside psychological considerations in delineating linguistics as a science, one should admit that the mechanistic principles of behaviorism were not only more compatible with the new linguistics, but also likely to be preferred.

Thus one comes full circle from B-1914 to B-1933. Though Bloomfield called his 1933 book a 'revised version of the author's Introduction to the Study of Language, ' his reviewers immediately comment on its being a totally new book. For example, Edgerton (1933; Hockett 1970:258), "this is really a wholly new book", Meillet (1933; Hockett 1979:264), 'au lieu de faire de son ancient ouvrage une édition corrigée, il a écrit un livre nouveau fonde sur des theories, purement linguistiques'; Sturtevant (1934; Hockett 1970:265), 'in reality, however, it is a new book.' One also detects a certain relief on the part of some that the early Wundtian allegiance has disappeared. For example, Kroesch (1933; Hockett, 1970:261), 'the author wisely emphasizes the facts of language througout rather than psychological interpretations'; Bolling (1935; Hockett 1970:278), 'the second drive has for its objective the elimination of "psychological explanations" from our work. Again I am in hearty agreement with the author . . . such theories add nothing to our understanding of our own problems . . . '

The fact of Bloomfield's being subject to Kuhnian intellectual

atmosphere considerations does not in any way diminish Bloomfield's stature at either point in his career. As Koerner (1976:708) has suggested, the fact that scholars are reflections 'of their time and not *creatores ex nihlo* does not by any means diminish their attainments; their creativity and originality lie in the very fact that they were capable of making use of the things that were in the air and put forward a synthesis, a general theory of language, in a rigorous manner not proposed by any of their contemporaries.' In Bloomfield's case, this is all the more enlightening for us, for we can observe both the intellectual tenor which went into the molding of the discipline as a separate entity, and then its shift to positivism. For those who fail to see the archeological merit in this stratigraphic layering of the evolution of ideas within our own discipline, it will at least allow them to glimpse their own mentalism through the eyes of an earlier mentalism.

We are at a critical turn in the development of our own understanding of the discipline. Lest we allow the pendulum swings between mentalism and objectivism to presage yet another pendulum swing, we must make the most of what we have learned from the past. As Blumenthal (1974:1131) has noted, 'the real successes of both the comparative linguistics in the twentieth century were concerned with methodology, procedures, and techniques. Those times in both centuries were perhaps paralleled by similar movements within psychology in general. The Wundtians, no less than some recent psychologists, then discovered that positivistic psychology was in need of explanatory theory, of a more sophisticated cognitive psychology . . . 'What we perhaps need now is an informed experimental mentalism, one which allows us to understand mental events, but by inductive means to balance out our deductive speculations about language and cognition. Unless we do so, we risk another swing, perhaps even another exciting new paradigm, but one which ultimately is as informative as the experimental mentalism alternative.

Finally, it might be said that we are in many ways what we were and many of our questions have been also asked in different times and different places. As Percival (1976) has suggested, the history of our discipline, as all others, is a history of the progression of ideas, and B-1914 is a reflection of ideas that come together from a variety of intellectual sources to focus at one point in the history of the discipline that has become the one we are. One welcomes the re-issuing of a classic in our field, and given Bloomfield's intellectual origins and his academic associations, a classic in psycholinguistics as well.

\*Note: This is a preliminary version of a paper prepared in connection with the re-issuing of Leonard Bloomfield's (1914) An Introduction to the Study of Language in the Classics in Psycholinguistics series, Amsterdam Studies in the Theory and History of Linguistic Science.

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## The Genesis of the Pronoun \*ngali in Australia Geoffrey N. O'Grady University of Victoria

The grand historical panorama which unfolds in R.M.W. Dixon's major new work, *The Languages of Australia* (1980), is quite breathtaking. This prolific linguist is to be heartily congratulated on having produced a work which will enable the lay reader to appreciate the scope of the linguistic diversity within Australia, and which at the same time will be of great value to the specialist linguist. Yet the picture which the writer paints of the routes through which this diversity unfolded is, to put it in a nutshell, *too* stupendous. He would have us believe, no less, that he has been successful in reconstructing the ancestor of virtually *all* of the two hundred or more modern Australian languages - languages whose roots in the continent go back to an almost unimaginably remote past.

In the realm of pronominal elements, Dixon has reconstructed, with very great professional skill and insight, plausible protoforms which are in fact essentially ancestral within the context of Hale's Pama-Nyungan construct - a genetic construct which Dixon is at pains to down-grade (e.g., pp. 226-7). He has, indeed, gone on to ascribe these forms, in what can only be described as a complete turnabout of logic, to what he calls 'Proto-Australian'. While this *tour de force* provides us with a far-flung vista extending back in time through an awesome gulf of, conceivably, 10,000 to 70,000 years, it represents a quite unrealistic approach to the problem of linguistic reconstruction in Australia.

I would like further to challenge Dixon in connection with his assertion (p. 256) that Pama-Nyungan 'has not yet been shown

to have any genetic significance.' In this he is, to say the least, seriously in error. Ironically enough, it is he himself who provides, by his masterful reconstruction of verbal conjugations on the evidence of eastern and western Pama-Nyungan languages (pp. 402-26), convincing further evidence for the genetic unity of Pama-Nyungan! Once again, however, as in the case of the pronouns, he ascribes the reconstructed forms and patterns to 'Proto-Australian'. In all fairness, it should be acknowledged that he does single out a few stray nuggets of what looks like hard evidence indeed, in the form of several monosyllabic verb roots, for the positing of a more inclusive genetic grouping than Pama-Nyungan. In another twist of irony, part of the intended contribution of this paper constitutes support for Dixon's basic premise, involving as it does the addition of at least one more nugget, namely an extremely ancient conjoining particle, \*pa, to the Dixon 'Proto-Australian' collection. This accords, one would hope, with the principle that scholars should impartially allow the surfacing of both evidence and counterevidence relating to the hypotheses which they espouse.

On the Pama-Nyungan side of the coin, however, I would point out that as early as 1966 I had recognized over fifty radical elements as showing cognation between Ngayarda languages such as Yindjibarndi and Pama-Nyungan languages in general; and these, in fact, only represent the tip of the iceberg. O'Grady (1966:113) also cites the handful of old monosyllabic verbs, represented in such Proto-Ngayarda forms as \*nha.ku+Y-<sup>1</sup> 'to see', which he recog-

Forms cited are transcribed uniformly in practical orthography, in which  $t^y$ , n, ñ, n are written j, nh, nh, ng, and rhotics r, ř appear as r, rr. Gupapuyngu retroflexed sounds are indicated by underlining, and the symbol n is retained, in accordance with the established orthography. In other languages, retroflexed [+cons]

nizes as having wider than just Pama-Nyungan provenience. On the whole, though, it is becoming more and more evident that there exists a dense network of genetic strands - grammatical and lexical - connecting the various Pama-Nyungan languages. By another stroke of irony, the full documenting of this evidence, because of its very richness, will require a huge expenditure of labour; and this, in turn, means that many years or decades will pass before *all* of the evidence needed for a full riposte to Dixon's attack on Pama-Nyungan can be presented. Later in this paper, I offer an estimate of the number of man-years of labour that will be necessary.

The strands which may be seen as connecting pairs of languages such as Ngandi and Maung are indeed sparse by comparison. This sparseness is emphasized, if anything, by the presence of a tiny handful of extremely hardy individual fibres such as are represented by Ngandi  $m\alpha$ - 'to get, to grab, to pick up' / Maung  $-m\alpha$ -'to take'.<sup>2</sup> These forms must be regarded as having survived through untold millenia with truly exceptional resilience.

O'Grady and Klokeid (1969) attempt to provide an exemplar for the systematic lexicostatistic comparison of all Australian languages, citing in full the 100-item Test List used in twelve communalects of southern Australia, and illustrating problems in the detection and counting of cognates. Cognate densities are seen to range in Australia from values nearing 100% in the case of very close sub-

segments are analyzed as clusters /rt rn rl/. Postposed L, Y on verbs are conjugation markers. Forms from sources dating from the pre-phoneme era are given in capitals. Capitalization of vowels in protoforms implies an absence of cognate material from languages which are diagnostic for vowel length. Dots indicate fossilized morpheme boundaries.

See Heath (1978:154) and Capell and Hinch (1970:173).

2

dialects of a dialect (the figure was 90% in the case of the comparison of the geographically adjacent dialects Yankuntjarra and Antikirrinya) to a putative low of 2% arrived at when comparing, for example, Nyulnyul with Bandjima. The authors take this extreme variation to provide a basis for at least preliminary hypotheses concerning language relationships.

At the same time, O'Grady and Klokeid urge upon linguists the desirability of entirely independent comparisons of Australian languages using, for example, grammatical indices in order to arrive at estimations of the degree of genetic closeness between languages. In addition, they point out that none of these approaches could be seen as superseding in any sense the ultimate massive use of comparative method techniques.

Blake (1977) provides, in effect, a stunning vindication of Pama-Nyungan through his thoroughgoing study of case marking in Australian languages, building on earlier work in this area, especially that of Capell (1956) and Wurm (1972).

O'Grady (1979), for yet another time, pursues the theme of the genetic unity of Pama-Nyungan languages, and seeks to trace some of the interconnecting strands which make up the complex web of Pama-Nyungan diachronic semantics, involving such mechanisms as metaphor, metonymy and, unquestionably, antonymy. Despite ongoing scepticism on the part of Australianists, I will continue to assert my claim that a systematic innovation is beginning to sweep the lexi-con of the well-documented Western Desert Language, and that that innovation is the well-known Australian phenomenon of Initial-Dropping. I plan to demonstrate, when time permits, that Pintupi, as documented in Hansen and Hansen (1974), had made a substantial start along the road leading to the eventual loss of all word-initial  $\begin{bmatrix} +int \\ -cor \end{bmatrix}$  segments, i.e. \*p- and \*k- (and, by implication,

ultimately of all initial consonants). Up to the time of the Hansens' work, at least, speakers appear to have been 'patching up' initial syllables by substituting, in the hiatus arising from initial loss, a non-nasal resonant, effectively y or l (in that order of preference). The structural constraint requiring all words to begin with consonants was thus still inviolate.

The reconstruction of at least Proto Nuclear Pama-Nyungan (PNPN) root elements, as discussed in O'Grady (1979:107-8), thus gains further impetus though the fully justifiable inclusion of Pintupi yiku within the roster of reflexes of the PNPN root \*piku(+ny). In the following tables, initial consonants are enclosed in cartouches.

Gawurna	Р	Ι	K	0,	PIKUPUTI	'eyebrow'
Nuguna	р	i	k	u		'forehead'
Pintupi	У	i	k	u		'face area'
Gupapuyngu	Ъ	u	k	u		'forehead,cliff, face'
Yidiny	р	i	k	u.	ny	'finger- and toe-nail, claw'

Table 1 Reflexes of PNPN \*piku(+ny)

Anticipatory vocalic assimilation is in evidence in Gupapuyngu, as in PNPN \*ngali > Gup.  $\eta ali+nyu \sim \eta ili+nyu$  'we DUAL EXCL(USIVE)'. In the case of Gup. buku, ancestral \*i can reasonably be regarded as having assimilated to the  $\begin{bmatrix} +hi \\ +bk \end{bmatrix}$  tongue position of the following \*u, or of both the \*k and the \*u. Further, the preceding \*p can be seen as a rounding influence operating on the \*i.

Final judgment on the assigning of Yidiny *pikuny* to the same etymon should come from *within* the Australian indigenous culture area. This form is included here because of clear evidence from Nyangumarda and Umpila of a semantic association between 'eye, forehead, nose, face' and 'nail': PNPN \*pIrri(+rn) 'nail' > Nya. pirri.rn 'forehead', and PNPN \*miilpa(+ny) 'eye, face' > Nya. milpi.ny 'fingernail, toenail'. The latter form is also discussed in O'Grady (1979:120). Concerning the status of the fossilized endings .rn and .ny, Alpher (pers. comm.) argues convincingly that historically they had an independent suffixal status.

Evidence from Umpila which dovetails with that of Nyangumarda and Yidiny, above, comes in the form of a doublet: PNPN \*nugrru 'nose' has twin reflexes in this language, namely uu'u 'forehead' and wuti 'fingernail, toenail, claw'. Though the exact lines of evolution of these forms are not known, it is clear that both passed through stages whereby \*ngurru > \*wurru > \*wutu (cf. \*nguna+Y 'to lie, sleep' > Ump.  $wuna+\phi$ ); in the one case \*w then assimilated to the following \*u, resulting in \*uutu ( > uu'u - cf. also Proto-Paman \*kuta.ka 'dog' > Ump. ku'a.ka); and in the other, the final \*u fronted to i following \*t, so that \*wutu > wuti, a development corroborated by PNPN \*paarntu 'all' > Ump. paanti.ku.

As a reminder to the reader that I continue in my attempts to add to the ancestral Pama-Nyungan lexicon, I would urge consideration of the problem of the ultimate etymology of the wellknown Western Desert word yinma 'corroboree/song and dance'. When the possibility is left open that the initial y is historically prothetic, and is 'filling in' for a deleted p- or k-, then things start falling into place: plausible cognates come into focus in Pama-Nyungan languages which are geographically far removed from Western Desert, as is indicated in Table 2.

Pintupi<sup>3</sup> 'corrorboree' i n а m 'music sticks'<sup>4</sup> i 1 Gupapuyngu m а Umpila Guugu Yimidhirr 'to clear the ground Yidiny a+L v.tr. i 1 m р (for a camp)'

Table 2 Reflexes of PNPN \*pirlma

This set, too, may well occasion raised eyebrows on semantic grounds (granted that \*1 undergoes assimilation in manner to a following nasal in the Western Desert language, so that \*pirlma is indicated as the ancestral shape, based on the Pintupi and Gupapuyngu evidence.

The resolution of the semantic 'problem' posed by Yidiny pilma+L, especially for a non-member of a traditional Australian indigenous society, is achieved by returning to the Hansens' large Pintupi dictionary with the question, 'what shapes do verbs of "clearing (the ground)" have in this language?' Appropriate search yields the following further set:

Pintupi	y.	u	r	i+L-	'to wipe, clean an area of prickles, objects etc.'
Gupapuyngu					
Umpila		aa	1	i+L-	'to play, dance, sing'
		aa	1	i+nyu	'corroboree'
Guugu Yimidhirr		uu	r	ii-	'to play, dance'
Yidiny		<del></del>			
Table 3 Reflexes	of	PNPN	**	muri+L	

<sup>3</sup> Ernabella dialect. Cf. also Nyangumarda *yinma* 'corroboree', an evident loan from the direction of Pintupi.

With which compare Rembarrnga prilmurr 'music sticks' (Graham

Here one finds direct semantic evidence supporting the connections implied in the \*pirlma set, but now it is the *phonological* discrepancies that need to be reconciled. This is achieved by postulating ancestral \*yuuri+L, with subsequent independently attested developments as follows:

- (1) Original long and short vowels merge in Pintupi
- (2) Initial \*y drops in Umpila; \*uu assimilates in tongue height to the following [+low] rhotic<sup>5</sup>; and finally, \*r is replaced by glottal stop.
- (3) \*y assimilates for the feature [bk] to the following\*uu in Guugu Yimidhirr.

The semantic 'problem' involving the reflexes of \*pirlma and \*yuuri+L becomes a non-problem if viewed in the context of the semantic POTENTIAL:ACTUAL relationship in Australian languages, brought into focus in O'Grady (1960) and discussed further in Dixon (1972, 1980): the action of clearing the (dancing) ground of sticks, thorns, etc. is conceptualized in the mind of the speaker as 'POTEN-TIALLY (or perhaps better: VIRTUALLY<sup>6</sup>) singing and dancing', just as such meanings as

McKay, pers. comm.). Note that the Gup. forms in Tables 1 and 2 ' could equally well be written with p- since the fortis/lenis opposition for stops in this language is neutralized word-initially.

<sup>5</sup> In sub-environments which are not fully controlled as yet. But it appears that this lowering rule does not affect short \*u - cf. \*kuru(+N) > Pintupi *kuru*, Umpila *ku'u.n* 'eye', and \*ngurru > Gupapyungu Ourru, Warrgamay wutu, Nyawaygi wuru 'nose', Umpila *uu'u* 'forehead' and wuti 'nail'. For further details concerning the Umpila reflexes, refer back to the discussion of \*piku(+ny). The phonetic realization of /uu/ in Umpila ranges as low as [0:], lending added plausibility to the claim that \*uu, but not \*u, lowered to *aa* preceding \*r.

6 Compare the use of the term *virtuel* in Herique (in this volume).

'firewood/fire', 'animal/meat', 'track of snake/snake' and 'hit/ kill' are united within single lexemes. The speaker has the OPTION of making further specification, as in Nyangumarda marlkarri+ngi wirla+rna+rna, 'death+LOCATIVE hit/kill+NONFUTURE+I', 'I killed 3sg'.

Despite possible success in the above attempt at reconciling semantic differences for the reader, (which are certainly real enough from an Indo-European point of view), he or she may nevertheless be led to reject a hypothesis of 'creeping Initial-Dropping' as accounting for the appearance in Pintupi of yiku and yinma, above, rather than, say, \*piku and \*pinma. Some Australianists may incline rather to a borrowing hypothesis, in which case the logical donor language would be Aranda, a full-fledged initial-dropping language lying immediately to the east. Granted that certain individual Pintupi lexemes in y- may well be Aranda loans, the fact nevertheless remains that statistical evidence points decidedly in the direction of *internal change* as being the basic mechanism involved: phoneme-count studies of Pama-Nyungan languages reveal that the relative frequency values for initial p in fully initial-retaining languages cluster around 17%<sup>7</sup>. The corresponding value in Pintupi for this segment is 13%, a figure which I take to be indicative of the loss of roughly one-quarter of all original p- initials across the lexicon. What is crucial to bear in mind is that initials such as j, w and m do not show comparable attrition.

Contrary to Dixon's assertion (1980:256), cited earlier, namely that Pama-Nyungan 'has not yet been shown to have any genetic significance', I would reiterate that Pama-Nyungan is indeed a genetic construct, consisting of clearly related and relatable languages.

<sup>7</sup> Dixon (1977:38) reports this figure for Yidiny, for example, and the same value holds for Bayungu, spoken on the opposite side of the continent.

I would like to round out my foregoing remarks with the following observation: linguists owe it to their students to use the terminology which they have generated in a reasonably consistent manner. It is completely misleading to the student, as well as to the non-Australianist linguistic specialist, to speak of, say, an 'Australian language family' and an 'Algonquian language family' in one breath. The newcomer to Australian linguistics will naturally assume that the detailed and systematic mapping of Proto-Algonquian forms, segment by segment, into daughter languages such as Ojibwa (as in PA \*ke?tike:wa > Oj. *kihtige* 'he farms' - a fully inflected verb form<sup>8</sup>) also has an Australian counterpart: reconstructions numbering one to two thousand, with adequate attestation of the descent of each segment appearing in a given environment or sub-environment (e.g. rhotic flap flanked by high front vowels) in at least half a dozen or so representative daughter languages.

In point of fact, Dixon has been able to propose considerably less than a score of primal shapes which one can confidently conceive of as at least hearkening well back toward 'Proto-Australian'. In sum, then, I urge all who would pause for reflection to do their own careful weighing of the relative degrees of reality attachable to the labels 'Australian Language Family' and 'Pama-Nyungan Language Family'. I, for one, will continue to represent Pama-Nyungan to students and colleagues as the largest coherent genetic construct, i.e. 'language family', in Australia, and 'Australian' as a *phylum*, a more inclusive and more vague grouping of languages, supposedly genetic, but with so few shared elements that the *detailed* mapping of innovations into daughter languages is not possible. I would

8

This reconstruction is #771 given in Aubin (1975).

envision the time depth for a language *family* as ranging from perhaps 3,000 to 6,000 years; that for a *phylum*, possibly 8,000 to 15,000 years - beyond which even the tentative identification of cognate material is probably not possible; cf. Bolinger (1975: 307). This leaves an indeterminate intervening area, the six to eight millenia range, where evidence for the reconstruction of *some* details might be adequate, but lacking for others.

The present paper should be thought of as at least a fourth battery of hard evidence marshalled directly for the defense of Hale's Pama-Nyungan construct - the first three being O'Grady (1966), O'Grady and Klokeid (1969) and O'Grady (1979). I am thus directly contradicting Dixon's claim about Pama-Nyungan's not having been shown to have any genetic significance. I let loose this salvo, then, on the premise that if one waits for the requisite two hundred to three hundred man-years of work on the assembling of all relevant attestation within Pama-Nyungan to be performed before seriously attempting to defend the theory, it could well come to pass, not for the first time in history, that a wrong-minded theory will have gained general acceptance.

In saying this, I intend in no sense to convey the impression of an overall condemnation of Dixon's latest work. It represents, in point of fact, a deeply impressive achievement. I feel that its author is entirely correct to the degree that he ascribes extreme antiquity to the handful of monosyllabic verb roots on which his postulated 'Australian Language Family' mainly rests (1980:402-11). But this antiquity, clearly recognized by Capell forty years ago, is surely of an order vastly greater than that normally associated with language FAMILIES. I therefore reiterate my assertions made over the years, e.g. in O'Grady (1966:113; 1979:108-9) to the effect that a handful of radical elements evidently predate Pama-Nyungan, but

that the wast majority of widely reconstructible grammatical markers and word roots in Australia, appearing in languages ranging from Bayungu to Pittapitta to Umpila, and surely to number well over a thousand eventually, will turn out to be reconstructible only on the evidence of Pama-Nyungan languages, or on the basis of cognate sets established within various subgroups thereof.

To pursue this vein further, I would challenge Professor Dixon to follow up whatever Pama-Nyungan reconstructions I and others may put forward, e.g. \*piku(+ny), \*pirlma, \*pIrri(+rn), \*miilpa(+ny) \*ngurru, \*paarntu and \*yuuri+L posited earlier in these pages, and demonstrate plausible cognates (not reflexes!) in well-documented non-Pama-Nyungan languages such as Alawa, Ngandi, Maung and Ungarinyin. To the extent that cognate shapes fail to appear, he will increasingly have to rely on his basic thesis that many (most?) elements of 'Proto-Australian' have fallen into desuetude in the non-Pama-Nyungan languages of northern Australia. This, I take it, is the argument which he advances to account for such absences as that of the 'Proto-Australian' first person dual pronoun \*ngali through virtually the whole of the area of Capell's 'prefixing' languages (largely coterminous with Hale's non-Pama-Nyungan) in the north. He has thus, in effect, adopted a 'Desuetude Hypothesis' to account for the gap and the instances of non-cognation seen in the following array.

Family <sup>9</sup>	Language	First Person Dual Inclusiye Pronoun
Pama-Nyungan	Wembawemba	ngalein
	Gumbainggir	ngalii
	Nyawaygi	ngali
	Umpila	ngali
	Wadjuk	NGALLI <sup>10</sup>
	Bayungu	ngali <sup>11</sup>
	Nyangumarda	ngali
	Gupapyungu	ŋali
Maran	Alawa	nyalu
Gunwinyguan	Ngandi	nyaka
Iwaidjan	Maung	
Tiwian	Tiwi	mua

Table 4 First Person Dual Inclusive Pronouns

According to the Dixon scenario, \*ngali is almost unimaginably archaic in Australia (10,000 years old? 20,000 years?), and in the more recent history of northern languages such as Alawa, Ngandi and Maung (if not Tiwi) has simply disappeared without a trace. For untold eons, in other words, generations upon generations of speakers of virtually all Australian languages pronounced the segments [n], [ $\Delta$ ], [1] and [i] in that sequence, with nary an umlauting of the [ $\alpha$ ] by the [i], palatalizing of [1] preceding [i],

<sup>9</sup> Family affiliations are as originally postulated by Hale, and reported on in O'Grady, Voegelin and Voegelin (1966).

<sup>10</sup> Glossed in Moore(1884) as 'p. dual - we two; brother and sisters; or two friends'.

11 The inclusive: exclusive distinction is not present in Bayungu.

the weakening of  $[\eta]$  to [w], or whatever<sup>12</sup>.

To my mind, such a claim is simply not plausible. I would like to put forward the proposal, again *contra* Dixon, that \*ngali is a quite modern innovation on the Australian linguistic scene. I would claim, in fact, that this form probably made its first appearance as a pronoun only after at least 90% of the period of unchallenged tenancy of Australia by its original settlers had run its course; \*ngali, in other words, could well be less ancient than the oldest of the Egyptian pyramids - dating to perhaps only 4,000 years ago. The elements which went into the creation of \*ngali, I would like to suggest, were the extremely archaic Australian first person pronominal base \*nga- and an old conjoining particle, \*li. The resultant \*nga+li 'I+AND (thou)' had specific first person dual reference.

So long as the Dixon scenario is more or less turned turtle in this manner, perspective suddenly reasserts itself, and a direction of focus more in keeping with the uncanny overall Sprachgefühl of such linguistic giants as Hale emerges. We now see a non-Pama-Nyungan language such as Maung (which lacks dual pronouns entirely) as never at any point in its history having had a dual pronoun con-

<sup>12</sup> Until a relatively few centuries ago, that is. Developments whereby \*ngali > Thargari ngadi Yinwum le- (with umlaut followed by  $C_1$  and  $V_1$  loss) Awngthim lay (with  $C_1$  loss and metathesis), and Kala Lagaw Ya ngoey ~ ngoel- (with raising of \*a to /ə/) are all evidently quite recent. The KLY form has undergone a switch in pronominal function in addition to sound shift: it is now a first person plural exclusive pronoun; by way of exchange, a cognate of Proto-Paman \*ngampa  $\approx$  cited in Hale (1976:57) as a first person plural inclusive pronoun, descends in KLY as ngaba 'we DUAL INCL'. The KLY pronoun ngalpa 'we PLUR INCL' evidently reflects ancestral Pama-Nyungan \*ngali \*pa, and compares well with the Warlpiri plural form given in Table 5, as does the secondary KLY form ngalbay 'we DUAL EXCL'. stituted as ng-a-l-i; in other words, dual pronouns simply never evolved in Maung. Yet uncannily enough, the seeds still exist in this language which elsewhere, namely in an early ancestral stage of Pama-Nyungan, were to fuse into this form.

It remains to make a case for the proposed development \*nga-+ \*li > \*ngali. This will be attempted by first citing what has to be entirely independent and incontrovertible evidence pointing to pronominal genesis elsewhere in Australia, in which the two radical building blocks have been a pre-existing pronominal and a conjoining particle. The evidence will involve a close examination of an extremely archaic, possibly Proto-Australian(!) particle \*pa, briefly mentioned at the beginning of this paper (and see also footnote 12).

In their Maung Grammar, Capell and Hinch (1970:91) make it clear that among Australian languages this northern tongue is exceptionally rich in monosyllabic conjoining elements. One of these, pa, joins clauses, as in *kiwuwunlar*, pa kapijalakaken 'they finish it and they scatter' (Text 7, p. 125). From the opposite geographical extreme of Australia, Hercus (1969:213) cites Wembawemba *mir* pa kanyuk 'eyes and nose' as an example of the function of pa 'and' in that language. In the west of the continent, Nyangumarda shows a cliticized use of \*pa, as in *mayi+pa* kyui+pa 'food and meat', comparable to Sanskrit -ča, Greek -te and Latin -que (PIE \*k<sup>W</sup>e), but appearing on *both* conjoined constituents. Nyangumarda kujarra+pa kujarra ('two and two') 'four' is evidently a fixed locution.

In the east, Yidiny appears to lack an AND-like particle, or any `any conceivable reflex of \*pa, except - and this is crucial - as a final syllable on the pronoun *nyuntuu+pa* 'you NON-SING'. That

this is transparently a suffix is seen by comparison with the second person singular pronoun *nyuntu*, discussed in Dixon (1977;165, 177). Dixon argues, quite correctly I feel, that *nyuntuupa* has supplanted earlier \*nyurra, the well-attested Pama-Nyungan second person plural pronoun. Yidiny *nyuntuu+pa*, then, is taken to be historically 'thou+AND (thou)'.

In the centre of the continent, Warlpiri, as reported on in Hale (1974:5), shows a directly comparable but quite independent development, in which an old first person plural inclusive pronoun has been replaced by a formation which is transparently based on *ngali* 'you and I':

Language	First Person Dual Inclusive Pronoun	First Person Plural Inclusive Pronoun
Nyangumarda	ngali	nganyjurru
Warlpiri	ngali	ngali+pa
Yidiny	ngali <sup>13</sup>	nganyji

Table 5 First Person Dual and Plural Inclusive Pronouns It is clear that the Nyangumarda and Yidiny forms are cognate, while the Warlpiri plural form reflects \*ngali+pa 'we two+AND (thou)'. Ultimately, then, Warlpiri *ngalipa* shows historical cliticization twice over: \*nga- \*li > \*ngali, followed by \*ngali \*pa > *ngalipa*, etymologically 'I AND (thou) AND (thou)'. And so history repeats itself in the selfsame language, leaving to the historical linguist the task of peeling off successive

<sup>13</sup> Dixon (1977:166) describes Yidiny ngali as 'a marked "dual" form, making a further, optional distinction within non-singular'.

layers of evidence.

A further instance of the fusing of \*pa on to a pronoun is to be seen in the second person dual pronoun in Pama-Nyungan languages, commonly attested as nyu(m)pala/u. If, *contra* Dixon (1980:336-46), we take the Pama-Nyungan second person singular pronominal base to have been \*nyun<sup>14</sup>, then the second person dual form can be viewed as being transparently based upon \*nyu(n) + \*pa (> \*nyu+pa in some dialects of PNPN, \*nyu+pa in others). At a very early stage in Pama-Nyungan, a further conjoining particle, \*la<sup>15</sup>, came to be cliticized in turn, resulting in \*nyum+pa+la, \*nyu+pa+la. Through paradigm pressure from such forms as \*nyun+tu 'thou+ERGATIVE', variant forms such as Bayungu *nhupalu* 'you two' arose.

If first and second person dual pronouns in Pama-Nyungan are to be seen as relatively recent compound forms based on much older elements, it is reasonable to conclude that 'pronouns' such as Gawurna *PURLA*, Nyangumarda *pulany* 'they two' are innovatory in function at least, if not in form. That this is so is borne out by Bandjalang *pulaa*, Wadjuk *BULA*, Gupapuyngu *bulal'*, the referents of which are in each case 'two' rather than 'they two'. The old numeral was pressed into service as a pronoun, thus completing the fleshing out of a dual pronominal paradigm.

All of these developments completely escaped languages such as Maung, perhaps even to some extent *because* this language had - and has - a very natural device used in expressing such notions as

<sup>14</sup> Dixon may be correct (p. 344) in his assertion that monosyllabic *nyun* appears in no Australian language. But note Wirangu *nyurni* 'you (SG)'.

<sup>15</sup> In Maung, according to Capell and Hinch (1970:91), la is used in conjoining either nouns or clauses. 'you and me': a conjoining particle which was, and is, used specifically and solely in linking *pronouns*; and the shape of this particle is *li* ! The crucial example given by Capell and Hinch is *ngapi li yanat*<sup>16</sup> 'I and he'. Here, then, is the evidence which, I feel, clinches the hypothesis that *compounding* has been the basic process by which dual pronouns in Pama-Nyungan have been formed.

If Maung is to be seen as a language which is related to Pama-Nyungan, albeit very remotely, the data for this language indicate very clearly that the period during which Maung has been evolving independently of Pama-Nyungan has been very long indeed - perhaps eight to twelve millenia. Among northern Australian languages Maung, at least, is not in any sense a Pama-Nyungan language: it essentially lacks the pronominal forms which are generally diagnostic for Pama-Nyungan languages, for the simple reason that it never had The languages with ngali 'we two', set out in Tables 4 and them. 5, show such striking agreement in meaning and form for this and other pronouns because they inherited them in single chunks which had coalesced out of older elements at the time when Proto Pama-Nyungan had hardly begun to differentiate; and Pama-Nyungan itself is quite modern, relatively speaking - almost certainly not as ancient as Proto Indo-European.

The lines of investigation pursued here have probably not been exhausted. In particular, the archaic conjoining particle \*pa has probably come to perform double, or even treble, duty in languages

<sup>&</sup>lt;sup>16</sup> In view of the appearance which Maung gives of extensive wordinitial lenition, this form should be noted as a possible cognate of PNPN \*jana 'they PLUR'.

such as Nyangumarda, where a marker -pa occurs in relative clauses, as in *nyarra+lu yi+nya+ny+a tupawurn*, *Ngaru+ngu+pa wani+nyi* (Ngulibardu dialect) 'that (remote)+ERGATIVE give ( $yi+\ldots+a$ ) PAST+ me two pounds, white sand+LOCATIVE+RELATIVE live+NONFUTURE', 'that man who lives in Port Hedland gave me £2'. The third function of \*pa which has evolved in Nyangumarda is its appearance as a phonological device which has the effect of preventing non-permitted consonant clusters from occurring. In the process, it has lost all semantic content. Thus verb forms such as ka+nya+rna 'carry+PAST+I, 'I carried it' and ka+nya+n 'you-SING carried it' can be further combined with -lu 'for him': ka+nya+rna+lu 'I carried it for him'; but since \*nl is a non-permitted sequence, for 'you-SING carried it for him' one must say ka+nya+npa+lu.

In languages such as Western Desert and Yinggarda, a phonological device consisting of the syllable *pa*, placed in word-final position, ensures that all inherited forms with final consonants end with a vowel. So, for example, in Bayungu the absolutive (uninflected) case form of 'woman' is *nyanyjil*, and the ergative *nyanyjil+tu*. The corresponding forms in Yinggarda are *nyanyjil+tu*. Here again, *pa* is to be thought of as being totally devoid of any lexical or grammatical content.

Perhaps this paper has helped pave the way, in a sense, to a vindication of *both* Hale's 'Pama-Nyungan Family' Hypothesis and Dixon's 'Australian Family' Hypothesis. To the extent that further reconstruction of affixal and root elements in Australia has to proceed on the basis of Pama-Nyungan evidence *alone*, Dixon's 'Proto-Australian' theory is left high and dry. But to the degree that Maung nga(pi), pa, li and la can be plausibly assigned cognation to segments long since fused into pronominal forms in PamaNyungan, and because of further nuggets, such as -ma- 'to take', Maung must be seen as being ultimately related to Pama-Nyungan. In this regard, Dixon is indeed vindicated, in the sense that there *is* evidence - thin and sparse though it be - for the genetic relatedness of most Australian languages. In no way, however, can a dozen or so probable cognate elements be equated to the concept 'protolanguage' as exemplified by Algonquian, Salishan, Wakashan, Indo-European - and, yes indeed, by Pama-Nyungan!

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On Japanese Verb Morphology

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In this paper, I will attempt to present a possible analysis of Japanese verb morphology in the light of the several different derivations of the past tense forms of Japanese which have been offered in the literature. Selected data will be considered in the frameworks of Transformational Generative Phonology (henceforth TGP) and Natural Generative Phonology (henceforth NGP) for the purpose of accounting most effectively for the alternations observed in the language.

Selected data from Japanese verbs shown in three different forms, i.e., simple present, polite present, and past tense are illustrated in Table 1.

	Simple Present	Polite Present	Past	Gloss
(1) (2) (3) (4) (5)	tabe+ru noke+ru mi+ru ki+ru ne+ru	tabe+mas+u noke+mas+u mi+mas+u ki+mas+u ne+mas+u	tabe+ta noke+ta mi+ta ki+ta ne+t <b>a</b>	'eat' 'remove' 'see' 'wear' 'sleep'
(6)	kir+u	kir+i+mas+u	kit+ta	'cut'
(7)	ner+u	ner+i+mas+u	net+ta	'knead'
(8)	mat+u	mat+i+mas+u	mat+ta	'wait'
(9)	kat+u	kat+i+mas+u	kat+ta	'win'
(10)	yob+u	yob+i+mas+u	yon+da	'call'
(11)	asob+u	asob+i+mas+u	ason+da	'play'
(12)	yom+u	yom+i+mas+u	yon+da	'read'
(13)	tum+u	tum+i+mas+u	tun+da	'pile'
(14)	sin+u	sin+i+mas+u	sin+da	'die'
(15)	das+u	das+i+mas+u	das+i+ta	'put out'
(16)	hanas+u	hanas+i+mas+u	hanas+i+ta	'speak'
(17)	kak+u	kak+i+mas+u	ka+i+ta	'write'
(18)	suk+u	suk+i+mas+u	su+i+ta	'become empty'
(19)	oyog+u	oyog+i+mas+u	oyo+i+da	'swim'
(20)	kaseg+u	kaseg+i+mas+u	kase+i+da	'earn'

Table 1.

It is a well-known fact that Japanese verbs fall into two categories according to whether the stem ends in a vowel or a consonant, and are hence referred to as vowel-stem and consonantstem verbs, respectively. The first five verbs in Table 1 contain the stems, *tabe*, *noke*, *mi*, *ki*, and *ne* to which are added a present marker /-ru/, a politeness marker /-mas-/ and the past tense marker /-ta/. The remaining verbs have /-u/ in place of the /-ru/ in the simple present and have an /-i-/ before the /-mas+u/ in the polite present. As the items from (6) to (20) show, all of these verbs have stems ending in consonants. Thus, the verbs (1) to (5) can be referred to as vowel-stem verbs which end either in /-e-/ or /-i-/, and the rest of the verbs can be referred to as consonant-stem verbs.

Before taking up the problems of the several past tense formations, let us consider items (4), (5), (6) and (7) in Table 1. As the data show, (4) and (6) have the same surface forms and so do (5) and (7), as in /kiru/ and /neru/, respectively. However, the polite present forms of these verbs vary, as illustrated in Table 1; that is, /ki+mas+u/ versus /kir+i+mas+u/ and /ne+mas+u/ versus /ner+i+mas+u/. Here the distinction between vowel-stem and consonant-stem verbs is necessary to arrive at the proper derivation. That is, setting up /ki-/ and /ne-/ as underlying forms for the former and /kir-/ and /ner-/ for the latter, /ki+mas+u/, /ne+mas+u/ and /kir+i+mas+u/, /ner+i+mas+u/ would be respectively obtained for the polite present forms. Without this distinction of the verb stems, proper derivations of the polite present forms would not be obtained. It is thus necessary to establish the two categories of vowel-stem and consonant-stem verbs.

The next problems to be raised are as follows:

1. Why does /-ru/ appear for the vowel-stem verbs while /-u/,
which is regarded as the basic form of the present morpheme,
appears in consonant-stem verbs ?
2. How is /i/, which is manifested between the stem and the polite present marker in the case of consonant-stem verbs, motivated ?

Here, I would like to explore the application of the notions of 'Syllable Structure Constraints' and 'Strength Hierarchy',<sup>1</sup> posited by Hooper (1976) to an explanation of the above facts. With regard to srength hierarchy, Foley may be the first linguist advocating this concept in the discipline; thus, both Foley's (1977) and Hooper's treatments are used in this paper.

As far as syllable structure constraints are concerned, Japanese has a very restricted set of possibilities for syllable structures: syllables are basically open; moreover, there appears to be preference for consonant-vowel syllable structure over a vowel, and even a vowel-vowel sequence is not preferred. Consonants are thus inserted in the environment of VV sequences, such as /haru/ 'spring' + /ame/ 'rain' → /haru+s+ame/ 'spring rain' or /ko/ 'little' + /ame/ 'rain' → /ko+s+ame/ 'drizzle'. Also, in Modern Japanese, /y/ glides are found in a hortative formation of the verb, such as  $/de/ + /oo/ \rightarrow /de+y+oo/$  meaning 'let's go out' or  $/kari/ + /oo/ \rightarrow /kari+y+oo/$  meaning 'let's borrow'. Further epenthesis is exemplified in Miller (1967:209). He notes that the appearance of Japanese /-r-/ is often found where Old Japanese borrowed Chinese words as loans or transcription value. For instance, tsan meaning 'praise' was borrowed as /sa+r+a/ and chün meaning 'rapid' came into the language as /su+r+u/. Miller shows great interest in the sporadic appearance of Japanese /-r-/ in these items; however, he does not fully explain the source of the /-r-/.

<sup>1</sup> The universal strength hierarchy scale proposed by Hooper (1976:206)

5. voiceless continuant, voiced stop, 6. voiceless stop. (strongest)

<sup>1.</sup> glides (weakest), 2. liquids, 3. nasals, 4. voiced continuant

These examples show the strategy of avoiding the VV sequence. It is thus necessary to have the insertion of /r/ in the case of the present form of /tabe+r+u/ 'to eat' or /oki+r+u/ 'to rise' to avoid a VV sequence in \*/tabe+u/ or \*/oki+u/. The reason for the inserted consonant being /y/ or /r/ is to be seen in the light of Hooper's strength hierarchy; that is, these two consonants are classified as the weakest consonants of all. However, /s/, which is placed at the higher end of Hooper's universal scale, is also involved in epenthesis.<sup>2</sup> It is thus appears that Hooper's scale is not sufficient to account for this fact. With respect to this point, Foley (1977) proposes several different strength hierarchies among phonological elements based on an abstract relation. One of his phonological relations is called the  $\rho$  phonological relation whose parameter is relative resonance. Adhering to his notion of the  $\rho$  relation, the strength scale is as follows (1977:35).

(weak) t s n 1 (strong)

According to this scale, /s/ is placed at the weaker position and the following phonetic change from Vulgar Latin into Modern Italian, as in *collis*  $\rightarrow$  *colle* 'hill', *vocalis*  $\rightarrow$  *vocale* 'vowel', involving the loss of final /s/ suggest that /s/ is a weak consonant in terms of the  $\rho$  phonological relation. Korean and Turkish both show evidence of a similar rule of consonant epenthesis. In the case of the former, /r,n/ are inserted and in the latter, /y,n,s,ş/. They have the effect of preventing a VV sequence in the language in much the same fashion as happens in Japanese. This is illustrated by forms like in Table 2. It is interesting to note that Turkish shows the strong group of consonants, i.e., /s,ş/ in terms of Hooper's scale, also acting as epenthetic consonants. Further investigation is needed to account for this fact, even if the epenthetic /s/ seems to be accounted for adopting Foley's  $\rho$  phonological scale.

<sup>&</sup>lt;sup>2</sup> Japanese /s/ epenthesis occurs only with one lexical item /ame/ 'rain'. e.g. /kiri/ + /ame/  $\rightarrow$  /kiri+s+ame/ 'foggy rain'. Thus, the /s/ may be regarded as a morphological rule.

Korean:

/r/: Accusative case -c: /nom/ + /u1//nom+w1/ 'body (accusative)' -v: /namu/ + /w1//namu+r+w1/ 'tree (accusative)' /n/: Nominative case -c: /pyə1/ + /un/ /pyə1+un/ 'star (nominative)' -v: /pi/ + /un//pi+n+wn/ 'rain (nominative)' Turkish: /y/: Definite object case  $-c: /k_{1z} / + /i/$ /k1z+1 gordum/  $\rightarrow$ 'I saw the daughter' -v: /kedi + /i/ /kedi+y+i gördum/ 'I saw the cat'  $\rightarrow$ /n/: Genitive 'of' -c:  $/k_1z/ + /in/ \rightarrow$ /k1z+1n/ 'of the daughter'  $-v: /kedi/ + /in/ \rightarrow$ /kedi+n+in/ 'of the cat' /s/: 'his'  $-c: /k_1z/ + /i/$ /k1z+1/ 'his daughter' -v: /kedi/ + /i/ /kedi+s+i/ 'his cat'  $\rightarrow$ /ş/: 'each' 'one each' -c: /bir//er/ /bir+er/ +→ -v: /iki/ /er/ /iki+s+er/ 'two each' + → -c: /dokuz/ +/er/  $\rightarrow$ /dokuz+ar/ 'nine each' 'six each' -v: /alt1/ + /er/ /alt1+s+ar/  $\rightarrow$ 

N.B. The above high-vowel suffixes undergo a four way i/ü/1/u alternation and the non-high suffix an e/a alternation according to the vowel of the preceding syllable. Table 2.

Just as epenthetic consonants appear in certain phonetic environments between underlying adjacent vowels, so are vowels inserted in almost all contexts where a CC sequence would otherwise occur.<sup>3</sup> Thus morpheme structures such as CVCV and CVCVCV are

<sup>&</sup>lt;sup>3</sup> Since mora phonemes such as /N/ or /Q/ appear in the morpheme structure in Japanese as in CVNCV or CVQCV, we cannot say that vowel insertion occurs in all phonetic environments of CC sequence. These /N,Q/, however, have moraic status, and thus are naturally excluded from a true consonant value. This being so, all phonetic environments of CC sequence undergo the vowel insertion rule.

generated in the surface realization of forms. Evidence for this process comes especially from recent loanwords in the Japanese language. For example, *labour strike* is borrowed as /sutoraiki/; *brake* is /bureeki/; and *beefsteak* is assimilated into the language either as /biFuteki/ or /biiFusuteeki/<sup>4</sup>. In most cases, an epenthetic high vowel has the effect of preserving the basic consonant-vowel (CV) syllable structure of Japanese. It is interesting to note that the high vowels, i.e., /i,u/ are the only vowels that undergo the devoicing rule in the appropriate phonetic environment, such as [sika] 'deer', [nasi] 'nothing', [kuse] 'habit' or [ongaku] 'music'. Furthermore, consider the following example:

karasu	to	kitsutsuki	wa	tori	desu
[karasw	to	kįcucukį	wa	tori	des]
raven	and	woodpecker	SUB	bird	are
' The raven	and th	e woodpecker are	birds.'		

Here, the /u/ of /desu/ which appears in phrase-final position is completely deleted. Suffice it to say that these high vowels are the weakest vowels in terms of strength hierarchy. They thus readily appear as epenthetic vowels and by the same token, /u/ is optionally deleted in phrase-final position as illustrated above.

Since the two Japanese high vowels /i,u/ are considered as the weakest vowels in terms of strength hierarchy, we can say that the aforementioned /i/ in the polite present of consonant stem verbs is to be regarded as a vowel whose insertion has the effect of preserving the consonant-vowel (CV) syllable structure. Otherwise, forms such as \*/kir+mas+u/ or \*/asob+mas+u/ would appear in the surface representation.

<sup>4</sup> F = bilabial fricative.

Now, I turn to the discussion of the past tense forms of Japanese verbs. At a glance, the several allomorphs of the past tense marker /-ta/, such as /-da/, /-i+ta/, and /-i+da/ are recognized in Table 1. With respect to these allomorphs, McCawley (1968:96) states:

The simplest generative description of these forms is clearly one which takes /ta/ as the basic form of the past tense morpheme and has rules for assimilative and other changes which take place when the /t/ of the ending is immediately preceded by a consonant. A voicing assimilation rule is needed to account for the forms which have /da/ instead of /ta/.

As for the derivations of items (6) to (9) in Table 1, the regressive assimilation rule which was first posited by Kuroda (1965:210ff) and developed by McCawley (1968:124,5) seems to be sufficient to account for these derivations. The rule which is proposed by McCawley is given below;

R1. regressive assimilation rule A:

 $[+cons] \longrightarrow \begin{bmatrix} \alpha grv \\ \beta cmp \\ \gamma shp \end{bmatrix} / \begin{bmatrix} \alpha grv \\ \beta cmp \\ \beta cmp \\ \gamma shp \end{bmatrix} (McCawley 1968:125)$ 

R2. regressive assimilation rule B:

 $\begin{bmatrix} +\cos \\ +voc \end{bmatrix} \longrightarrow [+obs] / \_ & \delta^5 [+obs]$  (McCawley 1968:124)

The above assimilation rules, however, will be revised later with respect to the other phonological rules.

Let us next consider items (10) to (14) of Table 1. Here, two other rules should be considered as yielding the correct surface representations. The voicing and nasalization rules which were also posited by Kuroda (1965:212) are the two new ones. These two rules are shown in R3 and R4.

5 & = morpheme boundary.



These rules, however, should be ordered as R3, R4, and then either R1 or R2, otherwise the correct surface representations cannot be obtained.

In items (15) and (16) of Table 1, the high front vowel /i/ appears between the two morphemes, i.e.,  $/das/ + /ta/ \rightarrow$ /das+i+ta/ and /hanas/ + /ta/  $\rightarrow$  /hanas+i+ta/. Since these stems are categorized as consonant-stem verbs, the high front vowel must be regarded as being epenthetic. The high front vowel is thus treated as an inserted vowel which has the effect of retaining the consonant-vowel (CV) syllable structure. This is accounted for by incorporating the notions of strength hierarchy and syllable structure constraints.

Finally, consider the last four items (17) through (20) of Table 1. These verbs contain either underlying final /-k/ or /-g/. They undergo deletion of final consonants, i.e., /-k,-g/, and the high front vowel /i/ appears between the verb stem and past tense morpheme as illustrated in Table 1. The inserted high front vowel /i/ seems to be accounted for in the same way as in the polite present forms (6) through (20) as well as the aforementioned underlying final /-s/ stem verbs. The problem here is to account for the deletion of the /-k/ or /-g/. McCawley (1968: 96) proposes a rule making velars [+cont] before the past tense ending, i.e., turning /k,g/ into /h/. This rule is given in R5.

<sup>&</sup>lt;sup>6</sup>The features,[+son] and [-nas]are added by the author in order to block incorrect application of the R3 in the case of some forms, as in  $/kir/ + /ta / \rightarrow */kir+da/$ .

R5. /h/ conversion:



Following this analysis, the /h/ created by rule R5 is turned into /?/. McCawley (1968:80) maintains a rule whereby an intervocalic /h/ is converted into glottal constriction /?/ unless an external boundary /:/, which is marked in the lexicon between the two components of a Noun + Noun compound, appears before the intervocalic /h/. The retention of intervocalic /h/ is illustrated below.

Retention of intervocalic /h/:

/hoo:hoo/	'method'	/ke:hai/	'sign'
/ku:huu/	'schme'	/ya:hari/	'as expected'

The above examples are historically compounds and are all written with two Chinese characters in the standard orthgraphy as shown below;

/hoo:hoo/	/ku:huu/	/ke:hai/	/ya:hari/
方法	工夫	负配	矢張

It should be noted that McCawley (1968:96) proposes a rule which deletes /?/ when it occurs between certain vowels, but he does not discuss it completely. I will therefore consider this problem in more detail. Additional verb roots with final underlying velars are illustrated in Table 3.

	Present	Past	Gloss			Past	Gloss
(1)	kik+u	ki+i+ta	'listen'	(6) *CVC	Cig+u		
(2)	sek+u	se+i+ta	'hasten'	(7) huse	eg+u	huse+i+da	'defend'
(3)	mak+u	ma+i+ta	'sow'	(8) kag+	-u	ka+i+da	'smell'
(4)	tok+u	to+i+ta	'solve'	(9) isog	g+u	iso+i+da	'hurry'
(5)	huk+u	hu+i+ta	'wipe'	(10) nug+	-u	nu+i+da	'take off'

Table 3.

With respect to the /k,g/ deletion , R5 applies following all five of the Japanese vowels, /i, e, a, o, u/ and preceding the past tense suffix. Furthermore, the rule is reminiscent of the diachronic change from Vulgar Latin into Modern French, whereby eventual loss of medial \*k occurred following its palatalization. This diachronic change can be observed in the following examples:  $factum \rightarrow fait$  'fact',  $lactem \rightarrow lait$  'milk', and  $noctem \rightarrow nuit$ 'night'. In Finnish, the deletion of /k/ is apparent in both noun and verb morphology. For instance, joki 'river (nom. sing.)' appears in the genitive as joen, i.e., with deletion of the /k/: This, and further examples, are set out below.

joki 'river (nom. sing.) joen 'river (gen. sing.)' : 1 virka 'government service 'government service : viran (gen. sing.)' (nom. sing,)' lukee 'he reads' : luen 'I read'

This phonetic change is well accounted for by using another strength hierarchy: Foley's  $\alpha$  and  $\beta$  strength hierarchy (1977:34).

According to Foley (1977:107), weak stops, such as /k,g/ which appear in weak environments, such as intervocalic position further weaken and will be deleted under certain phonetic conditions. With regard to the inserted /i/ of Table 3, the insertion is well motivated in terms of syllable structure constraints. Accordingly, the following ordered rules are set up with respect to the verb roots with final underlying velars. R6. /h/ conversion:



R7. /i/ insertion:

$$\phi \longrightarrow \left[ \begin{array}{c} + \text{syll} \\ + \text{high} \\ + \text{front} \end{array} \right] \left/ \left[ \begin{array}{c} + \text{cons} \\ + \text{cont} \\ - \text{nas} \end{array} \right] \right|_{-----} + C$$

R8. /?/ conversion:

$$\begin{bmatrix} +cont \\ -back \\ -high \end{bmatrix} \longrightarrow [-cont] / V \qquad V$$

R9. /?/ deletion:

$$\begin{vmatrix} -\operatorname{cont} \\ -\operatorname{cor} \\ -\operatorname{ant} \\ -\operatorname{high} \\ -\operatorname{back} \end{vmatrix} \longrightarrow \phi / V + \begin{pmatrix} +\operatorname{syll} \\ +\operatorname{high} \\ +\operatorname{front} \end{pmatrix}$$

Before proposing the ordering of rules, I would like to consider the aforementioned regressive assimilation. According to McCawley (1968:85), when a consonant sequence is created, the first consonant becomes either the mora nasal /N/, or the mora obstruent /Q/, depending on the nature of the first consonant. However, with regard to a CC sequence, as in /hanas+ta/ or /oyog+ta/, neither the mora nasal nor the mora obstruent are generated by the rules. Thus, such forms as \*/hanaNta/, \*/hanaQta/, \*/oyoNda/ or \*/oyoQda/ are never manifested as surface forms. What is needed then is a rule which excludes segments such as /s/ and /k,g/ from assimilating to a following /t/. In other words, the regressive assimilation only applies to /r/ and /m/ (including instances of /m/ which have been derived from /b/ by the nasalization rule.). Accordingly, a revised form of McCawley and Kuroda's regressive assimilation rule is given in R10.

R10. revised regressive assimilation rule:



After considering the possible phonological problems with regard to past tense formation, the ordered rules which are necessary in order to yield the proper output with respect to consonant-stem verbs in the past tense are illustrated in Table 4.

	U.R.		/kir+ta/	/mat+ta/	/asob+ta/
1.	voicing	(R3)			/asob+da/
2.	/h/ conversion	(R6)			
3.	nasalization	(R4)			/asom+da/
4.	regressive assimilation	(R10)	/kit+ta/		/ason+da/
5.	/i/ insertion	(R7)			
6.	/?/ conversion	(R8)			
7.	/?/ deletion	(R9)			
	S.R.		/kit+ta/	/mat+ta/	/ason+da/
	U.R.		/hanas+ta/	/kak+ta/	/oyog+ta/
1.	voicing	(R3)			/oyog+da/
2.	/h/ conversion	(R6)		/kah+ta/	/oyoh+da/
3.	nasalization	(R4)			
4.	regressive assimilation	(R10)			
5.	/i/ insertion	(R7)	/hanas+i+ta/	/kah+i+ta/	/oyoh+i+da/
6	/?/ conversion	(R8)		/ka?+i+ta/	/oyo?+i+da/
••	/1/ соптетьтоп				
7.	<pre>/?/ deletion</pre>	(R9)		/ka+i+ta/	/oyo+i+da/
7.	<pre>/?/ deletion S.R.</pre>	(R9)	/hanas+i+ta/	/ka+i+ta/ /ka+i+ta/	/oyo+i+da/ /oyo+i+da/

Table 4

So far, I have discussed the phonological problems of Japanese verb morphology with emphasis on past tense formation. I have tried to account for these derivations within the frameworks of TGP and NGP from a synchronic point of view. My analysis, however, is based on the hypothesis that consonant-stem verbs did not derive from vowel-stem verbs; these stems historically ended in consonants, i.e., CVC syllable structure was characteristic of proto-Japanese. Based on this hypothesis, I analysed Japanese verb morphology making use of the notion of 'insertion' in terms of strength hierarchy and syllable structure constraints. It is necessary to consider the Japanese language from a diachronic point of view in order to support my hypothesis.

It is a widely accepted theory that the ancestor of the Japanese language was proto Ural-Altaic. Adhering to Miller (1971) and Martin (1966)'s principle, the origin of Japanese can be traced back to the proto-Altaic language, and it is therefore genetically related to the Korean, Mongol, Turkish, and Tungus languages. With respect to this point, Martin (1966)'s treatise entitled *Lexical Evidence Relating Korean to Japanese* and Miller (1971)'s book *Japanese and the Other Altaic Languages* have very interesting and far-reaching implications. Recently, a noted Japanese linguist, Hattori (1959:99) suggested that Japanese might be related to Ainu. These views suggest that Japanese in a proto stage might have had a closed syllable structure, since the dominant present-day syllable structure in the above languages is certainly CVC.

The same idea was expressed by the Japanese linguist Atsushi Hamada in 1951. Since then, Ohno (1953) has developed this idea and has tried to account for the origin of the Japanese verb conjugations by basing his thinking on the hypothetical notion of closed-syllable structure in the proto-Japanese language. The following examples show how Ohno (1978:180) demonstrates this by comparing certain forms

of Old Japanese, such as *isi* 'stone', *isago* 'sand', *isunokami* 'on the stone', and *iso* 'seashore'. Among this group of lexical items, there can be seen a common element /is/. Similar examples are shown in *asita* 'morning', *asa* 'morning', *asate* 'the day after tomorrow', and *asu* 'tomorrow', in which /as/ is regarded as the common element. Ohno (1978:180) postulates that only the /is/ of the former group designates the meaning 'stone', while /as/ of the latter designates 'dawn'. This is why the vowels following /is/ and /as/ are free to vary. This being so, our hypothesis is that the proto-Japanese language had lexical items with final consonants, such as *is* and *\*as*. It is interesting to note that Miller (1971:97) offers cognates of *isi* 'stone' among other Ural-Altaic languages:

pA.<sup>7</sup> \*tal<sub>2</sub> <sup>8</sup>, OT. taš, Chu. cul, pKJ. \*dyoš, MK. tolh, K. tol. It will be observed that all the cognate lexical items end in final consonants, as hypothesized for proto-Japanese.

With respect to the verbs, the forms given in (4), (5), (6), and (7) in Table 1 show superficial homophony involving the shapes /kiru/ and /neru/. I maintain that the /r/ of items (4) and (5) appeared as a result of consonant insertion. In fact, Old Japanese counterparts of (4) and (5) were ki and ne, respectively. The /r/ of items (6) and (7) was the original consonant of the stem, which means that these verbs had final consonants. According to Miller (1971:72), /kir-/ 'cut' has cognates in other Altaic languages. The cognates of kir-u shown by Miller are given below.

pA. \*kïr, Mo. kir-ga-, Osm. qïr-, Ma. giri-, Go. geri-, pT. \*qïrï-, pKJ. \*kyɔr-, MK. kɔl-, K. kal-.

<sup>&</sup>lt;sup>7</sup> pA. proto-Altaic, pKJ. proto-Korean-Japanese, pT. proto-Turkish, Chu. Chuvash, Go. Goldi, K. Korean, MK. Middle Korean, OT. Old Turkish.

<sup>&</sup>lt;sup>8</sup> For an extensive description of  $*1_2$ , see Miller (1970) and (1971). Miller traces the development of  $*1_2$  as /1/, /s/ or /s/.

Consequently, Miller (1971:73) maintains that the final /r/ of the Japanese reflex represents a Japanese inheritance and preservation of the original Altaic root-final consonant /r/. Similar examples are found in the case of /sir+u/ meaning 'know' or /hasir+u/ meaning 'run'. These stems also preserve the original Ural-Altaic /r/ as the Japanese reflex in their stems, and accordingly, these verbs have a typical consonantstem morphology, as in /sir+i+mas+u/, /hasir+i+mas+u/ and /sit+ta/, /hasit+ta/ in their derivations of polite present and past tense, respectively. They are thus never conjugated as \*/si+mas+u/, \*/hasi+mas+u/, \*/si+ta/ and \*/hasi+ta/, since the /r/ of those stems are not to be confused with inserted /r/.

Furthermore, even contemporary Japanese forms such as /tabe+ru/ which are characterized as vowel-stem verbs had a consonant ending ancestrally. According to Miller (1971:86), the proto-Altaic form of this verb is \*žėp-, since the other related forms are proto-Turkish \*žäp-, Evenki žep, Lamut žeb-, Manchu že-, proto-Korean-Japanese \*cab-, and Middle Korean ca(ps)-. With respect to the etymology of /tabe+ru/, Miller (1971:95) makes the following proposal:

In the formations ultimately responsible for the forms that are attested in all these languages there was no original vowel following the final /-p/ of the root and preceding any subsequent consonant-initial suffixed element. The /-e-/ of the modern Japanese forms (taberu, tabeta, tabeyo, etc.) which here goes back to OJ /-e-/, is a Japanese innovation, inserted according to the canons of Japanese syllabic structure and morae accounting but nevertheless still in vowel harmony with the original vocalization of the root element  $*\breve{zap-}$ . The striking point is that the evidence for this original vocalism survives in the Japanese verbal morphology even though early in the history of pre-Japanese the vocalism of the root itself was leveled out to its now familier /tab-/ shape, probably through assimilation to the position of articulation of the root-final consonant ( 'labial attraction' ).

Here, Miller also maintains that the /ë/ emerges as a result of 'insertion'. With respect to this point, I hypothesize that first, /i/ appeared as the epenthetic vowel just as in the other instances. Then, /i/ of /tab+i/ was lowered under the influence of the stem vowel /a/; this can be viewed as being a instance of vowel harmony.

It is a well-known fact that Old Japanese had an eightvowel system, i.e., /i,e,a,o,u/ + mid high /ï, ë, ö/ which are known as Otsu-vowels or Series B vowels, in contrast to the former group which is referred to as *Koo*-vowels or Series A vowels. Old Japanese shows certain structural limitations in the distribution of its eight vowels. These restrictions on the occurrence of vowels are sometimes referred to as 'vowel harmony' which was commonly observed in Old Japanese as well as other Ural-Altaic languages. Some examples of this vowel harmony are illustrated below.

/kökörö/	'heart',	/itoko/	'dear',	/uko/	'foolish',
/Fadara/,	/Födörö/	'fall lig	ghtly'		

As we have seen above, there was a strong possibility that Old Japanese underwent a change from closed-syllable to open-syllable structure, while other Ural-Altaic languages escaped such a change.

My hypotheses are thus as follows:

1. As far as we adhere to the theory of the Ural-Altaic origin of the Japanese language, proto-Japanese had closed-syllable structure.

2. Proto-Japanese was possibly affected by contact with a language of some other family in which the syllable structure was open; possibly, one of the Austronesian languages. Alternatively, the language had enough time in its history for its syllable structure type to change through internal evolution.

According to Hattori (1959:13), it is wrong to claim that the difference between the open-syllable structure of Japanese and the closed-syllable structure of other Ural-Altaic languages is an important point in terms of genetic relationships. Since this difference is not so important, he maintains that we cannot deny genetic relationship between Japanese and Ural-Altaic only on the strength of this argument. Hattori (1959:13) enumerates the following examples to support his argument. Manchu, which is one of the Tungus languages, has open-syllable structure as a result of the deletion of final consonants, and Mongol and the Kagoshima dialect of Japanese are now in a stage of closed-syllable structure because of the loss of second-syllable vowels. Moreover, Hattori (1959:43) cites Ramstead's statement that some of the CV-structured words in Japanese developed from CVC shapes. Thus CV sequences, such as /ka/ can be traced back to one of the following forms: \*/kak/, \*/kag/, \*/kar/, or \*/kal/. This being so, the final consonant in such cases has since undergone deletion.

After open-syllable structure was firmly established in Japanese, every consonant-final lexical item had thus been modified to conform with the present-day basic open-syllable structure. Originally, /u/ appeared as an epenthetic vowel and then /i/, in conformity with the emerging basic syllable structure. The appearance of epenthetic /i,u/ is well motivated in terms of strength hierarchy and syllable structure constraints. It is interesting to note that even without a consideration of strength hierarchy and syllable structure constraints, the historical documents evince this fact. According to Sakakura (1966:285), the /u/ was highly involved in verb formation, since all the indicative verb forms end with /u/, and the old nominal forms ended with this /u/, possibly representing a usage analogous to verb formation. The basic vocabulary thus contained a suffix /-u/,

as in haru 'spring', natu 'summer' Fuyu 'winter', i.e., in the names of seasons. Further examples are shown below. day and time: Firu 'day', yoru 'night', keFu 'today', kinoFu 'yesterday', yuFu 'evening' necessities of life: midu 'water' usu 'mortar' situ 'textile goods', kinu 'silk' animals or plants: kitu 'fox', saru 'monkey', tadu 'crane', karasu 'raven', kudu 'arrow root', matu 'pine tree'

However, the /u/ could be replaced by /i/, since /i/ itself had a lexical meaning. That is, it designated 'person' or 'thing'. Ohno (1978:201) cites the following sentence taken from Zoku Nihonki (written in 720 A.D.) in exemplifying /i/ 'person' :

Kore	0	motu	_i	wα	homare	0	itasi
this	OBJ	possess	person	SUB	honour	OBJ	do
suturu	i	wa	sosiri	C	o mar	ekitu	
ab <b>a</b> ndon	persor	n SUB	censure	OF	3J bri	ng upon.	

'A person who possesses this will be honoured, but one who abandons it will bring public censure upon himself.'

Consequently, /-u/ in old nominal forms was sometimes replaced by /-i/, as in tuku(yo) 'a moonlit night'  $\rightarrow$  tuki 'moon', Kamu(sabi)'very Godlike'  $\rightarrow$  kami 'God', or kuku(tati) 'a spike of rape, a kind of plant with oil-yielding seeds'  $\rightarrow$  kuki 'a stalk or a spike'. The former compound forms of tuku, kamu, and kuku are the Old Japanese forms of Modern Japanese tuki, kami, and kuki, respectively. According to the old records, the /i/ of modern forms, however, were all written in Series B-/ï/ in the above instances, as in /tukï/, /kamï/, and /kukï/. Ohno (1978:198) thus maintains that Series B-/ï/ emerged as a result of coalescence, whereby /u/ + /i/

 $\rightarrow$  /i/. He posits a hypothetical transitional form \*/tuku+i/ as being a mid point in the derivation  $/tuku/ \rightarrow /tuki/$ . This hypothesis is based on the idea that some old nominal forms in /-u/ were affected by the new nominalizer /-i/ and yielded the new /u+i/ form. Ohno also suggests that  $/o/ + /i/ \rightarrow /i/$  is an another possible source of Series B-/i/. Furthermore, Ohno (1978:198) proposes that three other vowels, i.e., Series A-/e/, Series A-/o/, and Series B-/ë/ were also the result of coalescence. That is,  $/i/ + /a/ \rightarrow /e/$ , /u/ + /a/ $\rightarrow$  /o/, and /a/ + /i/  $\rightarrow$  /ë/. The main reason for the vowel coalescence is that Old Japanese prohibited the use of V + Vsequences. For instance, nagëki meaning 'grief' comes from nagaiki meaning 'deep sigh', literally, 'long breath', since the word nagaiki is a compound which consists of naga 'long' and *iki* 'breath'. Here, the VV sequence /ai/ of /naga+iki/ coalesced into Series  $B-/\ddot{e}/and$  yielded a new lexical item nagëki. There is special reason for believing that the analysis of Series B-/i/as a vowel arising from coalescence may be correct since there is a strong possibility that normal epenthesis occurs in a weak phonetic environment, such as syllable-final /-u/, which was originally an epenthetic vowel. Accordingly, a new nominalizer /i/ had a strong possibility of acting as an epenthetic vowel in addition to the original epenthetic vowel /u/. After a VV sequence, i.e., /u/ + /i/ was created, these two epenthetic vowels coalesced and yielded a new segment of Series B-/i/. This is partly because of the nature of the two weak epenthetic vowels and partly because of the prohibition on the use of V + V sequences. This phonetic change is thus reasonably accounted for by adopting the notion of strength hierarchy and adhering to Ohno's proposal.

Consequently, Ohno (1978:199) postulates that the basic vowels of pre-Old Japanese were only four in number, that is,

/i, a, ö, u/, since other four vowels, i.e., /ï, e, ë, o/ are assumed to have emerged later as a result of coalescence. The high front and back vowels, i.e., /i, u/ are considered weak vowels in terms of the strength hierarchy of vowel. The /i/ had thus a great potential to serve as an inserted vowel. In these ways, the /i/ certainly acted as an epenthetic vowel in proto-Japanese, either appearing as a lexical suffix which acted as a nominal marker or maintaining its status as an epenthetic vowel which had the effect of bringing about the CV pattern.

With respect to the CV syllable structure of the language, the reader may notice the appearance of a VV sequence (in contrast to the CV dominant structure), such as /ka+i+ta/ 'wrote' or /oyo+i+da/ 'swam' (see Table 1 and 3) resulting from the /k, g/ deletion. It seems that the above examples are counter-examples of the basic CV structures. This phonetic change, i.e., deletion of /k, g/, however, occurred in more recent times. Thus, transitional forms, such as /kak+i+ta/ or /oyog+i+ta/ were found in the literature. One might hypothesize that the consonant insertion took place in a very early stage of the language, i.e., pre-Old Japanese, and the language possibly had the following syllable structure change.

> \*CVC → CVC-V → CVØ-V (i.e., CVV) insertion deletion

Accordingly, the present-day VV sequence was created later as a result of the deletion of the second syllable consonant. This change occurs faily commonly in languages. This being so, the analysis involving the insertion of /i/ or /r/ in a pre-Old Japanese stage seems quite reasonable in terms of strength hierarchy and syllable structure constraints from both the synchronic and diachronic point of view. Consequently, the hypothesis of CVC syllable structure in the proto-language is not such an extravagant idea; rather, it is quite natural and useful notion reflecting the history of language as well as providing answers to some previously unsolved quiestions relating to verb morphology, the origin of modern verb conjugations and word formation in the Japanese language.

It is of course impossible to conclude that proto-Japanese roots definitely had a CVC structure. However, if the widely accepted view of the origin of the Japanese language is correct, then it is probable that syllable structure in the proto-languge was basically CVC; otherwise the above theory could be incorrect.

This paper represents only a preliminary attempt to incorporate the current theory of linguistics and traditional comparative method, and the subject certainly needs further investigation in order to account fully for the phonological processes in Japanese verb morphology and for the origin of the Japanese language. REFERENCES

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# 1.0 INTRODUCTION

Assuming that intralanguage differences derive from social differences between groups within a speech community, it follows that some type of standardization of language is inevitable. Furthermore, language norms are usually based on the linguistic behaviour of the dominant or prestigious class of that community (Wolfram and Fasold 1974:17-18). In England, for example, standard usage was once based on what has been termed Received Speech; the language used by the aristocracy. However, with the changes in the structure of English society in recent history, changes in language norms in England are no longer derived solely from upper class usage. Especially in matters of pronunciation, modified regional standardized dialects have emerged to compete with, and perhaps to influence, the standard dialect centered in London. (Gimson 1961:83-84). In such circumstances standard usage no longer remains uniform. Similarly in English-speaking Canada, although there is much less regional dialect variation than in Britain, it is impossible to identify a single model upon which to base a uniform standard. The situation in Canada is perhaps unique in that, rather than dialects competing for supremacy, through the course of our short history there have been two recognized models, both external to the community itself. To varying degrees and in various aspects British and American English have both had a direct influence on the perceived standards of English language usage in Canada.

Uniformity of standard usage can be achieved by fiat. One

such instance is recorded in the history of Canada when on July 14, 1890, an order in council was issued requiring that British spelling be used in all official documents of the Government of Canada. (The order made specific reference to the letter *u* in words such as *honour*, which is omitted from American spelling conventions.) In the absence of a regulatory body, such as the Académie Française in France, the model for standard usage is often provided by educators, lexicographers, and others who are recognized as authorities in matters of language use; e.g., celebrated writers and orators. Linguists have, by and large, eschewed involvement in this matter, except to condemn attempts to prescribe correct use of language.

Standardization of language usage is unavoidably and inextricably bound up with notions of correctness and, hence, often with negative value judgments concerning nonstandard usage. It is for this reason that linguists, until recently especially in North America, not only have failed to deal constructively with normative usage, but have bitterly attacked traditional prescriptiveness in English language education. Most linguists hold the view that all varieties of language are equally adequate for communication and that it is not a question of one being better than the other, but merely that they are different from each other. This view of language differences, rather than language deficiencies, derives from the anthropological tradition of cultural relativity dating back to the turn of the century. What linguists are objecting to, of course, is the notion that nonstandard varieties are deficient in some sense; a view that would make objective investigation impossible. Although this principle is unassailable, its adherents have tended to overlook an important practical aspect of language communication. For formal expression, especially written, a uniform or common standard is essential in a large society which includes

a number of divergent dialects. In such situations members of the non-literate dialect will find themselves at some disadvantage when wishing to communicate with other groups by having to acquire a second mode of language communication. In this context only, one might venture to suggest that the non-literate dialect is inadequate for its members to function in the larger community. Be that as it may, the need and existence of standards of usage are undeniable and one of the problems facing Canadians is the lack of clearly defined norms.

With the disappearance of grammatical studies from the school curriculum there is a growing uncertainty concerning the distinction between usages appropriate to formal and informal expression. This situation, if prevalent throughout the country, could open the way to major changes in the perceived norms. This paper addresses the issue by describing an experiment to determine and measure the perception of formal and informal registers in respect of grammatical usage amongst a sample population of university students.

The aim of the experiment was two-fold. One purpose was to explore a methodology through which acceptable norms of usage might be established. Appealing directly to speakers' own perception of appropriateness of expression in formal contexts may be one way of arriving at a definition of normative usage, provided that the survey population is fully representative of those who are likely to provide an acceptable model. Our experiment falls short in that respect. Another reason for this undertaking was to investigate the status, with respect to formality, of a handful of grammatical forms that are known to be subject to variation and suspected of giving rise to changes in normative usage.

#### 2.0 DESIGN OF THE PROJECT

### 2.1 The Instrument

The investigation reported here was part of a more comprehensive study in which several aspects of English language usage were examined. Thus, the instrument contained items measuring lexical, phonological, and morphophonemic variation, in addition to morphosyntactic variability relevant to the present discussion. The grammatical or morphosyntactic items were presented to the subjects in three sets, each of these sets consisting of the same thirty-two examples of morphosyntactic variation. Each item was followed by a scale with the more standard form at one end and with the alternative, less standard form at the other, as the following example illustrates.

It's really/real hot in here.

really / A / B / C / D / E / real

In the instructions preceding the first set of these items, subjects were asked to use the scale following each example to report the relative frequency with which they <u>heard</u> the two forms used in conversation. Points A and E indicate that the adjacent items are exclusively heard, C means that both forms are heard with equal frequency, and B and C represent intermediate points between A and C, and E and C, respectively. For the second presentation of the set of items the subjects were asked to report the relative frequency with which they used the two forms in conversation with people to whom they wished to show respect. For the third and final set the subjects were instructed to report relatively how often they used the two forms in both the second and third presentation of the set, subjects were enjoined 'to be as frank and honest as possible', and they were advised that the authors 'do not regard one form as more correct than the other'.

For present purposes the first encounter was considered to comprise a pretest familiarization with the content of the questionnaire. Since there exists considerable sensitivity regarding possible judgments on correctness of usage, we wanted to put our subjects as much as possible at their ease in order to reduce the natural tendency on their part towards reporting only what they believed to be the correct form. It was felt that, having reported on what they heard other people saying, the subjects would then be prepared to give their best performance in reporting what forms they themselves used in the formal register. Finally, in their third encounter with the same material, after having created their best impression, it was hoped that subjects would feel free to report honestly what usage they employed in informal situations. In fact, this study is interested only in the differences reported between formal and informal usage.

## 2.2 The Items

The thirty-two morphosyntactic items selected for inclusion in the survey instrument are listed in Appendix A. These particular examples were chosen on the basis of previous observations of what appeared to be indiscriminate use of alternatives. For example, it has become increasingly common to hear between you and I used in preference to between you and me, even in fairly formal situations. In order to measure the extent to which they are used and the formal versus informal distinction made, the much-discussed who and whom are included. In addition to a few examples of case confusion, examples of other common problems were included which the reader no doubt will recognize. Although the items in the Appendix are grouped according to the particular grammatical problem each exemplifies, they were randomly ordered in the questionnaire and, of course, without mention of the grammatical problems represented. Also varied at random was the order of presentation of the standard and nonstandard forms so that, for some items the standard form was placed at the left-hand end of the scale while, for other items, it was placed at the right-hand end of the scale.

The terms standard and nonstandard are somewhat artificially used. The standard form is that which may by some still be considered the more appropriate for the most formal expression. All others are designated as nonstandard without regard to their perceived degree of acceptability for use by educated speakers, as this is precisely what the experiment is intended to measure. 2.3 The Subjects

The population selected for this study consisted of students in two introductory English linguistics courses taught in the Department of Linguistics at the University of Victoria. One of these courses was at the first-year level while the other was a third-year course intended for students in the language arts stream of the Faculty of Education. In fact, all but a few in both courses expressed interest in including the teaching of English as a possible career. Thus, the sample could be considered as being comprised primarily of prospective English teachers. There were approximately equal numbers of first and third year students in a sample totalling sixty-eight subjects. Having eliminated from the sample subjects who returned unusable responses, we retained a total of sixty-four subjects.

3.0 ANALYSIS AND RESULTS

3.1 Data Processing

The responses of the subjects, having been marked directly onto the questionnaire forms, were transcribed to optical marksense forms for computer data entry. Transcription of responses to computer-readable form resulted in the literal codes A through E used in the questionnaire being translated into numeric codes O

through 4, respectively. These numerical values were then used in the calculations.

In subsequent steps in the processing of the data, attention was restricted to the results of the subjects' second and third encounter with the test items, during which they were to report their formal and informal usage. The first step consisted of transforming the response values so that the code value 0 always corresponded to exclusive use of the nonstandard form and 4 corresponded to exclusive use of the standard form. A mean and standard deviation were then computed for each of the sixty-four variables.

The next processing step consisted of computing for each item the difference in usage reported by each subject, this computation being accomplished by subtracting a subject's informal response value (i.e. in the context of conversation with friends) from that of his formal response (i.e. in conversation with superiors). Thus, data on thirty-two new variables were generated from the original sixty-four in two sets of thirty-two each. Finally, means and standard deviations were computed on the thirty-two difference variables. These means and standard deviations, and those calculated for the original sixty-four variables, were subsequently employed in the computation of the test statistics described in the following section.

## 3.2 Statistical Testing

The purpose of the study was to identify those morphosyntactic items for which subjects exhibit a statistically significant change in usage from that which is reported as informal to that of the more formal register. Detection of a change or shift in usage with circumstance was based on observation of departure from zero of the mean for the difference variable derived for the item. A positive mean was taken to indicate a shift towards relatively more frequent use of the standard form for the item by subjects in the formal context; a negative mean was regarded as revealing a shift towards relatively more frequent use of the nonstandard form in the same context. A t-statistic was used to test significance of these departures from zero. Since items were identified and treated separately according to whether the mean on the associated difference variable was positive or negative, a onetailed test of significance was applied.

The power we might expect for this test with a sample of sixtyfour subjects was determined by using tables published by Cohen (1977). We considered that we would want to detect a mean shift in relative frequency of usage of approximately one-quarter of a scale unit. A scale unit might be taken as the distance between response B and C, for example, on the questionnaire response scales. In order to use the power tables, the mean shift must be converted to an effect size by dividing it by the population standard deviation. The population standard deviation was estimated by computing the mean and standard deviation of the standard deviations of the thirty-two usage difference variables. A mean and standard deviation of approximately 1.06 and 0.08, respectively, were obtained; and it was therefore concluded that a reasonable estimate of the population standard deviation was unity. Hence, the mean shift of one-quarter scale unit could be treated as the population effect size to be detected. Since the tables are for a one-sample test, the two-sample paired-comparison effect size was multiplied by the square root of two, as prescribed by Cohen, to obtain an effect size of 0.35 with which to enter the one-sample tables. Interpolation yielded a power of 62% for a significance level of 5% for a one-tailed test. Application of the 1% and 10% significance levels resulted in powers of 36% and 74%, respectively. Thus, if the shift in relative frequency of usage in the population were one quarter

of a scale unit and if we were to apply a 5% significance level, we would stand approximately two chances in three of detecting this shift in usage with a sample of sixty-four subjects, and we would run a risk of one chance in three of failing to detect this difference. The 1% and 10% levels would give us approximately one chance in three and three chances in four, respectively, of detecting the one quarter scale unit shift in usage. The 1% and 10% significance levels yield the complementary risks of two chances in three and one chance in four, respectively, of failing to detect a difference of this size. Of course, if in the population the shift in usage with circumstance were greater, then the likelihood of detecting it in our sample would increase also. For example, if the shift in the population were one half a scale unit and we were applying a 5% significance level, then the probability of our detecting this difference with a one-tailed test in a sample of 64 subjects would exceed 99%.

In the following section we report the results obtained when applying the 1, 5 and 10% significance levels to the one-tailed t-test of the significance of the departures from zero of the mean shifts in usage. For our test of the null hypothesis that the mean shift of usage in the population is zero against an alternative that the mean shift is one quarter of a scale unit (in a specified direction), these levels of significance yield ratios of the likelihood of type II error to the likelihood of type I error of approximately 44 (for the 1% level), 8 (5% level), and 3 (at the 10% level). Thus, for the small mean shift in the population usage of one quarter of a scale unit, the test is conservative in the sense that we are more likely to deny the existence of this shift in usage than to accept it. At the 5% level, for example, we are eight times more likely to conclude that there is no shift in usage. Of course, if the shift in the population were greater than one

quarter of a scale unit, then the test would become more liberal. For example, if the shift were one half a scale unit, then application of the 5% significance level would yield a type II error rate of 1%; the ratio of likelihoods of the two types of errors then would be reduced to one-fifth (1/5).

The critical effect size reported in Cohen's power tables can be used to obtain approximate values for the mean shifts of usage which might be considered significant. If we assume a standard deviation of unity for all items, and if we convert the one-sample critical values to two-sample by dividing them by the square root of two, then for a one-tailed test we consider a mean shift in usage in a specified direction of 0.30 scale units significant at the 1%, a shift of 0.20 units significant at the 5%, and a shift of 0.15units significant at the 10% level. It must be stressed that these values are very approximate since we have observed that the standard error of the mean shift varies from item to item. We cite these values here only to illustrate what magnitudes of shift in usage from that employed with peers to that used with superiors might be considered statistically significant. The results reported in the next section were obtained by applying the conventional t-test.

The objective in this experiment was to identify those items for which subjects display a significant shift in relative frequency of use of the standard and nonstandard forms with change of social circumstances. Having discovered these items, we then were interested to learn which of the two forms subjects might favour significantly in their conversation with each of the two social groups. This information was sought through additional statistical testing. Since the value "2" represents the midpoint on the five point scale from "0" to "4" on which responses were recorded, and

since the code "2" corresponds to equal frequencies of use of the standard and nonstandard forms, a t-statistic was computed to test significance of departure of the mean for the original variables from the value "2". A negative value for this test statistic could be taken to show preference for the nonstandard form while a positive value could be regarded as revealing a disposition toward the standard. Thus, having separated the items according to the sign of the associated t-statistic, we could apply a one-tailed test of significance of the identified disposition. The results of these tests are reported in the following section.

3.3 Results

3.3.1 Negative Shift (summarized in Appendix B)

In this experiment we were interested particularly in those items for which subjects reported relatively more frequent use of the nonstandard form in the formal than in the informal context. Ten such items were identified, among the thirty-two, with mean shifts in the negative direction. In the case of seven of these items, mean shifts were significant at least at the 10% level. The two items (1) between you and me/I, and (17) purer/more pure resulted in highly signifcant (1% level) mean shifts in usage. (The item numbers correspond to those in Appendix A where the full text of the items is given.) A further two items (17) in a moment/ momentarily, and (16) I hope/Hopefully yielded mean shifts significant at the 5% level. The remaining three items of this set of seven were (26) behind/in back of, (32) visit/visit with, and (27) at/to home. These yielded mean shifts of usage towards the nonstandard form from the informal to the formal context significant at the 10% level.

Results of the one-sample tests for these seven items showed that, in conversation with friends, subjects exhibit a highly statistically significant (the 1% level) preference for the standard

form; however, in conversation with people to whom they wish to show respect, subjects display a highly significant disposition toward the standard form with only five of the seven items. In the case of (17) *purer/more pure*, their preference for the standard form is significant at the 5% level while for the remaining item, (1) between you and *me/I*, although the t-value is positive, it is too small in magnitude to be considered significant at any reasonable level. Thus, it would appear that, for six of the seven items, subjects tend towards use of the standard form in conversation with both peers and superiors; but, this disposition toward the standard is more pronounced in the context of conversation with friends.

3.3.2 Positive Shift (summarized in Appendix B)

We were interested also in identifying those items for which shift in reported relative frequency of usage was toward more frequent use of the standard form with superiors. This shift would be in the more normal or more commonly expected direction with the nonstandard form being used relatively more often in informal situations in conversations with friends. We found twelve items yielding shifts significant at the 10% level in the positive direction; among these, we found the following eight to exhibit mean shifts significant at the 1% level: (1) Whom/Who, (3) It's I/me, (4) for you and him/he, (29) as if/like, (9) sneaked/snuck, (23) wants to go/wants out, (24) wants to get/wants off. Two items, (19) do well/good and (8) saw/seen, yielded mean shifts in usage in the positive direction that were significant at the 5% level. The remaining two of the twelve (30) since/seeing as and (31) the reason is that/because exhibited positive shifts significant at the 10% level.

On applying the one-sample tests to these twelve items, a

highly significant (at the 1% level) preference was discovered for the nonstandard forms in conversation with both peers and superiors for the three examples (2) whom/who, (3) It's I/me, and (31) the reason is that/because. For the items (29) as if/like, (13) If he were/was, and (9) sneaked/snuck, although the test statistic was positive in sign for the formal register, the magnitude was not sufficiently large to be regarded as significant at any reasonable level. For the informal register the statistic was negative in all the above six instances. For the item (29) as if/like, the disposition towards use of the nonstandard form was significant at the 1% level; for (13) If he were/was, this disposition was significant at the 5% level; however, for the item (9) *sneaked/snuck*, the tendency towards the nonstandard could not be regarded as significant. For the remaining six of the twelve items, the one-sample t-test showed a highly significant (at the 1% level) preference for use of the standard form by subjects in conversation with superiors. In conversation with friends, however, subjects displayed a highly significant preference for the nonstandard forms in the two items (23) wants to go/wants out, and (4) for you and him/he, while with the item (24) wants to get/wants off, although the statistic is positive, it is not large enough to be significant. With the remaining three items, namely, (30) since/seeing as, (19) do well/good, and (8) He saw/seen, subjects show a highly significant preference for the standard form in conversation with friends. 3.3.3 General Disposition (summarized in Appendix C)

Although it was not the major concern in this experiment, we were interested peripherally in discovering in which of the thirtytwo items subjects demonstrated preference for the nonstandard form in both registers. On applying the one-sample t-test, we found five such items. In the case of three of these, namely, (2) *whom/who*,

(3) It's *I/me*, and (31) the reason is *that/because*, we discovered that subjects exhibited a highly significant (1% level) tendency toward use of the nonstandard form with superiors, while with the items (28) different *from/than* and (21) Everybody gets *his/their*, subjects showed tendencies towards the nonstandard which were significant at the 5 and 10% levels, respectively. For the four items (1) between you and *me/I*, (29) It looks *as if/like*, (13) If he *were/was* and (9) *sneaked/snuck*, although the test statistic was positive in sign, it was not large enough to allow us to conclude that subjects exhibited a statistically significant preference for the standard form; however, for the two items (12) He *lent/loaned* me money and (17) *purer/more pure*, the tendency toward use of the standard form with superiors was significant at the 10% level. Subjects exhibited a highly significant preference for use of the standard form with superiors for all other items.

The one-sample test was also applied to responses by subjects in the context of the informal register. This yielded ten items for which a negative value for the test statistic (i.e. a tendency towards nonstandard usage) was observed. In the following six cases this tendency was highly significant (at the 1% level): (2) whom/ who, (23) wants to go/wants out, (29) It looks as if/like, (3) It's I/me, (31) the reason is that/because, and (4) for you and him/he. In the two cases (13) If he were/was, and (28) different from/than the tendency towards the nonstandard form was significant at the 5% level while in the remaining two cases, namely, (21) Everybody gets his/their, and (9) sneaked/snuck, this tendency was not sufficiently large to be construed as significant. Among the twentytwo items for which the test statistic was observed to be positive, in all except two cases the disposition toward use of the standard form with peers was found to be highly significant (1% level); in

the case of the item (12) He *lent/loaned*, this disposition was discovered to be significant at the 5% level, and only for the item (24) *wants to get* off/*wants* off, did we observe the magnitude of the statistic to be too small for us to conclude subjects exhibited as significant preference for the standard form. 4.0 DISCUSSION

Eliciting formal register by appealing to respectful style raises a problem. By specifying the interlocuters as people to whom respect is due, the questionnaire may be suggesting a focus on parents and older members of the community rather than formal situations in general. Therefore the responses, on occasion, appear to reflect an accommodation to the linguistic behaviour of the respondents' elders by indicating nonstandard or dialect forms as preferred in respectful usage. Examples of these in the present corpus are (26) *in back of*, (27) *to home*, and (32) *visit with*. A possible way of avoiding such an outcome might be to appeal directly to the respondents' perception of correctness of usage, even at the risk of offending our philosophical sensibilities.

Amongst the items involving grammatical case it will be observed that, except for (1) between you and me/I, the standard form is still being recognized by respondents as being more formal. The tendency to replace the accusative by the nominative form may be related, by way of analogy, to the change from *it is me* to *it is I* which has been foisted on the English-speaking world by the school grammars. Halliday (1967:67-71) argues that, in a four-way classification, the verb *be* has the status of being a transitive (lexical) verb, thus making the expression *it is me* perfectly grammatical. Halliday observes that the replacement of Middle English *it cm I* by *it is me* represents a shift in subject function from *I* to *it*. The two forms, *it is me* and *it is I*, existed side by side in the sixteenth century. Halliday suggests that *it is I* may have been a transitional blend,
analogous to he hits I with he as subject, and to him hits I when the subject is I. In view of this, one might speculate that the form it is I would not have survived had it not been for the insistence of the school grammars that this was the grammatically correct form. Presumably it is I was preferred to it is me because of nominative complements in Latin which provided the model.

The occurrence of the accusative whom in interrogatives appears to be rare in Canada, judging by the results of The Survey of Canadian English (Scargill and Warkentyne 1972:74). Of the total survey population, 23% adults and 11% students reported the regular use of whom did you see. Professor Scargill (1974:27) draws the reader's attention to the pronouncement of Edward Sapir (1921:156-164) on this form. Sapir predicts the demise of the accusative in this context within a century. Among the reasons cited for this change are that sentence initial position is strongly associated with the nominative case in English, and the fact that the other interrogatives, which, what, where, when, how, are invariable in form influences speakers also to treat who as invariate. Whilst indulging in speculations as to the causes for the disappearance of the accusative pronoun in formal expression, one might also suggest that a contributing factor is the reaction against its over-use which is regarded as substandard.

Some items in our corpus designated as nonstandard have already received wide acceptance in the United States as appropriate to formal use. *Momentarily* meaning 'in a moment' is included by *The American Heritage Dictionary* (1969) without comment as to its acceptability. For *hopefully*, meaning 'let us hope' *Heritage* informs the reader that it is unacceptable for formal use to 56% of its usage panel. Fowler's *A Dictionary of Modern English Usage* (1965 edition) does not comment on *hopefully*, but condemns the use

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of momentarily to mean 'immediately' as 'foolish novelty-hunting'. The use of analytically constructed comparison for inflectional adjectives, as in more pure, is permissible in formal English in certain contexts; e.g. where more is to contrast with *less* as in *This item is more pure but less interesting than the other*. However, disyllabic inflectional adjectives collocate with *more* more readily than do the monosyllabic ones.

The experiment reported in this paper in no way claims to have provided firm answers to questions on standards of English usage in Canada, Any serious attempt to do so would entail a survey on a much larger scale, perhaps similar to the Survey of English Usage directed by Randolph Quirk at the University College London.

#### APPENDIX A

Test items (with a tentative classification) Case erosion

- 1. It's between you and me/I.
- 2. Whom/Who did you see.
- 3. It's *I/me*.
- 4. This letter is for you and him/he.

Preterite/participial confusion

- 5. He has drunk/drank the water.
- 6. I'm worn/wore out.
- 7. I've gone/went there often.
- 8. He saw/seen it happen.

Strong/weak verb formation

- 9. He sneaked/snuck by when my back was turned.
- 10. He sought/seeked political asylum in Canada.
- 11. Look what the cat dragged/drug in.
- 12. He lent/loaned me some money.

Subjunctive of be

13. If he were/was here, things would improve.

Semantic shift

14. It was lying/laying on the floor.

15. Don't go away, I'll be back in a moment/momentarily.

16. I hope/Hopefully we will have nice weather tomorrow.

Loss of adjective inflection

17. 24K gold is purer/more pure than 18K gold.

Adjective/adverb confusion

18. It's very/some hot out there.

19. I hope you do well/good on your exam.

20. It's really/real hot in here.

Number agreement

21. Everybody gets *his/their* reward.

Collocation of *less* with count nouns

22. He's taking *fewer/less* courses than he should.

Unusual ellipsis

23. The dog wants to go/wants out.

24. He wants to get/wants off the bus.

Loss of negative agreement with anymore.

25. A lot of people are away at present/anymore.

Prepositional variation

26. It's behind/in back of the house.

27. He's at/to home.

28. My book is different from/than yours.

Clause conjunctives

29. It looks as if/like he will go.

30. Since/seeing as he's gone, we'll leave.

31. The reason I can't go is that/because the road is washed out.

# Verb reclassification

32. I am going to visit/visit with my friend.

### APPENDIX B

Summary of items exhibiting statistically significant shifts in usage between formal and informal registers. The significance levels of the shifts are given in percent, and the t-values for each register are given only as plus or minus indicating a disposition towards standard and nonstandard usage, respectively. Where the signs of formal and informal are the same, one is duplicated to indicate the direction of the shift. In cases where the t-value is not statistically significant, it is shown as (+) or (-).

		Sig. Level	t-value	
		Neg. Shift	Informal	Formal
(1)	between you and $me/I$	1%	+	(+)
(15)	in a moment/momentarily	1%	+ +	+
(16)	I hope/hopefully	5%	+ +	+
(17)	purer/more pure	1%	+ +	+
(26)	behind/in back of	10%	+ +	+
(27)	at/to home	10%	+ +	+
		Positive Shift		
(2)	<i>Who/whom</i> did you see	1%		-
(3)	it's I/me	1%		-
(4)	for you and him/he	1%	-	+
(8)	he saw/seen	5%	+	++
(9)	sneaked/snuck	1%	-	(+)
(13)	) if he <i>were/was</i> here	1%	-	(+)
(19)	) do well/good	5%	+	+ +
(23)	) wants to go/wants out	1%	-	+

(24) wants to get/wants off	1%	(+)	+
(29) it looks as <i>if/like</i>	1%	-	(+)
(30) <i>since/seeing as</i> he's gone	10%	+	+ +
(31) the reason is that/because	10%		-

### APPENDIX C

Summary of items showing a tendency towards nonstandard usage and only a marginal tendency towards standard usage in either register.

	Item		t-value
		Informal	Formal
(2)	who/whom	-	-
(3)	it's I/me	-	-
(21)	everybody gets his/their	(-)	_
(28)	different from/than	-	-
(31)	the reason is that/because		-
(13)	if he were/was	-	(+)
(29)	it looks as if/like	-	(+)
(9)	sneaked/snuck	(-)	(+)
(1)	between you and $me/I$	+	(+)
(4)	for you and him/he	-	+
(23)	wants to go/wants out	-	+
(24)	wants to get/wants off	(+)	+
(The	remainder of the items fro	m the total se	t of 32 items had

a statistically significant positive t-value in both registers.)

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