

# Linguistics Today – Facing a Greater Challenge

Edited by Piet van Sterkenburg

*Typology*

*Endangered Languages*

*Methodology and Linguistics*

*Language and the mind*

## Linguistics Today - Facing a Greater Challenge



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*Edited by*

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# Table of contents

Preface	VII
Evidentiality: Problems and challenges <i>Alexandra Y. Aikhenvald</i>	1
Towards a less 'syntactic' morphology and a more 'morphological' syntax <i>Stephen R. Anderson</i>	31
Linguistic universals and particulars <i>Emmon Bach</i>	47
Language planning and language policies: Issues and prospects <i>Ayo Bamgbose</i>	61
Computational lexicons and corpora: Complementary components in human language technology <i>Nicoletta Calzolari</i>	89
Historical linguistics: The state of the art <i>Lyle Campbell</i>	109
Coherent fieldwork <i>Daniel L. Everett</i>	141
State of the art in Computational Linguistics <i>Giacomo Ferrari</i>	163
State-of-the-art paper: Lexicology and lexicography: Milestones in metalexicography <i>Rufus H. Gouws</i>	187
Pragmatics: State of the art <i>Robert M. Harnish</i>	207
Phonological dialectics: A short history of generative phonology <i>Harry van der Hulst</i>	217

Types of languages and the simple pattern of the core of language <i>Petr Sgall</i>	243
Attempt at the revival of Warrungu (Australia): Its cultural and scientific significance <i>Tasaku Tsunoda</i>	267
The future of creolistics <i>Kees Versteegh</i>	305
How the study of endangered languages will revolutionize linguistics <i>D. H. Whalen</i>	321
Index	343

## Preface

Every five years the Permanent International Committee of Linguists (CIPL) organises a world congress for linguists. And every five years the Committee faces the challenge of presenting a programme at the highest possible level. In order to guarantee such quality for the Congress planned for July 2003 in Prague the CIPL's Executive Committee established a Scientific Committee consisting of representative of the CIPL and the Local Organizing Committee. Professor Ferenc Kiefer chaired the Scientific Committee.

Monitoring the scientific quality of the Congress is not the only challenge facing the CIPL. The Committee has also to select relevant linguistic themes both for the plenary gatherings and the parallel sessions.

Since the Congress addresses the entire international community of linguists, every linguist must be able to find in the Congress something of his/her own particular research field. At the same time the themes chosen and the experts introducing the themes must give the average linguist the feeling that by attending the congress he will obtain a bird's eye view of current developments within the most authoritative linguistic paradigms and topics at the start of the 21st century.

Against the background of the above considerations, the CIPL Executive Committee decided to focus on four major topics which play an important role in today's linguistic debate: 1. Typology, 2. Endangered Languages, 3. Methodology and Linguistics (including fieldwork) and 4. Language and the mind. We are pleased to have been able to find leading experts who will introduce the four themes in the course of the congress. We are firmly convinced that their introductions should be placed as quickly as possible in the hands of fellow linguists wishing to go into greater detail in their areas of specialisation.

Accordingly, our idea has been to produce a volume containing the lectures given during the plenary sessions, and also those given in the parallel sessions which depict the state-of-the-art of: 1. Language planning and language policies, 2. Pidgins, Creoles, Language in contact, 3. Comparative Linguistics, 4. Computer Science and Linguistics, 5. Language and Fieldwork, 6. Techniques for Language Description, 7. Syntax and Morphology, 8. Phonetics and

Phonology, 9. Lexicology and 10. Social Pragmatics. In this case, we believed, brevity is the soul of wit.

We hope that this book will be a welcome tool for all our fellow linguists wishing to find their way quickly in current developments.

We would like to thank the John Benjamins publishing house in Amsterdam for their willingness to print and distribute this book.

Paolo Ramat  
Interim President CIPL

Piet van Sterkenburg  
Secretary-General CIPL

# Evidentiality

## Problems and challenges

Alexandra Y. Aikhenvald

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### 1. Evidentiality: Some key concepts

No two languages are entirely the same, nor are they entirely different. As Franz Boas (a founding father of modern linguistics) said, languages differ not in what they can say but in what they must say, that is, in what kind of information must be obligatorily expressed. In a number of languages, the nature of the evidence on which a statement is based has to be specified for every statement – whether the speaker saw it, or heard it, or inferred it from indirect evidence, or learnt it from someone else. This grammatical category, whose primary meaning is information source, is known as ‘evidentiality’. In Boas’s (1938: 133) words, “while for us definiteness, number, and time are obligatory aspects, we find in another language location near the speaker or somewhere else, source of information – whether seen, heard, or inferred – as obligatory aspects”.<sup>1</sup>

Consider Tariana, an Arawak language spoken in the multilingual area of the Vaupés in northwest Amazonia. In this language, one cannot simply say ‘José played football’. Just like in all other indigenous languages from the same area, speakers have to specify whether they saw the event happen, or heard it, or know about it because somebody else told them, etc. This is achieved through a set of evidential markers fused with tense (structurally similar to the ones described for Tucanoan languages by Barnes 1984, 1999; and Malone 1988). Omitting an evidential typically results in an ungrammatical and highly unnatural sentence (see details in Aikhenvald 2003b, 2003d).

If one saw José play football, (1) would be appropriate (here and elsewhere evidential morphemes are in bold type).<sup>2</sup>

- (1) *Juse irda di-manika-ka*  
José football 3sgnf-play-REC.P.VIS  
'José played football (we saw it)'

If one just heard the noise of a football game but could not see what was happening, (2) is the thing to say:

- (2) *Juse irda di-manika-mahka*  
José football 3sgnf-play-REC.P.NONVIS  
'José played football (we heard it)'

If one sees that the football is not in its normal place in the house, José and his football boots are gone (and his sandals are left behind), with crowds of people heading for the football ground, this is enough for us to infer that José is playing football. We then say (3).

- (3) *Juse irda di-manika-nihka*  
José football 3sgnf-play-REC.P.INFR  
'José played football (we infer it from visual evidence)'

If José is not at home on a Sunday afternoon, we can safely say (4). Our inference is based on the general knowledge about José's habits: he usually plays football on Sunday afternoon.

- (4) *Juse irda di-manika-sika*  
José football 3sgnf-play-REC.P.ASSUM  
'José played football (we infer it from general knowledge)'

The difference between the two inferential evidentials ('assumed', as in (4) and 'inferred', as in (3)) lies in access to visual evidence of something happening and to the degree of 'reasoning' involved. The less obvious the evidence and the more the speaker has to rely on reasoning based on general knowledge (or common sense), the more chance there is that the generic inferred will be used. A 'specific inferred' evidential is used to refer to something one has not seen, but which is based on obvious evidence which can be easily observed.

**If one learnt the information from someone else, then (5) – with a reported evidential – is the only correct option:**

- (5) *Juse irda di-manika-pidaka*  
José football 3sgnf-play-REC.P.REP  
'José played football (we were told)'

Languages which have 'evidentiality' as a grammatical category vary in how many types of evidence they mark. Some distinguish just two terms (eyewit-

ness and non-eyewitness, or reported and everything else), while others six or more terms. Evidentiality is a category in its own right, and, contrary to some assertions, not a subcategory of epistemic (or some other) modality, or of tense-aspect-mood.

Every language has some way of making reference to the source of information, or type of evidence. This does not mean that every language has grammatical evidentiality. Having lexical means for optional specification of the source of knowledge is probably universal – cf. English *I guess, they say, I hear that* etc. as well as lexical verbs such as *allege* (e.g. *the alleged murderer of X*). These lexical means often include adverbial expressions such as *reportedly* in English. Many languages have introductory clauses, such as English *it seems to me* or particles, such as Russian *jakoby, mol, deskatj* ‘hearsay’ (Rakhilina 1996). Modal verbs are often used to express meanings connected with information source (cf. discussion of French *devoir* in Kronning forthcoming). These expressions are not obligatory and do not constitute a grammatical category; consequently, they are tangential for the present discussion. They may, however, provide historical sources for evidential systems. The expression *dizque* ((one) says that) has become a reported speech marker in some varieties of Brazilian Portuguese (Appendix 4 in Aikhenvald 2003c). Travis (forthcoming) is a fascinating study of how the construction, *dizque*, has become virtually grammaticalised in Colombian Spanish as a reported and general non-eyewitness evidential.

Grammatical categories – such as conditional mood, perfective aspect or complementation – can each acquire a secondary evidential-like meaning without directly relating to source of information. Such extensions of grammatical categories to evidential-like meanings are called ‘evidentiality strategies’ in Aikhenvald (2003a, in press) and are outside the study of evidentiality proper.

Evidential markers may gain semantic extensions such as conjecture, the probability of an event or the reliability of information (often called ‘epistemic’ meanings). Evidentials may extend to cover unusual and ‘surprising’ information (termed ‘mirative’ in DeLancey’s seminal 1997 paper). Types of extensions depend on the system: for instance, in two-term systems the non-eyewitness or reported evidential often acquires an epistemic or a mirative extension, while in larger systems it is often the inferential evidential which does. Some typical semantic extensions depend on the choice made in another category – see the examples in §2 below.

Evidentiality may be independent of clause type, modality or tense-aspect choice. The marking can be achieved in a variety of ways: some languages have dedicated affixes or clitics, while others have their evidentiality marking fused

with another category, for instance, with tense (this is discussed at length in Chapter 2 of Aikhenvald in press). The choice made in the evidentiality system may depend on tense, aspect, or clause type (see Aikhenvald & Dixon 1998a).

An excellent summary of work on this category is in Jacobsen (1986); and other papers in the seminal collection by Chafe and Nichols (1986). Another attempt – albeit from a different theoretical stance – is in Guentchéva (1996). Johanson and Utas (2000) present an informed study of two-term evidentiality systems in Turkish, Iranian and a few other neighbouring languages. A typological overview accompanied by studies of several individual systems is in Aikhenvald (2003a). This typology is based on the examination of grammars of over 500 languages; a fuller discussion with examples is in Aikhenvald (in press).

I first present a brief overview of types of evidentiality system and their major extensions in the world's languages (§2). Unlike most other categories, evidentiality can be marked more than once in a clause – this is discussed in §3. The scope of negation and truth value of evidentials are discussed in §4. The choice of evidentials can be determined by speech genres and cultural attitudes – this is addressed in §5.

## 2. Evidentiality systems, and semantics of evidentials

Languages with evidentiality fall into a number of subtypes, depending on the number and kinds of sources specified. The simplest evidentiality systems consist of just two distinctions; more complicated ones involve up to six (or possibly more). The semantic content of each distinction depends on the type of system (hence, labels may sometimes be misleading). For instance, in a two-term system 'eyewitness' hardly ever implies just visually acquired information. It may cover visual and auditory information (and may also cover information obtained through other senses), as in Jarawara (Arawá: Dixon 2003). The opposition of something seen and non-seen within a single sentence is shown in (6).

- Jarawara  
(6) *Wero kisa-me-no*,  
name get.DOWN-BACK-IP.NONEYEW.M  
*ka-me-hiri-ka*  
be.in.motion-BACK-RP.EYEW.M-DEC.M  
'Wero got down from his hammock (which I didn't see), and went out  
(which I did see)'

In (7), also from Jarawara, the eyewitness evidential refers to the noise of the boat that the speaker hears (before he can see the boat itself):

- (7) [*moto*            *ati*] *ka-tima-re-ka*  
 motorboat(m) noise be.in.motion-UPSTREAM-IP.EYEW.m-DEC.m  
 ‘The noise of the motorboat was coming upstream (the noise could be heard)’

In some languages, the eyewitness term may cover both visual information and something personally witnessed, but may never refer to strictly auditory data – as in Kalasha and Khowar (Dardic: Bashir 1988:48–54). In other languages, e.g. Yukaghir and Shipibo-Konibo, the eyewitness term can refer to any appropriate sense. A non-eyewitness term is unlikely to refer to second-hand information if there is a special evidential used for reported information (such is the case in M̄ky (isolate: Monserrat & Dixon 2003). The ‘auditive’ in Samoyede languages (e.g. Nganasan, masterly analysed by Gusev forthcoming) mostly describes information acquired through hearing, and is also used to describe perception through other senses (except for vision). One expects more semantic complexity of individual terms within smaller systems than within larger ones.

Two-term systems cover:

A1. EYEWITNESS AND NON-EYEWITNESS. Such a distinction often – but not always – applies just in the past tense, as in Jarawara, Yukaghir, and M̄ky, Godoberi (Northeast Caucasian: Dobrushina & Tatevosov 1996:94–97), Yanam (Yanomami: Gomez 1990:97), Kalasha and Khowar (Dardic: Bashir 1988:48–54). The two evidentiality values in one sentence are illustrated in (11):

- Kalasha
- (8) *a aya’ a*                            *âgar’ Zot*    *ka’da*  
 I here come(PAST.EYEW) fire    already do(PAST.NONEYEW)+3  
 ‘I came here (which I witnessed personally), (and someone) (had) already made the fire (in my absence so I didn’t see it being done)’

The non-eyewitness terms often have overtones of ‘conceptual distance’ and ‘unprepared mind’ (involving unexpected, new and surprising information), and also non-commitment of the speaker to the truth of the utterance (see Bashir 1988, on these in Kalasha and Khowar). With the first person, non-eyewitness forms often have an overtone of unconscious and inadvertent

action, as in (9) (similar examples are in Dixon 2003; also see Curnow forthcoming).

Kalasha

(9) *a*            *âga'*    *ne*    *hu'la him*

I(NOM) aware not become(PAST.NONEYEW)+is

'I didn't remember (to get up and make your breakfast, although I had intended to)'

A2. NON-FIRSTHAND AND EVERYTHING ELSE. The non-firsthand term typically covers inference based on visible traces and reported information, as in Abkhaz (Hewitt 1979; Chirikba 2003), and in a number of Northeast Caucasian languages (e.g. Hunzib: van den Berg 1995), Ugric languages Mansi (Rombandeeva 1973: 137–138, 141–142); Khanty (Nikolaeva 1999), and a few Samoyede languages, e.g. Nenets (Décsy 1966: 48), Enets (Künnap 1999), and Selkup (Kuznetsova et al. 1980: 241). The non-firsthand term may refer to indirect evidence and inference gained through non-firsthand experience (as in Meithei: Tibeto-Burman: Chelliah 1997: 221–224). In Hare and a few other Athapaskan languages (Delancey 1990), the non-firsthand evidential covers inference and hearsay; it has a mirative extension. Non-firsthand evidentials may also be employed to focus the listener's attention on some crucial point in the narrative, as in Abkhaz (Chirikba 2003); compare 'argumentative' meanings of evidentials in Western Armenian (Donabédian 2000).

Non-firsthand evidentiality in Turkic languages presupposes that the information was acquired 'indirectly', through a mediator (Johanson 2003). This kind of evidentiality is called 'indirectivity' by Johanson (2000, 2003).

With the first person, non-firsthand evidentials may refer to inadvertent action – as in Hare; – to something surprising for the speaker – as in Archi (Kibrik 1977: 230) – or to both – as in Western Armenian (Donabédian 1996: 94). In Meithei and in Archi, a non-eyewitness evidential may indicate that the speaker has evidence about the proposition which the hearer does not have – that is, the perspective of the hearer (similarly to 'perspective' questions in Sherpa, Tibeto-Burman, when the speaker 'takes the perspective of the hearer in order to establish empathy with the hearer': Chelliah 1997: 222).

The two-term systems of types A1 and A2 often have no epistemic extensions. Probability is indicated with moods or other epistemic markers. For a discussion of further functions, developments and semantic complexities within such systems see Johanson (2003), Csátó (2000) and other papers in

Johanson and Utas (2000), papers in Guentchéva (1996), and a summary in Chapter 2 of Aikhenvald (in press).

A3. REPORTED (OR ‘HEARSAY’) AND EVERYTHING ELSE. Cross-linguistically, this is by far the most widespread kind of system. The reported term is marked, and the non-reported (‘everything else’) term is not marked; there are no systems of the opposite sort. In Enga (Engan family, Papuan), quoted utterances are marked with the suffix *-na* added to the last syllable of the predicate (Lang 1973:xli). Similar systems are found in many other Papuan languages, in Lezgian (Lezgian, Northeast Caucasian) (Haspelmeth 1993:148), in Tibeto-Burman, e.g. Kham (Watters 2002), in numerous South American languages (Aikhenvald & Dixon 1998b), and in Estonian and Livonian (Balto-Finnic) (where the reported forms go back to participles: see Stolz 1991:45–50; Perrot 1996; Campbell 1991).

The reported evidential – which may subsume secondhand and thirdhand unless there is a special way of expressing a thirdhand (see (17) below, from Tsafiki) – shows a considerable degree of semantic uniformity in any type of evidential system. It often acquires an overtone of information the speaker does not vouch for, both in small systems and in large ones. In Tsafiki, the reported is used to indicate ‘non-participation’ of the speaker in the event; while in Tauya (Papuan area: MacDonald 1990) it signals false presupposition. In the following example from Estonian (Ilse Lehiste and Reet Bergman, p.c.), “reported” implies ‘shifting’ responsibility for the information and relating facts the speaker does not vouch for (cf. Fernandez-Vest 1996:171).

Estonian

- (10) *Ta olevat arsti-teaduskonna lõpeta-nud*  
 he be.REPORTED doctor-faculty.GEN finish-PAST.PARTICIPLE  
 ‘He is said to have completed his studies of medicine (but I wouldn’t vouch for it)’

Similar extensions are also found for the marking of reported information in the process of grammaticalisation: *dizque* ‘it is said that’ in Colombian Spanish marks not just reported speech. Its other meanings include uncontrolled action (especially with first person – cf. the first person effect of non-eyewitness evidential discussed above), or something the speaker finds new or inapt (Travis forthcoming).

Three-term systems involve at least one sensory specification.

B1. VISUAL (OR DIRECT), INFERRED, REPORTED. Quechua languages have three evidentiality specifications: direct evidence (*-mi*), conjectural (*-chi*, *chr(a)*) and reported (*-shi*) (Floyd 1997). Similar systems are found in Jaqi languages such as Aymara (Hardman 1986), Shasta (Shastan family: Silver & Miller 1997:38), Maidu (Shipley 1964:45), in Northern Embera languages from Colombia (Mortensen 1999:87–88), and in Qiang (LaPolla 2003).

B2. VISUAL, NONVISUAL SENSORY, INFERRED. An example is Washo (Jacobsen 1986:8), which has visual, auditory and a marker of “ex post de facto inference with some connotation of surprise”.

B3. NONVISUAL SENSORY, INFERRED, REPORTED. In Retuarā (Central Tucanoan: Strom 1992:90–91) the three suffixes that give evidential information refer to: (1) strictly auditory information, (2) assumed information, and (3) secondhand information; see Gusev (forthcoming) for a similar system in Nganasan (Samoyedic).

Four-term systems involve at least one sensory specification. If there is just one sensory evidential, additional complexity may arise within evidentials based on inference (C2, C3), or on reported information (C4). Visually obtained data can be contrasted with data obtained through hearing or smelling (as in C1), or through inference (which may be of different kinds, as in C2–3); many of these systems have a marker for reported information.

C1. VISUAL, NONVISUAL SENSORY, INFERRED, REPORTED. Such a system is found in a number of East Tucanoan languages spoken in the same area as Tariana, and possibly in Xamatauteri (Yanomami family) (see Aikhenvald & Dixon 1998b).

C2. VISUAL, INFERRED (1), INFERRED (2), REPORTED. In Tsafiki (Dickinson 2000:407–409) the visual (used for ‘directly witnessed events’) term is formally unmarked; there is one suffix marking information inferred from direct physical evidence, another for inference from general knowledge, and an additional one for reported, or hearsay. A similar system is found in Pawnee (Caddoan: Parks 1972) and Eastern Pomo (Pomoan: McLendon 2003).

C3. EYEWITNESS (DIRECT), INFERRED (1), INFERRED (2), REPORTED. Shipibo-Konibo (Valenzuela 2003) has an eyewitness evidential (which refers to firsthand sensory knowledge) *-ra*, an inferred evidential based on reasoning

or observable evidence *-bira*, another inferred, *-mein*, based on speculation, and a reported *-ronki*.<sup>3</sup>

C4. VISUAL, INFERRED, REPORTED<sub>1</sub>, REPORTED<sub>2</sub>. Southeastern Tepehuan (Uto-Aztecan: Willett 1991:161–166) is said to distinguish eyewitness (‘perceived by the speaker’), inferred, and two kinds of reported. One is ‘previously known to the hearer’ (it marks a reference to a report that both speaker and hearer have previously heard, to something the hearer has previously said to the speaker), and the other marks information ‘previously unknown to the hearer’.

We have very few clearly attested instances of evidentiality systems with over four terms. Known five-term systems include:

D1. VISUAL, NONVISUAL SENSORY, INFERRED, ASSUMED, REPORTED is found in Tariana (examples (1)–(5) above); Tuyuca, and in a few other Tucanoan languages.

D2. NONVISUAL SENSORY, INFERRED (based on reasoning with overtones of surprise), INFERRED (based on assumption), INFERRED (based on physical appearance), REPORTED is found in Western Apache (de Reuse 2003).

Further examples of complex multi-term systems include the Nambiquara languages from southern Amazonia (Lowe 1999:275–276). There is an obligatory marking on the verb for whether a statement is (i) eyewitness (implying that the speaker has seen the action they are reporting); (ii) inferred; (iii) assumed (“the speaker’s claim. . .based either on seeing an associated simultaneous action and making an interpretation THEREFROM, OR ON seeing a set of circumstances which must have resulted from a previous action and making an inference; different suffixes mark these two options”); (iv) reported (“the speaker is simply passing on information they have heard from another speaker”) and (v) ‘internal support’ (“the speaker reports their ‘gut feeling’ that which they assert must be so”). More detailed investigation is required for such systems.

Systems with more than three evidential terms show considerable similarities in their semantic extensions. Within such multi-term systems, the visual or the direct evidential indicates events perceived through seeing and may be extended to cover direct observation, participation, control, generally known and observable facts, and also certainty. See the detailed analysis of the direct evidential *-mi* in Quechua by Weber (1986, 1989) and Floyd (1997).

The nonvisual evidential in multi-term systems always covers evidence which was heard and sometimes also information obtained through senses other than hearing – such as taste and smell, and also cognitive processes, as in Eastern Pomo, Nganasan and Tariana. Nonvisual may acquire epistemic extensions: it often implies that the speaker was not in control, or did something involuntarily and is generally unsure of what was happening. In Tariana and East Tucanoan languages, these meanings are characteristic of the nonvisual evidential with first person.

In systems with more than one inferential, the difference lies in the type of evidence for inference: for instance, in D1 systems, the inferred evidential refers to the inference based on visual evidence (example (3)), while the assumed evidential refers to the inference based on reasoning (example (4)). And in C3, the inference based on speculation is marked differently from inference based on other types of evidence. The inferred in a three-term system may acquire epistemic extensions of uncertainty and probability, as in Qiang and in Tsafiki. In Shipibo-Konibo (with a four-term system), the two inferred evidentials (*-bira* and *-mein*) have overtones of uncertainty and doubt. In Western Apache (D2), inferentials have epistemic and mirative extensions, while in languages of the D1 type they do not. In fact, in many languages with multi-term evidentials, epistemic meanings are expressed through an array of verbal categories other than evidentiality: this is the case in Tsafiki, Tariana and Tucanoan languages.

The meaning of each evidential can be demonstrated by its ‘lexical reinforcement’ in actual narratives. For instance, Tariana speakers often add a comment ‘I saw it’, to strengthen a visual evidential; or ‘so-and-so told me’, to justify using a reported. Such occurrences, together with native speakers’ intuition, help identify the core meanings of evidentials as marking information source.

In systems with three or more evidentials, the visual (or direct, or eyewitness) evidential may acquire a zero-realisation, i.e. be formally unmarked: such is the case in Pawnee (Caddoan: Parks 1972), Eastern Pomo, Tsafiki, Qiang (see discussion in LaPolla 2003) and Koreguaje (Gralow 1993). In Tariana, the visual evidential is the least formally marked. In Nganasan, a verb without any evidential morpheme is usually – but not always – interpreted as describing visually acquired information, or information of which the speaker is certain (Gusev forthcoming: 3); see §5.

Or the absence of any marking can simply ‘imply that the speaker was an eyewitness of the event’, as in Western Apache. In Hixkaryana (Derbyshire 1985:255), absence of a member of the set of ‘verificational particles’ – which express some evidentiality-related meanings – specifically marks ‘eyewitness’ as

contrasted with ‘hearsay’. This suggests a tendency towards interpreting visual or eyewitness perception as ‘default’.

An overwhelming majority of languages with evidentiality make no evidentiality distinction in commands; examples include Sochiapan Chinantec, Samoyedic languages, Jarawara, Wakashan and Turkic languages. Just some languages – for instance, Epena Pedee (Chibchan) – do have a reported evidential in both affirmative and imperative clauses (see Harms 1994).

One language can have several subsystems of evidentials, depending on clause type. Tariana distinguishes five evidentials – visual, nonvisual, an inferred, an assumed and a reported (see examples (1)–(5)) in affirmative clauses, only three in interrogatives and in apprehensives (where the reported and the inferred based on reasoning is not used), and just two (reported and non-reported) in commands. Purposives have a visual-nonvisual distinction (A1 system). Northern Embera languages have visual, inferred and reported (B1) in affirmative clauses, and Shipibo-Konibo has four evidentials (C3 above); but words in both languages, there is just reported versus non-reported evidentiality (an A3 system) in commands. Maidu has a three-term system of visual, inferred and reported (B1) in affirmative clauses, and just a visual-nonvisual distinction in commands (Shipley 1964:45, 51).

In most cases, the largest systems are attested in affirmative clauses. An exception is Nivkh (isolate: Gruzdeva 1992:60), where the visual-nonvisual distinction is found just in apprehensives. What kinds of systems can coexist – and how – remains a matter for further investigation.

Evidentiality specifications may enter into paradigmatic relations with morphemes of different sorts. In M̄yky, the reported and the inferred evidentiality markers occur in the same slot as negation and are thus mutually exclusive. In Ladakhi (Bhat 1999:86–87), most Samoyedic languages (cf. Gusev forthcoming on Nganasan, Kuznetsova et al. 1980 on Selkup), Yukaghir (Maslova 2003) and Abkhaz (Chirikba 2003) evidentiality markers occur in the mood or modality slot in a verbal word, and are thus mutually exclusive with conditional, imperative, interrogative markers and so on. As a result of such restrictions, evidentials are incompatible with various categories.

The choice of an evidentiality subsystem may depend on a choice made in the tense system or in the mood system (see Aikhenvald & Dixon 1998a). Jarawara has an eyewitness-non-eyewitness distinction restricted to the three past tenses; reported, and inferred are marked independently of tense distinctions, and of each other. M̄yky (Monserrat & Dixon 2003) distinguishes visual versus nonvisual as an obligatory specification in the past tense of the declarative mood; reported and inferred occur in a different slot in verb structure,

independently of tense (and no evidentials are distinguished in commands or questions). Archi has an A2 system in past tenses; the language also has a reported evidential, independent of an A2 system (Kibrik 1977: 231–232).

A language may mark evidentiality, but not have it as a single grammatical category. Evidentials may occupy different slots in the verbal word, and differ in the restrictions on co-occurrence with other categories. The evidentiality marking itself may be obligatory, but different evidentiality specifications may be ‘scattered’ throughout the verbal system, so that they by no means constitute one paradigm, let alone a unitary category. Makah (Wakashan), as described by Jacobson (1986), has obligatory evidentiality marking, ‘scattered’ among suffixes of different orders; they enter into different paradigmatic oppositions with other (not necessarily evidential) affixes and thus cannot be considered a morphologically unitary category. Japanese is another example of a language with different ways of marking source of information, but without evidentiality as a unitary grammatical category (Aoki 1986; Aikhenvald 2003a). In West Greenland (Fortescue 2003), evidential meanings are expressed with several kinds of verbal derivational suffixes, a cooptative enclitic and an adverbial particle. These affixes do not form a category of evidentiality – they are in opposition to other derivational suffixes, most of which have nothing to do with information source.

These are just a few problems concerning the status of evidentials. In the remaining sections, I concentrate on features of evidentials which set them apart from most other grammatical categories. These are: double marking of evidentiality (§3); the scope of negation and truth value of evidentials (§4); and evidentials and their use in discourse (§5).

### 3. Double marking of evidentiality

Some grammatical categories can be marked more than once in a constituent or a clause. Multiple number marking usually implies the expression of the same morphosyntactic category, with the same meaning, more than once in one form, thus creating redundancy (Anderson 1993; also Chapter 8 of Aikhenvald 2003d and 2003e, for Tariana; and Hill in press, for Cupeño). Double case consists in marking several clausal functions on one noun phrase (for Australian languages, see Dench & Evans 1988; Dixon 1998, 2002: 147–152; see Aikhenvald 1999 for a similar phenomenon in Tariana). Double marking of gender may involve agreement with several different heads marked within the same noun phrase (Aikhenvald 1999). And see Dixon (forthcoming) on the

semantic implications of marking person more than once within the Jarawara predicate.

Marking evidentiality more than once appears to be totally distinct from multiple expression of any other category: it is never semantically redundant. Having several evidentiality markers in one clause allows speakers to express subtle nuances relating to types of evidence and information source, either interrelated or independent of one another. The instances found so far are discussed below, (i)–(iv).

(i) Two different evidentials mark information acquired by the author of the statement in different ways for different constituents of a clause. E1 appears on the predicate and E2 on a noun phrase (core or oblique).

Example (11), from Jarawara, illustrates this. Jarawara has an obligatory eyewitness versus non-eyewitness distinction in all past tenses, and also a reported evidential. The oblique noun phrase in (11) is marked for reported evidentiality: the speaker knows just by hearsay that the place where the day dawned was the mouth of the Banawá river. The story itself relates the personal experience of the speaker which took place a long time ago, and is thus cast in remote past eyewitness (marked on the verb) (R. M. W. Dixon, p.c.). Constituents are in square brackets. (Similar examples are found in Nambiquara: Lowe 1999: 274.)

- (11) [[[*Banawaa batori*]-*tee-mone*]-*jaa*] *faja otaa*  
 Banawa          mouth-CUST-REPF-AT    then    Insg.exc.S  
*ka-waha-ro*                                  *otaa-ke*  
 APPL-become.dawn-RPEF    Insg-DECL  
 ‘Then the day dawned on us (EYEWITNESS) (lit. we with-dawned) at the  
 place REPORTED to be (customarily) the mouth of the Banawá river’

Such differential marking of information source on different clausal constituents is reminiscent of nominal tense marking whereby the time reference of a noun or a noun phrase may be different from that of the clause, as in Tariana *waha panisi-pena alia-ka* (we house-FUT exist-REC.P.VIS) ‘there was our future house (which we saw in the recent past)’. For a typological account of nominal tense marking, see Nordlinger and Sandler (forthcoming).

(ii) Information acquired by the author of the statement comes from two different sources, one marked by E(evidential)1 and the other by E2. E1 and E2 either confirm or complement each other.

Qiang (Tibeto-Burman: LaPolla 2003) has a three-term (B1) system consisting of visual, inferential and reported evidentials. The visual evidential can occur together with inferential if visual information were used to confirm the statement made on the basis of an inference. The situation described in (12) is as follows: the speaker first guessed that someone was playing drums next door, and then went next door and saw the person holding a drum or drumsticks. The combination of two evidentials has the sense of ‘as I had guessed and now pretty-well confirm’ (LaPolla 2003:69–70).

Qiang

- (12) *oh, the: z̥bə z̥ete-k-u*  
 oh 3sg drum beat-INFR-VIS  
 ‘Oh, he WAS playing a drum!’

Shipibo-Konibo has a direct evidential (which covers information acquired with any physical sense, be it visual or auditory), two inferentials and a reported. The direct evidential can combine with an inferred evidential, to indicate that the reasoning or speculation is based upon evidence coming from the speaker (Valenzuela 2003:44–46). Unlike in Qiang, the speaker uses the inferential evidential as a way of interpreting the evidence acquired visually, rather than a visual evidential to confirm one’s inference. In (13), the mother hears her child cry (hence the direct evidential), and infers that her mean relatives have buried her son alive (the inference is marked with the evidential *-bira*). The two evidentials appear on different constituents within the same clause. (The two clauses in (13) are separated by a comma.)

Shipibo-Konibo

- (13) *koka-baon-ra jawe-bira miin-ke,*  
 maternal.uncle-PL:ERG-DIR.EV what:ABS-INFR bury-CMPL  
*bake-ra sion i-t-ai*  
 child:ABS-DIR.EV ONOM do.INTR-PROG-INC  
 ‘What could your uncles have buried (direct evidence as a basis for inference), a boy is crying (direct evidence: the speaker can hear him cry)’

The inferential evidential can combine with the reportative *-ronki*. In (14), the narrator comments on the meaning of a possibly archaic word *benta*. The reported evidential is used to refer to the source of information on *benta* (hearsay), while the inferential *-bira* implies an attempt to make a guess about the meaning of the word. In fact, it turns out that the speaker does have a good guess of the meaning: *benta* means ‘sexual partner’.

## Shipibo-Konibo

- (14) *jawe joi-ronki i-bira-[a]i benta ja-boan ak-á*  
 what word-REP DO.INTR-INFR-INC *benta* 3PL:ERG DO.TRANS-PP2  
*joi-bi-ribi*  
 language-EMP-also  
 ‘What could *benta* (which I learnt of by hearsay) have meant in their  
 language (that I could infer)?’

In Xamatauteri (Ramirez 1994: 317), the ‘non-eyewitness’ evidential – which implies an inference based on general assumption – can combine with the inferential – which implies inference based on visual evidence – to indicate that there is additional support for an assumption: for instance, an assumption that someone was bitten by a snake will be confirmed by the fact that the speaker did see the wound caused by a snake bite.

Kamaiurá (Tupí-Guaraní: Seki 2000: 344–347) has six evidential particles. Two of them, reported (*je*) and ‘attested by the speaker’ (or firsthand) (*rak*), express source of information. The meaning of the other four is to do with type of evidence, direct or indirect. Direct evidence is either visual (*ehe/he*) or previously existent and now gone (*heme*). Indirect evidence (or inference) can be based on visible traces of an event (*inip*); or on the speaker’s opinion or deduction (*a’anj*). Markers of source of information and of type of evidence can co-occur in one sentence. In (15), the firsthand evidential *rak* appears because the speaker saw the snake who bit a man. Since the snake had already gone by the time the sentence was produced, *heme* ‘previously existent direct evidence’ has to also be added.

- Kamaiurá (Seki 2000: 346)
- (15) *moĩ-a rak ij-u’u-me*  
 snake-NUCLEAR.CASE FIRSTHAND 3-bite-GER  
*heme-pa*  
 PREV.DIR.EV-MALE.SPEAKER  
 ‘It was a snake that bit him (the speaker saw it but the snake is gone now)’

(iii) Information is acquired by the author of the statement from different but interconnected sources: E1 marks that the information was reported to the narrator; E2 marks the source of reported information.

In Tsafiki (Dickinson 2000: 408), with a four-term evidential system of the C2 type, a combination of a non-eyewitness with a reported evidential specifies the source of reported information. Reported evidential can combine with any

of the other three, indicating “the type of information the original informant had for the assertion”. In (16), those who told the speaker that Manuel had eaten had direct physical evidence of this happening (such as dirty dishes).

Tsafiki

- (16) *Manuel ano*  
 Manuel food  
*fi-nu-ti-e*

eat-INFERENCE.PHYSICAL.EVIDENCE-HEARSAY-DECLARATIVE

‘He said/they say Manuel has eaten (they didn’t see him, but they have direct physical evidence)’

Tsafiki (Dickinson 2000:408) allows the reported marker to be repeated to indicate up to three sources ‘between the speaker and the original event’. Each source is connected to the previous one:

Tsafiki

- (17) *Man-to=ka ji-ti-e ti-ti-e ti-e*

other-earth=LOC go-REP-DEC say-REP-DEC say-DEC

‘They say that they say that they say that he went to Santo Domingo’

Bora (Bora-Witoto: Thiesen 1996:97; Wise 1999:329, Ex. 33) has a non-firsthand and a reported evidential. When the two co-occur in one clause, the reported evidential indicates that the speaker was told about the event by somebody else. The non-firsthand evidential implies that the one who told the speaker about the event had not seen it.

Bora

- (18) *Hotsée-βá-ʔhá-p<sup>h</sup>e umiβá khuuβá-ʔóó-ha-tu*

Joseph-REP-UNSEEN-PAST escaped dark-room-house-from

‘Joseph escaped from jail a while back (the one who told me was not a witness)’

The position of evidential markers is language-specific. In Tsafiki, the final evidential in a string refers to the source of information for the speaker who produces the actual utterance. In Bora, the evidential with the same type of reference comes first in the string.

(iv) Information can be acquired from several different independent sources by different recipients.

Eastern Pomo (McLendon 2003) combines double evidentiality marking of type (iii) and (iv), depending on the type of evidential. The language has four evidentials: direct knowledge, nonvisual sensory, inferential and reported. Reported can combine with nonvisual sensory or with inferential in myths where reported is the basic evidential. The semantic effect is different.

When the reported co-occurs with the nonvisual sensory evidential, the reported evidential – in which the narrative is cast – refers to the way the storyteller acquired the information. The nonvisual sensory evidential refers to the way the information was obtained by the main character. That is, the sources and the recipients of information are different. (19) illustrates such marking of two different sources of evidence on one verb: here, the nonvisual sensory *-ink'e* refers to the fact that the blind old villain could hear the hero walk out; and the hearsay suffix *-le* is the evidential typically used in traditional narratives:

Eastern Pomo

- (19) *bá=xa=khi*  
 then=they.say=3person.agent  
*xówaqa-nk'e-le*  
 outwards.move-NONVISUAL.SENSORY-HEARSAY  
 'Then he started to walk out, it is said (the old man villain, who is blind, heard the hero start to walk out)'

In contrast, if the reported evidential co-occurs with the inferential evidential, the inferential reflects the narrator's inference, while the reported indicates that the narrator acquired the story from someone else. This is a clear case of (iii) above: the two evidentials highlight the two ways in which the information was acquired by the same person, the narrator. An example is (20), from a story about the Bear who killed his daughter-in-law, the Deer. Such examples also suggest that "the narrator is not quite certain as to what happened at this point in the narrative, perhaps because he/she didn't recall exactly what was said by the person from whom he had heard the narrative" (McLendon 2003: 111–112).

Eastern Pomo

- (20) *ka lél=xa=k<sup>h</sup>í*                      *maʔóral*                      *q'á-ne-le*  
 simply=they.say=3pAgent    daughter.in.law    leave-INFRR-REP  
 'He must have simply left his daughter-in-law there, they say'<sup>4</sup>

Summarising: evidentiality is rather unique in the possibility of such double marking. While double marking of type (i) is comparable to nominal and verbal tense, there is no simple analogy to (ii)–(iv) for any other category.

The co-occurrence of evidentials described above presents a piece of evidence in favour of several distinct evidentiality specifications forming different systems. In all these cases, it is the reported, or the inferred, specification that forms a system distinct from others, since a reported or an inferred specification can co-occur with another one. If a language has two sensory evidentials, one would not expect them to belong to different subsystems.

One should bear in mind that the co-occurrence of several evidentials may be of a different nature from those discussed in (A)–(D) above. Firstly, the semantic outcomes of having two evidential morphemes co-occur may be altogether different. In Western Apache, when two morphemes with a primarily evidential meaning happen to co-occur, only one of them keeps its evidential meaning (de Reuse 2003). The language has one experiential evidential (describing something acquired through firsthand experience, but heard or felt rather than seen), three inferentials and two reported markers. One inferential, *l̥q̥q̥*, has an additional mirative meaning (that is, describing surprising new information; see DeLancey 1997; and Aikhenvald in press). This morpheme can combine with a reported, as illustrated in (21) below. In such combinations, it marks only surprise, and not inference.

Western Apache

- (21) *ishikin nakhi n'í dáta'á dayits'isx̣j̣ l̥q̥q̥ ch'iṇṭṭ*  
 boys two the.former one 3pl+3sgp.kill MIR QUOT  
 'They killed one of those two boys surprisingly, it is said'

The co-occurrence of evidential morphemes may have to do with language specific requirements. The reported suffix in Jarawara (Dixon forthcoming: Chapters 6, 10; Dixon 2003) is used to emphasise that the source of information was hearsay. Reported can be used on its own, as in (22).

Jarawara

- (22) *Kamo awi naboe-himonaha Fahabiri jaa*  
 name(m) tapir kill-REPM place AT  
 'Kamo is reported to have killed a tapir at Fahabiri'

Ninety percent of all occurrences of far past non-eyewitness (the preferred tense for traditional stories) are followed by the reported suffix (when an ir-

realis suffix is included, the reported suffix cannot occur within the same verb: Dixon 2003). An example is under (23).

- (23) *mee tabori-mete-mone jokana boto joro*  
 3nsg home+f-FPNf-REFP real clearing(f) sit(du.S)  
*ni-kimi-ne-ke*  
 AUX-TWO-CONTF-DECf  
 ‘The two clearings of their reported past villages are there’

Tense in Jarawara – immediate past, recent past and remote past – is fused with evidentiality. Tense specification is not obligatory on the clause level. If the speaker decides to overtly specify tense within a traditional story (cast in reported evidential), the choice in evidentiality needs to be made. For obvious semantic reasons, reported is not compatible with eyewitness evidentiality. The only choice that remains is non-eyewitness. Thus, what looks like double evidentiality specification is in fact a result of the requirement to mark tense (already fused with one evidentiality specification) and reported evidentiality separately.

#### 4. The scope of negation and truth value of evidentials

There is yet another way in which evidentials differ from other categories. Evidentials can be within the scope of negation (pace Willett 1988 and others).<sup>5</sup> In (24), from Akha (Tibeto-Burman; see Hansson 1994:6; and discussion by Egerod 1985:104), a negated evidential particle implies that the speaker cannot figure out from the photo what is happening. That is, the visual perception (marked with the particle *ηa*) is within the scope of negation:

- Akha  
 (24) *é náa, hə à, àdjé ə rínmjð dján ə*  
 then this NOUN.PART what NOUN.PART thing make VERB.PART  
*mà ηá é, hə bə*  
 not EV:NONPAST.VIS.PERCEPTION FINAL.PART this one  
 ‘Then, as (for this photo), what kind of things they are making (I don’t know: negated visual experience), this one’

Thus, semantically an evidential can be a sort of predication of its own. Examples of how to tell a lie in Tariana show how one can distinguish the truth value of an evidential and of the actual event. One can deliberately use a wrong

evidential, with the correct information. This is summarised in Table 1 (with examples referred to by their numbers). No instances of wrong evidentials with false information have been attested in my corpus.

Table 1. How to tell a lie in a language with obligatory evidentiality

STATEMENT	EVIDENTIAL	EXAMPLE
true	correct	(1)–(5)
true	wrong	(25)
false	correct	(26)
false	false	not found

In (25), the frog woman uses a reported evidential instead of an expected visual evidential to show that she has nothing to do with her estranged husband. In fact, she had seen him come, so the evidential is wrong.

- Tariana  
 (25) *hī-pida*                      *di-uka*              *wa-dalipa-se*  
 DEM:ANIM-PRES.REP 3sgnf-arrive 1pl-near-LOC  
 ‘This one (her ex-husband) has (reportedly) come to us’

The other option is false information accompanied by correct evidential. In (26), the man tells a deliberate lie to the evil spirit: he pretends he does not know what day it is. In fact, he did know that it was Good Friday, when, according to modern Tariana beliefs, one is not supposed to go hunting. The nonvisual evidential is always used with verbs of knowledge in such contexts.

- (26) *ma-yekade-mahka*              *nhua*  
 NEG-know+NEG-REC.P.VIS I  
 ‘I didn’t know (what day of the week it was)’

In all these cases evidentials behave in some way as predications in their own right, unlike most other grammatical categories.

## 5. Evidentiality and discourse

Evidentials may acquire specific uses in discourse as a means of backgrounding or foregrounding information – see the discussion of A2 systems in §2 above, on how the non-eyewitness evidential in Abkhaz is employed to focus on a crucial point in discourse. The ways in which evidentials are employed often correlates with narrative genres. In languages with a two-term evidentiality

system and no reported evidential the non-eyewitness marker may be used in narratives, as in Meithei (Chelliah 1997:224) (cf. also Lazard 1999). This usage, however, is by no means universal. In Tsez eyewitness past is used in the body of traditional texts, probably comparable to the historic present for past narration in English (Bernard Comrie, p.c.).

If a language has a reported evidential, it may be the unmarked choice for narratives, as in many North American Indian languages (see McLendon 2003) and South American languages (Aikhenvald & Dixon 1998b), no matter whether the system is large or small. Quechua (Floyd 1997:183–188) employs reported in folktales and in riddles. In the languages of the Vaupés area in northwest Amazonia, only some myths are told using reported evidential. Inferred evidentials are told in those myths which relate travels of ancestral heroes: one can still see the stones, the mountains, the rivulets and the caves these heroes created or simply passed by.

In contrast, in Nganasan evidentials are not used in myths and folktales. The reported evidential ('renarrative': Gusev forthcoming: 3, 11) occurs only in narratives describing something that happened to a particular person, most often the narrator's ancestor. This evidential is also used by shamans recounting what spirits had told him. That is, unlike in many other languages, the use of reported evidential presupposes the existence of a firsthand information source. And reported can also occur in day-to-day speech, to create a special stylistic effect emphasising the importance of a statement.

This is very much unlike Tariana, Tucanoan languages and Shipibo-Konibo, where shamans use visual evidentials to relate their knowledge and supernatural experiences, since these are viewed as real. Prophetic dreams of shamans are cast in visual (since shamans are omniscient) (see also Gomez-Imbert 1986), while dreams of simple people are cast in nonvisual since they are not supposed to belong to the 'real world'. This brings us to differences in the perception of dreams in different cultures which goes together with different evidentials. Shipibo-Konibo speakers employ the reportative *-ronki* when describing their own dreams. In Jarawara descriptions of dreams are cast in visual evidential since they are supposed to be 'seen'. In contrast, in Yukaghir dreams are cast in non-eyewitness (see Jochelson 1905:400). In Qiang evidentials are simply not used in talking about dreams.

Unexpected evidentiality choices may be made to achieve additional discourse effects. In Pastazo Quechua (Nuckolls 1993) the choice of an evidential specification in narratives of different genres depends on the perspective of the speaker. In particular, the reported *-shi* could be used to focus on 'an assertion from someone else's perspective', in order to distance oneself from the event

(p. 249). In Huarochirí texts – the oldest known source written in Quechua – manipulating evidentials is a kind of stylistic device. There is a transparent link between the type of narrative and the choice of an evidential. The reportative suffix *-si* ‘characterises a genre of story-like texts removed from the narrator in time and frequently situated in a non-defined space; the characters often belong to the mythical sphere’; this suffix also ‘serves to mark events that happened in a not very remote past connected with historical persons, or events not witnessed by the narrator’. In contrast, the visual *-mi* is typical of personal accounts, and ‘of a descriptive genre of rituals and ceremonies’. It is also used ‘in connection with certain supernatural beings, probably marking their integration into human lived experience’ (Dedenbach-Salazar Sáenz 1997: 164).

The choice made in the evidentiality system correlates with the narrator’s attitude towards the information. The narrator ‘automatically uses the appropriate evidentiality suffix to testify his personal knowledge or ignorance of a phenomenon; on the other hand, he employs this linguistic means in order to deny or confirm his knowledge of certain events he relates, thereby conveying some information about himself and about how he wants to be seen by the addressee’ (Dedenbach-Salazar Sáenz 1997: 159). For instance, the narrators tend to use the ‘direct’ (visual) evidential *-mi* to describe feasts and rites which he seems to have witnessed; but they switch to reported *-si* ‘to deny personal knowledge’ of ‘barbarian’ customs, that is, ‘in order to document the non-witnessing of an event and/or to deny having witnessed it’ (p. 160). The detailed analysis of the use of evidentials in this text shows ‘a combination of the evidential aspect (how the narrator obtained their knowledge) with the validational aspect (the narrator’s commitment with respect to the truth of the narrated account)’; and the way evidentials are used may help in discerning the voice of the narrator from that of the commentator (p. 164).

In modern Quechua languages, evidentials often have a rhetorical effect (Weber 1986: 143–152). The inferred (‘conjectural’) evidential never initiates a conversation and is often employed as a challenge, and may have the rhetorical force of a negative, sarcastic, haughty or flippant remark.

Further insights on evidentials could be obtained from the ways in which evidentials are used to describe emerging cultural practices. In Qiang, reported is used for relating something seen on television (Randy LaPolla, p.c.), while the Tariana use visual evidential. When the Tariana spoke on the phone for the first time in 1999, they used nonvisual evidential to report their conversations. Now, after a few years of practice, they do not hesitate to use visual evidential (as if visualising the conversation partner). Another newly introduced practice

is reading. In Shipibo-Konibo, the reported *-ronki* is used for information read in a newspaper (similar usage in Quechua was reported by Floyd 1997:104). Literate Tariana speakers tend to use *inferred* when retelling stories they have just read, or when translating. We can offer a tentative explanation for this difference. In Shipibo-Konibo and in Quechua reported does not have overtones of ‘unreliable’ information, unlike in Tariana. In contrast, *inferred* in Tariana does not have any epistemic connotation and can be safely used for information transmission.

To formulate cross-linguistically significant conclusions as to what discourse genres and types of experiences tend to correlate with which evidentiality choices we need further studies of large evidentiality systems, and their changes concomitant with cultural innovations.

## 6. Further studies: Prospects and challenges

In this paper I have touched upon just a few properties of evidential systems, whose detailed cross-linguistic study will provide valuable insights into the nature of human cognition. The many fruitful lines for future inquiry include: investigating dependencies between evidentials and narrative genres; investigating how the ways in which cultural changes may affect changes in evidentiality systems, and analysing the various origins, patterns of areal diffusion and grammaticalisation paths of evidential systems. To elaborate on these, and numerous other issues, we urgently need theoretically informed studies of evidentials from across the world.

Evidential systems of varied size are scattered all over the world; they are particularly frequent in South American and North American Indian languages, in the languages of the Caucasus, and in the languages of the Tibeto-Burman family. However, until recently, there was no comprehensive typological framework for the analysis of varied evidential systems, their semantics and the ways in which they interact with other grammatical categories (such as person, negation, clause types). This made writing grammars of previously undescribed or poorly documented languages with evidentiality a particularly daunting task.

One of the major challenges for a comprehensive typological analysis of any category is the relative lack of comprehensive typologically informed grammars. Evidentiality is no exception. European-oriented researchers often face difficulties in determining the exact meanings of this ‘exotic’ category. Hardman (1986:113–114) provides a fascinating account of the ‘blindness’ of

numerous researchers to evidentiality, or ‘data-source marking’, in Aymara, within the period from 1603 up until the late 1960s – evidentials were simply disregarded as ‘ornate’ optional particles. Sticking to one particular highly restrictive formalist framework often proves to be particularly detrimental in analysing unusual categories. Migliazza (1972), in his cross-dialectal grammar of Yanomami – done within the framework of the transformational grammar of the time – missed evidentiality altogether. In fact, some Yanomami dialects have as many as four evidentials, as does Xamatauteri (Ramirez (1994: 169–170, 316–317) with eyewitness, non-eyewitness, inferred and reported. Sanuma, the Yanomami dialect described by Borgman (1990: 165–173), has three terms: eyewitness, ‘verification’ (by seeing evidence or by hearing from someone who has first-hand knowledge of the state or event) and inferential. And Yanam (described by Gomez 1990: 97) shows just two evidentials, eyewitness and non-eyewitness.

The problem of detecting evidentials in a language may be aggravated by inadequate methodology of fieldwork. Basing one’s grammar exclusively on asking questions and grammatical elicitation – translating from a lingua franca into the native language – and on sentences taken out of their context, leads to getting only a small part of the grammatical structure right. Speakers of Kamaipurá, a Tupí-Guaraní language with a very complex system of evidentials, often omit the markers of source of information in elicited sentences. Such sentences come out as unnatural, ‘something artificial, sterile, deprived of colour’ (Seki 2000: 347). Typologists must rely on careful grammatical descriptions unconstrained by any time-line formalism, in order to bring together language facts and their typological assessments.<sup>6</sup>

## Notes

1. The term ‘evidential’ itself appears to have first been introduced by Roman Jakobson (1957); he described it as a verbal category ‘which takes into account three events – a narrated event, a speech, and a narrated speech event’.
2. I am grateful to R. M. W. Dixon and all the participants of the International Workshop on Evidentiality, for comments and inspiration. Thanks also go to Janet Barnes, Lyle Campbell, Hilary Chappell, Bernard Comrie, Éva Csató-Johanson, Victor Golla, Ilse Lehiste, Reet Bergman, Terry Malone, Tom Payne, for insightful comments, discussion and data. I gratefully acknowledge my debt to my teachers of Tariana and Tucanoan languages. Abbreviations used throughout this paper are: ABS – absolutive; APPL – applicative; ASSUM – assumed; AUX – auxiliary; CL – classifier; CMPL – completive aspect; CONT – continuous; CUST – customary; DEC – declarative; DEF – definite; DEM:ANIM – demonstra-

tive animate; DIR.EV – direct evidential; du – dual; EMP – emphatic; ERG – ergative; EV – evidential; exc – exclusive; EYEW – eyewitness; f – feminine; FPNf – far past non-eyewitness; FUT.NOM – future nominalization; GEN – genitive; GER – gerund; INC – incomplete aspect; INDIR.COP – indirective copula; INDIR.PAST – indirective past; INFR – inferred; INTR – intransitive; INTRATERM.ASP – intraterminal aspect; IP – immediate past; LOC – locative; m – masculine; MIR – mirative; NEG – negative; nf – nonfeminine; NOM – nominative; NONEYEW – non-eyewitness; NONVIS – nonvisual; nsg – nonsingular; ONOM – onomatopoeic; p – person; PART – particle; pl – plural; PL – plural; PP2 – completive participle; PREV.DIR.EV – previous direct evidence; PROG – progressive; QUOT – quotative; REC.P – recent past; REP – reported; RP – remote past; RPef – remote past eyewitness; S – intransitive subject; sg – singular; STRANS – transitive; VIS – visual.

3. An additional system of NONVISUAL SENSORY, INFERRED, ASSUMED, REPORTED was reported for Wintu (Wintun family: Schlichter 1986), with four terms: *-nt<sup>h</sup>Er*, for ‘nonvisual sensory evidence’; *-ke* ‘hearsay’; *-re* ‘inferential’; and *-el* ‘assumed’ whereby the ‘speaker believes his proposition to be true because of his experience with similar situations’ (occurs with conditional, never followed by other suffixes). It is not clear how visual information is marked.

4. The difference between the clitic = *xa* ‘they say’ and the reported evidential is unclear.

5. An evidential cannot be questioned (see discussion by LaPolla 2003).

6. Such excellent grammars include Kibrik’s analysis of Archi, Northeast-Caucasian, or Seki’s description of Kamaiurá.

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# Towards a less ‘syntactic’ morphology and a more ‘morphological’ syntax\*

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The task of reporting on the full range of current views on either morphology or syntax, let alone both, is a completely daunting one, and could not possibly be accomplished in anything like a serious way within the scope of a single talk. Instead of attempting it, therefore, I will present some of my own views on some areas where the two can usefully be compared, and suggest that the result of that comparison does not come out quite the way it is often assumed to.

A long tradition, reaching back to the beginnings of scientific linguistics in the early twentieth century, has seen morphology and syntax as quite similar areas of structure, at least in terms of the appropriate theoretical principles which underlie them. In structuralist theory, syntax was often seen as an essentially morphological study, extended to structures above the word but otherwise exhibiting the same general organization and accessible to the same methods. Somewhat ironically, perhaps, generative theory long maintained the converse: that morphology was just the syntax of word-internal domains, with no real properties of its own.

The last three decades or so have seen a revival of interest in morphology for its own sake, and with that, a recognition that word structure really does have some properties that distinguish it from sentence structure. A close look, indeed, suggests that there are virtually no principles of any substantive sort that the two share, apart from a vocabulary of features that serve at the interface between syntax and the lexicon (Anderson 1992: Ch. 3). Despite this, the assumption persists in the field that morphology is ‘really’ like syntax, in that the structure of words consists of a hierarchical arrangement of fundamentally concatenative units (‘morphemes’) quite parallel to the arrangement of words into phrases. On that picture, the fundamental operations of both morphol-

ogy and syntax consist in (a) selecting a set of content units – morphemes or words, as the case might be; (b) grouping these into larger structures – words or phrases; and then (c) computing the appropriate phonetic forms which should be assigned to the result. I will suggest below that this conception of our knowledge of words is significantly flawed, and that morphology has a somewhat different character from that implicit in the traditional division of the field into the study of ‘morphotactics’ and ‘allomorphy’. So much, I think, is really just a summary of what we have come to know by taking morphology seriously as an area of inquiry in its own right. Somewhat less of a consensus exists, however, with respect to the other part of what I have to say. I will go on to suggest that in some areas outside the domain of word structure – specifically, the analysis of incorporation constructions, clitics, and ‘Verb-second’ structures, the insights we get from the study of words are also applicable, and that these traditionally ‘syntactic’ areas are thus more like morphology than is usually assumed.

### 1. What morphology is really like

The classic Saussurean picture of the linguistic sign as an irreducible link between form and content may suffice for *tree*, but it fails for words like *trees*, *tree-like*, and *palm tree*. Recognizing that words do not in general have the character of simple signs, Saussure allowed for the fact that many signs are *partially motivated*: a portion of the connection between their form and their content is founded on their relation to some other sign(s). The goal of morphology, indeed, can be seen as the explication of this notion, its possibilities and its limitations.

To approach this goal, we must begin by being clear on the information that words bear. Their content includes not only the core content we call their ‘semantics,’ but also much information about the relation of particular words to the syntactic structures in which they appear. A basic aspect of a word, for instance, is its lexical category (Noun, Verb, etc.), as well as its associated argument structure. Together, these determine the range of syntactic positions in which a given word can occur, a crucial aspect of the word’s identity.

Other relevant information which a word carries (and which may or may not be reflected in its form) can be arrayed on a number of dimensions of what is traditionally called its inflection. These include inherent properties, such as the grammatical gender of Nouns; configurationally assigned properties, such as case; agreement properties, such as gender in Adjectives or person-number inflection in Verbs; and the base generated properties of phrases which are re-

alized on particular words within them, such as tense in Verbs (reflecting the assignment of tense to the clause of which the Verb is the head) or number in nominal expressions. For discussion, see Anderson (1992).

The content of a word in the general case, then, involves a number of distinct components, even apart from the possibility of decomposing basic meanings into combinations of properties. We must then ask what kind of analysis is appropriate for the forms of words, with an eye to the relation that exists between form and content. Words clearly have a phonological organization, into features, segments, syllables, and larger prosodic constituents such as the foot, but the central question in this area has always been that of other sorts of complexity within words. Do the forms of words, that is, have an organization into non-phonological constituents of a sort defined by the morphology as well?

In more explicit terms, what is at stake here is the difference between theories of word structure based on an analysis into *morphemes* and theories lacking such units, relying instead on systems of Word Formation Rules. The differences are often subtle, but an important one is the extent to which a principle examining a morphologically complex word can or cannot refer directly to its composition in terms of non-phonological structural units. When we look for evidence of such structure in word forms, we do not find it. The kind of argument we look for would be a principle in some area of grammar which crucially requires reference to such morphologically motivated constituents, but it appears that morphologically defined units within a word are not referred to in a necessary way in any component of a grammar. Within the study of meaning, it turns out that compositional meaning and the relative scope of meaning components can be reconstructed quite adequately in terms of semantic representations themselves, together with the relation among the Word Formation Rules which may have participated in its formation. In Phonology, where this issue is usually presented in terms of the presence of non-phonological boundary elements within the representation of a word, these elements can in fact be dispensed with in a theory that makes appropriate use of cyclicity, as in various forms of Lexical Phonology. In syntax, the widespread acceptance of versions of the Lexicalist Hypothesis (on which the syntax neither manipulates nor has access to the internal form of words) entails the syntactic irrelevance of word-internal morphological structure.

The morphology itself might provide such evidence, but it is notable that we do not in fact find morphological principles that refer directly to morphological structure. We do not find, for example, infixes which appear precisely after the first morphological constituent of a form, regardless of its phonolog-

ical shape: rather, infixes appear after the first (or before the last) unit of some phonological type, such as a segment, a vowel, a syllable or foot, etc.

Two major classes of apparent counter-examples to this generalization have been suggested in the literature. Aronoff (1976) argued, for instance, that rules of truncation in morphology delete precisely a morpheme, as in the formation of *navigable* from *navigate* (which appears to involve the loss of the unit *-ate* in conjunction with the addition of *-able*). Corbin (1987) demonstrated, however, that what is lost in truncation processes is not in general a synchronic morphological unit, at least not in the language where the truncation takes place. In English formations such as *viral*, *rectal* from *virus*, *rectum*, the truncated material (*-us*, *-um*) may well have been a morphological unit in the language from which the words are originally borrowed, but this is not the case in English.

Another potential counter-example would be presented by cases in which some morphological element was restricted precisely to bases which are not themselves morphologically complex, as claimed for a number of morphological types in English in (Fabb 1988). This seems simply to be based on a flawed methodology, however: Fabb's study was based on the presence of forms in a standard rhyming dictionary, and it is well-known that the value of dictionary listings for an argument about the sorts of word that *cannot* occur in a language is extremely limited. A similar argument offered in (Aronoff & Fuhrhop 2002) fares no better, at least for English, in the face of numerous attestations of precisely the complex forms alleged to be impossible. It must be concluded that the existence of a morphological rule referring to morphological complexity *per se* has not yet been demonstrated. From the considerations above, we must conclude that the best theory of morphological structure would be one on which the forms of words have no internal structure at all, apart from what is motivated in the phonology. This is undoubtedly too strong a position: for instance, in Icelandic the word *kallast* 'to be called (something)' is undoubtedly a single word, and when it is inflected, the inflectional material is added to its internal stem (in e.g. *köllumst* 'we are called (something)'). Such examples show that in some cases, we need to recognize the existence of word-internal non-phonological structure; and as (Carstairs-McCarthy 1993) makes clear, it is not at all trivial to define exactly the circumstances under which this is appropriate. Such difficulties, however, should not be allowed to obscure the undoubted fact that in most cases, word-internal morphological constituency should not be accessible to rules of grammar.

We conclude, therefore, that the forms of words, at least in the general case, are organized in terms of phonological structure alone and not in terms of overt morphological constituents ('morphemes'). This raises the question,

obviously, of how the components of the content of a complex word are to be related to its form. Whatever the answer to this, it is likely to be quite different from the solution to the corresponding problem in syntax, where the existence and structural relevance of grammatical words and phrases as constituents of larger structures is not seriously in doubt.

### 1.1 Morphology is realizational

The traditional picture of morphology views complex words as built up out of 'morphemes,' local form-meaning correspondences that preserve the literal form of the basic, unanalyzable Saussurean sign. If this were in fact true, there are several consequences which would follow. For one thing, word formation on this model, since it always involves the *addition* of a new morpheme to a base, would always be *monotonic*: additive, in the sense that the new form would always contain its base as a proper sub-part, and concatenative. It has been known at least since Hockett's (1947) classic survey, however, that this is not at all the case. Morphological relations often involve not addition but substitution (*sit/sat, bath/bathe*), subtraction (e.g., Icelandic *pukr* 'concealment' from *pukra* 'make a secret of') or other non-concatenative changes such as metathesis. Many accounts of such phenomena have been offered, from Hockett's purely terminological presentation to rather more insightful analyses, but the fact remains that a substantial class of cases remains apparently ineliminable in which morphological elaboration rests on formal mechanisms other than the addition of an affix.

If morphological structure were as represented by the morpheme-based picture, we would also expect to find that the relation between components of form and those of content would be one-to-one, at least at some level of granularity. Instead, however, we find components of form that reflect no content at all ("empty morphs" in Hockett's terms), elements of content that must be assumed to be present but for which there is no formal reflection ("zero morphs"), unitary elements of form corresponding to multiple elements of content ("portmanteau" morphs, such as the ending of Latin *amō* which realizes first person singular present indicative, categories that are normally represented in Latin verbs by distinct pieces), and elements of content that are represented by several distinct aspects of the form ("multiple exponence"<sup>1</sup>). Morpheme-based views of word structure also imply that morphological relations should always have a specific directionality, since they are always the result of adding a particular morpheme to (one of) a class of possible bases. This prediction will be addressed in the following subsection, where it too will

be seen to be false. In summary, morphological relations do not in general have the character they would be expected to show if they were really based on the combination of minimal signs (or ‘morphemes’) into larger structures.

The morpheme-based view seems to follow rather directly from the existence of partially motivated signs, the observation that parts of word form may be related to parts of word content. This simple picture, based to some extent on a presumed analogy between morphology and syntax (the notion of morphology as “word-internal syntax”) is quite adequate to explain a large number of local observations under familiar circumstances. It is also quite intuitive, and easy to explain to beginning students. In these respects, however, it is somewhat similar to the view that the earth is really flat. Both yield to somewhat different pictures upon closer examination of a larger picture under a broader array of circumstances.

In the case of morphology, the more appropriate view appears to follow from the conception of a word’s content as involving a certain sort of feature structure – as including not only its semantic core but also a *Morphosyntactic Representation* (in the formulation of Anderson 1992) or the more elaborate representations of, e.g., Stump (2001). On these *Realizational* theories of word structure,<sup>2</sup> components of a system of rules specify (aspects of) the relation between form and content. Concatenative affixation, the sole possibility admitted by the morpheme-based view, is one type of such a relation, but by no means the only one.

Realizational views of morphology are thus quite different from the quasi-syntactic picture of word formation to which we are often tempted. Adopting such a theoretical position, in particular, reduces considerably the apparent similarity between the structures of words and of sentences, and accordingly leads to a much less “syntactic” picture of morphology.

## 1.2 Morphology is relational

On the usual picture, the rules of a language’s morphology serve to provide recipes for the construction of complex words out of simple or otherwise basic components – and that is all. In fact, however, morphological structure may play other roles as well.

Consider the class of agent nominals in English, words such as *diver*, *baker*, *actor*, etc. with meanings ‘one who dives, bakes, plays, etc.’ These appear to be formed from verbs by the addition of the phonological material *-er* to the form and the addition of something like ‘ONE WHO (Verb)S’<sup>3</sup> to the semantic content of the base. But now consider *butcher*, *carpenter*, *janitor*, and many

others: words that appear to have both the right form and the right meaning to have been constructed in exactly the same way, but for which no basic verbs (*\*butch*, *\*carpent*, *\*janit*, etc.) exist. There are good reasons for recognizing all of these as members of a unitary class (see Anderson & Lightfoot 2002, for some discussion of these matters), but if so, that class cannot be defined by a process of construction from simpler bases. As first argued in the generative literature in Jackendoff (1975), the regularities of morphology must be seen as having a function in parsing and recognizing existing words, as well as in constructing new ones.

Since these morphologically defined classes have a status in the grammar that is not based exclusively on the manner of their formation, relations among them might be expected to be more general than simply the construction of members of one class from members of another. In fact, in some languages we find fairly clear instances of inter-class relations that have no inherent directionality. In most of the languages of the Northwest Caucasian family, for example, the substitution of the vowel [ə] for [e] yields a transitive verb corresponding to an intransitive (e.g., 'to work (a field, the ground)' from 'to work'), but the opposite substitution, of [e] for [ə], yields an intransitive (e.g., 'to wash up, wash the dishes') from a transitive ('to wash (something)').

From these examples and others, we can see that the essence of a morphological regularity is to state the relation between two classes of words, in terms of systematic relations between them in terms of their form and their content, and the correlations between these aspects of their identity. Again, this relational aspect of morphology is quite different from the more constructional character of the rules of syntax.

### 1.3 Morphology is based on a system of constraints

Finally, we can ask what is responsible for the details of the formal composition of complex words. While denying that words should be seen as composed of 'morphemes,' we certainly accept that formal morphological markers appear within words as reflexes of their morphological organization.

In some sense, the order of these morphological markers within (the form of) a complex words reflects relations of relative scope among the content elements to which they are linked. This is an intuitively obvious idea, explored in some detail in (Baker 1985) under the name of the "Mirror Principle." What is it that is responsible for this word-internal sequence? In the absence of morphemes as structural units of analysis, we cannot say that it follows from a quasi-syntactic, hierarchically organized structure into which these units are

inserted, as on Baker's original picture. In Anderson (1992), I attempted to represent the relevant structure in terms of an order of application obtaining among the word formation rules, but that view cannot be maintained if one takes seriously the non-constructural aspect of word formation argued for in the previous section: if *janitor* is indeed an agent nominal, but not formed by the addition of an agent ending to a basic verb, why should the formal characteristic of this class nonetheless appear at the right edge of the word, just like the *-er* of *baker*? Other arguments as well, not rehearsed here, seem to militate against the conception of the grammar as involving sequentially applied processes of word formation, adding formal markers one at a time to a base. But in that case, we must seek elsewhere for a mechanism that can encode the sequential aspects of word form. The answer to this problem in morphology appears to come from recent developments in phonology. Instead of construing a stage in the derivation of phonologically complex forms as involving the sequential application of a series of rules of phonological modification, phonologists have increasingly come to see such derivations as based on a system of hierarchically ranked, potentially violable constraints. Given an input form, that is, such a grammar defines the corresponding output not by a series of steps gradually modifying the input, but rather as *whatever* possible form best satisfies the constraint system in its relation to this input. Empirical arguments can be offered to the effect that a constraint system is better suited than a system of ordered rules to account for the placement of morphological markers within the word.

The grammars countenanced by such a view (generally known as "Optimality Theory," following the foundational work of Prince & Smolensky 1993) are founded on the regularities of 'derived' structure, rather than on the process by which that structure is composed. As such, they turn out to be quite appropriate for the expression of morphological regularities as well as phonological ones. While the "classical" version of Optimality Theory probably goes too far in stipulating a one-step relation between the most abstract underlying structure and concrete, phonetically realized output, recent developments in integrating a constraint-based perspective into the relation between morphology and phonology within an overall architecture like that of Lexical Phonology have shown considerable promise (see Kiparsky to appear for extensive discussion).

I conclude, therefore, that morphology deviates in a number of important ways from the classical picture of word structure as simply the combinatory syntax of 'morphemes'. As we have seen, morphology is best seen as a system that describes relations among word structural types in terms of the way the

forms of words realize the properties that compose their content. These relations seem best characterized by a system of interacting constraints on form, rather than a set of procedures by which a complex word can be built up step by step from elementary components. In all of these ways, morphology turns out to be much less 'syntactic' than is generally assumed.

## 2. Much of "syntax" is like that, too

Since the days when structuralist linguists saw it as simply an extension of "morphotactics," syntax has been thought of primarily as a matter of the construction and manipulation of hierarchical arrangements of discrete constituents. The proliferation of 'functional categories' and their phrasal projections in recent decades, for example, can be seen as an attempt to integrate information about syntactically relevant non-lexical properties into a framework in which the only way content can play a structurally significant role is through the mechanisms of X-structure. In the previous section, I have argued that this is not the kind of picture within which to seek the regularities of word-internal structure.

In fact, however, much of what we normally think of as clearly part of syntax (because of its relevance to phrasal categories manifestly larger than those of the individual word) seems to have some of this same character. Rather than being exclusively matters of the construction and manipulation of hierarchical constituent structure, a number of areas usually considered syntactic in character also turn out to be realizational, relational, and governed by a system of constraints rather than (solely) by rules of X-structure, displacement, and other manipulations of phrasal structure. Three of these will be discussed briefly below: the analysis of clitics,<sup>4</sup> incorporation (especially noun incorporation, and perhaps most surprisingly, verb-second constructions).

### 2.1 Clitics

The syntactician's standard approach to the grammar of clitics is to introduce them as structural elements entirely comparable to other pronouns (though also characterized in some way that can be used to trigger their special distinctive behavior), and then count on rules of syntactic displacement to ensure that they occur in the proper position in Phonetic Form. In a series of works (Anderson 1992, 1996, 2000c, 2004) and a more extensive treatment to appear (Anderson in preparation), I have defended quite a different account of clitics.

On that picture, special clitics are regarded not as autonomous syntactic units but rather as the ‘morphology’ of phrases: a view often abbreviated by describing clitics as ‘phrasal affixes’. Just as morphological material (affixes and other sorts of morphologically significant modification of word form) is introduced as the realization of features characterizing the word, so also clitics expressing aspects of the functional content of a phrase are introduced as modifications of the phonological shape of that phrase. Certain other clitics represent a sort of phrase-level ‘derivational morphology’ (in contrast with the ‘inflectional’ nature of functional material), and as argued in Anderson (2004), for example, we find the same sort of relation between ‘inflection’ and ‘derivation’ at the phrase level (in the relative placement of clitics) that we find at the word level (with respect to affixes and other morphology).

There are many reasons to doubt the adequacy of standard mechanisms of syntactic description for the treatment of clitics (see Anderson 2000c for some discussion). While a great deal of the literature simply assumes that the introduction and placement of clitics is a problem for the syntax, and proceeds from there to make inferences about what syntactic theory must be like to accomplish this task, the results are quite often less than appealing. Special mechanisms (such as “Long Head Movement”) must be introduced specifically to deal with properties of clitics, clear and simple generalizations (such as the appearance of clitics in second position in many languages) must be treated as epiphenomenal, and other unfortunate distortions of both the analysis of specific languages and syntactic theory in general also follow.

On the other hand, the properties of clitics are virtually identical (modulo the difference between phrasal and word level domain) with those of morphological markers, as discussed in Anderson (1992) and elsewhere. Regarding (special) clitics as the realization of features associated with larger phrases, just as morphological markers are the overt realization of properties of words, brings out this analogy while freeing the syntax of responsibility for a class of formal elements which it is ill-suited to accommodate.

## 2.2 Incorporation

The analysis of Noun incorporation constructions has been a matter of contention at least since Sapir (1911) argued that these were a form of compounding rather than syntax in a strict sense. Treatments based both on syntactic displacement and on lexical compound formation have been defended within a variety of grammatical frameworks; but in recent years the work of Mark

Baker (1987, 1995) has persuaded a great many syntacticians of the plausibility of the movement-based account.

Elegant as Baker's work in this area has been, closer examination (Anderson 2000a, 2000b) shows that his description of incorporation (of nouns and other lexical categories) as syntactic displacement is not at all ineluctable. While a great deal of his account of the structure of heavily incorporating languages (such as Mohawk) is persuasive, these structural notions are actually quite compatible with a lexical description of incorporation itself, and indeed there are some clear advantages to be gained from such a move. It appears, in fact, that Sapir was basically right: incorporation is a type of morphology (typically a variety of compounding), rather than syntax.

If this conclusion is accepted, it has consequences that go far beyond the treatment of a single construction (however interesting) across a variety of languages. Baker's treatment of incorporation as movement is the principal support that has been offered for the existence and properties of an operation of "Head Movement," a notion which has simply been accepted since (at least) the publication of Baker (1987). Head Movement itself is the fundamental 'technology' underlying the analyses that have proliferated in subsequent years of grammatical structure as based on enormously ramified arrangements of (frequently non-overt) functional categories. But without the evidence from incorporation, the existence of Head Movement is itself much less well supported empirically. Accordingly, the hypothesis of elaborated hierarchical functional structure, which relies on Head Movement for its conversion to overt form, is correspondingly much shakier. The morphological character of some domains to which syntacticians have often laid claim has quite fundamental implications for the structure of syntactic theory.

### 2.3 Verb-second

Another central problem in the syntactic literature of the past quarter century or so has been the analysis of "Verb Second": the requirement in a number of languages (primarily, but not exclusively, Germanic) that finite inflected verbs appear in second position in certain clause types (main clauses but not subordinate clauses with complementizers in German; all finite clauses in some other languages, etc.). And unlike the case with clitics and incorporation, there is no real alternative to saying that the regularities underlying these constructions are matters of hierarchical organization, displacement, and in general, pure syntax.

But there is another way in which verb second constructions do have a kinship with morphological structure which could easily go unnoticed. The link becomes apparent when we ask what verb second and second position clitics have in common. Wackernagel (1892) for one believed that these were in some basic sense the same phenomenon. His derivation of verb second in German from the original second position placement of (unaccented, hence arguably clitic) finite verbs in Indo-European is somewhat implausible for a variety of reasons, but it must still be asked whether the appearance of “second position” in both of these cases is merely a coincidence. In connection with the second position placement of clitics in a number of languages, it is possible to argue that this follows from their introduction and placement in accord with a system of hierarchically organized, violable constraints (Anderson 2000c). I suggested above that this is also true from the placement of morphological markers within the word; and the independent evidence for this conclusion in the two domains reinforces the essentially morphological nature of special clitic phenomena. In fact, however, analysis of the bases of second position placement of the verb in languages like Icelandic and Breton suggests that the same conclusion is valid here as well. That is, instead of trusting the independent mechanisms of the syntax to put the verb in second position in these languages, we have to regard this phenomenon as resulting from the intersection of *possibilities* for displacing the verb with a set of constraints on the results – just the same families of constraints, when properly formulated, as those responsible for second position placement of clitics in languages where this is the rule, and of infixes within words where these occur postinitially. This conclusion leads us to suggest that (as proponents of Optimality Theory have maintained), the locus of explanation in grammar is often not the class of manipulations that relate abstract structure to overt form, but in regularities of that form itself. The role of constraint systems comes to be seen as pervasive, in phonology, morphology, and syntax (and perhaps semantics as well, as some have suggested).

We should conclude from this not that verb second is “really” second position clitic placement, as Wackernagel suggested, but rather that morphology and syntax are both subject to influences that are, at bottom, the same: constraints (indeed, essentially identical constraints) on overt form that are logically separable from the mechanisms by which that form is constructed. Since the beginnings of generative grammar, attention has been focused on the rules of construction (in phonology, morphology, and syntax). In part that is a consequence of the fact that generative grammar in its early years developed quite powerful tools of this sort, tools that made it possible to bring coherence to

phenomena that had long puzzled grammarians. And when what you have in your hand is a hammer, everything tends to look like a nail. But it is precisely when we look for generalizations that transcend any particular rules system within a grammar (such as a unified account of “second position” phenomena) that we are led to rethink this approach.

### 3. Conclusion

I have argued above that the similarities and differences between morphology and syntax are rather different from the picture that is often assumed. On the one hand, morphology really does have its own character, quite distinct from the more constructional nature of syntax. Word structure involves the realization of properties, not simply the concatenation of meaningful elements, and is often based on non-directional and non-monotonic relations. On the other hand, once the nature of the two systems is clarified somewhat, we find that some phenomena that had been taken as syntactic really belong to the morphology, appropriately construed. And finally, when we make these comparisons explicit, we find that there are aspects of grammatical structure that cut across lines in the architecture of grammar in ways that make us suspect that that architecture is somewhat different from what we had assumed. All of these notions seem to me important potential advances in the theory of grammar, but they are the kind of thing we can only see when we consider morphology and syntax together, and not separately. Perhaps the organizers of the Prague Congress were entirely correct in combining these as a single subject area, despite the somewhat distinct populations of linguists who normally think of themselves as doing “morphology” or “syntax.”

### Notes

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1. Several authors have seen that multiple exponence poses an important problem for morpheme-based views of word structure, and have attempted to deny its existence – quite unsuccessfully, in my view. For discussion, see Anderson (2001).
2. Many other morphologists have advocated realizational views of morphology, including (but by no means limited to) Mark Aronoff, Robert Beard, and Arnold Zwicky, among others in the recent literature.

3. We make no pretense about the significance of such ‘semantic’ representations, which are obviously nothing but place-holders for a serious theory of meaning.
4. More properly, “special” clitics in roughly the sense of Zwicky (1977) and Anderson (1992). The phonological dimension of clitics, as it manifests itself in “simple” clitics (as well as in the phonological properties of special clitics) is not considered here.

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# Linguistic universals and particulars

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*in memoriam Ken Hale*

Reflections, retrospective and prospective, about the activities and results of linguistics. Theory and description: methodological desiderata. Linguistic universals and linguistic particulars. Language extinction and the politics and ethics of linguistics. Linguistic creativity at the level of grammar creation and transmission: humans as members of the species *homo loquens grammaturgicus*.

## 1. Some truisms

Let me start with some general points and questions.

### i. Linguistics comprises many and varied activities.

This should be a truism. But within the field there are conflicting views and emotions about what “real linguistics” is. (Hudson 1972, gave an insightful picture of the hierarchies of prestige in academic and scientific disciplines.) Part of what I want to do here is call for mutual respect and cooperation among all who deal with language from various points of view, with various interests and various agenda.

### ii. The world’s languages are surprisingly similar *and* surprisingly different. Whenever I undertake the study of a “new” language I am constantly struck by two things:

- How often the language conforms to my expectations;
- How often I am surprised by some totally unexpected phenomena.

The similarities and the divergences come in all levels and aspects of languages. Some samples will be given below in Section 4.

iii. Linguistic theory must account for the similarities *and* the diversities among languages.

iv. Linguistic theory needs lots of different languages.

This is an obvious point: If only one language were spoken in the world, we would not know what features of the language were particular and which universal. There is a kind of argument that goes like this:

**Poverty of stimulus argument:** Such and such a feature must be universal because it would be impossible for children to learn it on the basis of the evidence that they get. (To be found, for example, in many writings of Chomsky.)

But without some independent knowledge of what can and can't be learned, this argument has no force (I believe I heard this counter-argument first from Barbara H. Partee, p.c.).

The inescapable fact about language acquisition is that children and other people do learn languages, with their expected and unexpected, universal and particular characteristics.

v. **Where are all the languages going?**

Extreme language loss is a fact of our current life. There are two ways of responding to this fact, both important for linguists to think about:

A. record, document, analyze languages before they go, call this “documentary linguistics”.

B. try to stem the tide, call this “ecological linguistics”;

I assume that a third response – ignoring it – is not an option.

With limited resources, there can be conflicts between the two aims. I am heartened by the fact that many younger linguists, equally adept in theory and in description, devote a large proportion of their time to community-oriented projects. The profession of linguistics must recognize and support such work.

vi. **Do languages need linguistic theory?**

Obviously, they do not, in the simple sense. Can linguistic theories help attempts to revitalize and preserve endangered languages? Probably not. But if the aim is documentation, then there are properties of languages that generally will not be noted unless there is active invocation of the linguistic theories that have uncovered these properties.

vii. **Do languages need linguists?**

Here I think we can give a more positive answer. There are many things that linguists can do which can contribute to the health of languages as well as contributing to the scientific aims of documentation. The main thing

(in my opinion) to keep in mind here is the asymmetry between practical concerns and requirements and scientific practice. Linguists naturally tend to write for other linguists. To render their results, documentation and descriptions, useful for community aims, they should be prepared to present them in or translate them into a form that is accessible to nonlinguists. For example, with computers as a help it is easy to present dictionary and textual materials in an adequate practical orthography, or to have parallel representations using both practical orthographies and more arcane representations.

## 2. A little history: Will the real linguist stand up?

One of my first public presentations on linguistics was at the Ninth International Congress of Linguistics). I mention that not to brag on how old I am, but to give some sense of the temporal frame for my remarks. Unfortunately, my vision is narrower than it should be on this occasion, since I will be largely confining my purview to linguistics as it has gone on in North America. That Congress took place in Cambridge, Massachusetts, in 1962 (if my memory is correct). It was in the heady early years of the Chomskyan Revolution or Abberation. Noam Chomsky made a presentation in a plenary session. The second wave of young turks were mostly graduate students. So what has happened in the ensuing four decades?

When I first became interested in linguistics, a few years before that, linguistics was all about going out and describing languages, especially languages that had not been documented. The “real linguist” was the fieldworker. The attitude toward “theory” was often apologetic. In the American linguistic scene, theory was mostly about procedures. Zellig Harris’s (1951) book was originally called “Methods in Structural Linguistics.” Its main motivation was theoretical. Harris wanted to be able to compare the structures of different languages. The point of the book was to try to ensure that different linguists could produce comparable or intertranslatable descriptions. Harris tried to define procedures of segmentation and classification by distribution that would lead to uniform results. Various choices – for example, introducing “long components” – would lead to certain kinds of statements, but in the ideal case, making different choices would lead to statements that could be converted readily into equivalent descriptions given other choices. Chomsky’s early characterizations of what he called “taxonomic linguistics” was not far off the mark for his teacher

Harris. I think it was pretty far off the mark for many other linguists, including probably most linguists in other parts of the world.

What was most original in Chomsky's early work was the very idea of a generative or formal grammar, considered as a theory that would specify all and only the (infinitely many) expressions in a language, and assign structural descriptions to them. In a kind of Kantian turn, linguistic theory became then the study of the general structure of the grammars that would be just adequate to capture natural languages as formal systems. The idea that a language could be described as a formal system might well be called "Chomsky's thesis," even though, as is often the case, the idea was "in the air" at the time (Bar-Hillel, Harwood, Greenberg, Hockett, among others). A decade and a half later, linguistics saw the formulation of "Montague's Thesis," the idea that a natural language could be characterized as an *interpreted* formal system. ("Chomsky's Thesis" and "Montague's Thesis" were so-called in Bach 1989.) Again, it was not just Montague who was coming up with this idea (Keenan, David Lewis, Parsons, among others).

There is no doubt that these new conceptions of theory became dominant in American linguistics and beyond over the next decades. The cultural clash between proponents of the two images of what a "real linguist" is and does, was – and still is – vigorous, often acrimonious. The split went along with differences in fundamental views of what scientific activity should be (see Bach 1965, for a then contemporary view of this philosophical disagreement).

In 1967, MIT made a brilliant appointment, when Ken Hale, arguably the best field linguist of his era and a gifted theoretician as well, joined the department. I believe a good deal of mending between the two impulses or sides of linguistic research in the following decades can be attributed to his influence.

### 3. Three kinds of language study

Think of a diagram, something like a schematic of the solar system. Ask: what is the Sun and what are the Planets? In one view, some language is in the center, arranged around it are various theories and subdisciplines. In the other, linguistic theory is the sun and the planets are languages and subdisciplines and related areas. These diagrams are supposed to represent crudely two kinds of linguistics: descriptive and theoretical.

Now another truism:

**There is no such thing as a theory-free description.**

Whenever you undertake to describe a language you carry with you a set of expectations about what languages are like. These are really hypotheses about Universal Grammar. They may be completely formal: you expect phonemes, recurrent structures of form and meaning. They may include much more specific, but still formal, hypotheses about the form of a grammar for arbitrary languages. And they may include substantive hypotheses about the content of a language, at all levels: phonological, phonetic, syntactic, semantic.

What are the results of these two kinds of activities?

**a. Primary Linguistic Description**

Primary descriptions of languages are the basis for everything else:

**There is no such thing as a language-free theory of language.** Descriptive linguistics traditionally results in descriptive grammars, dictionaries, texts, recordings – nowadays, audio and video, one hopes. In the United States and Canada, the great descriptive grammars, dictionaries, and text collections of the late nineteenth and twentieth century are the heritage of Boas and his tradition. True, there are many questions left unanswered in the products of this stream, but there is not nothing: the results are a rich mine for successive workers. Think, for example, of the careers that have been built on the grammars and text collections of this heritage.

**b. Linguistic Theory**

The results of linguistic theorizing are theories or bits of theories (hypotheses). In line with the schematic I drew in your minds, languages are drawn upon to give evidence for or against some point of theory or, in grander attempts, a whole theory. So the typical result of a graduate work in linguistic theory might be a dissertation with a title like: “The ABC Principle in Language X,” where X might be some language that had not been dealt with in depth by theoreticians. Such a study might be followed by a whole string of papers or dissertations taking the same material to argue against the ABC Principle in favor of the A'B'C' Principle, or for a whole new approach or theory. But usually there is no new data, often not even checking of the original sources, and only in rare cases are these studies based on new primary work.

It would seem that both of these activities would share a concern about accuracy, “getting the facts straight.” This concern for accuracy is central to a third kind of activity.

**c. Philology**

Besides the activities just mentioned – descriptive and theoretical linguistics – there is another kind of study devoted to languages. Unlike those two

disciplines, **philology** is devoted not to uncovering the system of a language or coming to understand the general abilities of humans to acquire and use such systems. It is devoted rather to the products of users of the languages, memorable products for the most part. The name is apt: ‘lovers of the word.’

The proper cultural matrix for philological efforts should be the community most directly associated with the texts or oral traditions in question. But in the colonialistic and post-colonialistic world in which we live, it is more often than not up to linguists from outside these communities to do the primary work that is a necessary foundation for such activity. Why is this so? It is clearly because the healthy functioning of the native traditions of story-telling or recording have become debilitated for reasons that are too familiar to need rehearsing. Languages and cultures change and sometimes die as a matter of human history. But sometimes they are killed. Ken Hale’s life work on many languages was distinguished by one insistent maxim: the best work on any language could only come from native speakers of that language. Therefore, it was incumbent on the foreign linguist to try as hard as possible to teach as well as take, to treat speakers of the language not as “informants” or “consultants” but as fully equal co-workers, linguists in their own right.

The activity of fieldwork, primary linguistic description has changed as the result of political change, the insistence by many First Nations people, that the work of linguists be responsive to community needs. Thus in the contemporary world, fieldwork of the old style is usually just not an option.

#### 4. *Homo loquens grammaturgicus*

Work within the Chomskyan paradigms has emphasized “linguistic creativity.” What this has generally meant is that adequate theories of Language and of languages must accommodate the fact that speakers can produce and understand novel utterances without bound. I believe there is another kind of creativity in the world of language, at the level of grammar creation. Adequate theories of Language must make room for this kind of creativity as well. In short, accounts of Universal Grammar must give room for the quite astonishing variety that we find in particular grammars. The best way to appreciate this is to look at a lot of languages in detail. Since none of us can look at all languages directly, we must make do with descriptions of the languages. As a matter of strategy, descriptions of languages that are theory-driven must be balanced by descrip-

tions that are carried out in the spirit of the advice often given in older and other traditions than those of some of the dominant theoretical stances:

We need to cherish and study linguistic diversity for reasons that are as important scientifically as they are politically and ethically. It is not a bad idea to let a language unfold itself to you on its own terms for a good long while before you jump to fitting it into your theory or testing your theories against it.

(Bach 1996)

There are two reasons to believe that the human linguistic organ (UG?) has an inherent creativity, and hence two reasons to believe that theories of universal grammar that are too constrained cannot be adequate:

- **Persistence** Some very special aspects of languages and language families can persist over long stretches of time and space.  
**Examples:** consonantal roots in Semitic, special systems of pronominal marking in Algonquian.
- **Diffusion** **Examples:** areal features in the Pacific Northwest of North America: phonology, word-grammar, phrase grammar; word order characteristics from substrata or surrounding languages: Amharic SOV syntax as opposed to general Semitic patterns. See Thomason (2001), on areal and contact phenomena.

## 5. Three meanings for “language”

In various publications, Chomsky has distinguished between various meanings of the word ‘language.’ Two notions have been constant: (1) a language considered as a set of utterances, sentences or whatnot, (2) a more abstract and idealized object connected to a grammar and more a virtual than an actual matter. These two meanings went with his early distinction between ‘observational’ and ‘descriptive’ adequacy, as attributes of linguistic theories, specifications of the form and content of grammars. The latest incarnations of these notions appear in these two terms (Chomsky 1995):

### i. E-language

...“E is to suggest ‘external’ and ‘extensional’” (p. 16)

### ii. I-languages

...“I is to suggest ‘internal,’ ‘individual,’ and ‘intensional’” (p. 15).

In the surrounding text, Chomsky makes an explicit connection to the earlier concepts of observational and descriptive adequacy. It is fair, in view of

the discussion here to also link the ‘I’ to another word, ‘idealised.’ Chomsky hypothetical speaker Jones does not ‘have’ such a language in a pure state. “Rather, Jones will have some jumble of systems, based on the peculiar pattern of his experience” (p. 19). And:

Furthermore, even if a homogeneous speech community existed, we would not expect its linguist system to be a “pure case.” Rather, all sorts of accidents of history would have contaminated the system, as in the properties of (roughly) Romance versus Germanic origin in the lexicon of English. The proper topic of inquiry, then, should be a theory of the initial state that abstracts from such accidents, no trivial matter. (*ibid.*)

Some years ago I suggested a third term, to complement Chomsky’s two (Bach 1996):

### iii. R-languages

R is intended to suggest “real.” An R-language is supposed to be precisely what Jones or an actual speech community might “have.” Of course, idealization is still in order, but dealing with an R-language means dealing precisely with the results of the accidents of history, and whatever jumble of systems might result. However, unlike Chomsky, I contend that theories of acquisition must make room for how people learn such sets of systems, and to understand language change, linguistics must deal with just how such jumbles and historical accidents can become systematized and approach and mold I-languages. Moreover, I believe that it is only through such study that we can approach an understanding of the surprising diversity of languages.

## 6. Tensions

There is a natural affinity between the three notions of language and the three kinds of language study distinguished above. The theoretical linguist is, like Chomsky, focused on I-language(s), the descriptive linguist will be primarily concerned with R-languages, or at least will not be able to avoid them, while the philologist will be deeply involved with R-languages, and the products of users of the language, that is to say, with E-language: sets of texts, corpora, memorable utterances by particular Joneses. There will probably always be tensions among the practitioners of these various activities, often within the heart and mind of individual workers. A few hours spent on reading articles and reviews in prominent journals of the various activities will bring this home.

But there is a fourth group that we must add to our consideration: the **native speakers**, the members of the communities whose languages we study. This group also has a notion of language, and it is different from all the above. I say “notion,” but probably we should think of pluralities. In any case, I am thinking of a language as embedded in a culture, and alive in the consciousnesses of the members of the culture. The concepts of language that linguists use often abstract away from the cultural matrix of the language. I believe that many of the disagreements and tensions that arise when native speakers and linguists confront each other arise simply from differences in what the several groups mean by “language.”

We can appreciate this point by considering the problem of translation. One of the cardinal results of the linguistic study of the last two centuries has been the realization that languages are all completely adequate toolboxes for expressing anything that their users want to express. This thesis is often embodied in the slogan:

**All languages are created equal!**

This means that given time and patience and the possibility of paraphrase it is possible to translate anything from any language into any other. But what do we mean by ‘translate’ here? There is an opposite view of expressibility that is equally often expressed, perhaps more by non-linguists than linguists: “Translation was never possible” writes Margaret Atwood in a poem ‘Marsh Languages’ (Atwood 1995). And this sentiment is equally true and valid but we must understand here “translation” in the sense of reproduction of a piece of language in its flesh and bones and skin, not just the discursive content.

## **7. A case study: Syntactic categories**

Ideas about syntactic and morphological categories provide a good mirror of changing stances toward universal and particular grammar and grammars. Put briefly, the development has been from the assumption that all languages potentially share the same categories, namely, those familiar from Latin and Greek, to the opposite assumption that such categories were purely language specific, then back to the universalist view, but presumably grounded in a theoretically more defensible foundation, and then back to an emphasis on the language-particularity of such categories. The history of morphological/inflectional concepts is similar. These stages or stances can be characterized by appropriate slogans:

- **Universal I: all languages are the same, namely just like Latin.** But we must qualify immediately, as some languages are defective, in lacking various features of the quasi-Universal model. This is the era, where in the “Western” world, you find statements like these:
  - i. **Language X lacks a clear distinction between Verbs and Adjectives.**
  - ii. **English noun paradigm:**
    - nominative: John**
    - accusative: John**
    - genitive: John’s, of John**
    - dative: to John**
    - ablative: from John**
    - vocative: O John**

**BUT (see below)** It is easy to laugh at such examples, but we should not be too snifty. The idea may be good and the execution bad, that is, Latin or Greek do not provide the right set of categories to start with but the idea that terms like Noun, Verb, Adjective, or names for cases have some theoretical and universal content is not foolish. The much maligned and admired Grammar of Port Royal (Anonymous 1676) contains a beautiful example of an explanation of very language-particular facts of French based on assumptions about the universal nature of such categories as Verb, Adjective, Participle.

- **Bloomfieldian Particularism: it is not expected that languages share categories.** In this era and tradition, linguists were reluctant to use terms from traditional grammar. Instead of Noun, Verb, and the like, descriptions used terms like Class I, Class II, and so on.

There is a way of understanding syntactic categories under which the Bloomfieldian mode makes perfect sense. If a category is simply the name for a set of expressions which in the strictest construal can stand in all and only the same environments, then it is completely obvious that Noun in Japanese grammar and Noun in English grammar, for example, cannot name the same sets. So either the idea of universal categories has to have some different interpretation, or we must think of the categories as “really” meaning ‘Noun-in-Japanese,’ ‘Verb-in-English,’ and so on.
- **Universal II: all languages are the same, namely just like English.** The earliest transformational grammars took over with no substantial justification the categories of traditional grammar, but refined so as to reflect the existence of phrasal categories or ‘projections’ (speaking anachronistically) of those traditional categories. The initial empirical base was English and

as this base was broadened to include more and more different languages these categories were naturally taken over for the ‘new’ languages.

- **Early discussions of categories in transformational grammar.** There are two streams in the early years of the transformational-generative tradition that are directly relevant here.

The first may be traced to a paper by John Lyons (1966) who asked why there should be phrase structure rules such as “NP – Det + N” and not “NP – Det + V.” In other words: Is there any substantive connection between the use of “N” on both sides of the arrow, or is it just a kind of pun? There is a connection here to Harris’s (Harris 1952) use of symbols like N, N<sup>2</sup>, on the one hand, and to Categorical Grammar, on the other, and Lyons suggests using a categorial base instead of phrase-structure for what was then called the “kernel” (the notion of “kernel” was itself borrowed from Harris). This stream led eventually to so-called X-Bar theory (Chomsky 1970; Jackendoff 1977).

The other was a line leading up to and incorporated into the Universal Base Hypothesis of yore (see for example Lakoff 1970 [1966]; Bach 1968). As in recent discussions (see below), the strongest hypothesis was taken to be that all languages shared the same set of base rules, and hence categories. This was enforced by the idea that the base rules provided a direct representation of meaning. But it was not assumed that the form of the Universal Base was just what had been posited for English. In Bach (1968), for example, it was argued that English categories were on closer consideration rather like those one might posit for a language like Nuuchahnulth (Nootka), which traditionally had been assumed to have a single category of Contentives or Predicates in place of the traditional division into Noun, Verb, Adjective. (The best review of this question that deals directly with the Southern Wakashan facts as instantiated in Makah is still Jacobsen 1979.)

James D. McCawley (1982) gave a critical review of these early discussions of syntactic categories. The whole discussion lost a good deal of its urgency over the 70’s as linguistic theory absorbed model-theoretic semantics as a more adequate theory of (some aspects of) meaning. Another blow to the Universal Base Hypothesis was delivered by the demonstration by Peters and Ritchie that the hypothesis had no empirical force, given the excess power of then current transformational theories. (As far as I know recent revivals of the Universal Base Hypothesis have not given the requisite formal attention to showing that the Peters and Ritchie result no longer applies.)

- **Recent discussions.** The last few years have seen a revival of interest in the controversy about basic syntactic categories across languages. Eloise Jelinek argued on an entirely new basis for the lack of Noun-Verb distinction in Straits Salish (Jelinek 1995; Jelinek & Demers 1994). Demirdache and Matthewson (1995) argued against Jelinek, but on the basis of a different Salish language. In my opinion, the verdict on this controversy is still open, but the question still needs to be clarified:  
Is the claim for and against universality relevant to lexical categories or categories of the syntax proper?  
How does the discussion relate to questions about the mappings from syntax and lexicon to the semantics proper?  
For the discussion here let me just focus on one point:  
It may be true that the strongest or null hypothesis is that all languages have the exact same set of syntactic categories, as Demirdache and Matthewson argue. The immediate next question is: just what is this set? I would no longer argue for the position of Bach (1968): English categories are those one might arrive at from the point of view of a Wakashan language. But on the face of it that is a priori just as strong and no stronger than the null hypothesis of Demirdache and Matthewson.
- **Semantics of Syntactic Categories.** As a final point on the matter of this brief review of discussions about the universality of syntactic categories let me refer to a different line of inquiry that relates to semantics. This line asks questions about the universality of semantics and the models that we use to interpret natural language. One approach is to say that the basic model structures made available for interpreting natural language are universal, but that the mappings that are made from the syntax to these structures may and do vary from language to language (Bach 2001). A fruitful series of inquiries has dealt with questions about the nominal domains of plurality, mass, count and so on as related to various languages (see Krifka 1995; Chierchia 1998; Cheng & Sybesma 1999).

## 8. Outlook: The discussion continues

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# Language planning and language policies

## Issues and prospects

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### Introduction

Language planning as a sub-discipline of sociolinguistics is not as popular today as it was in the two decades from the 1960s to the 1980s. During this period, high expectations were raised about how language planning could contribute to development, and scholars working in the field were excited about formulating models in whose framework they hoped to be able to find solutions to “language problems”. Disenchantment rapidly set in as the expected returns from the efforts of so-called language planners and theoreticians did not materialize. With the exception of aspects of corpus planning, scholars have discovered that the ideal of planning, which they hold dear, is hardly borne out by what happens in practice. While they prescribe “initial fact-finding” before “policy formulation”, they find, to their dismay, that much of policy-making is arbitrary and unplanned. Where they expect a decision made at the top to trickle down to the bottom, they discover that contradictory policies are adopted at different levels and what is implemented at a lower level is often different from what is prescribed at a higher level.

The context in which language planning was done, particularly in the then newly independent countries, offered challenges of language management in a multilingual situation, particularly, language policy in education. In time, governments shifted their focus to more pressing issues related to the economy and welfare programs, oblivious of the fact that language was equally important for a successful execution of these programs.

Faced with decreasing support and the failure of models to account for language behavior, some scholars of language planning turned their attention

to other areas of sociolinguistics. In any case, language planning as a field of research has always been viewed with condescension by some scholars in the core areas of linguistics as not being rigorous or demanding. An example of this was an incident at the University of Cambridge in the U.K. in 1987. After I had given a paper on Language Planning at a Departmental Seminar, the Professor of Linguistics, whose specialization is syntax, commented that he had never bothered himself to know what language planning was all about nor could he understand why anyone else would! With such prejudice around, one needs little impetus to move to more prestigious areas of linguistic research.

Notwithstanding the disenchantment with language planning research, the field continues to feature in the literature. Books, monographs and papers in learned journals continue to be published, and a new journal, *Language Policy*, edited by Bernard Spolsky and Elana Shohamy, has just made its appearance in the year 2002. The fact that language planning continues to be important is amply demonstrated by its inclusion in the list of eleven Sections of the 17th International Congress of Linguists, 2003.

Why has language planning as a sub-discipline continued to survive? Basically, I think there are two reasons. First, the issues that gave rise to the concerns of the study of language planning and language policy have remained, even though the emphasis may have shifted from one major concern to another. For example, in the immediate post-independence era, many African nations were concerned with the question of identity and authenticity. Hence, they paid much attention to the issue of evolving a national language as a symbol of nationhood and national integration. With the growing (often misplaced) emphasis on modernization and globalization, which roughly translate into Westernization and adoption of a Language of Wider Communication, notably the English language, there is less and less talk of a national language or languages. Rather the emphasis has shifted more to the role of language in education, which inevitably includes a child's first language or the language of its immediate community. The whole question of educational language failure in the context of the use of an imported language as a medium of instruction is also very relevant in this regard. Similarly, so is the concern for language allocation in a multilingual situation, particularly the status of minority languages, immigrants' language, literacy, and language rights (Bamgbose 2000: 7–29).

Second, the situation in which some linguists find themselves inevitably compels them to show interest or be involved in language planning and policy matters. If one lives with a problem, one cannot help being a part of it. This position is summed up in a comment I made in an inaugural lecture I delivered in 1972:

The linguist in a developing country cannot afford to be a 'pure' linguist. He must be prepared to 'dirty his hands' (if need be) if and when he is called upon to assist with such things as the problems of language teaching, devising orthographies, writing text-books, or even primers. It is only in this way that he can show that he is genuinely concerned with the problems of the society in which he lives and works. (Bamgbose 1973: 17)

In addition to those who live with the problem, there are other committed scholars who continue to occupy themselves with the concept and implications of language policies and language planning, revisiting previously held positions, refining formulations and generally advancing the state of the art.

Given this background, what should a "state-of-the-art" paper attempt to do? I believe it should carry out a survey of developments, but more importantly, it should raise issues, particularly those issues that are of continuing relevance.

### Link between language planning and language policy

Language planning and language policy are so closely associated that they are reported to be used in some of the literature either interchangeably or considered to be two sides of the same coin (Schiffman 1996: 3; Kaplan & Baldauf 1997: 14; Calvet 1998: 114). This notwithstanding, it is useful for analytical purposes to make a distinction between the two. The link between language planning and language policy may be represented in terms of three dimensions: rigor of treatment, inclusion, and sanction by an authority.

Because of the fluid nature of policy and the supposedly rigorous nature of language planning, it has been suggested that any actions taken on language, whether to expand or limit its range of uses or to modify its form, is to be regarded as *language treatment*. Such treatment forms a continuum, at one end of which is non-rigorous treatment, which is equivalent to *language policy*, and rigorous treatment at the other end, which is equivalent to *language planning*. In between the two ends of the continuum are a series of activities ranging from language teaching to translation, which may be called *cultivation* (Neustupný 1978). In this conception of the link between policy and planning, one is radically different from the other and the only relationship between them is the rather tenuous approach of language treatment.

A more satisfying account of the link between policy and planning is a relationship of inclusion. This, in a way, is a 'chicken-and-egg' situation. One could either say that language policy embraces and includes language plan-

ning or that language policy is an aspect of language planning. Both views are represented in the literature on language planning. Take, for example, the following sample:

- a. Planning is an integral feature of policy in the sense that its function is to seek to formulate and/or administer policy. (Williams 1992:129)
- b. *Language planning* is understood as a set of concrete measures taken within language policy to act on linguistic communication in a community, typically by directing the development of its languages.  
(Quoted in Schiffman 1996:3)
- c. To the extent that policies are deliberately and consciously created, they usually involve some form of planning.  
(Herriman & Burnaby 1996:3)
- d. Language policy is official planning, carried out by those in political authority, and has similarities with any other form of public policy.  
(Ager 2001:5)

Postulates (a) & (b) support the priority of policy over planning. The arguments for this are that without policy, there can be no planning (Calvet 1998:115) and that even language policy itself is grounded in 'linguistic culture', i.e. norms and beliefs about language in the community (Schiffman 1996:5). Postulates (c) and (d), on the other hand, put language policy squarely under language planning, since policy formulation is one of the series of steps in language planning. In fact, it is even possible to say that language policy is a type of language planning, popularly known as *status planning*. The arguments on both sides boil down to the scope of language planning. If it is narrowly defined to exclude policy decisions, it will be seen to be consequential to language policy. If, however, its scope is wide enough to embrace all kinds of action on language, irrespective of whether such actions affect language status or language corpus (i.e. to cover roughly what has been referred to earlier as "language treatment"), it makes sense to see language policy as an aspect of language planning.

The third dimension of the link between language planning and language policy is the role of sanction by an authority. Postulate (d) above already points to sanction by political authority. All that needs to be added is that the authority that sanctions a language policy need not be political. In fact, there could be a hierarchy of authorities and levels of policy decisions matched by the status or power of the authority concerned. For example, higher-level decisions are fundamental decisions taken at the level of the government. An example of such a decision is conferment of status on a language (for instance, making it a

national language). Lower-level decisions are consequential decisions taken in the course of implementation by bureaucrats, commissions, committees and private institutions (Bamgbose 1989:26, 1991:111). The higher the authority in the hierarchy, the weightier the policy matter that will be referred to it for a decision and, most probably, the greater the likely force of the policy. A useful complement to “policy” is the dual notion of “ideology” and “practice”. While “policy” implies that “the maker of the policy has some form of authority over the person expected to follow it”, “ideology” refers to “a community’s generally accepted beliefs”, and “practice” refers to “the deducible implicit rules that seem to underlie the language use of the defined community” (Spolsky & Shohamy 2000:2). The importance of ideology is that it may have a bearing on policy or the way policy-makers arrive at particular policy decisions. On the other hand, one of the persistent problems in language planning is the divergence between policy and practice. By comparing policy with practice, we can easily evaluate how successful a policy is and whether there is need for policy revision.

Considering the three dimensions of the link between language policy and language planning, we can conclude that language policy is an aspect of language planning and that it is subject to the sanction of an authority.

### Characteristics of language policies

Language policies differ not only in terms of where they appear, how they appear, but also in terms of what they are intended to achieve and how. Arising from this, is it likely that one policy will have much in common with another? The question has even been raised about the monolithic nature of the term “policy” and whether, in view of the range of referents that it has, cross-country comparisons of policies are ever possible (Herriman & Burnaby 1996:3). The problem posed here is not different from that of any other term that is vague or ambiguous. In any case, there is need to talk of policies and to compare them. A useful starting point is the typology of language policies proposed by Noss (1971:25):

- *Official language policy*: Languages recognized by the government and for what purposes.
- *Educational language policy*: Languages recognized by education authorities for use as media of instruction and as subjects of study at various levels of public and private education.

- *General language policy*: Unofficial government recognition or tolerance of languages used in mass communication, business and contacts with foreigners.

Much of language policy decision continues to revolve around this three-way paradigm, with undoubtedly greater emphasis on educational language policy, since it is assumed that one of the most effective ways of implementing a language policy is through the educational system.

Individual language policies in each of the above categories may differ in the following respects: *force* (degree of compulsion or promotion), *gestation* (the deliberative interval between initiation and decision), *stability* (predictability over a reasonable period of time), *feasibility* (probability of implementation), *formulation* (content in light of expected goals), and *visibility* (whether overt or covert).

The force of a policy may be seen in how it is expected to affect the languages or the subjects for which the policy is intended. Schiffman (1996: 28–29) referring to Kloss (1977) has drawn attention to the distinction between *promotive* and *tolerance* policies. The former are designed to encourage the use of particular languages in specified domains through laws and regulations or through provision of resources, while the latter are intended to allow the use of languages without explicitly promoting them. In terms of Noss's typology, official and educational language policies are likely to be promotive policies, while general language policies are likely to be tolerance policies. In relation to persons, Cooper (1989: 89–90) also points to the distinction between *regulatory* and *symbolic* language policies. Regulatory policies aim to influence language behavior, while symbolic policies assign status as a result of achievement or adoption of the policies concerned. Examples of the two types respectively (as given by Cooper) are the promotion of Hebrew through its exclusive use in certain domains and the feminist campaign against the use of male-dominated gender pronouns.

The gestation period of a policy refers to the time it takes between the conception of an idea and its realization in terms of a policy decision. It is customary to talk of policies as if they all arise from careful deliberation and rational planning over a period of time: involvement of experts in the preparation of a plan, setting of targets, authorization, and systematic implementation (Jernudd & Das Gupta 1971: 196). Obviously, this is merely an idealization, as most policies hardly match this abstract standard. Many policies are in fact arbitrary or taken in respect of a one-off situation, otherwise called "policy-on-the-run" (Herriman & Burnaby 1996: 3). A possible test of arbitrariness of

a policy is the gestation period. As stated in an earlier study, “The arbitrariness of policy decisions may be measured by the interval between the time the idea is conceived and the decision itself. The shorter the interval, the more arbitrary the policy is likely to be” (Bamgbose 1991: 114). The language planning history of several countries is full of instances where policy measures are introduced by fiat and required to come into force “with immediate effect”. To take ‘change of script’ as an example, the introduction of the Latin script for the Somali language took effect from the same date as the decision was taken on October 21, 1972 (Andrzejewski 1979). Similarly with the change of Turkish script by Kemal Ataturk from Arabic to Roman in 1928, and that from the Latin script for Tajik in Tajikistan to Cyrillic by a decree of May 21, 1940 (Shorish 1984). Different from change of script is the introduction of French in Nigeria in 1998 as a second official language. Faced with the mounting opposition to his military junta, General Sani Abacha, the then Nigerian Head of State, took this decision to spite the U.S. and Britain, which, at that time, were in the forefront of the opposition to the regime by the international community.

A language policy may be evaluated on the basis of how stable or consistent it is. In most Western democracies, major policies are settled on a bipartisan or multi-partisan basis, and once broad agreement is reached, fluctuation in policy is reduced to the barest minimum. In many developing countries, competition between political parties often leads to jettisoning of previous policies. Other factors that foster instability in language policy include frequent changes of government, including the sudden and often violent disruption typical of a military take-over of government, new ideas or practices recommended by specialized commissions or adopted on the advice of foreign experts or organizations. Quite often, educational language policies come as a package in new educational policies or reform. My favorite example is that of the mother-tongue medium policy in Ghana, which, within the period 1950–1974, changed six times with each change virtually coinciding with a change of government (Bamgbose 1991: 115–117). Needless to say, frequent policy changes entail repeated reorientation, a certain amount of waste, and an inevitable lag between policy and practice.

When we say that a language policy is feasible, it means that, all things being equal, such a policy has a reasonable chance of being implemented. Not all language policies are feasible. In fact, making a language policy that is not feasible is a well-known avoidance strategy. There may well be social or political pressure for a policy, which those responsible for know cannot be implemented or have no intention of implementing. In many African countries, for example, it is politically correct to pay lip service to the importance of indigenous

languages. Some countries elevate them to the status of “national languages”, while happily continuing to use French in all official domains. Others promote one language to the same status, but only as a decorative symbol, while the *de facto* language actually playing that role is English. In educational language policy, it is not unusual for all sorts of games to be played with policy statements, which turn out to be empty. When a country that does not have, or has no control over, pre-primary schools prescribes a medium of instruction for such schools or when a country that has a shortage of teachers of French in secondary schools wishes to make French a primary school subject, it is obvious that such a policy is not intended in any way to be implemented.

The way a policy is formulated can be a guide as to how seriously it is intended to be taken. Quite often, a policy is clear-cut and the implementation steps are well articulated. Some policy formulations are, however, vague or riddled with *escape clauses*. A policy may be said to be vague when it has no particular focus and it is not really capable of being implemented. An example of such vagueness is the 1970 language policy of Kenya’s ruling party in respect of the adoption of Swahili as a national language. This policy stipulated that all Kenyans were to speak Swahili at all times in all situations and all communication with government officials was to be in Swahili (Gorman 1973:76). Obviously, it is simply impossible for anyone to use any language in all domains at all times. Consequently, what this policy is about is more of an aspiration than a realizable goal. Similarly, escape clauses are a clever device of making it possible for the implementation of a policy to be avoided. Some examples of escape clauses highlighted in italics below (see Bamgbose 1991:117, 119; Republic of South Africa 1996:4, 14) are:

- The requirement that Nigeria’s three major languages should be used for conducting business in the National Assembly, “*when adequate arrangements have been made therefor*”.
- The teaching of the same languages in secondary schools, “*subject to the availability of teachers*”.
- The adoption of any official language by the national or any provincial government of South Africa for government business, “*taking into account usage, practicality, expense, regional circumstances and the balance of the needs and preferences of the population as a whole or in the province concerned*”.
- The educational language policy of South Africa which gives the right to receive education in the official language or the languages of one’s choice in public educational institutions “*where that education is reasonably practicable*”.

Although it can be argued that such clauses are a legal requirement to prevent litigation in case of non-implementation, it is also true that they make non-observance of a policy so much easier.

The dimension of visibility relates to whether a policy can easily be noticed or not. The norm is for a policy to be stated in form of the constitution, laws, regulations, decrees, proclamations, pronouncements, and judicial interpretations. These are overt policies, which generally can be referred to in a document. On the other hand, there are policies, which are covert and only left to be inferred from observed practices. A common, but faulty, assumption that is often made is that absence of an overt policy means that there is no policy. In fact, absence of a policy is indeed a policy. Nowhere is this made clearer than in the post-independence context of many developing countries, which continue to perpetuate colonial language policies, while overtly avoiding the promulgation of new policies.

A summary of the characteristics of language policies will show that, if we expect the default policies to be regulatory, non-arbitrary, stable, feasible, goal-oriented, and overt, actual experience of language policies shows that these are ideals, which are rarely attained in practice. It is one of the major challenges for language planning models to be able to account for those policies that do not seem to conform to the ideal.

### Motivation for language policy

Why are language policies necessary? In the heydays of language planning, the need for language policy was predicated on observed language problems. Such problems may affect a whole nation or the school population or even individuals. Thanks to the work of Ager (2001), we now know that motivation for language policy goes far beyond the need to correct language inadequacies. The motivating factors he postulates, which may be abbreviated as I-7, are identity, ideology, image, insecurity, inequality, integration and instrumentality.

The quest for identity is bound up with nationalism, cultural distinctiveness, and language rights (including rights of minorities). The use of language as a marker of identity is what leads to the search for national language, regional language and revival of languages associated with ethnic minorities within a larger multilingual community. Much of policy-making is about such issues, but as Ager (2001:39) has correctly pointed out, “appeals to cultural identity need political power to put the relevant policy into place, and the relevant policy must demonstrate economic advantages if it is to stick”. Given this position,

one of the major motivations for language policy that needs to be recognized is economic. The experience of Malaysia in this regard is instructive. In the immediate post-independence period, the emphasis of language policy was on national identity, which manifested itself in the replacement of English with Bahasa Malaysia in most domains. After many years, economic considerations have now led to the re-enthronement of English. It is argued that if rapid industrialization is to be achieved and the country is to become a “multi-media super corridor” and a regional center for education, traditional nationalism needs to be jettisoned and former “traditional nationalists” are to be transformed into “pragmatic nationalists” who will sponsor and support active bilingualism in English and Bahasa Malaysia (Gill 2002:37–42). This experience shows the enormous influence of economic motivation. If a country that has had such a long history of linguistic nationalism can make a roundabout turn in this manner, the strength of the economic factor needs to be duly recognized.

A language policy may be motivated by ideology in one of two senses. First is ideology as shared belief by the community, which, as mentioned earlier, may influence the way policy-makers view and arrive at policy decisions. Second is ideology as determined by a party in power. In this connection, reference has been made to the return to traditional values by the Conservative Party under the leadership of Margaret Thatcher, an outcome of which is the emphasis on Standard English in the curriculum (Ager 2001:42–43). This is in contradistinction to tolerance of dialectal varieties and bilingual education practices, particularly for immigrant children. In this second sense, ideology is not different from a program advocated by a party and sometimes even submitted to the electorate as a party manifesto prior to an election. What follows from this is the need to fulfil an electoral pledge.

Image projection is closely linked to identity in the sense that once identity has been established through association with a language, a possible corollary is the engineering of favorable attitudes to such identity through cultural diplomacy. The activities of language agencies such as the Goethe Institute, Alliance Française, the British Council, the Japan Foundation, the English-speaking Union (Kaplan & Baldauf 1997:6) are all designed to project the image of the countries associated with the languages of those countries. Globalization has further intensified the drive for propagation of certain languages and cultures, with the result that the dividing line between cultural diplomacy and linguistic imperialism (à la Phillipson 1992) has become blurred.

It has been suggested that insecurity is one of the motives for language policy and the examples given are the exclusion of Gypsies from mainstream language policy and the resentment of French authorities to English-inspired

French neologisms (Ager 2001: 77–88). In neither of these cases can insecurity be said to be a factor. Gypsies are a minority on the outside fringes of society and the so-called neologisms are too insignificant to affect the enormous vocabulary of French. Perhaps what is more likely in evidence here is purism, either in terms of protecting a language from contact with a stigmatized variety or dialect or shielding the vocabulary of a language from the influence of an external code.

Language policy may arise from a perception of the need to correct inequality either in terms of access or prestige. Quite often, policy decisions of this type are not spontaneous but arise from pressures brought to bear on policy-makers. The example frequently cited in the literature is the feminist movement's opposition to sexist vocabulary in English as well as male-dominated generic nouns or pronouns, such as the use of *man* to refer to males and females or *he* as a default reference to persons or professions (Cooper 1989: 14–21; Ager 2001: 89–93). Inspired by a desire to right the inequality usually associated with the assumption that the male is the norm, the groundswell of pressure forced learned societies, publishing houses, and even government departments to review their stylistic conventions and, eventually, a non-sexist policy emerged and was generalized, to the extent that anyone who persisted in sexist language was stigmatized. Similar pressures may lead to the recognition of minority languages for certain purposes or even the adoption of an educational language policy. For example, policies favoring the use of the first language as a medium of instruction are often due to the relentless advocacy of mother tongue education by UNESCO in meeting after meeting and resolutions addressed to member states.

Integration and instrumentality are not so much motives for policy as motives by individuals for supporting particular policies. When a government provides facilities for language acquisition, it does not overtly insist that citizens should choose particular languages for the purpose of getting integrated or only as a means of getting along in the community. Immigrants who have no desire to return to their original home and who have no community support for their home language are more readily likely to choose integration. On the other hand, immigrants who still keep in touch with their original home or have strong community support for their language are most likely to opt for bilingualism and to favor instrumentality over integration. It should be said, however, that the situation is never static, as the younger generation as well as children born to immigrants may not consider themselves bound by their parents' choice. Where there is strong community support for a language, there may be pressures to give some status or attention to the language

as is the case in California, where bilingual education involves Spanish and English and concessions are made for Spanish-speaking citizens, particularly in terms of official documents and information dissemination (Macías 2001). Australia's National Language Policy not only recognizes other languages in addition to English, it makes provision for their maintenance and support (Ozolins & Clyne 2001:384). There is, of course, the possibility of counterpressures to enforce integration through a dominant language. Proposition 187 which was submitted to voters in California in 1994 was intended to deny education and welfare services to illegal immigrants, and consequently weaken the position of citizens who are not proficient in English (Ricento 1996: 150–151). The English Only movement, which would like to have English entrenched in the Constitution, and discourage the use of other languages (Baron 1991; Crystal 1997: 117–122) is another example of pressure to achieve compulsory integration.

It should be clear from the foregoing that motivation for language policy is complex and may even be contradictory. For example, the quest for identity may serve to generate integration, but it may also favor instrumentality where the cultural distinctiveness of minority groups is involved. Ultimately, the translation of any motive into policy depends to a large extent on the sociopolitical and economic clout in support of the policy.

### Language policy-makers

Given an earlier characterization of language policy as decisions on language requiring the sanction of an authority, it follows that to be considered a policy-maker, the person or entity involved must be in a position of authority. Ager (2001: 175–176) identifies three categories of major actors in policy formation: individuals (such as powerful rulers and opinion leaders), the ruling group or elite, and the state. Cooper (1989: 88–89), on the other hand, citing Ellsworth and Stahnke (1976), identifies the three categories of *formal elites*, *influentials* and *authorities*. Formal elites are those mandated to make policy. Their composition includes governments (presidents, governors, ministers, council chairmen), the legislature (senators, representatives, members of parliament), commerce and industry (chief executives, boards), education (school boards, principals, college councils, vice-chancellors), etc. Influentials are the privileged persons in the society who usually have a vested interest in given policies and do their best to influence the emergence of such policies. Authorities are those who actually make the decisions.

Three observations may be made about this division. First, the types are not mutually exclusive. A formal elite may also be an influential as well as an authority. For example, a minister is formally vested with the authority to make policy, she or he could also be a member of the privileged class having vested interest in a given policy, and the eventual policy decision may have to be taken by her or him. Second, those who do not have authority may, through the sheer force of influence or pressure, force policy decisions. In this sense, we could talk of policy initiatives. Such initiatives, however, do not become policies until an appropriate authority empowered to make policies has sanctioned them. Third, decisions are taken at different levels and it is sometimes difficult to tell when certain lower-level decisions taken by operatives in the implementation of given policies are still to be regarded as policy-decisions or merely implementation steps. For example, while a decision by a minister of information to use a certain language in broadcasting is validly a policy decision, the same cannot be said for the decision of a newscaster to use one term rather than another in that language.

There is a definite correlation between power and policy-makers. Those who make policy tend to be those who have access to power and privilege and it is to be expected that they will exploit it to their own benefit. Nowhere is this power relation more obvious than in post-colonial situations, where the elite tries to maintain its hold on power through the entrenchment of the colonial language, the mastery of which has enabled them to attain their privileged position. This phenomenon, which has been called “elite closure” (Scotton 1990:27), ensures that the majority of the population who are not literate in the colonial language remain disadvantaged. Since those who are privileged are also favored with power and prestige, it follows that the language they use is also valued as the language of power. Those who cannot speak that language are powerless and so are their languages (Webb 1991:5). In fact, such is the force of power in policy-making that the claim has been made that “Language policy is one mechanism available to the state for maintaining its power and that of groups which control state policy” (Tollefson 1991:10).

### Language planning models

The practice of language planning existed long before *language planning* as a term was introduced by Haugen (1959) in connection with the language situation in Norway. Variations of the term and associated terms have subsequently emerged and these include *language engineering*, *language development*,

*language treatment, language cultivation, language management, language regulation and language reinforcement.* Because of the diversity and complexity of language situations which language planning is required to account for, it is not surprising that approaches to it are varied. This has also resulted in a proliferation of models of language planning. Since it is practically impossible to discuss all the models that have been proposed in the literature, what I intend to do is select seven models that I consider representative of the range of models and situations. The selected models are language improvement, form-function, process, correction, accounting, eco-system and value-chain.

The language improvement model is associated with Tauli (1968). Its basic assumption is that natural language is imperfect and could do with improvement, which language planning provides. This is facilitated by the conception of language as an instrument, which, like any tool, can be reshaped and improved. Hence, language planning is concerned with evaluating language so as to conform to the principles of *clarity* (conveying all of necessary information and shades of meaning), *economy* (least possible number of linguistic units and shortest expressions possible), *aesthetics* (harmony, euphony, and brevity of speech) and *elasticity* (ease of adaptability to new tasks). The application of these four principles is expected to lead to an “ideal norm”, which, by the minimum of means, attains the maximum of results. Tauli’s model of language planning is a powerful machine, which can take the raw data of natural language and manipulate it so as to bring out the ‘best form’, whether in terms of a standard language or style or speech. It is excessively preoccupied with language as a tool (see Haugen 1971b) and language as a means of communication to the neglect of the social and cultural significance of natural language. Its scope is narrowly geared to corpus planning. This probably explains why this model has had little influence in the literature on language planning.

The form-function model (Haugen 1966, 1983) is chronologically the first model of language planning to be proposed. In its original version, it involves processes of language development in a four-cell matrix in which *form* and *function* are coupled with *society* and *language*. The processes are

1. *selection of a norm* (one of a number of competing languages, modification of an existing language variety or creation of a new standard),
2. *codification of form* (establishing the selected norm by adopting an appropriate script, devising an orthography and linguistic description),
3. *elaboration of function* (expanding the language to cope with use in wider domains, particularly vocabulary expansion) and

4. *acceptance by the community* (stamp of authority on the selected norm by the government).

Process 1 involves form and society, 2 involves form and language, 3 involves function and language while 4 involves function and society.

The usefulness of Haugen's original model lies in its simplicity, which may have accounted for its general acceptance with or without modification (Fishman et al. 1971; Jernudd & Das Gupta 1971; Jernudd 1973; de Vries 1991). Its weak point is that it is silent on who is responsible for selecting a norm and what the inputs are to the selection process. Reacting to proposed modifications and other suggested models, Haugen (1983) attempted to improve his original model by incorporating concepts suggested by others such as "status and corpus planning" (Kloss 1969), "identification of problem", "correction", "policy and cultivation" (Neustupný 1978), "language allocation" (Gorman 1973), "evaluation" (Rubin 1971) and "graphization" (Ferguson 1968). Together with the change of nomenclature from *acceptance* to *implementation*, and its reordering to come before *elaboration*, the resulting revised model is less satisfactory as it incorporates elements that are not necessarily compatible. In the revised model, *selection* is said to be concerned with "decision procedures" which will include "identification of problem" and "allocation of norms". This is, at least, an attempt to pay some attention to how norms are selected.

The process model (also known as the decision-making or planning model) of language planning (Rubin & Jernudd 1971; Rubin & Shuy 1973; Ager 1996:13–14) takes off where the form-function model stops in that it sets out to account for how language planning decisions are made and why. The basic assumptions of the model are that language planning arises from perceived inadequacies, that language planning is part of the political and economic process, that language is a resource like any other economic resource, that the processes applicable to social and economic planning must be applicable to language planning, and that language planning is tied to a central authority, preferably the government and its agencies. The two major aspects of the process model are its procedure and its orientation. Its procedure begins with fact-finding which is wide ranging and could include a survey of language situation, dialect research, evaluation of competing media of instruction, language needs, etc. Then follows the actual planning, which consists of establishment of goals, selection of means, and prediction of outcomes. After this comes implementation. However, there is evaluation at every stage. According to this model, language planning is future-oriented, i.e. the expected outcomes must be specified in advance of the actions taken.

Attractive as the process model may be, particularly in view of its rigor and association with decision-making in other areas of social planning, it is no more than an ideal, which is hardly ever realized or even realizable. The fact that this model does not account for much of language planning, particularly when it involves policy-making, has been pointed out repeatedly in the literature (Bamgbose 1987; Cooper 1989; Ager 1996: 14–15). As Cooper (1989: 92) has rightly observed,

There is general agreement that policy-making does not conform to the rational model of decision-making. This paradigm assumes a single decision-maker or decision-making unit, with a single set of preferred outcomes, knowledge of a reasonable full range of alternatives and of their consequences, the intention of selecting that alternative which maximizes benefits and minimizes costs, and the opportunity, willingness, and ability to make necessary calculations.

Quoting Bauer (1968: 11), Cooper concludes, “In the process of policy formulation, every one of these assumptions is violated”. In an earlier critique, I pointed out five deficiencies of the model: “It forces language behavior into the narrow mould of economic planning, its operative processes are idealistic and Eurocentric, its emphasis is on the negative aspects of language, it is weighted in favor of corpus planning, and it is too much government-oriented” (Bamgbose 1987: 7). It is no surprise then that the influence of the model, which was very high in the seventies, has waned considerably.

The correction model (Neustupný 1978: 243–278, 1983) has three basic assumptions: First, that there are many activities about language which may not fall under the narrow scope of language planning, but all such activities may be regarded as a type of *language treatment*. Second, that correction systems are triggered only when there is a language problem. Third, that language planning is only one kind of language treatment. Language inadequacies are observed in the language situation through inquiry or research, and the identification of a language inadequacy leads to an action program involving language correction or treatment. Once decisions are taken on the appropriate course of action, implementation follows. The main advantage of the correction model is that it provides a broad framework for dealing with language issues that need to be addressed. Its weakness is that the scope it gives to language planning is as narrow as that of the process model. As Neustupný (1983: 2) claims, “language planning is most suitably used to denote only such language treatment that is informed by language planning theory: normally it will be systematic, theoretical, rational (in other words, ‘rigorous’ and future-oriented”. Much of

language planning activity, particularly of the status planning type, will simply be regarded as language treatment minus planning.

The accounting model or scheme (Cooper 1989:98) is a descriptive framework for handling language planning situations and processes. Cooper begins by looking at four alternative models, which conceivably may be used for language planning. These are: language planning as the management of innovations (who adopts what, when, where, why and how?), language planning as marketing (product, promotion, place, and price), language planning as the pursuit and maintenance of power (who gets what, when and how?) and language planning as decision-making (who makes what decisions, why, how, under what conditions and to what effect?). While each of these models could throw some light on language planning processes, none of them by itself can satisfactorily account for all the aspects of language planning. This is the basic premise for the accounting model whose objective is to make a comprehensive description of what goes on in the language-planning situation. Following his definition of language planning as “*deliberate efforts to influence the behavior of others with respect to the acquisition, structure, or functional allocation of their language codes*”, Cooper’s summary of the accounting model is as follows: What *actors* attempt to influence what *behaviors* of which *people* for what *ends* under what *conditions* by what *means* through what *decision-making process* with what *effect*? (Cooper 1989:45, 98). One innovation made by Cooper is the addition of “acquisition planning” (language learning-related activities) to the status/corpus planning dichotomy.

The main advantage of the accounting model is its wide coverage as well as its neutrality in respect of language planning activities, actors and their actions. For example, it does not restrict the kind of activity involved in language planning nor does it expect actors and their actions to conform to a set scheme. For those who insist on a rigid model, this framework may be too loose to facilitate easy comparison, but one thing it certainly will do is provide a lot of information on the language planning process in each situation or country.

The eco-system model (Kaplan & Baldauf 1997:311) is another descriptive model, but unlike the accounting scheme, it concentrates mainly on the linguistic situation and the influences on it. Central to the model is the linguistic situation (i.e. eco-system) that planning is intended for, and, isolated within this situation, are national/official language and several minority languages, including those that may be on the verge of extinction. Impacting on the linguistic eco-system is a set of forces as well as agencies and organizations. The forces are language death, language survival, language change, language revival, language shift and language spread, language amalgamation, language contact

and Pidgin and Creole development, and literacy development. The agencies and organizations are government agencies, education agencies, communities of speakers, non-government organizations, and other bodies (Kaplan & Baldauf 1997:6, 296, 311).

The emphasis of the eco-system model on the linguistic situation facilitates cross-country comparison while the model itself is dynamic in that changes over time can be reflected in changes in the linguistic eco-system. The agencies and organizations reflect the reality that much of language planning is done, not by governments or its agencies, but by other bodies, including the community of speakers as well as individuals. It is difficult to explain why those eight forces selected as impacting on the eco-system are the “key language change elements” considered crucial for the model. Some of them, such as language revival and literacy development are goals of language planning while others, such as language death, language change, language amalgamation and language contact are facts of the linguistic situation. Obviously, this sort of selection is perhaps influenced by one’s experience of particular situations. For example, while language status changes are subsumed under “language shift and language spread”, someone dealing with post-colonial language policies would most probably have had “language allocation” as one of the crucial forces impacting on the linguistic eco-system. More importantly, what the planning agencies do and how can only be inferred from insights from other models; yet this should be an important aspect of language planning.

The value-chain model (Donnacha 2000) is an adaptation of a business planning approach (Porter 1985) to language planning. Just as in business, there are *primary activities* (product creation, sale, and after sales service) and *support activities* (purchased inputs, technology, human resources, etc.); an integrated model for language planning can be conceived of as involving two types of language-reinforcement effort: *primary activities* intended to influence language behavior and *secondary activities* designed to support the primary activities. The primary activities are those designed to nurture positive attitudes to the language, increase ability and use, strengthen the language community, and increase intergenerational language transmission. The secondary activities are: putting in place appropriate organizational structure and ensuring its effectiveness, the language planning process, human resource management, research, corpus planning, and convergent planning as related to language planning (Donnacha 2000:16).

In spite of the author’s claim that the value-chain model is of general applicability, it is clear that it has been influenced by the Irish situation, where active language promotion is desired. That is why all the primary activities

are for influencing language attitudes towards greater use and transmission to the younger generation. This bias manifests itself in the very choice of the term “language-reinforcement”, which is a cover term for language promotional activities of which language planning is merely a sub-set. No account is taken of “Negative language planning” (Kaplan & Baldauf 1997:230–232), where the aim is to discourage linguistic pluralism. Besides, language planning as conceived by Donnacha excludes status planning, which is the focus of much language planning activity in many developing countries. The value-chain model is, in effect, a descriptive framework, which is heavily weighted in favor of the so-called primary activities, which are largely the expected goals of successful language planning.

Based on the literature on language planning, six dimensions of language planning activity may be established as shown in Table 1.

**Table 1.** Dimensions of language planning activity

Determining Factor	Components
ACTOR	government, agencies, groups, individuals
ACTIVITY	treatment, cultivation, policy, planning
TYPE	status, corpus, acquisition, prestige
METHODOLOGY	process, management, marketing
GOAL	modernization, reform, revival, maintenance, standardization
MOTIVE	identity, ideology, image, insecurity, inequality, integration and instrumentality

Most of the components are either self-explanatory or have featured in the discussion of the sample models. The only components that call for some comment are those related to the dimension of “TYPE” where “status planning” and “corpus planning” have long been recognized. Although a division between these two is necessary for analytic purposes, it is generally accepted that dealing with one has implications for the other (Ager 1996:5; Kaplan & Baldauf 1997:5; Fishman 2000:44). Cooper’s introduction of “acquisition planning” has also been favorably received and incorporated in most language planning discourse. In spite of this, I am of the view that language spread through language teaching, which has motivated the introduction of a third type of planning, is really an aspect (though a major one) of implementation of language policy. As Kaplan and Baldauf (1997:121) have pointed out, the failure of “independently implemented” language acquisition plans may be traced to their exclusion from the larger framework of implementation of status and corpus planning. In many language-planning situations, there is excessive reliance

on language educational policies and the educational system as the major instrument for implementing such policies. In effect, language planning is often reduced to educational language planning. The caution expressed by Donnacha (2000:21) about the inadequacy of reliance on the school without adequate attention to “intergenerational transmission” of a language through the conventional family and community channels is entirely justified. The component of “prestige planning” (Haarmann 1990: 120–121) is meant to account for the promotion that is done by the different groups of actors. For instance, government actors engage in official promotion, agencies in institutional promotion, groups in pressure group promotion and individuals in individual promotion. Again, there is no reason why all kinds of promotion should not be seen as an aspect of implementation of status planning.

There are certain features that characterize the different models of language planning. First, they comprise elements borrowed from other disciplines. Such disciplines include economic planning, policy studies, business planning and organization, marketing, psychology, medicine, etc. A positive way of looking at this is to see the elements borrowed as enrichment, i.e. “Language planning as a field of study draws on a variety of different disciplines for expertise, and each of these fields in turn contributes its own unique methods and techniques for collecting data as a basis of language planning” (Kaplan & Baldauf 1997: 87). On the other hand, the need to borrow elements from different disciplines may be said to betray a certain lack of independence of language planning as a field of study. Besides, the choice by different models of elements from different disciplines contributes to a divergence in models.

Second, key concepts are arranged and re-arranged. Consider the arrangement and directions of the following:

- Selections > Codification > Elaboration > Acceptance (Haugen 1966)
- Selection > Codification > Implementation > Elaboration (Haugen 1983)
- Policy formulation > Codification & Elaboration > Implementation (Fishman et al. 1971)
- Determination > Development > Implementation (Jernudd 1973)
- Fact-finding > Actual Planning > Implementation (Rubin et al. 1971, 1973)
- Inadequacy Identification > Action Program > Implementation (Neustupný 1978)
- Status Planning > Prestige Planning > Corpus Planning (Haarmann 1990)
- Status Planning > Language Planning > Functional Level Primary Activity/ Functional Level Support Activity (Donnacha 2000)

The impression of similarity given here is deceptive, as the concepts do not necessarily have the same coverage in terms of their meaning and significance.

Third, no single model has been able to capture all language planning situations. The reason for this may well be over-concentration on language issues to the neglect of social and political factors (Apter 1982:220; Cooper 1989:35; Williams 1992:124; Rannut 1999:101). In the words of Blommaert (1996:207), “More and more, language planning is seen as a primarily *political* phenomenon that has to do with societal disciplining, homogenizing or dividing, and which may or may not be guided by sociolinguistic insights”. To make models comprehensive is not merely to incorporate insights from politics and sociology, it requires a fundamental change of attitude on the part of scholars concerned with language planning.

Objectivity is a cherished canon in academic scholarship. In language planning, there could also be “pseudo objectivity”, if one restricts oneself to purely linguistic considerations and avoids anything overtly political. As put by Williams (1992:123–124), “most planners involved with language planning have seen their task as ideologically neutral, whereas the politicization of language in multilingual settings has increasingly drawn attention to the relationship between language planning and wider political issues”. My own experience with several planning situations reveals a reluctance by linguists to get involved with political issues to the extent that some foreign researchers claim that it would amount to unwarranted interference if they were to express an opinion on policy options. However, it is clear that

if a linguist presents with detachment all the facts and factors to be considered in making some decisions and refuses to go beyond this to evaluate possible options or even recommend a particular option, he may be forced to look on while less knowledgeable people pontificate on the virtues of an option he knows to be unworkable or disastrous. (Bamgbose 1991:129–130)

Hence, refinement of models basically entails expanding the underlying basis of language planning and attitudinal change to non-linguistic factors.

### Further issues in language planning

There are several other issues in language planning which may have a bearing on the language planning process. A selection of such issues includes the concept of language problems as a basis for language planning, the range of actors,

planning and non-planning, ambitious planning, and prospects of a theory of language planning.

In much of the literature on language planning, actions taken on language are assumed to have arisen from the existence of a language problem. This position is clearly illustrated in some definitions as well as the basic assumptions of several models. Typical of this is the oft-quoted definition of language planning as “the organized pursuit of solutions to language problems, typically at the national level” (Fishman 1974: 79). In addition, the basic assumption of many models is identification of a language problem through fact-finding. It is the presence of such a problem that leads to attention through correction, cultivation or planning. Such is the association of language planning with “problems” that the leading journal in the field has the title, *Language Problems and Language Planning*. Obviously, not all the sources of language planning can conceivably be traced to “language problems”. For example, multilingualism and status planning leading to policy decision on one or more languages for certain functions is traditionally viewed as a language problem. This is because multilingualism is negatively evaluated as impeding communication. Its practical value in terms of bilingualism and cultural enrichment is often conveniently ignored. Even when there is something that can truly be regarded as a language or communication problem, such is its nature that it is only a manifestation of an underlying political or economic problem. Consequently, “it is preferable...to define language planning not as efforts to solve language problems but rather as efforts to influence language behavior” (Cooper 1989: 35).

It is widely accepted that actors in the language planning process can be governments and government agencies, non-government agencies, groups and individuals. The point of disagreement is whether action by non-government bodies can validly be regarded as planning. Some will exclude from language planning instances of language decisions taken by private companies, media houses, learned societies, and individual authors. What such bodies do will be merely language treatment or language cultivation, but *not* planning in the strict sense (Jernudd 1973: 18–19; Haarmann 1990: 120–121). Hence, the stigmatization of such practices as a “happening” (Jernudd & Das Gupta 1971: 199) rather than “planning”. Although this distinction between strict planning and other forms of language intervention is still found in the literature, it is becoming difficult to sustain, because of the weakness of the purportedly rational and idealistic decision-making model on which it is based. In relation to non-planning, mention must be made of “unplanned” language planning (Kaplan & Baldauf 1997: 297–299), which may refer to non-planning,

unplanned outcomes of specific planning measures or behavior contrary to planning objectives.

While it is true that policy decisions, if they are to be meaningful, must have a stamp of authority, the role played by non-governmental organizations should not be ignored. To do so will be to make language planning too restrictive (Cooper 1989: 31). My experience with orthographic reform in the Yoruba language is a pointer to the role played by groups and individuals. Because of inconsistencies in spelling and conventions of word-division, a society devoted to language promotion invited me to give a lecture, which was subsequently expanded into a booklet. Arising from the awareness thus created, the State Government appointed an Orthography Committee to study the proposals and make recommendations. The recommendations were further submitted to an enlarged Committee, which produced a final set of recommendations. However, such was the sensitive nature of the recommendations that they were not enforced. In the meantime, teachers continued to be worried about which spellings to accept or reject, particularly in examinations. Consequently, the Teachers' Association in the language put pressure on the Federal Ministry of Education to accept the recommendations and make them general for all the States in which the language is taught. That was how the National Council on Education came to adopt the recommendations, which have become the norm for almost thirty years. A series of actors have been involved in the language planning process in this case: the Language Society that commissioned a study of the inconsistencies in the orthography, the expert who produced the study, the State Government, the Orthography Committees, the Teachers' Association, the Federal Ministry of Education, and the National Council on Education. Can there be any justification for excluding any of these actors from the language planning process or for saying that some are involved in planning and others in language treatment?

The bureaucratic approach to language planning has meant an undue emphasis on top-down as opposed to bottom-up language planning. Yet it is understandable why such emphasis continues to pervade language-planning scholarship. First, as has been pointed out repeatedly in this paper, sanction by an authority is required for policy decisions. Second, a bottom-up approach is easier postulated than actually practiced. This is because initiatives at individual or group level often end up as pressures, which may not necessarily result in policy decisions. In spite of this, provision must be made for different types of intervention such as lower level planning in a hierarchical situation, implementation structures and processes at lower levels, initiatives outside official channels, and involvement of consumers of policy through a survey of

preferences and attitudes (Ager 1996:12; Webb 1991:14; Adegbija 1994:79; Bamgbose 2000:114).

In most language planning, it is almost a credo that planning goals must be matched with available resources. One factor that is often ignored in all this is commitment, as reflected in the political will to prosecute the stated objectives. This could even lead to a reordering of priorities. Hence in language planning, it is sometimes necessary to think “big”, even beyond the limit of rationally objective possibilities. The concept of *strategic intent* (Hamel & Prahalad 1991) as reported in Donnacha (2000:24–25) which is used in business planning has been borrowed and applied to language planning to cover the overwhelming ambition that makes one work outside the limitations of current factors. The example of the revival of Hebrew as a spoken language is cited in this regard. But for the commitment and the will to transcend logical possibilities, the venture would not have been started at all:

Rational thinking would have had its way and the world would not have known what was irrationally possible. Many other language-reinforcement efforts continue on in similar circumstances, struggling to succeed where rational analysis suggests that success is not possible. (Donnacha 2000:25)

This concept is extremely important for developing countries trying to cope with status planning in the face of inherited colonial policies.

The question has often been asked, “Is there a theory of language planning?” The answer that one gives depends on how one evaluates the approach. In other words, is it simply a question of “no theory” or “bad theory”? (Blommaert 1996:204–205). There are three possible positions in this matter. The first is to say that no theory is possible, since each language-planning situation may not be exactly the same as the other. It is easy to fault this position, as there are enough similarities that make significant generalizations possible. The second is that from the observed generalizations, it is possible to postulate a theory which is designed to account for the general characteristics of language planning. That this is the position taken by some analysts can be shown in the inclusion of the words, “*theory of . . .*” in the titles of their papers. The third position is to say that what we have does not yet attain the rigor of a theory and that we need to engage in more descriptions before we can begin to talk of a theory (Haugen 1983:274; Cooper 1989:41). This position, as put by Cooper (1989:41), is as follows: “As for being based on theory, we have yet to move beyond descriptive frameworks for the study of language planning. We have as yet no generally accepted language planning theory, if by theory we mean a set of logically interrelated, empirically testable propositions”. If we equate Cooper’s

modest “framework” with a “model”, we can say that language planning is yet to go beyond models, and the more descriptions we have, the greater will be the possibility of refinement and improvement of existing models.

## Conclusion

Language planning activities continue in most countries of the world and our study of it continues to be enriched by the variety of policies and practices encountered. While, on the one hand, there are pressures to empower hitherto disadvantaged languages, there are also contradictory pressures toward globalization. For instance, in South Africa, where there were previously two official languages, Afrikaans and English, there are now eleven official languages, with the addition of nine African languages to the previous two. On the other hand, Malaysia, which at independence opted for Bahasa Malaysia, has now moved in the direction of active bilingualism with English becoming prominent, particularly in tertiary education. These changes in the linguistic ecology call for a refinement in the instrument for accounting for language planning practices. An emphasis on description of a wide range of practices is to be encouraged. These may include information on specific situations or matching of proposed models with actual practices to see how well they fit. (See, for example, Dua 1991; Jones 1997; Kam & Wong 2000 for language policy and planning in India, Brunei Darussalam, and East Asia respectively.)

Judging from the abstracts of the papers to be presented at the Section on Language Planning and Language Policies of this 17th International Congress of Linguists, a variety of language planning activities continues to be represented in the field: language spread, language maintenance and language shift, educational language policy, language policy-makers and power, language choice, globalization, language rights, language instruction, and terminology. It is becoming increasingly clear that language planning is not merely a reaction to communication difficulties or language problems. Language planning of the future needs to be all-embracing and sensitive to the underlying social, political, economic, scientific and educational planning. It should not merely show how policies are arrived at, it should also indicate what provisions are made for implementation, for without empowerment to implement them, language policies will not be worth more than the paper on which they are printed. Given the continued prevalence of the non-linguistic and linguistic situations that make language planning activities necessary, the study of language plan-

ning and language policies will continue to be an important preoccupation of sociolinguists.

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# Computational lexicons and corpora

## Complementary components in human language technology

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### 1. Introduction

Language resources<sup>1</sup> (LR) (both lexicons and corpora, but also grammars and basic and robust software components) are unanimously recognised as a necessary preliminary platform for developing an adequate human language technology (HLT). Large scale LR are therefore considered as the infrastructure underlying language technology (Zampolli 1998; Calzolari 1998; Calzolari & Zampolli 1999). In particular, every HLT system has to deal with ‘words’, at some level of linguistic description. It is vital to have the availability of computational lexicons which embody the types of word knowledge necessary for different systems, from the simpler requirements of spelling checkers to the complex semantic and world knowledge information needed by machine translation systems.

In this paper I touch on a few issues related to computational lexicons and textual corpora, highlighting how evidence shows that they are not only two closely interrelated linguistic objects, but should be considered as complementary views on the lexical space. They must be used together, as two methodologically complementary linguistic components, in HLT research and development.

A computational lexicon is a very complex – and expensive – component to be built adequately. It must contain, in an explicit and formalised way, all the information which a native speaker uses in everyday situations, from the simpler orthographic, phonetic, morphologic information, to the more complex syntactic, semantic, pragmatic, logical, ontological, multilingual in-

formation. A ‘complete’ lexicon should practically incorporate our ‘knowledge of the world’, and represent it in an explicit and formal way. It is already ambitious to formalise this large and manifold set of information, and to link the different properties to each other, but even when we succeed in designing adequate lexical models and creating adequate lexicons (both in size and coverage of the necessary properties), we are aware that in no way can we – cover all the potentialities of the human language in any ‘static’ lexicon.

It is an intrinsic property of human language to be not a fixed and forever stable object. Human language is a continually evolving system, adapting itself to the different needs of various types of situations, special domains, communication contexts. Therefore, it cannot be captured by a finite system of properties and rules, but is rather a *continuum*, where so-called exceptions often become a rule, and, vice versa, what traditional grammars give as regular phenomena may actually not be used in real contexts. We claim that it is theoretically not possible to achieve ‘completeness’ within any ‘static’ lexicon. The only way of reflecting and capturing all the potentialities of a language relies on trying to extract the linguistic and lexical information not only from ‘experts’, i.e. native speakers or linguists, but from the texts themselves in which the language is actually used, with a continuous process of enrichment. From these considerations the importance of corpora obviously emerges.

Which type of conclusion will be drawn? A sound language infrastructure must encompass both ‘static’ lexicons, such as the traditional ones, and ‘dynamic’ systems able to enrich the lexicon with information acquired on-line from large corpora, thus capturing the ‘actually realised’ potentialities, the large range of variation, and the flexibility inherent in the language as it is used.

In the following sections I briefly describe and discuss a few major European initiatives for building harmonised lexicons (Section 2), a few aspects of corpus based lexicography (Section 3), static lexicons vs. dynamic means for acquiring lexical information (Section 4), and finally conclude by looking to the future, stating the need of establishing a ‘virtuous circle’ between lexicons and corpora, and pointing at a possible change in the paradigm of LR (Section 5).

## 2. An infrastructure of language resources: EAGLES/ISLE, PAROLE, SIMPLE

### 2.1 EAGLES/ISLE: Interdependent standards for lexicon and corpus

Within the European LRE and LE EAGLES (*Expert Advisory Group for Language Engineering Standards*) projects (Calzolari, McNaught, & Zampolli 1996) for the development of standards both in Morphosyntax, Syntax, and Semantics, the awareness of the interdependence between lexical specifications and corpus tagsets / syntactic annotations has guided the formulation of the proposals for standards in both the Corpus and the Lexicon Working Groups (Leech & Wilson 1996; Monachini & Calzolari 1996, 1999).

The EAGLES recommendations constituted the common basis for the creation of corpora and lexicons within the PAROLE and SIMPLE EU projects (next section), both coordinated, as EAGLES, by A. Zampolli in Pisa. According to the subsidiarity concept, which is at the basis of many EU initiatives, the process started at EU level was then continued at national level. The PAROLE/SIMPLE lexicons and corpora are being enlarged to real-size lexicons in the framework of National Projects for a few European languages. These national initiatives show that the goal of the LR EC projects, aiming at providing a core set of resources to be extended with national support, is perfectly satisfied. The availability of these large, uniformly structured lexical resources in so many EU languages offers the users the benefits of a standardised base. This achievement is of major importance in a multilingual area like Europe, where all the difficulties connected with the task of LR building are multiplied by the language factor. This would have been absolutely impossible without the fundamental role played by the EC LR and standards projects. The ENABLER thematic network, which groups the National Projects in Europe dealing with LR, now has the task of continuing to ensure harmonisation among EU LR.

The transatlantic project ISLE (*International Standards for Language Engineering*) (Calzolari & Zampolli 2001) – a continuation of EAGLES – financed both by the EC and by NSF in USA, has recently focused on the development of recommendations for multilingual lexicons. The Computational Lexicon Working Group of ISLE has proposed a general schema for the encoding of multilingual lexical information, the MILE<sup>2</sup> (*Multilingual ISLE Lexical Entry*) (Calzolari et al. 2002; Calzolari et al. 2003). This is to be intended as a meta-entry, acting as a common representational layer for multilingual lexical resources. Obviously MILE also includes previous EAGLES recommendations.

In its general design, MILE is envisaged as highly *modular* and *layered*. Main related concepts are the following:

- The *MILE Lexical Data Categories* provide the lexical objects (such as syntactic and semantic features, semantic relations, syntactic constructions, predicates and arguments etc.) that are the basic components of MILE-conformant lexical entries. Lexical Data Categories are organised in a hierarchy and defined using RDF schema to formalise their properties and make their ‘semantics’ explicit.
- The *MILE Shared Lexical Objects* instantiate the MILE Lexical Data Categories, to be used to build lexical entries in an easy and straightforward way. These will include main syntactic constructions, basic operations and conditions to establish multilingual links, macro-semantic objects, such as lexical conceptual templates acting as general constraints for the encoding of semantic units.
- The *MILE Entry Skeleton* defines in an Entity Relationship model the general constraints for the construction of ‘ideal’ multilingual entries, as well as the grammar to build the whole array of lexical elements needed for a given lexical description.

In defining the MILE we have also taken into account data categories needed to represent information acquired from corpora. One of the first objectives of the CLWG was to discover and list the (maximal) set of (granular) *basic notions* needed to describe the multilingual level. The principle guiding the elicitation and proposal of MILE basic notions in the recommendation phase has been, according to a previous EAGLES methodology, the so-called ‘edited union’ of what exists in major lexicons/models/dictionaries, at least as a starting point, enriched with those types of information which are usually not handled, e.g. those of collocational/syntagmatic nature, and obviously those pertinent to the multilingual layer. This method of work has proven useful in the process of reaching consensual *de facto* standards in a bottom-up approach.

One of the main aims of the CLWG activities, was to create a common parlance among the various actors (both of the scientific and of the industrial R&D community) not only in the field of computational lexical semantics and multilingual lexicons, but also in the areas e.g. of ontologies and the emerging semantic web, so that synergies will be enhanced, commonalties strengthened, and resources and findings usefully shared. ISLE intends to foster the vision of open and distributed lexicons, with elements possibly residing in different sites of the web. The MILE basic notions are formalised in the MILE Lexical Model (MLM), together with a concrete instantiation in RDF. The defined lex-

ical objects, collected in shared lexical repositories, will be used by the lexicon (or applications) developers to build and target lexical data at a higher level of abstraction. This is a step in the direction of simplifying and improving the usability of the MILE recommendations, and towards a new paradigm of language resources, sharable through an open and distributed architecture (for the semantic web).

## 2.2 PAROLE: Corpora and morphological and syntactic lexicons

The central goal of PAROLE (Zampolli 1997) was to produce in Europe an initial core of harmonised corpora and lexicons. For each of the following languages a corpus of at least 20 million words and a lexicon of 20,000 entries was produced: Catalan, Danish, Dutch, English, Finnish, French, German, Greek, Italian, Portuguese, Spanish (lexicon only), Swedish. In addition, a corpus of 20, 15, 3 million words was produced for Belgian-French, Irish, Norwegian respectively. The PAROLE Lexicon model for Morphosyntax and Syntax is based on the results of EAGLES (Sanfilippo et al. 1996) and GENELEX (Genelex Consortium 1994), further developed within the project (Calzolari, Montemagni, & Pirrelli 1996). All the lexical resources are declarative, and the model – with a high level of precision in the description – is designed to ensure that application-dependent models of data and applicative dictionaries can be derived from this repository of information, by mapping the application model from the generic one.

The PAROLE lexical resources (Ruimy et al. 1998) encode the following morphological and syntactic information, divided into optional and mandatory classes for entries:

- Morphology:
  - written forms (graphical morphological unit) including stems and variants
  - morphosyntactic category (part of speech) and as appropriate a subcategory
  - inflected forms
  - morphological features
  - derivation
  - abridged forms
- Syntax:
  - subcategorisation patterns (with optionality)

- grammatical relations of subcategorised complements
- control
- diathesis and lexical alternations
- pronominalisation
- linear order constraints
- constraints on the syntactic context where the lexical entry is inserted
- syntactic compounds (idioms, etc.)

### 2.3 SIMPLE: harmonised semantic lexicons for the European languages

Semantics is today – and will be in the coming years – the critical issue in HLT. Every application having to manage with information, in the ever-growing importance of the so-called ‘content industry’, calls for systems which go beyond the syntactic level to understand the ‘meaning’. Many theoretical approaches are tackling different aspects of semantics, but in general they have to be tested (i) within really large-size implementations, and (ii) with respect to their actual usefulness and usability in real-world systems both of mono- and multi-lingual nature. SIMPLE (Lenci et al. 1999) has directly addressed point (i) above, while providing the necessary platform to allow future projects to address point (ii).

Even more when we consider the multilingual aspect, with its problems and challenges – which is today one of the foci of attention in HLT programs in Europe and world-wide – again semantics is at stake. We cannot hope to successfully address the multilingual aspect without some solution to the semantic aspect (unless we use only statistical techniques). It may be easier to add a multilingual layer to a ‘harmonised’ set of semantic lexicons, addressing in a uniform way the core of what is needed for natural language processing (NLP), i.e. ‘semantic typing’ of heads and arguments, which is at the centre of the SIMPLE project.

#### 2.3.1 *The model: Basic issues*

SIMPLE, a follow-up to PAROLE, has added a semantic layer to a subset of the existing morphological and syntactic entries. The semantic lexicons (covering about 10,000 word meanings) are built in a harmonised way for all the 12 languages covered by PAROLE. The SIMPLE project (Bel et al. 2000) represents – to our knowledge – the first attempt to tackle harmonised encoding of semantic types and semantic (subcategorisation) frames on a large scale, i.e. for so many languages and with wide coverage. The SIMPLE model provides the formal specifications for the representation and encoding of the following information:

- i. *semantic type*, corresponding to the template that each Semantic Unit (*SemU*) instantiates;
- ii. *domain* information;
- iii. *lexicographic gloss*;
- iv. *argument structure* for predicative SemUs;
- v. *selectional restrictions/preferences* on the arguments;
- vi. *event type*, to characterise the aspectual properties of verbal predicates;
- vii. *links* of the arguments to the *syntactic subcategorisation frames*, as represented in the PAROLE lexicons;
- viii. '*qualia*' structure, following the Generative Lexicon, represented by a very large and granular set of semantic relations and features;
- ix. information about *regular polysemous alternation* in which a word-sense may enter;
- x. information concerning *cross-part of speech relations* (e.g. *intelligent* – *intelligence*; *writer* – *to write*);
- xi. *semantic relations*, such as hyponymy, synonymy, etc.

There are three main types of formal entities:

- *Semantic Unit* – word-senses are encoded as *Semantic Units* (*SemU*) and assigned a *semantic type* from the Ontology, plus other sorts of information specified in the associated *template*, which contribute to the characterisation of the word-sense.
- *Semantic Type* – each type involves structured information represented as a *template*. The semantic types themselves are organised into the *Ontology*, which allows for the *orthogonal organisation of types*.
- *Template* – a schematic structure which the lexicographer uses to encode information about a given lexical item. The template expresses the semantic type, plus other sorts of information characterising multiple dimensions of a word-sense.

The 'conceptual core' of the lexicons consists of the basic structured set of 'semantic types' (the SIMPLE *ontology*) and the basic set of notions to be encoded for each sense. A set of top common templates (about 150) has been defined, while the individual lexicons can add more language-specific templates as needed. The relevance of this approach for building consistent resources is that types both provide the formal specifications and guide subsequent encoding, thus satisfying theoretical and practical methodological requirements. The SIMPLE model allows to consistently generate concepts out of a set of ontological categories that are grounded in linguistic behaviour. The model has

a high degree of generality in that it provides the same mechanisms for generating broad-coverage and coherent concepts independently of their grammatical/semantic category (entities, events, qualities, etc.), an aspect which is often lacking in existing lexicons, where the focus is often on the representation of the clear, well-known cases while the semantics of more complex cases is neglected.

### 2.3.2 *Some theoretical underpinnings*

SIMPLE has tackled issues that are at the core of lexical semantics research, and provided a framework for testing and evaluating the maturity of the current state-of-the-art in the realm of lexical semantics grounded on, and connected to, a syntactic foundation. A coherent development of semantic lexical resources must be guided by an underlying theoretical framework for structuring word meaning and generating concepts which satisfies both ontological considerations as well as the need to capture linguistic generalisations (see Busa et al. 2001). The SIMPLE model is a concrete major step towards this objective. It is based on EAGLES Lexicon/Semantics recommendations (Sanfilippo et al. 1999) and on extensions of Generative Lexicon (GL) theory (Pustejovsky 1998). An essential characteristic – which makes it basically different from EuroWordNet (Vossen 1999) (where the main structuring semantic relations are synonymy and hyponymy) – is its ability to capture the various dimensions of word meaning which are equally important in language. The basic vocabulary relies on an extension of the ‘qualia structure’ for structuring the semantic/conceptual types, which is understood as a representational tool for expressing the componential multi-dimensional aspect of word meaning (Pustejovsky 1991, 1995; Calzolari 1991a, b). The perspective adopted is that all words have internal structure, based on different semantic types, and differ in terms of complexity, which affects the way they are composed in a sentence. The so-called ‘extended qualia structure’ of SIMPLE addresses the concern of capturing more or less subtle linguistic differences while maintaining a systematic and consistent structuring of the lexical representations. This is achieved by specifying, for each qualia role, its extended *qualia set*, namely subtypes of that role which are consistent with its interpretation. To the standard approach of defining semantic classes along one dimension – which fails to capture underlying generalisations along different dimensions –, we have thus opposed a framework whose development has been crucially concerned with capturing the multidimensionality of meaning. Assuming that lexical items differ according to which dimension of meaning carries most of the semantic weight, the GL-SIMPLE model also clarifies the nature of the underspecification of certain

items, which may be highly underspecified along one dimension while providing a rich semantic contribution along other dimensions. An example is given by words like *goal*, *target*, *aim*, which are characterised by the ‘telic’ dimension, but are underspecified from the perspective of the ISA dimension.

### 2.3.3 *Cross-lingual uniformity and text corpora*

The SIMPLE lexicons, even though monolingual, are designed with a view to their future cross-lingual linking. All the lexicons do in fact share and are built around the same core ontology and the same set of semantic templates. The multilingual component translates into the requirement of identifying elements of the semantic vocabulary for structuring word meaning which are at the same time independent of any individual language but are able to capture linguistically useful generalisations for different NLP tasks.

The semantic lexicons are partially corpus-based, exploiting the harmonised and representative corpora built within PAROLE. The main criterion for the selection of the senses to be encoded in SIMPLE was the frequency of occurrence in PAROLE corpora. The fact that these corpora share a common design with respect to text types and *genres* for all the languages ensures some uniformity in vocabulary (sense) selection.

## 3. Corpus-based lexicography

### 3.1 Lexicon ↔ corpus interactions

When we look attentively at the various ways in which lexicon and corpus are related to each other, we cannot avoid highlighting the complexity of their mutual interactions. Depending on different perspectives, the relation goes in one or other direction. In any case, we cannot safely separate these two linguistic objects from one another as if they were independent entities. We can summarise, without claiming to be exhaustive, the lexicon (L) ↔ corpus (C) interactions in the following list, where an arrow from L to C means, in general, the projection/mapping of some lexical information on the corpus, while an arrow from C to L means acquisition of lexical information from corpora.

- L → C part-of-speech tagging/lemmatisation
- C → L frequencies (of different linguistic ‘objects’)
- C → L proper nouns/named entity recognition
- L → C syntactic parsing
- C → L updating/tuning a lexicon

- C → L 'collocational' data (MWE, idioms, grammatical patterns, ...)
- C → L semantic clustering and 'nuances' of meanings
- L → C semantic markup
- C → L lexical (syntactic/semantic) knowledge acquisition
- L → C word-sense disambiguation
- C → L validation of lexical models
- C → L corpus based computational lexicography

### 3.2 Corpus evidence vs. introspection as a basis for lexicographic description: Actual vs. potential

One of the most interesting – and intriguing – aspects of using a corpus for a lexicographic task is that one is immediately confronted with the impossibility, based on textual evidence, of using any type of description which is based on a clear-cut boundary between what is permitted and what is not. It is evident that, in the actual usage of a language, a large number of properties are displayed which behave as a *continuum*, and not as properties of the 'yes/no' type. In fact, this is one of the main characteristics encountered in actual language usage. The same holds true for the so-called 'rules': we find in corpus evidence more of 'tendencies' towards rules rather than precise rules. A conclusion which must be drawn from these observations is that almost all information types must not be treated as absolute constraints, the violation of which makes a sentence totally unacceptable, but as preferences that make a given sentence more or less acceptable in a given context, without affecting its grammaticality. This poses a problem at the level of representation.

Moreover, the evidence of actual usage is often in contrast with what one would expect if judgement were based solely on introspection. A clear example of this is given by the behaviour of two Italian 'Speech Act' verbs, *chiedere* and *domandare*, both possible translations of the English verb *to ask*. They therefore present a problem of lexical selection at the translation level. These verbs display strong similarities. They are intuitively judged by native speakers to be synonymous and to display quite similar behaviour, also in their polysemy. They are also described in a similar way in traditional dictionaries (where one is frequently used to explain and define the other) and grammars. However, surprisingly, they behave quite differently in the corpus, producing some unexpected results. Whereas, theoretically, both of them permit exactly the same type of surface syntactic patterns and related semantic nuances of meaning, they actually display a completely different usage with respect to a number of

semantic/syntactic patterns. While both *domandare* and *chiedere* can convey both the ‘interrogative (ask to know)’ and ‘imperative (ask to have)’ meaning, which seem both quite natural to the speaker, the corpus analysis reveals that – when used with a clausal complement – *domandare* is almost always used in the interrogative sense, and *chiedere* very often in the imperative one. When the imperative message is expressed by a *che*-clause (*that*-clause), the verb used is almost always *chiedere* throughout the entire corpus (it appears 165 times), although again the same construction with *domandare* cannot be considered as ungrammatical from a theoretical point of view (in fact it is found 3 times).

An even more striking difference between the two verbs is evidenced by the analysis of a homogeneous portion of a corpus, constituted by about 900,000 occurrences from “*Il Sole 24 Ore*” (an Italian financial newspaper). The first fact to be noticed is that *chiedere* is used much more than *domandare* (342 occurrences of *chiedere* vs. 35 only of *domandare*), with a much larger disproportion than in the entire general corpus (where *domandare* is about one/half than *chiedere*). Moreover, the difference in meaning is even clearer. If we consider the two verbs followed by clausal complements, we find that: (i) *chiedere* is used much more in the “request” meaning (82 occurrences, 62 of which in the construction *di* + infinitive, and 20 with *che*-clause) than in the interrogative sense (18 occurrences of *wh*-clause, 12 of which with *se*, Italian ‘whether’); (ii) *domandare* is only used in the interrogative sense (a good 16 occurrences, 7 of which introduced by *se*). This is a striking example, but many other cases of evidence unlikely to have been discovered by introspection become apparent.

Computational lexicons, like printed dictionaries, often represent a sort of stereotypical/theoretical language. However, a (computational or traditional) lexicon has to faithfully represent ‘irregular’ facts and divergences of usage from what is potentially and theoretically acceptable. We should not decide what to encode in the lexicon relying on introspection only and on native speakers’ (even if lexicographers’) intuition, since this leads to a description of a ‘theoretical language’ instead of the language as it is used; we must provide, in the lexicon, some representation of (and distinction between) what is allowed but only very rarely instantiated, and what is both allowed and actually used. It follows that carefully constructed large corpora are essential sources of linguistic knowledge. Corpora help us in particular in those areas where more delicate categories than subcategorisation and broad semantic classes are necessary.

### 3.3 Intensional vs. extensional description: Innovative lexicon types

With respect to this broad issue, a number of dichotomies must be considered not as opposite views, but as complementary perspectives:

- rules vs. tendencies
- absolute constraints vs. preferences
- discreteness vs. continuum/gradience
- theoretical/potential vs. actual
- intuition/introspection vs. empirical evidence
- theory-driven vs. data-driven
- paradigmatic vs. syntagmatic
- symbolic vs. statistical.

We claim that the second element of the above dichotomies has to be highlighted, in order then to combine the two. To this end, robust and flexible tools are needed for (semi-)automatic induction of linguistic knowledge from texts. Because of the mixture in the lexicon – as indeed in language in general – of (i) core phenomena which can be encapsulated in general rules (or tendencies), and (ii) peripheral but pervasive phenomena which are flexible, variable and with loose boundaries, we have to handle these two types in different but integrated ways. This is particularly true when we enter the realm of semantics. Regular patterns of usage can be described intensionally by e.g. classifying them as members of lexical types. More elusive patterns can at least be partially extensionally recorded e.g. as collocational patterns. This can be achieved for instance by listing (possibly with their frequencies) those words which fulfil a particular role in relation to the entry we are describing, but cannot be classified through a general semantic type or relation or as a normal metaphorical extension of a semantic type. We must be aware that it is just this type of data, i.e. collocational patterns of usage, which are often the real clue for proper selection of the correct translation when we move to bilingual or multilingual lexicons.

Extensional descriptions will amount to creating virtual links between lexicons and examples (corpus/web samples, image samples, clips and videos, etc.), pushing towards new and *innovative types of lexicons*: a sort of ‘*example based living lexicons*’ that participate of properties of both lexicons and corpora. In such a lexicon *redundancy* is not a problem, but is even a benefit.

#### 4. “Static” lexicons vs. “dynamic” acquisition of lexical information

The lexical resources built in the projects mentioned above (in Section 2) do provide the essential basic infrastructure, but – as we said – they do not have enough coverage, for structural and inherent reasons. No ‘static’ resource can ever be adequate and satisfying, from more than one perspective: (i) in extension: it cannot obviously cover new formations, or all the possible domains, and (ii) in depth: not even for the existing lexical entries it can provide all the necessary and useful linguistic information (e.g. not necessarily all the sub-categorisation types actually occurring in a specific domain are covered by a general lexicon). For them to become really usable, it is essential that these generic, core LR are built in such a way that (i) they are open to different types of enrichments and customisations, possibly to be done in an automatic way, and (ii) the information is granular enough so that different applications can extract what they need in the format they need.

The common generic platform of LR needs therefore to be enhanced and fine-tuned in various ways – according to the domain, the task, the system (information retrieval, machine translation, . . .), etc. – to become actually usable within specific applications. This makes it vital, for any sound lexicon development strategy, to accompany core static lexicons with dynamic means for enriching and integrating them – possibly on the fly – with the types of information which are known to be structurally and intrinsically missing from available LR. This global view eliminates another apparent dichotomy, i.e. the one between static vs. dynamically built (or incremental) resources, encompassing the two approaches in a more comprehensive perspective that sees the two as complementary and equally necessary facets of the same problem.

Everything said above points to the need of working towards *semi-automatic construction of a new generation of computational lexicons directly from corpora*, otherwise coverage and/or accuracy will remain inadequate. The increasing availability and reliability of robust techniques (for chunking, shallow parsing, functional analysis, named entity recognition, etc.), and the ability to integrate them, makes the exploitation of text corpora of greater relevance in many HLT tasks, and allows the acquisition of lexical information which complements that available in static lexicons (see e.g. Bartolini et al. 2002). Automatic acquisition usually implies a bootstrapping methodology, because extraction presupposes some capability of automatically analysing the raw text in various ways, which first requires a (partial) lexicon. A cyclical methodology is of help in getting out of this loop. The induction phase must, however, be followed by a linguistic analysis and classification phase, if the induced data is to



tion formally represented in the lexicon is usually projected on the corpus and reflected by the annotation/analysis performed on it. At this stage, one consequence is that we have an ‘enriched’ corpus, from where – through usage of other tool types – we can acquire either more refined information at the same level of linguistic analysis or, more interestingly, information pertinent at another, ‘superior’, level of analysis. These information types can then be fed back into the lexicon which, still partial, nevertheless becomes richer and therefore able, in a successive implementation of this virtual circle, to achieve a deeper analysis of the corpus.

Obviously what is said here, in a concise and rather simplistic way, needs a very complex set of knowledge, tools, techniques, etc., in order to be fully implemented. It is only currently that these resources are becoming robust enough to allow a number of cycles to be implemented with the hope of success. In particular, the capability of ‘integrating’ what until now was used in isolation is essential: by using a good integration process the value of the individual components can be multiplied.

It is worth making a final observation: the implementation of such a cycle needs a strong compatibility both (i) between the lexical representation and the corpus annotation, and (ii) at the system/tools interface level (for input/output). From this consideration a clear need for continuing the standardisation efforts in HLT emerges.

A final note of concern is due with respect to the interaction between availability of adequate LR on one hand and of tools/systems able to use them on the other. If we had real-size lexicons with very fine-grained semantic/conceptual information, would there be systems (non ad-hoc toy systems) able to use them? It seems that there is still a loop between (i) the lack of suitable, large-size and knowledge-intensive resources (lexicons and corpora, with many types of syntactic and semantic information encoded), and (ii) the ability of systems to use them effectively: the two directions of work (resources and tools) should be pursued in parallel, should closely interact with each other, and be gradually integrated. Another virtual circle is needed here.

## 5.2 MILE and the Semantic Web as corpus

If we broaden our perspective into the future, it is clear that the need of ever growing LR makes it necessary to make a change in the paradigm. Coming from the experience gathered in developing advanced lexicon models such as the SIMPLE one, and along the lines pursued by the ISLE standardisation process, a *new generation of lexical resources* can be envisaged. These will cru-

cially provide the semantic information necessary to allow for effective content processing. On the other hand, they will in turn benefit from the Semantic Web itself. Thus, it is possible to state the existence of a bi-directional relation between the Semantic Web enterprise and computational lexicon design and construction. In fact, the Semantic Web is going to crucially determine the shape of the language resources of the future. Semantic Web emerging standards, such as ontologies, RDF, etc., allow for a new approach to language resource development and maintenance, which is consistent with the vision of an open space of sharable knowledge available on the Web for processing.

Looking to the future, a further step and radical change of perspective is now needed in order to facilitate the integration of the linguistic information resulting from all these initiatives, to bridge the differences between various perspectives on language structure and linguistic content, to put an infrastructure into place for content description and content interoperability at a European level and beyond, and to make lexical resources usable within the emerging Semantic Web scenario. This objective can only be achieved if we work towards an integrated *Open and Distributed Lexical Infrastructure*, based on open content interoperability standards, where not only linguistic experts can participate, but which includes designers, developers and users of content encoding practices, and also many members of society.

Semantic content processing lies at the heart of the Semantic Web enterprise, and requires to squarely address the complexity of natural language. Existing experience in language resource development proves that such a challenge can be tackled only by pursuing a truly interdisciplinary approach, and by establishing a highly advanced environment for the representation and acquisition of lexical information, open to the reuse and interchange of lexical data.

With MILE we put the basis for the realisation of a common platform for *interoperability between different fields of linguistic activity* – such as lexicology, lexicography, terminology – *and Semantic Web development*. The reuse of existing lexicons is achieved through the design of MILE, a lexical schema which (i) takes into consideration the basic notions employed in major available lexicons, (ii) is flexible enough to allow mapping from various lexical models into it, and (iii) allows the creation of user-defined lexical objects if needed. The platform will provide a flexible common environment not only for linguists, terminologists and ontologists, but also for content providers and content management software vendors, for development and communication. This will enable users to share lexicons and collaborate on parts of it. The lexicons may be distributed, i.e. different building blocks may reside at different

locations on the web and are linked by URLs. This is strictly related to the Semantic Web standards (e.g. RDF metadata to describe lexicon data categories). Overall, lexicons will perform the bridging function between documents and conceptual categorisation. The common conceptual model within the envisaged architecture will ensure content interoperability between texts, lexicons and ontologies.

## Notes

1. The term ‘language resources’, and also the recognition of their infrastructural role in HLT, was introduced by Zampolli (1991), to whom all the LR sector owes so much for his visionary ideas realised in so many initiatives with never ending efforts.
2. See also [http://www.ilc.cnr.it/EAGLES96/isle/clwg\\_doc.html](http://www.ilc.cnr.it/EAGLES96/isle/clwg_doc.html)

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# Historical linguistics

## The state of the art

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### 1. Introduction

The purpose of this paper is to survey major recent developments in historical and comparative linguistics. From colleagues on the HISTLING list, I discovered opinions concerning these developments differ widely and sometimes clashed. There was agreement on major areas of concentrated activity, but not on what constitutes significant contributions. Some work, in spite of the attention it receives, fails to be significant. I will attempt to distinguish real contributions from mere areas of high activity.

### 2. Historical linguistics' "health"

I begin with perceptions of the current state of historical linguistics. On the one hand, colleagues reported a downturn in historical linguistics in some quarters, e.g. major linguistic conferences with little historical linguistic representation, universities which offer no courses in historical linguistics, and linguistic institutes with scarce historical offerings. On the other hand, colleagues also report increased interest among the public and students in aspects of historical linguistics, citing as reasons for this media attention to long-range relationships, evolution of language, and advances in decipherment of ancient writing systems. Historical linguistics has been vigorously represented in other conferences – some dedicated to specific historical linguistic topics, e.g. language contact, grammaticalization, syntactic change, language evolution, etc. Numerous books dealing with historical linguistic themes have been published,

including a number of new textbooks. Four of the six winners of the Linguistic Society of America's Leonard Bloomfield Award, for the best book in linguistics of the previous two years, have been on historical linguistic topics, and all have some historical linguistic content. Historical linguistic scholarship, in spite of ups and downs, is flourishing and influential.

### 3. Time-honored research

Progress has been made in working out the history of individual language families and of particular languages. There have been advances in reconstruction, subgrouping, the phonological history, and other aspects of major language families, in Austronesian, Semitic, Uralic, and a number of Native American language families in particular. Understanding has advanced in the classification and history of languages of the least understood areas of the world – Papua New Guinea, South America, and Australia – as well as elsewhere. Naturally much remains to be done. Progress has also been made in the classification of African and Southeast Asian languages, though new questions have also arisen (cf. Heine & Nurse 2000). Activity has continued in Indo-European and its branches (including much in Germanic and on the history of English). Corpus linguistic research, especially in the history of English, has taken on major importance.

### 4. Language contact

All agree that language contact is an extremely active area of research. Some trace this to Weinreich (1953), others to Emeneau (1956); many attribute the upsurge in activity to Thomason and Kaufman (1988), and a few to Dixon (1997). (See Thomason 2001 for an overview.) Borrowing and language contact have always been important in historical linguistics. Still, language contact has not always received sufficient attention, making the increased recent focus welcome. For example, areal linguistics has been neglected in sub-Saharan African linguistics, and in various proposals of distant genetic relationship, in particular in “Altaic” and “Nostratic”. It is standard in American Indian and Australian linguistics, but typically received less attention in Europe, though it should not be forgotten that the best studied linguistic areas (the Balkans, Baltic, and South Asia [India]) involve Indo-European languages.

There have also been excesses in recent work on language contact. (See comments on Dixon's and Nichol's approaches, below.) The notion 'linguistic area' (*Sprachbund*) is generally acknowledged to be difficult to define. In Campbell (2003), I argue that there is no significant difference between areal linguistics and borrowing, that it is individual historical events of diffusion that matter, not the post-hoc attempts to impose geographical order on varied conglomerations of these borrowings. With focus on the diffused traits themselves and not on attempts to define the linguistic areas, the difficulties for defining "linguistic area" and specific areas cease to be problems. The task should be to answer the question, "what happened?" If we succeed in determining what changes have taken place, and how, when, and why they took place, we will have succeeded in providing all the information underlying traditional notions of linguistic areas. The geographical patterning to instances of diffusion thought characteristic of linguistic areas will be a natural consequence of this fuller historical account, read directly off the accumulation of history changes in the languages involved.

## 5. Kinds of change and their explanation

### 5.1 Sound change

The questions of how and why sounds change continue to receive much attention, but controversies continue. While most hold that sound change is still basically regular, some raise doubts (cf. Durie & Ross 1996), debating whether sounds change mechanically in Neogrammarian fashion, or by lexical diffusion, or through weak network ties (J. Milroy 1992), or for other reasons. Labov (1994, 2001) has contributed much in his study of the internal and social factors involved in sound change. His principles of vowel shifting and mergers, however, bear refinement (see Gordon et al. in press).

Some argue that push chains are illegitimate because they appeal to teleology: cases where one segment moves towards the phonetic space of another segment, and this segment which is encroached upon moves away "in order to" avoid merger and thus to prevent the loss of a phonemic contrast which distinguishes numerous words from one another. However, the existence of push chains is an empirical matter, and cannot be resolved by philosophical decree. For example, in the chain shift involving the short front vowels in New Zealand English documentation shows that what took place was indeed a push chain and not a pull (drag) chain. As demonstrated from real-time evidence, the

vowel shift sequence in New Zealand was: (1) raised /æ/ (of the TRAP lexical class) impinged upon /ɛ/ (of the DRESS lexical set), (2) which as a consequence began to rise; this raised /ɛ/ crowded /ɪ/ (of the KIT class of vowels), (3) which in reaction began to centralize ([ɨ] or [ə]). What changed, when it changed, and in what sequence the changes took place are documented in the recordings – /ɪ/ centralization was last and late, much after /æ/ raising, so that it is impossible to maintain that /ɪ/ centralization left a gap in the inventory into which /ɛ/ was pulled, and so on in pull-chain fashion (see Gordon et al. in press for details).

Labov's vowel shift principles need clarification. His early formulation was: "in chain shifts, short vowels fall" (see Labov 1994:116, cf. Labov 1994:31, "short nuclei fall"). Clearly, however, New Zealand English, as just seen, is an exception, where the short front vowels (/ɪ/, /ɛ/, /æ/) have risen, not fallen. Labov's (1994, 2001) revisions rely on "peripherality." He proposes that in chain shifts, peripheral vowels are raised and non-peripheral vowels are lowered (Labov 1994:601). This gives the impression that there is a precise physical definition of peripherality which determines directions of change in vowel shifts. However, it is not a precise concept: "peripherality is not an absolute location in phonological space but . . . a relationship determined by the elements of the system as a whole" (Labov 1994:285). Peripherality has no inherent phonetic content. A problem with determining peripherality is that its definition requires comparisons with neighboring vowels, but it is often not clear what comparisons should be made in order to determine whether a given vowel is [+peripheral] or [–peripheral]. The New Zealand short front vowel shift is an exception to the vowel shift principles. Appeal to peripherality to save the New Zealand vowel shift from being an exception becomes a gimmick. It is not clear why these New Zealand vowels are "clearly short, tense [+peripheral]" (Labov 1994:212) in New Zealand (particularly since the same vowels are short and "lax" in other English varieties), other than that they rise unexpectedly, rather than fall, as predicted by the principles. The vowel shift principles do not account for outcomes where the /ɪ/ vowel can centralize in New Zealand English (towards [ɨ]), but rise in Australian (to [i]), and where the originally somewhat raised /æ/ and /ɛ/ vowels can continue to rise in New Zealand, but went in the opposite direction in the South of England, that is, reversed their trajectory and lowered (see Gordon et al. in press).

Mergers have also claimed recent attention. Claims range from "mergers are preferred" to "mergers tend to be avoided" (cf. M. Gordon 2002:244). Statements such as the following, if accurate, would dispose us to believe that avoidance of merger (as in vowel shifts) should not exist or at least should be very rare: "The relative progress of sound change is determined by pho-

netic factors alone, without regard to the preservation of meaning” (Labov 1994:603). Some assert that “mergers are, in general, much more common than chain shifts” (M. Gordon 2002:253). Nevertheless, vowel shifts in which mergers are avoided are reasonably common, and it is not possible to argue generally for the dominance of merger over shift. In the end Labov (1994:270) concedes that “the study of chain shifting does not provide many examples of unexpected mergers; examples of unexpected avoidance are easier to locate.” He says, “there is no doubt that in some way or other, linguistic systems respond to change in ways that maintain meaning – more or less” (Labov 1994:569).

It has been claimed that in contact situations mergers are preferred over distinctions (cf. Hickey 2002; Trudgill 1986:105). This connects with what Labov (1994:35) calls “Herzog’s Principle,” the claim that “mergers expand at the expense of distinctions” in dialect geography (see Labov 1994:602, 2001:498). From the point of view of child language acquisition in social settings, there is no compelling reason why mergers should necessarily be preferred or should expand their geographical territory. That is, if there is a variety in the dialect contact situation which has not undergone a merger but which has the edge over the other varieties in terms of numbers of speakers or social advantage, then it is predictable that children acquiring their language in that setting will favor acquisition of the non-merged variety.

However, Labov brings up two reasons for why in dialect contact mergers might win out. He claims that “mergers rarely rise to the level of overt social consciousness” (Labov 1994:324), that “mergers are almost invisible to social evaluation, and it is difficult to think of them as diffusing under the social pressures of social imitation and association” (Labov 2001:27). However, even if mergers were invisible to social evaluation, children in a dialect contact situation might nevertheless give preference to the variety without merger if the number of speakers without merger is dominant. There are a number of clear examples of variables involving a merged and non-merged variant in which social factors have influenced the trajectory of change to favor the non-merged variant (to cite just one, the stigmatized /v/–/w/ merger in areas of southern England reversed so the contrast is again firmly established, due to the status of non-merger varieties). We cannot assume that mergers have an automatic advantage in dialect contact situations.

Labov’s second reason for believing that mergers win in dialect contact concerns strategies for cross-dialect communication. Following Herold (1990), Labov holds that to understand, speakers ignore the merged phonetics and look elsewhere for clues: “[non-merger speakers] learn that there are speakers who

do not make the distinction, and they cease to misunderstand those speakers by adjusting their own system accordingly” (Labov 1994:324). He claims that speakers of the non-merger variety “may continue for some time to produce the distinction [but] without using it for semantic interpretation” (Labov 1994:324). In the end, according to Herold’s and Labov’s claim, neither those who make the phonemic contrast nor those who do not make it attempt to use the contrast to understand, but rely on semantic, pragmatic, and non-phonetic clues to interpret cases of homonymy. This, then, is seen as justification for believing that mergers are preferred in situations of dialect contact. However, to what extent does this strategy for understanding interlocutors from a variety with merger hold for settings other than Herold’s situation in Eastern Pennsylvania? What numbers of speakers of the respective dialects might be required? To what extent do social factors condition what takes place? As Labov (1994:325–327) shows, in many situations people without a merger nevertheless continue to misunderstand those with a merger. Is it not just as possible that the children of parents who merge could grow up in a community where the non-merged variety has numerical or social advantages and as a consequence they acquire a non-merging variety of their own, even if their parents’ generation may decide to employ a strategy of no longer listening for phonetic contrasts in the speech of those who have merger? If the people after whom children and adolescents model their language do not make the contrast, the merger could win out; however, if in the broader context the variety with merger does not have dominance in numbers or social advantage to bolster it, this particularly local strategy for interdialectal comprehension may have little lasting effect.

An on-going debate about sound change relates to whether sound change spreads regularly (mechanistically changing a sound in all words which happen to contain it in the relevant phonetic environment, simultaneously), or whether lexical diffusion occurs (with a change spreading across the lexicon, reaching some words before others, and some perhaps not at all). Labov (1994) attempted to reconcile the differences, concluding there is evidence for both, but in different contexts. He argues that regular sound change is characteristic of the initial stages of a change, without lexical or grammatical conditioning or any degree of social awareness (change from below); he argues that lexical diffusion is most characteristic of late stages of a change that has lexical and grammatical conditioning or a high degree of social awareness or of borrowing from other systems (change from above) (Labov 1994:542, cf. also Campbell 1998a).

Recent work indicates that high frequency words tend to undergo greater reduction in speech (cf. Bybee 2001; Jurafsky et al. 2001), and so it is argued that they lead leniting changes and this is lexical diffusion of sound change. However, the connection between frequency (with reduced pronunciation) and sound change is not clear. For example, one of the well-documented frequency effects relates to final *t-d* deletion in varieties of spoken English (Bybee 2001; Jurafsky et al. 2001). Word-final /t/ and /d/ are more often deleted in high-frequency words than low frequency words. This has been taken to be evidence in the involvement of frequency in sound change, with words of high frequency changing faster than words of low frequency. However, just because more frequent words show greater rates of reduction does not mean that sound change is progressing through the lexicon. Such effects can exist for stable variables (not apparently undergoing change) as well. More research is needed here.

A related claim in need of more investigation is that the semantic domain of some words can significantly affect the likelihood of whether they will undergo a particular sound change or not. For example, Gordon et al. (in press) found that in New Zealand English farming and mining words in the speech of some individuals were much more resistant to /r/-loss than words of other semantic domains (for other examples of semantic areas resistant to on-going changes, see Yaeger-Dror 1996).

## 5.2 Semantic change

Understanding of major pathways of semantic change has increased, in particular of the role of invited inferences (and pragmatics generally) in change in meaning (see Traugott & Dasher 2002).

## 5.3 Morphosyntactic change

Syntactic change has received considerable attention. Harris and Campbell (1995) argue that there are only three principal mechanisms of syntactic change, reanalysis, extension, and borrowing. Approaches based on autonomous syntax have been found unable to explain how and why syntactic changes take place. A continuing controversy concerns whether syntax can be reconstructed by the comparative method; in spite of some continued expressions of doubt (e.g. Lightfoot 2002), there are now a number of persuasive demonstrations that aspects of syntax can be reconstructed (e.g. Campbell

1990; Harris 1985; Harris & Campbell 1995: 344–376; Janhunen 1982; Kroeber 1999, etc.)

### 5.3.1 *Grammaticalization*

Grammaticalization is a “hot” topic. While this work has amassed valuable examples, contributed significantly to the histories of individual languages, and stimulated much thinking concerning linguistic change, it is also controversial. The papers in Campbell (2001a) reveal that grammaticalization has no explanatory power of its own, but rather is explained by aspects of phonological change, semantic change, and reanalysis and extension in grammatical change. It has been demonstrated that the unidirectionality assumption (less grammatical > more grammatical, but never more grammatical > less grammatical) cannot be sustained in strong form, that there are a number of counterexamples (cf. Campbell 2001b). Some believe that some difficulties can be surmounted by imposing formal notions of “economy” and learnability; my own view is that this will not resolve the problems.

### 5.3.2 *Typology and change*

Much recent work relates typology to language change. Watkins’ (2001:60) opinion – that “the most important development in historical linguistics in the last decade or so has been the confluence between historical and typological lines of study” – is shared by many. Significant findings involve how the various grammatical constructions typically interact with one another and what constructions can change into other constructions (and what the typical paths of change for these are) and which changes do not (usually) take place (see, for example, Song 2001). Understanding of the typical directionality of many changes (e.g. that postpositions may become case suffixes, but the reverse is unlikely) has proven useful for explaining changes and for syntactic reconstruction. For example, when sister languages are compared where postpositions in Language<sub>1</sub> correspond to case suffixes in Language<sub>2</sub>, the proto-language is reconstructed with postpositions and a change postposition > case is postulated for Language<sub>2</sub> with the case suffixes. Given the known directionality, it is extremely unlikely from a proto-language with case suffixes that Language<sub>1</sub> would have changed case > postposition. Thus the directionality informs reconstruction. (Directionality does not mean “unidirectionality,” though this has not always been understood, cf. Lightfoot 2002; Campbell & Harris 2002.) Also, typologically commonplace traits have not been sufficiently distinguished in a number of proposed but still controversial proposals of distant genetic relationship, calling them into question when little other supporting evidence

was offered (cf. Campbell 1997b). Commonplace typological traits can develop independently and are thus not compelling evidence of genetic relatedness.

## 6. Distant genetic relationships

Judging from media attention, proposals of remote linguistic kinship appear to be one of the “hottest” topics in contemporary linguistics. Nevertheless, many of these proposals have been rejected by most historical linguists. For example, Greenberg’s (1987) “Amerind” hypothesis and the “multilateral comparison” method upon which it is based have been heavily criticized (see, for example, Adelaar 1989; Campbell 1988, 1997a; Hock 1993; Matisoff 1990; McMahon & McMahon 1995; Rankin 1992; Ringe 1992, 1996; Watkins 1990). The debate about proposed “macrofamilies” has sharpened understanding of the methods for researching potential relationships among languages as yet not known to be related. Throughout the history of linguistics the criteria employed for establishing language families consistently included basic vocabulary, grammatical evidence (especially morphological), and sound correspondences. For discussion of pitfalls and excesses, as well as successes, in the use of these criteria, see Campbell (1998a:311–326, in press a), Campbell and Poser (forthcoming). Controversy continues with respect to “Nostratic” (see Joseph & Salmons 1998; Campbell 1998b, 1999), “Altaic” (Campbell 1997b), “Eurasian,” “Dene-Caucasian,” “Austrian,” “Nilo-Saharan,” and others. There is a growing consensus that “Khoisan” is not a valid genetic unit, but may be an areal grouping (cf. Güldemann & Vossen 2000; Sands 1998) (see Campbell & Poser forthcoming for details).

## 7. Beyond the comparative method?

Some scholars have recently expressed dissatisfaction with perceived limitations of the comparative method. Nichols (1996:267) says, “since the tried-and-true Neogrammarian comparative method can only reach back a few thousand years before the evidence fades out, something else must be tried.” Nichols’s (1990, 1992, 1995, 1996, 1997) and Dixon’s (1997; Aikhenvald & Dixon 2001) proposals have been influential, though they have problems.

## 7.1 Nichols' program

Nichols (1996:267, 1997:363) believes the comparative method fades out at around 6000 to 8000 years ago. Her approach is based on a sample with one representative for each of some 200 "lineages" (from the c. 300 existing "lineages" – families and isolates). She tries to find ties among language populations and to gauge the relative age of linguistic traits in large-scale geographical areas; she attempts to infer the source and direction of spread of these structural features, and how the languages came to have their geographical distributions.

This distinction of "spread" vs. "accretion" zones is central in Nichols' work:

An *accretion zone* (termed *residual zone* in previous works...) is an area where genetic and structural diversity of languages is high and increases over time through immigration. Examples are the Caucasus, the Himalayas, the Ethiopian highlands and the northern Rift Valley, California, the Pacific Northwest of North America, Amazonia, northern Australia, and of course New Guinea. Languages appear to move into these areas more often than they move out of them.

A *spread zone* is an area of low density where a single language or family occupies a large range, and where diversity does not build up with immigration but is reduced by language shift and language spreading. (Nichols 1997:369)

However, the terms are misapplied. For example, Nichols treats Mesoamerica as a "spread zone," but by her criteria (Nichols 1992:16–17) it is a residual (accretion) zone; it has: (1) Lots of linguistic diversity, not the low genetic diversity characteristic of spread zones. (2) Lots of structural diversity, not the low structural diversity of spread zones. (3) The language families are not shallow (cf. Otomanguean glottochronologically at 6,400 BP, Uto-Aztecan at 5,000 BP, Mayan at 4,200 BP). (4) Mesoamerican families have not spread rapidly wiping out existing families. (5) There was no widespread lingua franca. Since Nichols (1992) deals with only five spread zones (and five residual zones), with even one misassigned (20%), all the counts involving these zones are seriously skewed. (For other misassignments, see Campbell & Poser forthcoming.)

Other problems involve the geographical and linguistic composition of these zones, the genetic classification of languages, the non-stability of traits thought to be stable, calculation of time depths based on an assumed number of average splits in genetic units, etc. (for details see Campbell in press b, in press c; Campbell & Poser for details).

The notions of spread zones and accretion zones have been influential, but the notion should be abandoned. Nichols (1992:291) has only four spread

zones – Ancient Near East, Europe, Central Australia, and Interior North America (Mesoamerica, a residual zone, is removed) – so different from one another that no generalization is warranted. The Ancient Near East is a linguistic area (Friedrich 1975; Diakonof 1990), with considerable genetic diversity. Central Australia has but a single language family, Pama-Nyungan. In any event, her Europe zone in later work (Nichols 1997, 1998) and appears to be more the recipient of impact from the Eurasian zone than a proper zone of its own. Europe appears to be thrown together on the basis of some arbitrary geographical decision. Interior North America appears even more arbitrary. It contains languages of several families and linguistic areas where there is nothing in the linguistics, anthropology, or geography that suggest these languages ought to be grouped together. They have nothing in common (except absence of coastline). It does not match the spread zone definition: “an area of low density where a single language or family occupies a large range.” In later work other spread zones are mentioned, the grasslands of central Eurasia, northern Africa, and Great Basin (Nichols 1997), but the later two are not clear. In short, there are so few spread zones and they have such disparate characters that the notion of “spread zone” should be abandoned. For those few instances of putative spread zones which do involve few but widely spread languages, there is no particular set of linguistic or other factors which unite them; rather, they appear to be mere artifacts of political and social history, better understood on a case by case basis, as products of contingent history. It is difficult to see that the notions of spread zone and accretion zone do anything more than restate the facts of language distribution, misleadingly suggesting that there is some underlying explanation that does not really exist.

Given the serious problems, Nichols’ approach does not see past the limitations of the comparative method and does not afford reliable insights into the remote past.

## 7.2 Dixon’s approach

Dixon’s (1997) “punctuated equilibrium” differs from mainstream linguists in the degree of emphasis placed upon areal convergence. Dixon (1997:67) characterizes his approach:

The hypothesis put forward here ... is that there have been long periods of equilibrium during which a number of languages have coexisted ... without any major changes taking place. From time to time the state of equilibrium is punctuated by some cataclysmic event; this will engender sweeping changes in the linguistic situation and may trigger a multiple ‘split and expansion’ ...

After the events which caused the punctuation have run their course, a new state of equilibrium will come into being.

The attention Dixon's work draws to areal linguistics and language contact is valuable, though misleadingly overstated.

Dixon (1997:28) asserts that "the family tree model, while appropriate and useful in many circumstances, is not applicable everywhere and cannot explain every type of relationship between languages. We need a more inclusive model, which integrates together the ideas of the family tree and of diffusion area." Mainstream historical linguists agree that the family tree is not everything and that attention must be paid to diffusion, but historical linguistics has never been limited to only the family tree – borrowing, the wave theory, and later areal linguistics, are taken into account (cf. Garrett 1999; Watkins 2001). There are other problems with Dixon's view.

The notion of punctuated equilibrium is challenged in biology. Dennett (1995) argues there is nothing special about punctuated equilibrium; evolution continues even without punctuated events disrupting equilibrium. Language change and differentiation into language families also continue in equilibrium. I argue (Campbell 2003, in press b, in press c) the problems detected in biology also hold for languages – changes of both sort, divergence and borrowing, take place both in equilibrium and in punctuation.

Another problem is the unrealistic assumptions about social structure and its relation to linguistic change. As Dixon (1997:78) sees it:

The necessary scenario for a period of equilibrium is a number of groups living in relative harmony with one another, each more or less respecting their neighbours and their neighbours' ideas and religion, and not trying to foist either themselves or their religion on others. The political groups would have been fairly small in size (ranging from a few hundred to a few thousand) and fairly anarchic in organisation. We can imagine them as being rather like Australian tribes before the white invasion in 1788; or being like some tribes that survive today in the Amazon jungle.

As Nettle (1999a:26) observes, "this Rousseauesque picture is anthropologically naïve." Ethnographers have repeatedly attempted to come up with a list of peaceful societies, but find at best four to seven possible examples (LeBlanc in press). The ethnographic literature does not support Dixon's picture of small non-industrial societies as egalitarian living in harmony; it shows enormous variation in social structure and political organization; harmony and equality are mostly absent. War is common – even non-human primates wage war (cf. Stanford 1998). Many anthropologists believe all societies are systems of

inequality (Salzman 1999). This is a problem for the equilibrium upon which Dixon's model relies.

Normal change leading to diversification into language families takes place also in situations of equilibrium, contrary to expectations of the model. We see cases where under stable conditions over long periods, with no evidence of punctuation, the languages of the region continue to undergo normal change and diversify into language families. Though Dixon (1997: 70–71) expects that in periods of equilibrium languages in contact will diffuse features and become more similar, linguistic diffusion does not always take place in situations of equilibrium. Languages in the same area over a long time may exhibit little evidence of contact-induced change, for example the Hano Tewa (Tanoan) and Hopi (Uto-Aztecan) harmoniously share the same tiny mesa top, yet extremely little borrowing or diffusion has taken place in either language (Kroskrity 1993). This is a problem for the model's expectation that equilibrium produces diffusion and convergence. Moreover, diffusion can be caused by punctuation and does not take place only in equilibrium. Conquest and political inequality promote diffusion among languages, and examples are common. The history of English is mostly of punctuation (Scandinavian invasion, Norman French conquest), but the outcome is like that envisaged for equilibrium: borrowed sounds and pronouns, levelled morphosyntactic complexity, massive impact on the lexicon. Both forced language contact [punctuation] and peaceful contact [equilibrium] can have similar outcomes with respect to diffusion. Furthermore, the formation of linguistic areas and the development of areal phenomena shared across languages of a geographical region, as Hill (1978) shows, can be brought about as a response to punctuating factors, not just equilibrium. Groups may join in areal associations in response to catastrophes of all sorts, structuring human organization at the areal level, supporting "the extensive networks of contact which allow survival of human groups even during periods of locally severe environmental stress" (Hill 1978: 18).

Both diversification in equilibrium and diffusion in punctuation, both unexpected, take place, as does expected diversification in punctuation and diffusion in equilibrium. The correlation envisaged, which equates equilibrium with convergence and punctuation with divergence, is not supported – both kinds of change take place in both kinds of situations. Punctuated equilibrium proves not very useful.

## 8. Linguistic prehistory

There has been considerable work on the linguistic prehistory of various families and areas of the world, on homelands, migrations, language dispersals, expansions, and attempts at dating (cf. Renfrew et al. 2000). (See Fogelberg 1999 on linguistic prehistory of Finnish and its relatives; Campbell 1997a for surveys of works on languages families of the Americas.)

Hypotheses about the diversification and spread of languages have attracted attention. Renfrew (1987, 1994, 1997, 2000, in press) and Bellwood (1991, 1994, 1997, 2001, in press) argue agriculture is the primary agent in language dispersals, though this is not well supported and leaves out many other factors involved of the diversification and spread of languages.

Agriculture does not always motivate language expansions; cultivation can make local resources reliable, allowing people to stay put, hence the numerous attested non-expansionist languages of agriculturalists, which go against the farming-language dispersal model. To test the model, it is important to survey the language families of the world to see whether they have spread significantly and whether they have agriculture. Exceptions to expectations are the many relatively non-spread language families with agriculture (e.g. Papuan families, Mixe-Zoquean, Mayan, Munda, Keresan, Panoan, Zuni, Basque, etc.), and the many widespread families which lack agriculture (Tungusic, Uralic, Pama-Nyungan, Eskimo-Aleut, Athabaskan, etc.). Thus, the hypothesis is not necessary, since there are widespread non-agricultural language families, nor is it sufficient, since there are non-spread agricultural languages. Other problems include the zones of intensive agriculture which exhibit great linguistic diversity. Agriculture in these zones has not led to language spreads, but seemingly has allowed the development and co-existence of numerous languages and language families. The agricultural dispersal model also does not explain the co-existence within a region of small languages and large languages. Bellwood (2001) has in mind agriculturalist language families spread over vast areas leaving virtually no enclaves, with Bantu, Austronesian, and Indo-European as paradigm examples. The model predicts, in effect, that the small languages in the geographical domain of larger languages should be eliminated by the expanding larger agricultural languages. The co-existence of such smaller languages with larger ones in a region thus constitutes a difficulty for the model. In short, widespread non-agricultural cases such as Pama-Nyungan and Uto-Aztecan and non-spread agricultural cases such as the “Papuan” language families and Mixe-Zoquean which go against the predictions are serious problems for the farming/language dispersal model.

There are problems even with cases that might appear to fit the model. For example, if the Indo-Europeanization of Europe and northern India took several millennia, is it appropriate to talk of a single expansion or dispersal, reflecting a single cause? Indo-Europeanists insist on a number of independent movements scattered over centuries to account for the distribution of Indo-European. (See Vansina 1995: 191 for a similar view of Bantu “expansion”; Evans & McConvell 1998 show that Pama-Nyungan “expansion” involved different processes and various phases over several millennia.) This telescoping of the events which resulted in the distribution of the languages by claiming a single spread with a single cause does a disservice to the prehistory under investigation. In summary, the cases where the farming-language dispersal approach might work are insufficient for generalizing about what might drive language spread and diversification. (For other causal factors in language diversification, see Campbell *in press c*; Campbell & Poser *forthcoming*.)

Nettle’s “ecological risk hypothesis” adopts an economic theory concerned with social networks to insure against risks of agriculture (Nettle 1996: 413), to which Nettle adds a linguistic component. In his view, “the greater the provisioning problem, the wider the social network necessary,” and “it is the larger networks of generalized exchange which ultimately give rise to different ethnolinguistic groups” (1996: 413, 414). He “predicts that the size of ethnolinguistic groups should increase in proportion to the amount of ecological risk that they face” (Nettle 1996: 414). He concludes, “the question of why there are more languages in some areas than others can therefore be more or less restated as the question of why households in some areas are involved in larger or more dispersed networks of generalized exchange than others” (Nettle 1996: 412). This view also has difficulties.

Nettle’s economic determinism which correlates size of language with extent of economic risk is too narrow. Social associations often follow linguistic lines, but linguistic lines are not the only ones along which people organize themselves in ethnic, economic, kinship, and other alliances. The idea of alliances to aid against economic risk may be correct, but reliance on alliances within single languages is overstated. The kinds of alliances (which might be called upon in times of food crisis) often extend beyond language boundaries. Linguistic homogeneity is not necessary for economic cohesion and exchange. These economic ends can be served as well through multilingualism, *lingua franca*, and trading pidgins. Indeed, West African society (Nettle’s test case) is characterized by a high degree of multilingualism. These modes of communication reflect organizing patterns of human population beyond individual language boundaries which help to maintain area-wide systems of adaptation

and support, “the extensive networks of contact which allow survival of human groups [beyond single language boundaries] even during periods of locally severe environmental stress” (Hill 1978: 18). Cross-linguistic alliances of various sorts can serve economic risk reduction.

Another problem is the presence of large and small languages in the same area, many instances of which are known, including in West Africa (see Campbell in press c). Such cases go against the predictions of the model: in circumstances subject to economic risk, small languages should not survive; all the languages should be roughly equally large to address the risk. Nettle’s ecological-risk approach also fails to take sufficiently into account the impact of large political units on language distribution, for example the Hausa state, with its political importance (see McConvell 2001). The area of “‘pure’ Hausa states” is more or less identical to that of the Hausa language, inconsistent with the fact that Nettle sees no evidence that state formation spread the language. There are numerous other counterexamples: the spread of Nahuatl with the Aztec empire, Quechua with the Inca empire (mentioned above), Latin with Romans, and the spread of Arabic, Chinese, Turkish, and so many others. Clearly, factors other than ecology figure in the spread and distribution of languages.

## 9. Sociolinguistics and language change

Several advances have been made in the study of change in its social context (see Labov 1994, 2001). The role of gender has been further confirmed that women lead most (non-stigmatized) changes, and principles of vowel shifting (above, Labov 1994) have been refined.

An assumption of sociolinguistic studies of change has been that people do not change their vernacular pronunciation significantly during adulthood; however, it is now clear that this was overstated, that speakers do sometimes change their vernacular later in life (see, for example, Gordon et al. in press; Harrington et al. 2000; Sankoff et al. 2001; and even Labov 1994: 112). This has implications for the apparent-time approach to the study of change on which so many sociolinguistic studies are based, where people of different ages are recorded during a single time period and differences in their speech which correlate with age are assumed to reflect changes at different times. It is assumed that the difference between the speech of older speakers and younger speakers represents change which has taken place in the language since the older speakers acquired their vernacular. However, this may not be the case if older

speakers accommodate later in life to younger speakers' norms. This has major implications for many sociolinguistic projects concerned with change.

An important, though controversial, topic is the role of speakers' choices in linguistic change. Linguists have believed at least since Labov's (1963) Martha's Vineyard study that speakers' attempts to signal their identity can condition linguistic change. In the Martha's Vineyard case, speakers who identified locally with the island centralized diphthongs more than others who did not have such a strong sense of local identity. Given this, it is surprising that the possibility that speakers can identify themselves with particular social groups and thus contribute to linguistic change has been questioned, even by Labov (2001) himself, while at the same time evidence supporting speakers' acts of identity influencing change continues to accumulate in other quarters (see Schilling-Estes 2002). According to Giles' (1973, 1977; Giles & Coupland 1991) accommodation theory, speakers may modify their speech in order to sound more like others they talk with to achieve greater social integration with them. However, Giles' approach deals not only with convergence through accommodation, but also with divergence, where deliberate linguistic differences can be employed by a group as a symbolic act for asserting or maintaining their distinct identity.

Many connect this sort of motivation with LePage and Tabouret-Keller's (1985) "acts of identity," defined as follows: "the individual creates for himself the patterns of his linguistic behaviour so as to resemble those of the group or groups with which from time to time he wishes to be identified, or so to be unlike those from whom he wishes to be distinguished" (Tabouret-Keller 1985: 181). They find "positive and negative motivation to identify with groups" as "by far the most important" of their constraints governing linguistic behavior (LePage & Tabouret-Keller 1985: 2). Recent work by Eckert (2000), for example, shows that adolescents in Detroit high schools engage in linguistic "acts of identity," selecting linguistic variables specifically to fit the social groups with which they want their identity to be associated. Labov (2001) shows how by subconsciously selecting the most advanced tokens of a variable correlated with a group, speakers can bring about change in the perceived core of the target.

Labov (2001: 191–192, 1994: 549–550), nevertheless, takes a dim view of claims that "acts of identity" and "attitudes" motivate change or acceptance of change:

[such views] attribute linguistic change to: (1) the association of positively regarded traits and social privileges with membership in a given social group,

and (2) the association of a linguistic form with membership in that social group. If such attitudes are to be used to account for linguistic diffusion [spread of innovations], it is necessary to posit a covert belief structure: that speakers feel that their adoption of the linguistic form will lead others to attribute to them the positive traits of the given group and allow them to share in the privileges of that group . . . such covert attitudes and beliefs . . . are not usually supported by material evidence . . . language change may simply reflect changes in interlocutor frequencies which are in turn the result of changes in social preferences and attitudes . . . The . . . pattern of a sound change from an originating group to neighboring groups may then be the simple product of frequencies of interaction. The account based on covert attitudes is redundant to the extent that the network of daily interaction brings people into contact with the new form in proportion to their distance from the originating group.

(Labov 2001:191–192)

These sociolinguistic matters deserve particular attention in the further investigations. Recent work from several quarters appears to converge as different scholars investigate the significance of social and cultural factors and speakers' choices in language change (see Thurston 1987, 1989; Ross 1996, 1997, 2001; Golla 2000; Hill 2001), and these views intersect with social network theory (J. Milroy 1992; L. Milroy 1987; Milroy & Milroy 1992; Ross 1997) and "communities of practice" (Eckert 2000).

Thurston (1987, 1989) and Ross (1996, 1997) speak of "esoterogeny," where speakers of a language add linguistic innovations which increase complexity in order to emphasize the distinctiveness of their language from that of neighboring groups; "esoterogeny arises through a group's desire for exclusiveness" (Ross 1996:184). In this way the community language – which Ross (1996) calls the 'emblematic' language, emblematic of ethnic identity in a multilingual situation – becomes the 'in-group' code which serves to exclude outsiders (cf. Thurston 1989:556–557; Ross 1997:232). In such situations, Ross (1997:239) believes "innovations leading to increased complexity and to differences from neighboring lects will be favoured." Ross (1996:183) points to processes of elision and assimilation which "result in phonological compactness, in allophony and allomorphy," mentioning also the accumulation of irregularities, elaboration of the lexicon with numerous near synonyms, much borrowing, and "an increase in the frequency of opaque idioms."

There are, nonetheless, difficulties with this interpretation. It is attractive to imagine that speakers of these "esoteric" languages have initiated particular changes for the purpose of distinguishing themselves from and thus excluding outsiders. However, it is not clear how this hypothesized cultural motive

for these changes – conscious exclusion of outsiders (Ross 1997:239) – could be tested or how the investigator might distinguish changes motivated for this purpose from changes which just happen with no such motive. It would be difficult to prove that such cultural factors were involved, since many languages have undergone rather extensive changes of these sorts, leaving them looking “esoteric,” but where no separatist motive behind the changes can be identified. English has undergone many changes which distinguish it significantly from its closest relatives (e.g. loss of Umlaut, unrounding of front rounded vowels, loss of verb-second properties, loss of verb-final properties, several sound changes, loss of much inflectional morphology, various lexical changes, addition of many idioms). This illustrates that languages can undergo changes which consequently – but not apparently on purpose – keep outsiders from understanding them, without necessarily having the cultural teleology of intending to exclude outsiders.

The opposite of esoterogeny for Thurston and Ross is “exoterogeny.” “If a community has extensive ties with other communities and their emblematic language is also spoken as a contact language by members of those communities, then they will probably value their language for its use across community boundaries . . . it will be an ‘exoteric’ lect” (Ross 1997:238). Thurston and Ross believe that use by a wider range of speakers makes an exoteric lect subject to considerable variability, so that innovations leading to greater simplicity will be preferred. “Exoterogeny” reduces phonological and morphological irregularity or complexity, and makes the language more regular, more understandable and more learnable” (Ross 1997:239). The claim that the use across communities will lead to the simplification of the language does not appear to hold up in numerous known cases. For example, Quechua, spread into many new regions by the Inca Empire, maintained its complexity (glottalized and aspirated consonants, uvular stops, complex morphology, etc.) in spite of being the lingua franca for a huge area. Other examples include Arabic, Turkic, Mongolian, Georgian, etc. Certainly there are plenty of instances where languages not involving state-level social organization also have become “exoteric” without undergoing simplification, for instance, Pama-Nyungan in Australia (Evans & McConvell 1998). Some languages in instances such as this appear instead to accept a wide range of variation rather than undergoing simplification (e.g. Shoshoni, cf. Miller 1971).

Other recent work reflects similar views. Ross (1997) argues that speakers’ attitudes can favor the spread of certain innovations and disfavor others. He uses social network theory to model changes in speech communities, including both fissure (diversification) and fusion (diffusion, borrowing, contact).

For him, “*language fissure* is the SCE [speech community event] that . . . occurs . . . when a subset of the links in the network undergoes a sharp reduction in density” (Ross 1997:218). This reduction of density (together with reduction in intensity and multiplexity), when sharp enough, results in division of a single community into two. The network, however, does not *cause* this reduction in density; rather, this is due to conventional factors known to be involved in communicative isolation, e.g. population growth, migration, etc. (Ross 1997:218, 222–223). For Ross, linguistic innovations in the network are socially relevant when associated with a particular group of speakers, and this can lead other speakers to “acts of identity,” either adopting or rejecting the innovative feature because of its social significance. This can change the network structure. Nevertheless, as Ross (1997:240) points out, “esoterogeny and exoterogeny cannot readily be captured in a social network diagram because it is not the distribution or the spread of innovations which is significant, but the *kind* of innovation.”

Hill (2001) generalizes from her work on Tohono O’odham (Papago) for “localist” and “distributed” strategies:

In a “localist” strategy, the speaker decides, “I will select a particular kind of person as my model, and I will try to sound as much like that particular kind of person as I can.” In a “distributed” strategy, the speaker decides, “I am not sure what kind of person I want to sound like. I will try to sound like a variety of different kinds of people.” The speech of any single person and the patterns of variation in any community will always be the product of a combination of these two strategies. (Hill 2001:260–261)

Hill associates each of these strategies “with a different set of ecological, socio-cultural, and biological constraints” (Hill 2001:261). She says:

in the localist case, speakers behave as if they hold an opinion . . . “I have a rightful and primary claim on valuable and dependable local resources adequate to sustain my well-being.” In the distributed case, speakers seem to have a different thought: “I have no rightful and primary claim on valuable and dependable local resources adequate to sustain my well-being. However, I might be able to add to my limited primary claims secondary claims on a sufficient range of a distributed inventory of resources to sustain my well-being. (Hill 2001:261)

And: “The way I can license my claim on resources is through speaking in a certain way” (Hill 2001:261). Hill shows that Tohono O’odham speakers with a distributed strategy, those from the drier, less reliable areas, speak dialects which incorporate aspects of other dialects, while speakers with a localist strat-

egy have dialect features which tend to set them off from others. She believes these different dialect features reflect who is an “insider” and thus has rights to resources.

Golla (2000) distinguishes two kinds of language communities based on geography and communicative patterns. *Spread languages* are:

language communities all or most of whose constituent dialect communities are sufficiently distant from one another geographically and socially, to make social contact sporadic and relatively unstructured. Such language communities are usually the result of the dispersal of speakers of related dialect communities across a wide territory, often by migration. (Golla 2000:60)

*Spread languages* often constitute chains of intelligibility. *Compact languages* are:

language communities whose constituent dialect communities are closely adjacent and share a common interaction sphere (connected by trade, intermarriage, ritual, and intergroup alliances, and hostilities. (Golla 2000:60)

“Compact language communities were common along the west coast, from Alaska to California, in the Pueblo southwest, and along the Gulf Coast from Texas to Florida” (Golla 2000: 60–61). Characteristic of compact language communities are phonological and grammatical differences among dialects that focus on a salient and easily dichotomized feature” (Golla 2000:60). In similar fashion, Golla distinguishes two kinds of language families. *Spread families* are:

those that have largely developed in the geographical and social contexts that are conducive to the development of spread languages. Dialect communities develop into language communities with mutually unintelligible linguistic patterns owing to lack of contact and the independent ‘drift’ of their linguistic systems. Boundaries among these groups remain informal, and where contact exists multilingualism is common, even encouraged, and innovations are rapidly transmitted. This frequently results in the language-level equivalent of dialect chains, where adjacent languages share more features than more distant languages, although the time depth of their split may be the same. Such language chains are typical of Northern Athabaskan languages . . . and Sahaptian languages. (Golla 2000:62)

*Compact families* are:

those that have largely developed in the geographical and social contexts that are conducive to the development of compact languages. Dialect communities develop into language communities in areas where the social boundaries

are rigid and stable and where close contact with neighboring groups is the norm ... patterns of interaction between adjacent dialect communities appear to have remained stable over many generations, with steadily increasing differentiation of linguistic systems. An important factor in this process is the social advantage of maintaining distinct adaptive systems focused on the exploitation of a relatively circumscribed territory. The continuance of such small-scale social units would appear to be dependent on encouraging monolingualism. (Golla 2000:63)

The localist-distributed strategies, the spread-versus-compact languages and language families, and “esoterogeny” are not primarily about economics, however. They are about people’s choices, social behavior, and how they restrict group membership and rights to participate in the cultural life of the group; they are about the whole fabric of social life.

Language is among the symbolic material communities use to construct their identity (Eckert 2000:44). Members of a community of practice, in their social and linguistic behavior, can choose social and linguistic traits that focus inwardly, which distinguish members from non-members of the community, as in Hill’s “localist strategy”, Golla’s “compact languages,” and Thurston and Ross’s “esoteric” languages. Other, less tightly integrated communities of practice can involve some social and linguistic traits which facilitate communication beyond the group, as in Hill’s distributed strategy, Golla’s spread languages, and Thurston and Ross’ exoteric languages. The different choices made in these different circumstances, different communities of practice, will have a strong impact on the kind of innovations which distinguish some groups and which link others.

Other similar claims about social and cultural attributes of speech communities determining sorts of linguistic change and non-lexical aspects of linguistic structure offer less promise. One such claim is that language becomes more complex in isolated communities or in small-scale societies characterized by face-to-face interaction – the opposite has also been claimed, that such languages simplify and become less complex (cf. Andersen 1988; Dixon 1997; Nettle 1999a; Nettle & Romaine 2000; Ross 1996, 1997; Trudgill 1989, 2002). Neither is correct. Hymes’ (1974:50) claim has influenced thinking; he says: “the surface structures of languages spoken in small, cheek-by-jowl communities so often are markedly complex, and the surface structures of languages spoken over wide ranges less so.” This view is often attributed to Jakobson (1929[1962]:82), who believed that “dialects which serve as vehicles of communication in large areas and gravitate towards the role of koiné tend to develop simpler systems than dialects that serve purely local purposes”

(cited in Andersen 1988:37), to which Andersen (1988:60) adds, “dialects that serve predominantly local functions are more prone to elaborate phonetic detail rules.” Later versions tie developing complexity with social structure. Andersen (1988:61) speaks of “relatively open” and “relatively closed communities,” arguing that “the greater potential for variability of usage in open communities favors a more active levelling of irregularities in these, and the lesser variability a more faithful transmission of morphological irregularity in closed communities.” He asserts that “the conservatism of relatively closed dialects is common knowledge” but argues that “phonetic norm elaboration” is also common in closed dialects (Andersen 1988:62), including “exorbitant’ phonetic changes” (Andersen 1988:73–74). He correlates “open” and “closed” dialects with “core/center and periphery,” respectively, where closed or periphery dialects are characterized by “lower density networks.” Trudgill (1989:227), speaking of “high- and low-contact varieties,” extends Andersen’s dichotomy to include different languages. For Trudgill, high contact leads to simplification, simpler systems. He asserts that “dialects which serve a relatively wide socio-spatial function tend to have simpler systems than dialects with a more restricted function” (p. 228), that “in low-contact situations . . . the speed of linguistic change will typically be slow” (p. 229), and that “many of the changes that take place in this sort of situation [low-contact] are of the type that move in the opposite direction. . . complication as opposed to simplification” (p. 229) – but at the same time that “lack of contact favors lack of change” (Trudgill 2002:709). Thus Trudgill feels that the relative greater isolation of Faroese over Danish explains the seeming less conservativeness of Danish (Trudgill 1989:227, 2002:711–712), but, how, then, do we explain the fact that higher-contact Danish is more conservative in some regards than its lower-contact Scandinavian sisters?, e.g. in Danish /k/ is preserved before front vowels, where Swedish and Norwegian have changed it to a fricative.

Views differ; Trudgill (1989, 2002) sees isolated low-contact communities as conservative (and resistant to certain kinds of change), while Nettle and Romaine (below) hold the opposite view, that such communities allow much innovation. Nevertheless, Trudgill (1989:234) suggests some kinds of “changes typical of low-contact social contexts.” One is the development of grammatical agreement, but, for example, case and number agreement on adjectives in Finnic languages is generally understood to be due to contact with Germanic languages. That is, not the result of low-contact, but of high-contact. Another is the “proliferation of clicks in the Khoisan languages,” but, then, the elaborate inventory of clicks in southern Bantu languages is due precisely to language contact, contact with Koisian languages. The Northwest Coast Lin-

guistic Area of North America is characterized by extensive language contact and extremely elaborate phonemic inventories. This is clearly against the belief that isolated low-contact communities tend to “develop small phonological inventories” (Trudgill’s 2002:725). Clearly there is no easy correlation between relative contact or isolation and structural complexity.

Nettle (1999a: 139) also relates “community size” with language structure, but in a different way:

If a group consists of just a few hundred people, the idiosyncrasies of one very influential individual can spread through it very easily. This is not the case if the group consists of thousands or tens of thousands of people. In general, the smaller the community, the greater the probability that a given variant that has no functional advantage at all, but is neutral or slightly disadvantageous, can replace the existing item and become the norm.

Nettle and Romaine (2000: 12) add that “languages which are used only for in-group communication in small groups can afford complexity.” “In small language groups innovations and new usages can quickly spread throughout a whole village.” The basic idea is that such communities, isolated or characterized by face-to-face communication where most speakers know each other, tolerate eccentricities, and so complexity can grow and unusual linguistic traits can become part of the structure of the language.

A major problem in this literature which claims correlations between community size or social organization and linguistic structure is the many counterexamples, many relatively isolated small but simple languages and many large non-isolated but complex languages (as well as simple small isolated languages and complex large non-isolated ones). For example, looking at phonological complexity (from which some of the proponents take their inspiration), we see counterexamples to claims that isolation correlates with complexity in numerous small isolated languages such as Rotokas, Pirahã, Hawai’ian, Māori, etc. which have extremely limited phonemic inventories: Rotokas (“Papuan” of Bougainville, c. 4,000 speakers), with only 11 segments, only 6 consonants; Pirahã (Muran family, c. 150 speakers), 8 consonants, 3 vowels (cf. Maddieson 1984); and Hawai’ian, 8 consonants. The small and isolated South Island Māori, instead of becoming more complex, reduced the already limited inventory by merging /ŋ/ with /k/, leaving only 9 consonants. At the same time, numerous large non-isolated languages with similarly limited phonemic inventories exist; for example, Indonesian and Malay, with many millions of speakers, do not have appreciably larger phonemic inventories than their Hawai’ian and Māori relatives. On the other hand, there are plenty of cases

of large non-isolated languages which are complex or exhibit unusual traits, e.g. the Quechua language spread by the Inca Empire, spoken by several millions of speakers, is phonologically very complex, with 3 series of stops and affricates (plain, glottalized, aspirated), at 5 points of articulation, including uvulars (some varieties at 6, with contrasting also retroflexed obstruents). Zulu (c. 6,000,000 speakers), not isolated, has 35 consonants; it acquired an extremely elaborate system of click consonants. There appear to be no causal correlations between size/isolation and complexity or simplicity.

## 10. Conclusions

It is encouraging that so much historical linguistic research is being done in so many distinct areas, and that understanding of how and why languages change is growing. To me personally, it is heartening to see the many advances in the working out the history of language families and individual languages around the world. As I have attempted to point out, not all initiatives which are receiving attention appear to be leading in productive directions – some are just misleading. Nevertheless, I am optimistic about what the future holds for us.

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# Coherent fieldwork\*

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## 1. Introduction

The history of research in general and field research in particular is the history of fallible humans, evolved creatures, struggling to understand nearly infinite complexity in an alien environment. No one person is up to the demands of fieldwork, requiring as it does an idealised character from Arthur Conan Doyle. The outputs of our fieldwork will necessarily be incomplete records of our progress in understanding parts of wholes that exceed our abilities. Thus, our research reports, whether grammars or articles or talks or webpages are never more nor less than our efforts to communicate with interested interlocutors about the beliefs we have come to form and hold, based on our experiences and how these beliefs affect our actions in science and in life. This is our canopy of epistemic humility. Further, I am more and more convinced that the beliefs we have come to hold about a particular language or grammar are constrained and shaped by the totality of our experiences, not merely our linguistic training. If this is correct, one immediate consequence for fieldwork that emerges is that compartmentalization of knowledge and its isolation from application, the notions of ‘pure’ ideas, ‘deep thoughts’, and ‘objectivity’ are recast from normal expectations to just ways of talking about what works.

The pragmatist view of fieldwork which I advocate here has a philosophical pedigree to claim for its integration of life, research, and application, tracing back to the three concepts of *usefulness*, *radical empiricism*, and *coherence* urged by William James (1842–1910) (see James 1896, among many others). These are the principal calibrators of research success in pragmatism. As such let us consider each in turn. Usefulness is just the idea that a theory need not be true or even falsifiable to be a good theory. Rather it must move us towards our

goals and have some utility in society. Coherence is the idea that each researcher accept and acknowledge the role of her temperament in her science and, more importantly, her status as an evolving creature with an ability to learn limited by tools provided by random mutations, natural selection, and perhaps a few other side effects of evolution. I say more about this below. Radical empiricism will be taken up directly. First, though, let us return to our fieldworker and some preliminary advice for her.

The first question the fieldworker must ask herself, i.e. before she applies for a research grant, packs her bags, or buys a plane ticket to some exotic place, is this: what do I plan to study? What is the exact object of my investigation? Am I studying something directly observable or something only inferable? Am I studying a set of rules that I can formulate about observed sentences or the characterization of a speaker's knowledge of her grammar? Am I studying Language? A language? The cognitive capacity behind Language? A corpus of texts? The behaviour of native speakers when exposed to certain questions? The answers to a questionnaire? The emergent communicative behaviour of a particular culture? All of the above? None of the above? And once she has decided what her object of study is, she might ask what it is that justifies her choice. Could one ever prove that any of the questions above is the right question to ask, for example? Further, once one has studied an object, is there any way to quantify what it is that one purports to have come to know?

Let's consider a concrete example. In Everett (2001:168), I defined fieldwork as in (1):

- (1) *Fieldwork describes the activity of a researcher systematically analyzing parts of a language other than one's native language (usually one the researcher did not speak prior to beginning fieldwork), within a community of speakers of that language, prototypically in their native land, living out their existence in the milieu and mental currency of their native culture.*

Why propose this definition? And could I ever prove that it is the best definition? Is there any truth to it? Well, I can say that I believe (1) to be maximally useful in grasping the coherence between a language and its culture, which I in turn find useful for understanding language itself. But that is about all I can say for it. I certainly cannot prove it. Still, it is a point of departure for coherent fieldwork, more useful than many alternatives, perhaps less useful than others, all applications of usefulness teleologically determined.

But can't we do better than to merely claim usefulness for our proposals? After all, many researchers I have spoken to over the years will say the quantifier of their findings is 'Truth', that they are after the 'Truth' about one of the

questions above. This is an interesting position, though in spite of its popularity, I think it is useless. Let's consider it a bit, therefore. Imagine that we can find truth. Many scientists believe this to be a legitimate goal, after all. In recent work, Chomsky (2002: 129ff.) is very explicit that he is after Truth: "So, the first question that has to be met is TRUTH [emphasis mine, DLE] for every state of the language faculty" (133) or "Minimalist questions are substantive: they ask whether TRUE THEORIES [emphasis mine, DLE] of states of the faculty of language satisfy the interface condition in an optimal way" (132).

But what would 'truth' or a 'true theory' look like? Would we ever be able to recognize either? Can we ever find this truth? Do we asymptotically approach it by ever-closer approximations to it? Or, paraphrasing Rorty, is truth just a compliment we pay to ourselves when we have made a well-justified statement? If my statement about 'x' at time 't' in context 'c' is true, then this implies that that statement is never in need of revision. But suppose I in fact do need to revise my statement as a result of a new fact coming to light. Was my previous statement true? Maybe, you say, in the part of it that did not need revision. But how can we ever know, in principle, what part will not need revision? The world and its languages are 'out there', of course. The denial of Truth need not be a denial of Reality, though it does imply that we can never claim to have apprehended Reality. It is difficult to see how saying that we are looking for truth is any better than saying that we will do our best to be convincing. The average field worker experiences a receding truth-line. Insisting on truth is just insisting that one drink the water from a mirage.

There are two applications of the explicit acceptance that Truth is ever-receding, or, in my view, non-existent. First, lack of guilt and arrogance. Second, the healthy refusal to be locked in to a given solution. All statements about a language are subject to revision in principle. But this just means that in principle no statement about a language is true. Let's consider these in turn. To begin, how does an eschewal of the concept of Truth enable us to work guilt-free? It does so by freeing me to follow my own interests with no sense of servitude to intellectual fashion. I can accept, say, a definition like (1) of my enterprise, or some other, or even none. If Truth existed, I might feel that I should not work on false ideas when other ideas had been shown to be true. But since nothing can be shown to be true, by my pragmatist reasoning, no such burden can be placed on me. And yet at the same time, just as I no longer feel inferior for my specific choice of goals, such as fieldwork over theory, I have no basis for feeling superior either. My choice is no truer than any other's choice. Pragmatism would simply urge us to do what is most useful from our perspective and, as I discuss directly, what 'coheres' most usefully with our life experiences

and goals. But this is not to say that theory has no role to play in fieldwork, that I should work alone, or that I should ignore other linguists and their results. All field researchers should reflect on the role of theory in the enterprise of fieldwork. And all should endeavour to learn from the past. As I have stated and as reflection should inform any experienced linguist, our field research is inescapably and rightly constrained and motivated by our life experiences, including reading, thinking, and other engagements with linguistic theory. The field researcher without knowledge of linguistic theory, without an on-going reading program in modern linguistic theory, whether eclectic (which I recommend) or focused on a single theory, is severely handicapped. But I suspect that most linguists know this. The real question is not whether the field researcher (should) know(s) linguistic theory but to what degree linguistic theory should constrain her fieldwork. I would say ‘up to coherence’, in the sense given.

Another question relevant to the fieldworker is what role she plays in the development of linguistic theory. Should fieldworkers and theoreticians be different people or, ideally, should each fieldworker be a theoretician as well? The answer is surely that this ultimately depends on the tastes and preferences of the individual fieldworker. At the same time, I believe that linguistics benefits when fieldworkers are doing more than merely gathering data for a theoretician to interpret. This would place fieldworkers in the same position as Gideon Mantell in relation to Richard Owen in the study of the fossil record. Owen considered Mantell at best a collector of data for him, Owen, to interpret (see Deborah Cadbury’s 2000 fascinating account of their relationship). Yet science suffered greatly because of Owen’s ascendance over Mantell and his failure to accept Mantell as a theoretician of the first order. As for Mantell and his fossils, so with the fieldlinguists and their data – there is really no one better placed to interpret field data from a theoretical perspective than the fieldworker who collected the data, given the theses of ‘radical empiricism’ and coherence discussed in this paper, so long as the fieldworker is hard on herself and develops the requisite stringency of thought. To get at the import of this, we return to the third component of James’s pragmatism, viz., radical empiricism. According to radical empiricism (James 1987 [1909]), ‘reality is just the flux of pure experience’ or ‘reality consists in nothing but experience’. It is our experience with an object that gives that object reality. But since no two experiences will be exactly the same, no object can be the same to two people (or for one person at two times). This includes grammars and the other outputs of fieldwork. James’ thesis seems particularly useful to answering the questions posed above about the object of fieldwork. But to fully appreciate it, we need to make the connection between radical empiricism and coherence.

If experience is all there is, as it is according to radical empiricism, then there is no 'Truth', only usefulness. Moreover, the experiences which have the greatest coherence with the rest of our lives will be those which are most useful to us. Radical empiricism almost by definition favours the specific experience over the less directly experienced reasoning behind 'big picture' theorization. Although James was by no means averse to generalizations, he advocated the view that the most useful 'big picture' was the generalization that best coheres with one's life experiences. This will require some exemplification. Let me first exemplify what coherence means for me in fieldwork and then exemplify what I think it means for my object of study.

Coherence for me personally means that I cannot objectify myself, i.e. prevent my own history and person from interacting causally with my observations and conclusions. It might be useful to try, but at the same time I realize that my efforts in this regard, should I expend them, will always fall short. (Kenneth Pike recognized something like this in his many references to the role of the 'observer' in the scientific process.) So, for example, I oppose much of the conforming power of Western culture. Therefore, I have noticed that what I often observe and most enjoying finding out about in my fieldwork are those things which make a language different from other languages, the 'relativity' among people and their languages found in fieldwork. I like to find and look for counterexamples to theory, rather than what is to me mere confirmation of some other linguist's beliefs. It took me some time to admit this personal aspect or that it affected my fieldwork. But I am happy now to acknowledge it and am unconcerned about it. My perspectives add to the tapestry of experiences emerging from fieldwork around the world and will be useful to others (or not). As they are useful to other linguists, then I am doing useful linguistics. That is about all I can hope for, frankly. If you take nothing else from this paper, remember this: *If your fieldwork is coherent for you and useful to others, then you must be on the right track.* Ascribing Truth to any part of the enterprise is little more than a religious incantation.

The other side of coherence, at least in my interpretation of James's notion, concerns my full experience with my object of study. That is, that whatever I say or might say about one aspect of my object should cohere with other statements I have made about the object, and the sum of my experience with the object. For example, as I reside in an Amazonian community, my understanding and reports of the language or grammar or phonology, etc. of the language of that community ought to cohere with what I know about the speakers of that language and, where possible, tell me something about the cultural matrix in which the language is embedded. In Section 2, I give an example from the

communities I know best, those of the Pirahã people scattered up and down the Maici River.

Before I turn to specific empirical cases, however, I would like to round out this introduction to fieldwork with a brief, impressionistic view of its history, focusing on the Americas. Arguably linguistics in the Americas and elsewhere began as an extension of colonial activity, specifically missionary work. In Brazil, the French Calvinist chronicler Jean de Lery (1534–1613?) compiled fascinating and extremely useful records of Tupinambá conversations. Today this once widely-spoken language lives as a communication system only in the conversations recorded by Lery. Contemporary with de Lery was the amazing Padre Jose de Anchieta (1533–1597), founder of the city of São Paulo, co-founder of the city of Rio de Janeiro, and the author of a brilliant grammar of Tupinambá and translator of many catechisms into this language. Anchieta could plausibly be called the founder of linguistics in the Americas. Following Anchieta was Padre Antonio Ruiz de Montoya (1585–1652), who produced brilliant studies of the Guaraní language. (Montoya has what I believe to be the first insightful discussion of Noun Incorporation and Possessor Raising anywhere, offering a brilliant account of how the verb's case is freed up after Noun Incorporation to be re-assigned to the possessed NP — over 300 years before, say, Mark Baker's 1988 theory of incorporation.) Of course, the study of language motivated mainly by science did not begin until much later. Still, these Jesuit priests in Brazil and elsewhere set standards of scholarship that endure through the centuries.

In the Americas, the scientific concern for fieldwork began with Franz Boas (1858–1942), who trained a core of linguistic anthropologists Ruth Benedict (1887–1948), Edward Sapir (1884–1939), and in some classes and via Sapir, Mary Haas (1910–1996), among others, responsible for the birth and growth of North American linguistics. During the years of Boas's influence, roughly during his life and following his death until the 50s, North American linguistics was concerned about describing specific languages in detail, producing integrated studies of texts keyed to cultural studies, grammars, and dictionaries, providing exactly the kind of pragmatist study that has proven to be so important to knowledge of little-studied peoples and their languages throughout the intervening years. In fact, though this is not the place to attempt a more detailed intellectual history, a case can be made that these earlier descriptive linguists were heavily influenced by the pragmatist philosophy underlying much American intellectual endeavor until at the least the death of John Dewey (1859–1952). Consider some remarks of Boas in his 1917 introduction to the first volume of the new **International Journal of American Linguistics** (IJAL).

According to Boas one of the principal goals of the new journal was to provide what I would call a ‘coherent’ report of languages. For example, he (1917:201) laments the fact that “. . . the available material gives a one-sided presentation of linguistic data, because we have hardly any records of daily occurrences, everyday conversation, descriptions of industries, customs, and the like. For these reasons the vocabularies yielded by texts are one-sided and incomplete.” That is, Boas felt that a full ‘picture’ of a given language was only possible by looking at the language in the cultural context. Or consider Sapir’s (1915:186) assertion that more studies are needed of cultural ‘modalities of attitude’ and consonantal alternations (I discuss this further in 2 below), thus explicitly connecting grammar with culture.

Thus, for the first half of the Twentieth Century, the normal conception of the linguist’s ‘job’ was to study little – or un-studied languages in the field and to produce coherent bodies of data on the interaction of culture, lexicon, texts, and grammar.<sup>1</sup> But by the 60s this had changed radically, with field research given more or less the intellectual status of butterfly collecting. The ‘withering of fieldwork’ began innocuously enough, in the restlessness of a graduate student at the University of Pennsylvania with his MA research:

*Harris suggested that I undertake a systematic structural grammar of some language. I chose Hebrew, which I knew fairly well. For a time, I worked with an informant and applied methods of structural linguistics as I was then coming to understand them. The results, however, seemed to me rather dull and unsatisfying. Having no very clear idea as to how to proceed further, I abandoned these efforts and did what seemed natural; namely, I tried to construct a system of rules for generating the phonetic forms of sentences, that is, what is now called a generative grammar.* (Chomsky 1975:25)

Chomsky’s intellectual frustration with (an extremely easy version of) standard fieldwork led indirectly to some of the most important developments in the 2000 years history of the study of language, so I am hardly complaining about the direction Chomsky decided to take. Nevertheless, the very intellectual vigor and power of Chomsky’s subsequent work sufficed, in my view, to pull most linguistics students and departments away from the traditional emphasis on field research to theoretical work on, for the most part, the linguist’s native language. Though there is nothing inherently anti-fieldwork in Chomsky’s research programme, his attitude, as expressed in the passage just cited, and his rejection of the intellectual priorities of Boasian linguistics led to an abandonment of fieldwork in the US and a nearly five-decade neglect of the study of indigenous languages and fieldwork throughout the linguistics world,

as his influence soon became massive and international. Over the past decade, as the spotlight has begun to shift to fieldwork once again, it has been primarily concerned with the study of endangered languages (see 5 below) and has not yet recovered the ‘Boasian imperative’ of coherent, integrated fieldwork. This is unfortunate and one hopes that we will continue to make our way ‘back to Boas’. Perhaps my reasons for this statement can be better understood by means of an example. I will now turn, therefore, to what I hope will one day be a new trend of ‘Ethnogrammatical studies’.

## 2. Coherent fieldwork: Ethnogrammatical studies

In his 1921 book on language, Sapir (p. 172) talks of the need to understand the ‘genius’ of each language. By this Sapir refers to that which makes each language unique, the essential core of a language, that part less subject to historical change (a sort of Heraclitus-inspired question of what changes and what remains). Judging by his intellectual output, Sapir was always concerned with coherent fieldwork. His concern was with difference, the *relative* value of a given language, as opposed to it as merely an exemplar of one setting for an absolute UG. One good example of what I mean is found in a study he undertook of Nootka (Wakashan, Canada) consonant alternations. In this language, as Sapir (1915: 181) observes, there are extremely interesting consonantal alternations that cannot be explained grammar-internally:

*It is possible and often customary in Nootka to imply in speech some physical characteristic of the person addressed or spoken of, partly by means of suffixed elements, partly by means of ‘consonantal play’. Consonantal play consists either in altering certain consonants of a word, in this case sibilants, to other consonants that are phonetically related to them, or in inserting meaningless consonants or consonant clusters in the body of the word. The physical classes indicated by these methods are children, unusually fat or heavy people, unusually short adults, those suffering from some defect of the eye, hunchbacks, those that are lame, left-handed persons, and circumcised males.*

Sapir exemplifies this ‘consonantal play’, concluding that to understand the grammar of a language, we must therefore understand the culture in which that grammar is found. Sapir’s study of Nootka is well-known, of course. But perhaps it has failed to exert modern influence because it is considered to be a marginal example. In fact, I believe such situations to be fairly common. So let

me give some other examples, this time from my own research, which buttress the case for coherent fieldwork.

As first pointed out in Everett (1982), Pirahã (Amazonian language isolate) has two rare sounds, one of which is found in no other language. These sounds are [B] and [L,]. The former is a voiced bilabial trill and the latter is a lateral-apical double-flap. These sounds are allophones of /g/ and /l/, respectively and, according to Everett (1979), derive historically from \*d and \*l. The special interest of these sounds for our present discussion is that they are not used in the presence of non-Pirahã-speaking outsiders. This means that (i) Pirahã speakers are able to control sub-phonemic elements (a bit problematic for traditional views of the phoneme) and that (ii) Pirahã phonology cannot be fully described or understood without a knowledge of how it interacts with culture. There are other examples from Pirahã phonology. Let me present two of the strongest, in ascending order of importance for coherence.

Pirahã women have a different phonetics and phonemics from Pirahã men. Pirahã women manifest a smaller articulatory space than Pirahã men. In general, females' points of articulation are retroflexed compared to men's and the 'guttural sound' one associates with their speech is the result of contracting the walls of their pharynx. Further, women's speech has one phoneme less than men's: where men's speech has both /s/ and /h/, women's speech has only /h/ where men would have /s/ and where men's would have /h/. A full statement of the phonetics and phonology of Pirahã must, therefore, include gender-based differences and would be seriously incomplete without this additional data. A formal phonology would not care about this, but coherent fieldwork.

But, one could ask, aren't these first two Pirahã examples just run-of-the-mill sociolinguistics? I would say 'not quite', though in fact, I did have something more ambitious in mind for this section. If extragrammatical considerations could in principle play a causal role in phonological structures (not merely selecting them but forming them, to use a distinction made in Everett 1994), how would our conception of phonology change? What would constitute a 'causal role' for these factors in the phonology?

Here is a possible scenario. Imagine that a language could have various systems/modalities of sound structure, beyond its phonetics and phonology. And then consider the possibility that one modality can affect another, but not necessarily via constraint-rankings or rules, the standard devices of phonological theory proper. If so, then to understand the sound system of language, L, at any level (e.g. 'what happens' or 'what native speakers know when they know the sound system of their language') we must look carefully at the modalities of expression made available via an ethnography of communica-

tion and not merely at a supposed universal formal apparatus. Corollaries of this scenario might include, for example, the appearance of new roles for old constraints (e.g. mode-faithfulness of segments being highly ranked to mark syllable types; syllables are maintained, a form of prosodic faithfulness, in order to parse the larger speech stream, not merely to enhance the perception of segments; and thus arguments for syllables may go beyond phonotactics and segmental enhancement and the syllable may have roles not envisioned by the so-called ‘phonological hierarchy’). If this were true, the coherent fieldwork would evolve from a curiosity or desideratum to an imperative. Is there such a case? Indeed. Consider the following facts about Pirahã phonology, beginning with its phonemes.

Table 1. Pirahã phonemes

Consonants ( ) = missing from women’s speech <sup>2</sup>			
p	t	*k	?
b	(s)	g	h
Vowels			
i			o
	a		

The first thing to notice about Table 1 is that the segmental inventory is one of the smallest in the world. The next is to recall that it includes allophonic sounds found in no other language, subject to cultural constraints. The third is that the /s/ is in parentheses because it is not found in women’s speech, but only in men’s.

Though this is one of the simplest segmental phonemic inventories in the world (the women’s inventory does seem to be the simplest known), we should juxtapose alongside this simplicity the complexity of Pirahã’s prosodies. Pirahã’s stress rule is a good place to begin, since it is well known.

This rule, from Everett and Everett (1984), is considered one of the more complex and unusual stress rules in the literature, mainly for its phonological consequences (rather than, say, any difficulty in stating or recognizing it):

- (1) Pirahã stress rule: stress the rightmost token of the heaviest syllable type in the last three syllables of the word.

The phonetic basis of ‘heaviness’ in (1) is just this: Voiceless consonants are always longer than voiced consonants and there are five syllable weights based partially on this contrast:

## (2) Pirahã's five syllable weights: CVV&gt;GVV&gt;VV&gt;CV&gt;GV

Pirahã is a tonal language, as well. But stress, tone, and syllable weight vary independently in the language. To see this, I will just review one simple set of examples, in (3), from Keren Everett (1998). In the examples in (3), tone is independent of stress. ´ = high tone; no mark over vowel = low tone. The stressed syllable is marked by !. There are no secondary stresses (7 = glottal stop).

- |     |    |         |                |
|-----|----|---------|----------------|
| (3) | a. | !tígí   | ‘small parrot’ |
|     | b. | !pigì   | ‘swift’        |
|     | c. | !sàbí   | ‘mean, wild’   |
|     | d. | !7ábì   | ‘to stay’      |
|     | e. | tíí!híí | ‘bamboo’       |
|     | f. | 7ì!tì   | ‘forehead’     |
|     | g. | tì!7í   | ‘honey bee’    |
|     | h. | tí!hì   | ‘tobacco’      |

Thus alongside Pirahã's extremely simple segmental phonology, it manifests an extremely rich set of prosodies. This leads us to ask a reasonable question, namely, does the language exploit this differential complexity in any way? Indeed, as Everett (1985) describes it, Pirahã communication makes crucial use of the CHANNELS in (4), below, where Hymes (1974) defines a channel as a ‘sociolinguistically constrained physical medium used to carry the message from the source to the receiver’. The four principal modalities or channels in Pirahã after ‘normal’ speech are:

- |     |                    |   |
|-----|--------------------|---|
| (4) |                    |   |
|     | CHANNEL            | FUNCTIONS   |
| a.  | <b>HUM SPEECH</b>  | Disguise<br>Privacy<br>Intimacy<br>Talk when mouth is full<br>Child language acquisition relation |
| b.  | <b>YELL SPEECH</b> | Long distance<br>Rainy days<br>Most frequent use – between huts & across river                    |



But does the discovery of channels like this imply any causal interaction between culture and grammar? Or are these channels outside the grammar proper? Notice that these channels rely crucially on the syllable weights and stress rule in (1) and (2) above. So, if nothing else, they help account for what is otherwise an anomalous level of complexity in the stress rule. Yet the facts cut deeper than this. Consider the following example of what Everett (1985) calls the ‘sloppy phoneme effect’:

- (6) tí píai ~ kí píai ~ kí kíai ~ pí píai ~ ʔí píai ~ ʔí ʔíai ~ tí píai, etc. (\*tí ʔíai, \*gí gíai, \*bí bíai) ‘me too’
- (7) ʔapapai ~ kapapai ~ papapai ~ ʔaʔaʔai ~ kakakai ~ (\*tapapai, \*tatatai, \*bababai, \*gagagai) ‘head’
- (8) ʔisihoái ~ kisihoái ~ písihoái ~ píhihoái ~ kíhihoái ~ (alternations with /t/s or involving different values for [continuant] or [voicing] are unattested) ‘liquid fuel’

Pirahã allows a tremendous amount of variation among consonants, though not for the features [continuant] or [voice]. This can be accounted for, but only if we refer to Pirahã’s channels. The ungrammatical examples in (6)–(8) show that the features [continuant] and [voice] are linked in the sense that they may never vary in the effect. Only place features may vary. With no reference to channels this is without explanation. But in light of the channels this follows because [continuant] and [voice] are necessary for stress placement (Everett 1988) which in turn must be preserved in every discourse channel, or the constraint in (9) is violated:

- (9) *Constraint on functional load and necessary contrast* (Everett 1985):
- a. Greater Dependence on the Channel → Greater Contrast Required
  - b. Lesser Dependence on the Channel → Less Contrast Required

Notice that I am not claiming that the absence of variation for different values of [continuant] is predicted by ‘channels’ alone. This case in fact demands that we further investigate the connection between [continuant] [voice]. There is no claim that ethnography replaces phonology! But I am claiming that without the study of channels and their role in Pirahã culture, not even an understanding of Pirahã’s segmental phonology is possible.

Moreover, this type of example is important for the theory of phonology, i.e. as part of UG, if indeed it is (which I doubt). The lesson is just this: as a modality-dependent channel, phonology may be subject to constraints that are (i) language specific and (ii) grounded not only in the physical proper-

ties of the instantiating modality (the phonetics) but also or alternatively on the culture-specific channels of discourse employed. This is a very important result because it shows that the ‘interface conditions’ of the HUMAN COMPUTATIONAL SYSTEM, in Chomsky’s (1995) terms, may range beyond PF and LF, if we define an interface system as a system setting bounds on interpretability for HC<sub>L</sub>. Such examples also show how coherent fieldwork can be useful for theory. Thus not only the fieldworker, but also the phonologist must engage the language as forming a coherent whole with culture. And this in turn means more fieldwork, the reconsideration of old phonological themes, new training for graduate students, new data-bases, and on and on.

It seems to me that such findings also provide support for the proposal in (10), harmonious with Jamesian philosophy:

- (10) The study of ‘Universals’ is no more vital than the study of ‘Particulars’.

This is so because, among other things, if UG is the CORE of linguistic knowledge, the study of particulars leads to knowledge of the perimeter, setting the outer boundaries. This is also consistent with research by Ladefoged and Everett (1996), in which it is found that there are phonetic rarities, particulars, which are simultaneously violations of distinctive feature theory yet not ignorable nor solvable by that theory. That is, that these rarities are particulars with general theoretical import qua particulars. Let us move now to some practical considerations in fieldwork.

### 3. Fieldwork and technology

Any inclusion of technology will date a paper quickly. So I do not intend to spend much time on this. However, there are a couple of things to say in this regard that will be somewhat impervious to time.

The first is that the most important equipment for the fieldworker are talent and training, the former greatly outweighing the latter. And this talent and training will most clearly manifest themselves in the linguist’s five senses (mental/physical data input devices) and her ability to interpret the results she gathers (mental data-processing). Does she think and read regularly about other languages? Does she have a well-developed ability to distinguish segments and prosodies? A talent for language-learning? An enjoyment of the exotic? A strong constitution? Ability to learn and teach with patience and clarity? These are by far the most important toolkit the field linguist will possess.

But technology is also very important. In my experience machines have been invaluable in helping me to notice sounds and patterns which my unaided ears had missed. Consider, for example, the significance of the portable cassette tape-recorder for the history of field research. It is true, trivially, that early fieldworkers got by without this device, just as everyone gets by without inventions yet to come, but wouldn't it now be priceless to listen to audio tapes or watch video tapes made by Sapir, Boas, Newman, and others, checking their facts and interpretations more carefully, or possessing a more complete record of the languages they studied? As we recognize the need to study, for example, endangered languages, technology capable of accurately preserving and measuring the sights and sounds of these languages becomes ever more important.

The questions to ask with regard to field-equipment are:

- i. Who will be able to use the output of your equipment now and in the future?
- ii. Is the equipment portable?
- iii. Does the equipment provide state-of-the-art accuracy, or as close to it as the fieldworker can afford?
- iv. Will the equipment help record both the grammar and its cultural matrix?
- v. Does the equipment use a practical power source for the location in which it will be used (such as solar power)?
- vi. Does the fieldworker's equipment include satellite-based communication equipment, for email and phone contact from any part of the world to any part of the world?

Point (iv) may seem strange, but it can be taken as a reason for using, in today's terms, high-quality camcorders in the field, rather than relying exclusively on audio recordings. It is also a reason to use portable computers in the field which have state-of-the-art video and audio editing capabilities (e.g. the Mac G4 laptop in 2003). In purchasing and planning, remember that quality is not something to be overly economical with – pay top prices if necessary to get top equipment. There are other areas to be frugal in, if that is necessary (and of course it always is).

Now let us turn to consider data-preservation and digitization when processing one's data back from the field, at one's home institution. This has become a vital issue in Twenty-first century fieldwork.

#### 4. Web-design, digitization, and data-preservation

Before travelling to the field, the field researcher and all members of her team (which may include no one else but the lone fieldworker) should be trained in the field-collection of audio, video and pictorial primary data and determine the form of the notes and metadata to be associated with the collected data in the data base, e.g. Name, Tribe, Dialect; Gender; Age; etc. Any less-experienced members of the field team should also practice data-collection and field-analysis prior to the fieldwork seasons. Once in the field, members of the team should follow their pre-agreed upon plan to collect, transcribe, and conduct preliminary analysis of captured data. Unprocessed data is almost always useless away from the field situation. It is also vital that the team ensure that all collected data are secured and backed up to alternate media, e.g. DVD s and copied onto each team member's computer in the field (i.e. each team member's laptop should contain a full copy of the entire team's data). Returning to the home institution the data should be further backed-up, preferably using institution-wide resources that enjoy a long-term commitment of the institution's resources and administration.

In today's world, it is important that the field researcher attempt to make her research results (at some stage of development, not necessarily including raw data) web-accessible. The planning process for this should include at least the following: (i) a list of desiderata for a preliminary website (e.g. data-retrieval, video-audio coordination on the website); (ii) a plan for the participation of a web-programmer in the site's design; (iii) a discussion or answers to questions like the following: (a) how can data be optimally accessible to linguists and other users via the Internet; (b) how the underlying data-base is best structured and constructed; (c) a consideration of how web-based users will interact with the site; (d) a determination of the efficacy of different kinds of material to be made available for retrieval from the web data base; (e) how best to link video, audio, and labelled files to the orthographic and phonetic transcriptions of the texts collected, etc.

Finally, in designing the final site, the field researcher/team should take a 'story-board' approach to experiment with design ideas, also inviting other university staff, colleagues, and students to participate. Presentations and discussions should be used to discover usability errors or conceptual design flaws before the latter implementation stages. At each stage of development of ideas on web-preservation expected outcomes should be clear to the field team.

These planning stages will then be followed by actual site construction. During this phase the research team, in conjunction with the web-

programmer, will: (a) construct the underlying database; (b) populate the database from analyzed field data; (c) construct the website front-end working from the agreed story-board; (d) program the agreed methodologies for the extraction and viewing of the underlying data; (e) conduct usability trials on the prototype system with interested parties (e.g. linguists experienced in web design and/or field linguistics); (f) consult with a wide-range of experienced experts to ensure best-practice and comply with emerging international standards in web-archiving of field data, e.g. the Open Languages Archive Community (OLAC); Electronic Metastructure for Electronic Languages Data (EMELD); Oxford Text Archive (OTA) and others.

It is also important to consider something often referred to as ‘data-development’. What kind of data will the field research collect? How will this data be processed in the field? How will the data be processed after the field experience? For example, some types of data from my own research include the following: (a) Speech: Digital tape in AU/WAV formats; (b) Video: Digital tape in Apple ‘QuickTime’ format; (c) Pictures: Digital images in PNG format; (d) Metadata related to the above data.

In my research, I attempt to pre-process field data along the following lines: (i) prior to a first team trip, team members agree on an initial system for annotating and tagging collected data, following standard linguistics field guides and my own previous experience; (ii) in the field, data collected will be checked with multiple native speakers. All video and audio recordings are downloaded onto each team member’s computer and annotated, summarizing analysis and discussions among team members. All these annotations and data are also copied to DVDs, two copies made of everything. Audio files are analyzed using PRAAT phonetics software or its equivalent. This analysis will be especially important for developing and testing hypotheses in the field, as well for training team members. For example, by viewing acoustic measurements of the data in the field, the research team can more effectively train members’ hearing to distinguish between unusual (i.e. non-Indo-European) combinations of tone, intonation, and stress placement; (iii) transcriptions of data are first done longhand in indelible ink in hard-backed field notebooks. These are then keyboarded and copied to each member’s computer and then to DVDs (it is useful to do this for various reasons. An important one is that it is always useful for field researchers to handle and process the data multiple times to better control it. But fieldnotes in longhand also provide for creating symbols not available on computers initially, and can be done when not near one’s computer, as often arises in field situations). These transcriptions will all include metadata, tagging them to the appropriate audio and video files (e.g. which

computer file they are in). Digital photos are made of each informant/language consultant and imported into the Word.doc files of the appropriate text transcription (or their equivalent); (iv) at my home institution (the University of Manchester), all data are copied to the Linguistics Department server. DVD copies are also made and then ideally would be made available to the appropriate institutions of the country of research, e.g. for Brazil, the Museu Nacional in Rio de Janeiro. At the home institution, initial field transcriptions are expanded to include as much of the phonetic nuances as possible, up to the point that members of the team to have reached the 'point of diminishing returns' relevant to the project. Sound files are also analyzed in more detail, again using an advanced acoustic measurement program, focusing on prosodic features in the case of my own research. Video files are studied and discussed, e.g. as to how hand gestures, facial expressions, and other features (might) correlate with grammatical and cultural features of the texts being told. A secretary is appointed at all group meetings in which we discuss the data and metadata. This secretary distributes minutes of these meetings to team members, maintaining copies on, say, a computer server at the level of the department or the university. Tagging of texts, audio files, and video files will include any special/additional information agreed upon by the team.

Attention must also be given to then assessing the final product and ensuring that it continues to serve the agreed-upon purposes of the particular language project, e.g. (i) transcriptions of written texts downloadable for each text, both separately and along with their supporting audio (and/or) files. Readers might also have the options of downloading with the transcriptions other information, e.g. ToBI labelings (i.e. any combination of files, including downloading only audio or video files); (ii) each individual sentence of a transcribed text should ideally be downloadable individually, along with its supporting sound file and various annotations/labelings; (iii) some thought should be given to a general constraint to serve all interested parties with Internet access, including many in the third world with slow, dial-up connections. Therefore, during the pilot-webpage development phase, the field researcher should be seeking feedback from all users to design a future site that can facilitate use by both high-end and low-end technology users. These are, of course, just a few ideas and their implementation and nature will vary dramatically from project to project.

## 5. Field research and language endangerment

There are nearly 7,000 languages spoken in the world today. Many of them will disappear beneath the waves of time without being documented or described in any depth. This is a tremendous loss. But there simply are not resources or personnel at present to cover all of these. Field research is still a minority activity in international linguistics. Even in anthropology it has lost its luster.

This seems a tremendous step backwards, given the history of our discipline, at least in North America. One might plausibly claim that North American linguistics began from the interest in documenting and describing endangered languages that was temporarily lost with the ‘withering of fieldwork’ mentioned in section one. In any case, the use of the phrase ‘endangered language’ is a fairly recent development. Its history can be traced from the early 1990s.

Appropriately, the term seems to have been introduced into modern linguistics by Kenneth Hale. At the 1991 annual meeting of the Linguistic Society of America, Hale organized and led a session on ‘endangered languages’, which led to the founding of an LSA Committee for Endangered Languages and their Preservation, in 1992, which was initially chaired by Michael Krauss. From 1993 on, the LSA meetings have included special sessions on field reports, focused on endangered languages. The Linguistics Association of Great Britain (LAGB) held a workshop on endangered languages at its 1993 Fall Meeting, with participation from Nigel Vincent, Greville Corbett, Alastair Walker, and Colette Grinevold (Craig) and has since emphasized the study of endangered languages. Since the early nineties, other meetings and organizations have come into being, with support from private foundations, specific governments, and UNESCO. It is probably fair to say that the most significant development has been the founding of the Endangered Languages Documentation Project (and other initiatives) at the School of Oriental and African Languages in London, under the direction of Peter Austin, with funds from the Lisbet Rausing Charitable Trust.

But in this paper I am less concerned with history than with the role of endangered languages research in the ethos of fieldwork. On the one hand, I am an active proponent of the need to document and describe endangered languages. But on the other hand, I do not believe that one should take on field research for this reason alone. Coherent fieldwork calls, again, for fieldwork that resonates with one’s total life experiences, not merely with a particular, externally-pressured objective. What is it that the fieldworker herself is most passionate about in fieldwork? That is what she should undertake to study. ‘Sal-

vage operations' per se are not the only nor even the most significant reasons for taking on the responsibility of fieldwork.

## 6. Field research desiderata

So what are the desiderata of fieldwork? Let me suggest just a few, with others to be filled in or substituted, according to one's own life experiences. First, and foremost, is that the fieldworker see her commitment to fieldwork as a human connection between herself and the people with whom she will work. This connection entails friendship, mutual support, and assistance in many ways. Naturally, because there is usually a power-differential in the relationship, favoring the fieldworker, the fieldworker may find herself offering more help than she might have ever anticipated. Let me give some examples. The fieldworker may be called on to provide medical assistance (and should, therefore, include first-aid and basic medical knowledge in her basic training – as can be found, for example, in the excellent book, **Where There is No Doctor** by David Werner). Or she may be asked to help arrange legal aid (e.g. for landrights), to represent or accompany representatives of the people to local or national government offices/officials, etc. or to bring manufactured articles otherwise unavailable to the community (e.g. fishhooks, ammunition for hunting, bicycles, etc.). The fieldworker unwilling to make this kind of commitment has not recognized the significance of pragmatist, coherent fieldwork. People are not library books to be consulted and then returned to their shelves when the fieldworker is done with her research.

But I also believe that it is desirable to undertake fieldwork for the purpose of advancing linguistic theory. This is certainly the principal scientific motive for fieldwork in my opinion. In my own case, I undertook fieldwork as a linguist to, I thought at the time, understand the nature of the human mind. On the other hand, each person can legitimately be motivated by her own theoretical concerns, or not. Field research could also model what pragmatist science is about, i.e. illustrating the principles of coherence, usefulness, and radical empiricism. This would be a marvellous object lesson, it seems to me, for scientists generally, and linguistic theory in particular.

Finally, let me inject a 'corny' desideratum here: field research should promote love and world peace. There is no better way I am aware of for developing meaningful relationships across cultures than by fieldwork, for showing, promoting, and developing love and respect and passing this on to one's home institution through training and example, as well as to the many people con-

tacted in the course of fieldwork. That is a worthy goal, perhaps the most worthy. It is certainly in the spirit of William James. And directly connected to this and all the other worthy goals and desiderata of field research is the fundamental task of training new generations of fieldworkers to continue on. This really is an area of science in which it makes sense to establish apprenticeships, where future researchers learn by example and doing from their mentors. A fieldworker will, I hope, also learn to love the specific and distrust (though not reject) the general, to think relative and to doubt absolutes.

I ascribe truth to none of the above. But I hope it is all useful. Peace.

## Notes

\* I would like to thank Caleb Everett, Joan Baart, Sally Thomason, Bob Van Valin, and David Weber for comments on earlier versions of this paper.

1. One should mention the very important role of the Summer Institute of Linguistics, through its leading linguists, e.g. Kenneth Pike and Eugene Nida. SIL has probably documented and described more endangered languages and fieldwork situations than any other organization in history. I do not mention it in the text, however, because its role and activities have been parallel to and outside the mainstream of scientific concerns, due to its connection with the missionary organization, Wycliffe Bible Translators. Nevertheless, its role should not be overlooked.
2. \**k* is somewhat problematic. It seems to be a phoneme, but in most of its appearances it can be analyzed as a portmanteau realization of /h/ + /i/ or /h/ + /u/. See Everett (1979) for details.
3. The length of the notes is determined by the relative lengths of the syllables, as is the height of the notes. The wedges under the lines indicate stress. The values are CVV = whole note; GVV = dotted half; VV = half; CV = dotted quarter; GV = quarter.

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# State of the art in Computational Linguistics

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## 1. The position of Computational Linguistics

Computational Linguistics has a long history, dating back to the Fifties, during which it developed a whole set of computational models and implementations, theories, methodologies and applications. It is difficult to give a sensible account of its present state without going back a little to the main steps through which this discipline evolved. Since its origins, Computational Linguistics has been in an intermediate position between Computer Science and Artificial Intelligence, Linguistics and Cognitive Science, and Engineering. Computer Science itself shares its roots with Computational Linguistics; *parsing*, which is central for the design of compilers for programming languages (Aho & Ullmann 1977:6), is also the building block of any natural language processing engine, and both are the realizations of the Chomskian theory of formal languages (Chomsky 1957). The same theory, together with the corresponding computational model, has given a contribution to the general hypothesis of Artificial Intelligence, that human behaviours usually judged intelligent could be simulated on a computer in a principled way. Oversimplifying, Artificial Intelligence aims at modelling a number of behaviours through three very general paradigms, theorem proving, problem solving and planning, and language understanding and production. The history of both disciplines is rich in intersections, especially between language processing and planning, as in SHRDLU (Winograd 1971) or, more recently, in ARGOT (Allen et al. 1982; Allen 1983), with all its practical and theoretical follow-ups; modern dialogue systems in all their forms and applications are derived from the dialogue model J. Allen designed for ARGOT.

Artificial Intelligence and the relevant part of Computational Linguistics share a commitment to “simulation of behaviour” and also to “cognitive modelling” of different human behaviours, including the use of language.

This is probably one of the reasons why Linguistics appears in the set of sciences originally interested in the dawn of the new discipline called Cognitive Science ([www.cognitivesciencesociety.org](http://www.cognitivesciencesociety.org)).

Since the Seventies, when language technology reached a state of maturity allowing the realization of some applications, Engineering has been interested in some of the language processing techniques, and it soon became apparent that the approach introduced by engineers might on the one hand be of lesser interest from the point of view of theory and cognition, but more effective in many ways. By now, we can say that while Computational Linguists were, and are, more interested in the correctness and plausibility of their models, Engineers were, and are, more interested in the usability of tools and techniques, even if this entails the risk of “dirty” solutions. The history of Computational Linguistics in the last few decades is much the history of the evolving relations between all these conjuring approaches.

## 2. Main achievements

It has long been mooted that Computational Linguistics, like Artificial Intelligence, created a number of models, programmes, and prototypes which claimed a lot but did nothing at all. This is a sweeping statement, but it may contain a grain of truth. The suspected chasm between theory and practice might have been due to the constraints imposed by sponsors and funding agencies. Nevertheless, despite various impractical and even failed attempts, some fields of Computational Linguistics made good headway in producing stable technologies, as a preparatory step to engineering. The following paragraphs will analyse the main areas in which some achievements have been made.

### 2.1 Parsing and parsing technology

Parsing is probably the field of Computational Linguistics where some results have been achieved since an early stage. The first parsers bearing some relation to grammatical theory go back to the early Seventies. The common background knowledge that made such a development possible is Generative and Transformational Grammar, but its computational interpretation gives rise to a number of different models, with different impacts both on a technical and theoret-

ical level. The key problem to solve is to reach a logical (deep) structure of the sentence, in order to deal with the need to map the sentence onto some sort of semantic (executable) representation. Transformational grammar does not offer a direct solution to this problem, leaving a free space to several interpretations. The most orthodox of these interpretations (Friedman 1971) proposed a parser that, after having produced a surface tree, by means of a context-free grammar, tried to yield such a deep structure using the same set of transformations proposed by Chomsky, inverting input and output.

In fact, the most successful solution was the one offered by ATN (Woods 1970), as not only was it a good answer to the question of dealing in a single process the mapping of a surface sentence/structure onto the corresponding logical form, but it also gave rise to a number of grammatical (Kaplan 1973, 1975; Kaplan & Bresnan 1982) and psycholinguistic (Kaplan 1972; Wanner & Maratsos 1978) debates. This was also the prototype of an entire family of natural language grammars for parsing, called Augmented Phrase Structure Grammars (APSGs, see Heidorn 1975); in general APSGs are ordinary context-free grammars, augmented with instructions to store the already reached (sub)trees and to carry linguistic tests.

From a plain computational point of view, the way rewriting rules are stored, accessed, and utilised identifies different groups of algorithms (Early 1970; Kuno 1966; Kasami 1965 etc.), and this is the basis of the parsing theory and practice in computer science.

From a natural language point of view, starting from these achievements it has been possible, over time, to develop further (computational) theories (see below §4.2.2.1), as well as grammar engineering practices (Bates 1978), or natural language parsing best practices (Tomita 1986, 1987).

Turning to the application side, when facing the problem of parsing real natural language, some inadequacies of axiomatic grammars became clearly apparent. For instance, people rarely use sentences, in the proper sense, but rather utterances which may be elliptical or really “erroneous”; some solutions have been tried, falling into the category called “treatment of ill-formed input” (Kwasny & Sondheimer 1981). Also, human language consists of sentences/utterances connected with one another, and such connections are realised by some pro-forma, like pronouns that represent nouns, but also sentence fragments; this is the “reference resolution” problem, that gave rise to a number of techniques for treatment (Sidner 1981, 1983; Grosz 1977; Grosz & Sidner 1986; Grosz, Joshi, & Weinstein 1983). In general, however, most of the proposed solutions failed to become technological achievements.

From the point of view of the structure of algorithms, grammatical theories like GPSG (Gazdar et al. 1985) or LFG (Kaplan & Bresnan 1982) have led to the adoption of a model in which semantic interpretation goes together with syntactic parsing. This idea of a direct correspondence between syntactic and semantic rules comes from the Montagovian approach to logic grammars (Montague 1973), which relied on a categorial grammar. A first attempt to associate semantic rules with context-free and transformational grammars was carried out by B. Hall-Partee (1973), but GPSG, in its early formulation (Gazdar 1982), makes this association systematic. LFG does not impose any constraint on the nature of the semantic rules, but makes the assumption that such an association always takes place. At present, the direct mapping of sentences onto a semantic structure is taken as a standard both in logic terms, in reference to the DRT model (Kamp 1981; Pinkal 1985; Kamp & Reyle 1993), and in practical applications, as embedded in (spoken) dialogue systems (see Dahl 2000).

Now, all this knowledge, technical know-how, models, programmes is the core technology on which any natural language application is based.

## 2.2 Interaction models

Since the early Seventies, natural language has been viewed as the most natural way of interacting with computers, capable of breaking the barriers of man-machine interaction. The notion of “naturalness” has evolved through the years: at the very beginning it seemed that the simple possibility of using English sentences was more natural than using some computer language; later it appeared that the “naturalness” of natural language was defective if some “tricks” of natural language are not adopted, like anaphora or paraphrase of an utterance; finally a real “blue-print” for graceful interaction was dictated, which included a number of ingredients for “naturalness” (Hayes & Reddy 1983). At the end, this notion of “naturalness” was overcome in the Eighties and Nineties by the introduction of easy graphical interfaces; nevertheless, natural language interfaces have caused a number of problems of interaction, some of which have larger import than plain natural language.

The most relevant result is the introduction of “dialogue”, i.e. the model of (natural language) interaction based on pragmatics and the inference of the user’s intentions (Allen 1983). The concept underlying this approach is that human-human interaction is part of a more general “cooperative planned behaviour”. An agent has a plan to reach some objective, but, if she/he meets an obstacle, she/he can call for help; among the obstacles, a very serious one is

the lack of the necessary information to carry out the original plan, and the call for help can take the form of a request for information. Thus, the agent that responds to the request of information often does not answer the question in its literal meaning, but the intentions of the calling agent which the respondent infers from the question itself. The plan, the planning activity and the plan reconstruction are represented in the *planning* formalism, well known to Artificial Intelligence, while Searle's theory of *Speech Acts* gives the basis for the inference of the meaning of an utterance underlying its propositional content. This "marriage" between Speech Acts and Planning has probably been the most theoretically original result in Computational Linguistics, and it gave birth to a set of researches in the field of natural language interaction in terms of planned behaviour (Cohen & Perrault 1979; Allen & Perrault 1980; Allen & Litmann 1986; Carberry 1999), but with the spreading of Spoken Dialogue Systems (Peckham 1993; Smith & Hipp 1994), these models became of practical relevance and are again a paradigm of interaction. On the other hand, the assumption that any given speech act allows a specific set of inferences (as in Traum 1993) promoted the idea that there is a strict correlation between the rhetorical features of an utterance – referred to as a dialogue act or discourse act, identifiable by a rhetorical label – and the set of inferences this utterance allows. By this conceptual bridge, Allen's theory of dialogue stands also at the origin of all the activities of pragmatic tagging of corpora (see §§4.2.1 and 4.2.4).

### 2.3 Morphology and dictionaries

Until the Eighties, natural language processing programmes used "built-in" dictionaries of a few thousand words. This was seen for a long time as the only solution to the problem of POS-classifying words before parsing. However, the small number of lexical items available and "searchable" was regarded as a strong limitation to the power of those prototypes.

The problem of finding a word in a list (file, database) of large dimensions could be seen as a plain technical problem of search optimisation. Nevertheless, the idea of making a lexical system able to predict incoming forms motivated some research on the morphological approach to dictionary look-up since the Eighties (see Byrd 1983; Byrd et al. 1986). The typical morphology-based look-up system consists of a set of dictionaries of lexical segments (bases, prefixes, suffixes, endings) and a set of rules of word formation, consisting of two parts, composition of segments and phonetic modifications.

The (casual) appearance of highly inflected languages on the stage of computational treatment showed the weakness of existing lexical analysis systems, mostly designed for English. It is not the case, probably, that the present standard for morphological and lexical analysis comes from a Finnish environment (Koskenniemi 1983). This is based on the above sketched framework, but the form of the (phonological) rules is particularly sophisticated, as it deals with complex morpho-phonetic phenomena.

## 2.4 The treatment of corpora

The treatment of corpora goes as far back as the origin of Computational Linguistics as Machine Translation. The first project of building a computational lexicon of the works of Saint Thomas (Busa 1980) dates to the end of the Forties. From this project a number of large projects for the acquiring and the treatment of corpora for different languages was encouraged. Following this initiative, many collections of (literary) corpora were started, such as the *Trésor de la Langue Française* started in 1971, and computerised since 1977 (see <http://atilf.inalf.fr/tlfv3.htm>), the Brown Corpus since 1963 for American English (see <http://www.hit.uib.no/icame/brown/bcm.html#bc3>), or the COBUILD for British English, started by Collins around 1980 (see <http://titania.cobuild.collins.co.uk/>).

In the early stages, much attention was given to the acquisition and coding techniques (how to code all the existing characters and diacritics, how to represent textual references), to the context delimitation techniques (how to define a context, how long should it be etc.), to the output procedures (how to print out special characters or diacritics). Obviously, all these problems were overcome with the introduction of mark-up languages, which allowed the definition of any subset of characters, while the introduction of high resolution screens, graphic interfaces, and laser printers made the representation of any kind of character easy. Thus attention moved from technical problems to more substantial problems like the techniques for acquiring and processing linguistic data, and the standards for tagging and mark-up (see §4.2.4).

## 3. From prototypes to engineering

One of the main objections to traditional Computational Linguistics was, and often still is, that it has produced a lot of theoretical work, but no practical application at all. Engineering is still a phase to be reached. The causes of this

situation have been, in general, recognised to be a general lack of empirical knowledge and, going farther, the lack of automatic learning processes.

The first argument goes the following way: Theoretical “Chomskian” linguistics deals with “competence”, whereas our programmes deal with “performance” phenomena. This causes two inadequacies; (i) what we know about language is based on highly theoretical assumptions, but it has little or nothing to do with the linguistics of “real language”, and (ii) even if our knowledge should be valuable, it would cover a smaller number of phenomena than “real language” would present.

The second, more serious, argument runs as follows: no theory or programme will be able to model the whole linguistic knowledge necessary to both analyse any text that is presented and keep aligned with language innovation. The only system capable of this is one that acquires documents and learns new linguistic structures from them.

Both trends have led to a revitalisation of corpus linguistics. The main features of this activity are:

- the attention given to the standards of the collection of corpora, be they written texts or spoken samples. Linguists have always been very careful in setting the principles for the collection of their data, but use in computational applications puts forward problems that have never arisen before, like the use of a standard for document exchange
- the development of methods to prepare the materials, like annotating them
- the development of a number of techniques for the inference of linguistic features from the corpus. These are methods based on the ordinary automatic learning techniques, such as, for instance, Hidden Markov Models, statistical methods, or even neural networks (see §4.2.3).

The emergence of these new trends does not imply that more traditional research objectives have been dropped; simply, priorities have been changed. Thus theoretical and formal studies are still carried out with less effort than in the early stages of Computational Linguistics, while the study of techniques for the treatment of massive linguistic materials acquires a high priority.

#### 4. Computational Linguistics, theoretical linguistics and corpora

Having examined the major trends of research and application, we can finally give an account of the present state of Computational Linguistics. This will run in two directions, a description of the major application areas, in order to

explain their impact on the research trends, and a brief account of the most popular research objectives and methodologies.

#### 4.1 Application areas

The studies in Computational Linguistics have always been affected in some way by the nature of prospective applications. Thus, when human-computer interaction in natural language was the focus of attention, sentence processing and dialogue models were extensively studied. It is, therefore, very important to have a notion of what are the presently preferred application domains.

##### 4.1.1 *Spoken dialogue systems*

The study of dialogue structures and dialogue modelling has been one of the research themes since the early Eighties, and different paradigms have been developed, most of which dealt with typed input and output. By the end of the Nineties some vocal services had been activated, especially in Europe, in which vocal input is interpreted by a signal processor and a parser, while the answer, also in vocal form, is planned by a dialogue manager module according to a number of contextual parameters. There are some systems that can be considered a sort of milestone, such as SUNDIAL (Peckham 1993), Dialogos (Albesano et al. 1997), or the DUKE University system (Bierman et al. 1993).

These systems have a large application in transaction situations, like itineraries, train, flight, or hotel reservations. The main features of these systems are a robust speech recognition component, a simple parser that performs semantic interpretation of the input, a dialogue manager that acquires the interpretation of the input and, depending on a number of parameters, decides whether it is necessary to ask the user to complete the parameters of her/his query, or, if they are sufficient, the query can be processed by the back-end component (data-base). The reactions of the dialogue module are translated into sentences and hence into speech utterances by a speech synthesis module.

Spoken dialogue systems integrate technological developments of early dialogue systems (see §2.2), based on a simplified theory of speech acts, then dialogue acts, and corpus techniques, largely used to infer dialogic rules. These applications are mostly responsible for the creation of dialogue corpora, annotated in terms of dialogue or discourse acts (see <http://www.sigdial.org> for a relatively exhaustive list of such initiatives).

#### 4.1.2 *Multilingualism*

Machine translation has been one of the first, if not the first application domain of computational linguistics. This has been experimented on in several areas, like military message translation, technical translation, institutional document translation. Machine translation has always been intended to be from one language to another, from Russian to English and vice-versa at the beginning, from Japanese to English and vice-versa in the large Japanese projects of the Seventies and the Eighties, or from French to English and vice-versa in Canada. In the Eighties, the European Community started the first project of many-languages-to-many-languages, Eurotra (Raw et al. 1988). Later, it is worth mentioning the large project Verbmobil (Kay et al. 1994), which couples machine translation techniques with spoken dialogue techniques.

The spreading use of the Internet proposes a new dimension of the translation problem. The scenario is the search for some information, or some documents, using a generic search engine; once key-words have been introduced in one language, they can be translated into other languages by means of bilingual dictionaries, and the search can be launched in the target languages. This limits the search to those documents into whose languages the query words have been translated, thus missing possible other documents in other languages. To overcome these limitations, the idea has been proposed that any document and any query is translated into a sort of (universal) interlanguage (Universal Networking Language, also UNL, see <http://www.ias.unu.edu/research/unl.cfm>); the match is carried out on expressions of this interlanguage, rather than on one-to-one translations.

#### 4.1.3 *Document classification and retrieval*

Studies in classification and retrieval of documents start in the field of information retrieval, which has been for years separated from any kind of natural language processing. Many statistical techniques have been tried to establish degrees of relevance of words in a text (see Salton & Buckley 1988), or degrees of relations between couples or triples of words (collocations; see e.g. Sinclair 1987; Sekine et al. 1992; Dagan et al. 1999). More recently, Computational Linguistics tried to propose contributions coming from linguistic analysis. On one hand, the grouping of key-words by concepts has been proposed, using the WordNet approach (see Mandala et al. 1998), on the other hand some (parsing) techniques have been developed to recognise and use phrases as queries (the Cristal LRE Project, see <http://www.ilc.pi.cnr.it/Mac/cristal.html>).

Document classification and retrieval has several applications. Web browsers are generally based on such a technique, but also more local services like

dedicated portals, *pull* information systems, technical information systems and any other service that relies on document management.

## 4.2 Research areas

In order to develop the above mentioned applications, a number of basic techniques and theoretical issues are now being developed. We will sketch, in the following few pages, the main lines of both theoretical and technical developments of research in Computational Linguistics.

### 4.2.1 *Linguistic resources*

By linguistic resources all kinds of repositories where some linguistic piece of knowledge is contained in an explicit or implicit way are meant. The study of the techniques for acquisition, storage, manipulation of information, standards, access, and use of these resources has become an almost autonomous branch of Computational Linguistics, with its own characteristics and a large community of practitioners, as is shown by the LREConference (Linguistic Resources and Evaluation Conference, see <http://www.lrec-conf.org/>), a biennial event that assembles several hundreds of researches. This branch will be dealt with by N. Calzolari, elsewhere in this volume. However I will briefly treat here those subjects that somehow are closer to traditional Computational Linguistics or interact with it.

Although linguistic resources may fall into different categories, the most common ones are dictionaries and corpora.

Dictionaries may take various forms. The most common dictionaries are the general coded dictionaries, where POS and other morpho-syntactic codes are associated to lexical items. There are also a number of special language dictionaries in specific (technical) fields. A very peculiar one is WordNet. This is a sort of “conceptual” dictionary, where words are presented in a structure that accounts for links like synonymy, hyperonymy etc. This dictionary comes from an initiative taken by the psychologist G. Miller in the early Eighties (Miller 1985); the general idea was to present lexical material in a more concept-oriented way. More recently it has been taken to represent real conceptual relations, and it has been tested in many programmes of document classification and document retrieval to group key-words in “ideas”. With this objective in mind, some projects have been started, and some finished, to build WordNets for other languages or multilingual ones. This particular usage of WordNet has stimulated a number of researches aimed at analysing single word classes,

proposing methodologies to carry word disambiguation, or statistically based inferences of different types (see for all <http://enr.smu.edu/~rada/wnb/>).

The other large family of resources are “corpora”. A “corpus” is a collection of language samples stored in specific formats and annotated in such a way as to distinguish different linguistic units. The utility of such large collections of texts is that they offer the empirical base for the inference of different linguistic regularities that may form a rule or a norm. The ground level of such an inferential function is the simple distribution of words; this is the basis of a large enterprise like COBUILD (<http://titania.cobuild.collins.co.uk/>), a corpus in continuous evolution, where texts are stored in a rough format and words and word sequences can be searched through the whole material. However, the most widespread opinion is that texts should be “annotated” or “tagged”. This means that linguistic units belonging to some level of analysis should be assigned labels that classify them. Thus, at the word level, any word is isolated and given the POS-tag, i.e. the label indicating its Part Of Speech and other morphological properties. Higher levels are also possible, like coreference or, more commonly, the pragmatic level, i.e. the discourse segments.

As it has been mentioned before, while POS-tags belong to a more or less stable tradition, discourse-act tagging has been originated and strongly influenced by previous studies on dialogue models. Thus, the DAMSL (Allen & Core 1997) tagging system is the result of a long involvement in dialogue modelling at the University of Rochester, as well as the RSTool by D. Marcu (2000), which is a derivation of the studies carried at ISI in the domain of text generation, not to mention the MapTask (Carletta et al. 1996) initiative, or the Verbmobil corpus (Alexandersson et al. 1997).

Linguistic resources are used mostly to develop learning techniques to be applied to different fields of natural language processing (see §4.2.3).

#### 4.2.2 *Parsing sentences and documents*

Parsing continues to be an important component of any process with natural language as the object. It has evolved in two main directions, quite opposite to each other. On one hand a substantial evolution in grammar formalisms has produced major modifications in the parsing techniques; on the other hand, the need for more realistic and engineered solutions has led to the use of some sort of mass-parsing techniques, based on substantial simplifications of traditional parsing.

**4.2.2.1 *New formalisms and new algorithms.*** Since the introduction of APSGs, it appeared that a neater parsing process should rely on both a structural

(syntactic trees) and a functional (labels) component, which could manage agreement tests (in terms of identity of feature values), “functional” transformations, like passive (in terms of inversion of SUBJECT-OBJECT labels) and other parsing situations. This awareness somehow percolated to theoretical linguistics, where a number of double-sided theories arose, like Lexical-Functional Grammar (LFG – Kaplan & Bresnan 1982), Generalised Phrase Structure Grammar (GPSG – Gazdar et al. 1985), Head-driven Phrase Structure Grammar (HPSG – Pollard & Sag 1994), or plain functional computational models like Functional Unification Grammar (FUG – Kay 1979, 1984, 1985). From a procedural point of view all these theories, FUG excepted, more or less agreed in identifying two main blocks, a structural process, which remains close to traditional parsing, and a functional process, which takes care of computing the values of higher nodes labels as a function of lower nodes labels values (in terms of computing new values or raising lower values). LFG proposed an algebraic system of meta-equations, GPSG proposed a set of constraints on the admissible values, while FUG completely eliminated structural computation, trying to use a single algorithm, called *functional unification*, which directly produced a single functional representation of any sentence.

It is not necessary, here, to discuss the viability of each of these solutions; functional computation has in itself many difficulties, and some attempts to solve them have been made (see Schieber 1986). All the grammars of this group, and the related algorithms, have been classified as feature-based grammars, feature unification algorithms, unification algorithms.

More recently, a general logic for the treatment of feature unification has been developed (Carpenter 1992), and some of the related algorithms have been implemented (see for instance Kepser 1994).

The solution offered by feature-based grammars and typed-feature logic seemed, for a while a sort of definite solution for every parsing problem, but despite its formal clarity and neatness it soon appeared inefficient for the needs that were emerging from the market of natural language products.

**4.2.2.2 Mass-parsing.** In fact, the idea of producing a complete and deep analysis of sentences was losing appeal, as the more promising applications of the end of the Nineties became text analysis, text classification, and any other domain where large quantities of text were to be analysed. In addition, it turned out that for the purpose of identifying some key-words or key expressions, no deep and accurate analysis was needed.

Thus the idea emerged that a complete parsing was not necessary, at least for “real” purposes, but some sort of “surface” analysis was sufficient. This job

is successfully carried out by “shallow parsers”, whenever a syntactic analysis is necessary, or even “chunkers”, where just the identification of some words is sufficient.

Shallow parsers are traditional parsers to which some complexity restriction has been added, especially in those areas where a deep analysis generates ambiguity with no interpretation benefit corresponding to it. An exercise for all parsers could be the PP-attachment problem. By PP-attachment the ambiguity in the interpretation of Prepositional Phrases is meant; in the sentence

John saw the man in the park with a telescope

many interpretations are possible according to whether “with a telescope” is attached to “park” (a park where a telescope is located), to “man” (a man walking with a telescope under his arm), or to “John” (John saw the man by a telescope). This gives rise to an explosion of ambiguities, which cannot be solved unless one knows exactly what happened. But, despite these difficulties, if one has to classify the sentence, or address a query to some search engine, or even translate the sentence into another language, the attachment of the PP does not make any difference. The conclusion is that a deep syntactic analysis introduces more information than actually needed. It is enough to identify the PPs without trying to attach them to each other, and any solution will hold.

In an extreme view, it could be possible just to delimit constituents on the basis of some separators, like articles, prepositions or conjunctions (see Abney 1991). In this case, no structure, even partial, is built, but sentences are simply divided into segments; segments are called “chunks” and this approach is known as “chunking”.

Actually there is no clear cut difference between “shallow parsing” and “chunking” as they simply lie in different locations on a continuous line. In “chunking”, a text is divided into chunks that may belong to the same phrase (but this is to be verified) while “shallow parsing” implies in any case the attribution of some shallow structure, as is the case in those resources known as Tree-banks (see <http://www.cis.upenn.edu/~treebank/>). It is usually believed that this preliminary processing of a text (in both cases) is an intermediate step towards deeper parsing, if it is necessary.

Thus syntactic processing of texts falls into two categories, according to whether hand-crafted patterns or machine learning techniques (see §4.2.3) are used. With the first method, the analysis still relies on some sort of rules, be they relaxed enough to allow chunking; with the second method it is assumed that parsing of large texts becomes feasible without much work on grammar writing, but simply by inferring new linguistic constructions.

**4.2.2.3 *Finite state transducers technology.*** A finite state transducer is an extension of a specific family of automata; essentially it is a finite state automaton that works on two (or more) tapes. The most common way to think about transducers is as a kind of “translating machine” that reads from one of the tapes and writes onto the other. Transducers can, however, be used in other modes as well: in the generation mode transducers write on both tapes and in the recognition mode they read from both tapes. Furthermore, the direction of translation can be turned around: i.e. the expression *a:b* can be read not only as “read *a* from the first tape and write *b* onto the second tape”, but also as “read *b* from the second tape and write *a* onto the first tape”. In procedural terms, translating consists of a set of states and transitions; a specified transition symbol stands for each transition, and associates to it a translation symbol, including the null one.

For many years Finite State Automata, and, therefore, Finite State Transducers, have been considered an efficient tool for the treatment of natural language, but less powerful and accurate than other formalisms such as Context-Free grammars. We are not interested, here, in a computational evaluation of FSTs, but it is a fact that in more recent times they have found a large employ in different areas, such as morphological analysis (see Clemenceau & Roche 1993; Karttunen et al. 1992; Silberztein 1993), following the work by Koskenniemi (1983), lexical recognition (see Pereira et al. 1994), speech processing (see Laporte 1993, 1996), and even syntactic analysis (Pereira & Wright 1996).

The appeal of this approach, started with the “two-level morphology” by K. Koskenniemi, and spread over other areas, is that it is possible to map something onto something else more or less at random. Thus, phonetic lattices can be mapped onto tentative words, idioms can also be mapped onto words, morphologically complex words onto their morphemes (and related interpretations), strings of words onto phrases. This allows an unprecedented flexibility, without missing computational tractability and neatness. Indeed, it seems that from a computational point of view FSTs are more efficient than CF grammars, and related algorithms. For these reasons FST technology is often regarded as the most promising technology for massive natural language applications.

#### **4.2.3 *Acquisition and learning***

Another relevant drawback for the building of efficient and robust natural language processing systems is the lack of flexibility and adaptability of linguistic knowledge embedded in these kinds of system. This is commonly attributed to the fact that it takes a long time and a considerable effort to build linguistic

knowledge sources, like dictionaries or grammars, but they remain unchanged through time, so they run the risk of becoming obsolete at some point; also they are not easily adapted to new kinds of applications. In addition, any single new linguistic item, be it a word unknown to the dictionary, a sentence type unforeseen by the grammar, or an unknown semantic frame, causes a Natural Language Processing System to stop or crash (see Armstrong-Warwick 1993). The answer to this problem seems to be the building of systems able to infer and learn new lexical items or linguistic regularities. In addition, experiments carried out in the field of speech recognition showed that learning approaches, especially based on statistic methods, were highly successful (Stolcke 1997; Jelinek 1998). Also, the collection of large corpora, started in early stages of Computational Linguistics, has produced a huge amount of data that can be used to induce new patterns or train learning systems.

Learning techniques have been mainly applied to morphologic and syntactic analysis (Charniak 1997; Brent 1999; Daelemans et al. 1999; Haruno et al. 1999; Ratnaparkhi 1999), semantic disambiguation (Ng & Zelle 1997; Dagan et al. 1999), discourse processing and information extraction (Cardie 1997; Beeferman et al. 1999; Soderland 1999), and machine translation (Knight 1997).

Natural language Learning studies have been carried out mostly without strict contacts with traditional Machine Learning; nevertheless some of the traditional techniques have much in common with this branch (Brill & Mooney 1997). The basic techniques are those based on statistics: a probabilistic model, such as Hidden Markov Models (HMM) or Probabilistic Context Free Grammars (PCFGs) can be roughly represented in terms of a finite state machine or a production system. Statistical data derived from a corpus can be attached to such models in order to serve as training for them. The corpus can be pre-annotated or not; in the first case training consists just in counting the already marked linguistic phenomena, in the second case some estimation techniques should be used.

Another important method is symbolic learning, which consists in automatic acquisition of rules; this always has a statistical base, but the model is a rule-based rather than a probabilistic one. A variety of this method consists in remembering past examples and making decisions on the basis of similarity to those examples. A third method relies on the neural networks, a computational model of neural cells (Rumelhart & McClelland 1986) that learns from examples.

It is evident, in all three cases, that the relevant role is played by a *training corpus*, i.e. a set of data that are used by the learning mechanism to infer

linguistic regularities in any of the ways outlined. Training can be *supervised*, if the data have been somehow manipulated before training, or *unsupervised* if the data have not been interpreted in advance.

Many research efforts are deployed in this area, although stable technological results are still far away. However, the assumption that the only possibility of improving our natural language systems and reaching a level of engineering close to real applications, makes of this research field, together with the strictly related corpus based (computational) linguistics, the domain to which the majority of efforts is devoted.

#### 4.2.4 *Text and dialogue studies*

As has been anticipated above, discourse and dialogue studies have evolved in two different directions. On one hand, many of the functional or speech-act based theories have been turned into sets of pragmatic tags, as a preparation for the analysis of corpora. On the other hand some of the traditional issues have been tackled with new methodologies.

A commonly accepted result is that discourse has an internal structure, however it is recognised and marked. It is also accepted that the discourse structure can be exploited in different natural language processing applications, such as summarisation, machine translation, natural language generation. Thus, research has been carried out to produce systems that automatically chunk discourse into segments and assign them labels according to some theory of discourse acts, the *discourse parsers*. Many of these attempts have been based on the recognition of the internal structure of discourse (see Kurohashi & Nagao 1994; Asher Lascarides 1993), but they failed to produce reliable results, especially with real texts. Also logical models, like Discourse Representation Theory (Kamp 1981; Kamp & Reyle 1993), could not be applied to large texts, despite their formal elegance and semantic depth. Better results have been attained by using incremental algorithms (see Polanyi 1988; Cristea & Webber 1997).

But the most successful approach to discourse parsing is considered, presently, to be Marcu's algorithm (see Marcu 2000). This is based on the recognition of cue phrases and discourse markers and the use of RST (Mann & Thomson 1988). Segments are split into three levels, *sentence*, *paragraph*, and *section*, and the rhetorical relations holding between them are hypothesised on the basis of the corpus analysis of the cue phrases. Then the trees, for each level of granularity, are built according to RST. The final discourse trees are built by merging the trees corresponding to each level.

A general issue strictly connected to discourse processing is discourse coherence. This is a strong theoretical issue, also connected to discourse parsing, as discourse segments are characterised and identified by coherence relations. In this field two basic approaches have been developed, Centering Theory (Grosz, Joshi, & Weinstein 1995) and Veins Theory (Cristea et al. 1998). Both conjure to improve discourse parsability, also providing methods to understand anaphora and other forms of co-reference.

Approaches to discourse and dialogue do not appear systematic, but rather as a collection of solutions for different issues. The original systematic and coherent studies have shown that the phenomenon is much more complex than one may have originally thought, and several different aspects are being studied now. However, it is to be seen as a high priority topic, as the large amount of (textual) documents available in the Internet demands intelligent, flexible and effective programmes for text retrieval, text summarisation, and content extraction.

## 5. Recommendations for linguists

Computational Linguistics has often been divided into two extreme positions. On one hand the community of researchers involved in lexicography and computational treatment of texts have developed tools for the treatment of large amounts of linguistic data, as well as linguistic studies based mostly on statistics. This was true in the Sixties and early Seventies, when text processing was based on punched cards, the software definition of special characters and diacritics, and printing chains for those special characters, and is true now, when texts are marked-up and standardised by the use of exchange languages like SGML or XML. The results of this trend of studies tend to be probabilistic.

On the other hand, the community of researchers involved in syntactic analysis, semantic interpretation, pragmatics, dialogue modelling, and discourse planning tend to look for regularities that can be expressed in rules that can generate corresponding behaviours. Sometimes these rules have been offered by theoretical linguistics, as for syntactic parsing, or logic, as for semantic interpretation, sometimes they have been derived by the interpretation of some empirical material, assisted by some general theories as has been the case for dialogue modelling.

This dichotomy somehow interacts with another opposition, sketched at the beginning of this article, between a highly “prototypical” approach and extremely low-level “engineering” solutions.

Till the early Nineties, researchers created highly sophisticated prototypes, where modules often integrated complex theoretical formal models. Prototypes were costly, in terms of required know-how, but it was often claimed that easy portability from one application to the other (say from one database to another database) was a source of revenue to cover these expenses.

In fact this turned out to be partly false. Portability was also costly and difficult, and, in addition, users seemed not to be happy with the few commercial products that were proposed in the early Eighties in the field of natural language interfaces. When the engineering of some products started, some of the strongest assumptions were torn apart. Modularity of early prototypes was changed into the view that modules should have been interchangeable and the notion of *interoperability* became fashionable, meaning that complex systems should be built by gluing together “off-the-shelf” pieces. This view destroyed the assumption that “the more complex a problem is, the more theoretically sophisticated the solution”, and, somehow, demotivated advanced research, in favour of less sophisticated empirical studies.

Extreme views have never helped research and produced real advances. It is obvious that no engineering view could have been even possible without those very principled prototypes. We are now able to assemble “off-the-shelf” commercial modules because, at a given point in time, people designed theoretically motivated objects. The mistake was, probably, to try to stand on the side of applications before being mature for them. With an *a posteriori* evaluation we should admit that Computational Linguistics is a theoretical, high-risk branch of research participating in the cycle of information technology and services. Engineering of products cannot be done by computational linguists; Computational Linguistics must produce advanced research, know-how, but not get involved in production. To draw a parallel, the fact that bridges and buildings stand because of some principles of physics does not purport the involvement of physicists in ordinary projects; nevertheless physicists have the duty to keep studying the principles of statics.

Talking of syntactic approaches, we observe that in many cases the move towards empirical studies is uncontrolled, because linguistics has no interpretation mechanisms. Computational Linguistics produced important results in syntactic and morphological analysis because it could rely on robust theoretical linguistic models like Chomsky’s grammar (s) and the theory of automata. This is not true in other fields like, for example, discourse and dialogue modelling.

Discourse and dialogue have been studied by linguists according to different paradigms, such as discourse analysis (Sinclair & Coulthard 1975), conversationalism (Sack & Schegloff 1973), or sociolinguistics (Tannen 1984). The

speech acts/planning approach and all its developments arose in the area of Computational Linguistics with no link to linguistics; thus the empirical studies necessary for a better modelling have often been carried out with a weak linguistic view. This is even worse in the field of corpus-based linguistics, where often tag-sets are chosen with the objective of getting a “pre-theoretical” or “theoretically neutral” classification of facts.

In the field of semantics, Computational Linguistics turned to logic because linguistics offered absolutely nothing for what concerns the meaning of sentences. Thus, the conclusion is that Linguistics is in debt here, and should take advantage of the stimuli coming from Computational Linguistics to build new theories which take into account computational modelling.

Again, Computational Linguistics is not an applicative domain, but a part of theoretical linguistics in its own right; it provides an extra view on language, besides historical linguistics, structural linguistics, generative linguistics, cognitive linguistics, etc. This is the reason why we should think of integrating Computational Linguistics approach and stimuli into linguistics, rather than just promoting co-operation between Computational and Theoretical Linguistics.

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# State-of-the-art paper: Lexicology and lexicography

## Milestones in metalexicography

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### Introduction

Lexicology and Lexicography have occupied the research programmes of scholars all over the world for many years. The influence of their research has not been restricted to the academic spheres. This is one of the fields of research from which a linguistic impact on the communicative needs and skills of the general public can be and has to be realised on a continuous basis. It is also a field in need of a continuous interaction with various other disciplines, including linguistics. Consequently it is important that lexicology and lexicography should be included in the programme of academic conferences like this one. Lexicology and lexicography do not only have something to gain from such a conference but also have much to offer.

To get an overview of the history of lexicography one has to go much further back than the Chomskyan revolution, the advent of structuralism or the contributions of the famous Prague School. The first work in the field of lexicographic practice dates back to between the third and the fifth millennium before Christ. Different scholars have different opinions on the origins of the first dictionaries but whether they were sources of reference written on papyrus leaves in Egypt or on clay tablets in Mesopotamia, these containers of knowledge were prepared to serve as practical instruments for their respective speech communities, cf. Al-Kasimi (1977), McArthur (1986). And now, in 2003, at the XVII Congress of Linguists, the first meeting of the CIPL in the new century, Lexicography and Lexicology have finally been awarded a slot on

the programme of this esteemed organisation. This gives a new status to Lexicology and Lexicography and one has to thank and compliment the organisers on including Lexicology and Lexicography as a topic for this specific congress.

In lexicography a distinction needs to be made between the practical component, the practice of the writing of dictionaries, and the theoretical component, the discipline of metalexigraphy. The writing of dictionaries is an ancient practice which developed in a pre-theoretical era. By contrast, theoretical lexicography is a relative late comer to the field of research activities. In this regard the venue of our congress, Prague, played no minor role. In 1960 UNESCO, the United Nations Education, Science and Culture Organisation, offered a contract to the International Council for Philosophy and Humanistic Sciences to inquire into the situation in the domain of lexicography (Zgusta 1971:9). The contract was then offered by the International Council for Philosophy and Humanistic Sciences to the Union Académique Internationale and an inquiry was undertaken resulting in a final report. Acting on recommendations made in this report UNESCO and CIPSH, the Conseil International de la philosophie et des sciences humaines, partly co-sponsored a special international colloquium in 1962. This colloquium was organised by the Oriental Institute of the Czechoslovak Academy of Sciences and its aim was to discuss the problems of lexicography.

In the final resolution of this colloquium the desire was mentioned that a manual for lexicography needs to be prepared. The Oriental Institute of the Czechoslovak Academy of Sciences accepted the task of preparing this book with the Prague academic Ladislav Zgusta as main author, cf. Zgusta (1971:10). For Zgusta this was an opportunity to accomplish something he had already planned – an idea resulting from his endeavours in the lexicographic practice and his realisation of the need for a handbook of lexicography. Since 1958/59 Zgusta had been involved in a project for the compilation of a Czech-Chinese dictionary. While working on this project Zgusta's bibliographical search of works dealing with theoretical lexicography rendered only a small harvest. This stimulated the idea to write a handbook of lexicography, cf. Zgusta (1992). As a result the first major publication to establish theoretical lexicography as a research field, the monumental *Manual of Lexicography*, authored by Ladislav Zgusta was published in 1971 by Mouton as a product of the Publishing House of the Czechoslovak Academy of Sciences in Prague. Ignoring some unfortunate political occurrences surrounding the preparation and publication of the *Manual of Lexicography*, it is good to see Lexicology and Lexicography on the programme of the CIPL and to know that lexicography has come home to Prague.

I would like to dedicate this paper to Prof. Ladislav Zgusta in recognition of the innovative and ground-breaking work he has done in the field of Lexicography and for his continued commitment to this field of research over many years.

## The state-of-the-art

To say something about the state-of-the-art of any given topic compels one to look at three questions, i.e. *Where do we come from? Where are we now? and Where are we going?* I will respond to these questions with the emphasis on the first one. I will not and cannot, however, endeavour to give a comprehensive account but will focus on some major aspects regarding the development especially of lexicography.

## Where do we come from?

Lexicography started as a practical endeavour with no theoretical base. Wiegand (1998:29) rightly argues that dictionaries are much older than the field known as lexicology. Prior to the distinction between lexicography and lexicology, theory and practice had been intertwined and vocabulary research, the forerunner of what is now known as lexicology, actually formed a basis for the practice of dictionary compilation, i.e. lexicography. Lexicography and lexicology were almost one and the same thing.

Towards the end of the 19th century, according to Geeraerts (1996), there was a strong relation between lexicology and lexicography, among other factors due to their mutual historical orientation. Linguistics then was primarily concerned with historical research and focused strongly on the lexicon. Lexicography was primarily aimed at the compilation of historical dictionaries. The historical dictionary was not only a lexicographic pinnacle due to its extent and its purpose to give a comprehensive description of the lexical history of a language but also on account of its scientific character and its contribution to historical linguistic research.

Wiegand (1998) regards lexicology as a late product of lexicography and linguistics. As recently as the 19th century a more or less independent methodological research of words, called lexicology, had been established within the field of linguistics. However, it still remained in close contact with lexicogra-

phy where especially historical and dialect dictionaries were seen as the typical results of word research.

An overview of the development of lexicography and lexicology indicates varying degrees of proximity in the relation between these two disciplines but in the course of time an increasing gap became noticeable. Changes in the field of linguistics during the 20th century, e.g. the development of the theory of word fields, led to a slackening in the relation between lexicology and lexicography, cf. Wiegand (1998:30). Various schools of thought in linguistic theory created fluctuations sometimes leading to an increase and sometimes to a slight decrease in this gap between lexicology and lexicography. The distance was decreased e.g. with the advent of cognitive linguistics. But, in spite of varying degrees of proximity, Geeraerts (1996:9) indicates that lexicography and lexicology have been expelled from the paradise of their erstwhile symbiotic interactive relation.

An interesting conclusion to which Geeraerts (1996: 14, 15) comes, is that lexicology originally presented the theoretical basis for the scientific historical dictionary but the dictionary was at the same time the large-scale empirical realisation of lexicological research programmes. The stronger pragmatic approach of lexicography stimulated the need for a separate theoretical component, which was established as the field of metalexicography. This ascertained the schism between lexicography and linguistics.

The newly found autonomy of lexicography, with its own theoretical and practical components, left lexicology stranded in terms of a large-scale realisation of its research programmes. Geeraerts (1996) concludes that just as metalexicography functions as the theoretical component of lexicographic practice, theoretical lexicology is in need of a broadly orientated descriptive lexicology. Only then would the gap between theoretical lexicology and lexicographic practice be bridged.

The combination of lexicology and lexicography as a topic for this conference implies or presupposes a definite relation between the two components of this topic. From a purely linguistic perspective it used to be argued that lexicography should be regarded as applied linguistics, cf. Van Sterkenburg (1984:2), Richards, Platt and Weber (1992), or even as a subdiscipline of lexicology, being the practical application with lexicology featuring as the theoretical variant, cf. Gouws (1989). Where lexicology and lexicography used to be intertwined and regarded as closely related the situation has changed dramatically. According to Wiegand (1998:31) lexicography should not be seen as a branch of lexicology. Lexicology is one of a number of fields within and outside linguistics to have an influence on lexicography. Tarp (2000:192, 193) goes further and states that

lexicography and linguistics should be viewed as two independent disciplines. This is also stated quite categorically by Hartmann and James (1998:vi) when they say that lexicography is often misconceived as a branch of linguistics. Lexicology, on the contrary, is regarded by Hartmann and James (1998:86) as a branch of linguistics.

Grouping lexicology and lexicography together in a single topic can be justified on historical grounds although it does not reflect the actual state of affairs.

### Lexicography and linguistics

It is also necessary to pay attention to developments in the relation between lexicography and linguistics. In the early years of its development theoretical lexicography had a much stronger connection with linguistics, albeit that it was no mean feat for lexicography to be recognised by linguists as a scientific research area. For lexicography to develop its own theoretical component it was necessary to have an established scientific discipline to provide a base for the theoretical model. Zgusta realised this and one of the major aims of his book was to put the lexicographic discussion within a linguistic framework. To Zgusta it was important that a “coherent statement and discussion of lexicographic problems will help to clarify them, and to demonstrate the importance of their being conceived in the framework of linguistic theory most effectively” (Zgusta 1971:10). It has to be regarded as one of the most important contributions of the *Manual of Lexicography* that it convinced people of the important role linguistics plays in lexicography and the role of lexicography in linguistics. It became clear that linguistic theory, often so abstract, should also be formulated to be used by lexicographers when planning and compiling their dictionaries. Linguists, on the other hand, should and could also utilise dictionaries and the results of lexicographic research in their own research.

Soon after the publication of the *Manual of Lexicography* the influence of Zgusta’s ideas was already noticeable. This resulted in the growth of theoretical lexicography, noticeable among others in the papers published since 1984 in the *Lexicographica Series Maior* and in many other publications, e.g. Geeraerts (1983) which is a fine example of a scientific discussion of issues regarding the relation between linguistics and lexicography. Many dictionaries have also been improved and display a much stronger theoretical basis. Wahrig (1983:449) states that a consistent lexicographic description depends on the use of theoretical models. In his discussion of lexicography as an academic subject Sinclair (1983:9–11) relates lexicography to linguistics and suggests a concentration on

newer disciplines of text linguistics and pragmatics which are concerned with language in use. He argues that lexicography as an academic discipline should lead to profound changes in linguistics. The evidence from language usage, snapped up by lexicographers, should force linguistics to broaden its base.

Drosdowski says: “Die Sprachwissenschaft hat nicht nur die Aufgabe, den Wortschatz einer Sprache zu einem bestimmten Zeitpunkt zu kodifizieren, sie hat auch die Aufgabe, neue – praktikable – Konzepte zu entwickeln, um die Qualität der Wörterbücher zu verbessern” (Drosdowski et al. 1977:143). Unfortunately this point of view was shared neither by all linguists nor all lexicographers. Stein (1985:43) implies a scepticism among linguists towards lexicography when saying that it should be self-evident that linguistic research has to influence the making of language dictionaries. What she finds astonishing is that linguists expect lexicography to incorporate their findings, yet they rarely assume that lexicography may further linguistic ideas. On the other hand, Haensch (1984:118) says that lexicographers continue with their purely empirical practice without interest in theoretical linguistics. This scepticism of lexicographers is echoed by Hanks (1979:37) when saying: “When theory comes into lexicography, all too often common sense goes out.” Malkiel (1980:43) talks about the “vexing issue of the fluctuating relations – intellectual and social – between representatives of linguistics and spokesmen for lexicography.” He also regrets the indifference of American linguists towards the theoretical component of lexicography and, especially, the lexicographic practice.

The *Manual of lexicography* clearly linked lexicography with linguistics, e.g. when Zgusta (1971:15) states categorically that “lexicography is a very difficult sphere of linguistic activity.” He places lexicography within the field of the study of the lexicon, including the domain of lexical semantics, when he argues that a lexicographer concentrates on properties of lexical units like e.g. the lexical meaning in its different aspects and the grammatical properties. In order to do this the lexicographer should, however, be familiar with linguistics in a much broader sense and has to take into consideration not only the whole structure of the language in question but also the culture of the respective linguistic community. By referring to the culture Zgusta makes way for an approach which compels lexicographers to contextualise the language in terms of the more general world of the relevant speech community. He quotes the editorial paper in the first issue of the *Lexikograficeskij sbornik* which says “The theory of lexicography is connected with all the disciplines which study the lexical system, semantics, lexicology, grammar, stylistics . . .” Zgusta (1971:19) would consider his *Manual* a success if it stimulates authors of lexicographic projects and of

dictionaries to publish descriptions or discussions of their methods and approaches to their specific problems and the solutions within the more general framework of the linguistic theory. In a much later work Zgusta (1988: vi) says:

When drafting ... the Foreword to my *Manual of Lexicography* ... I wrote. ... 'One of the strangest features of lexicography is the fact that lexicographers have only rarely exchanged methodological experiences ... coherent discussions of lexicographic theory and practice are also very rare.' These were true statements at that time; ... Since then, ... there... is a steady and increasing flow of publications on the theory of lexicography, its methodology and procedures, ...

The ideals of his *Manual* have evidently been realised.

From these statements by Zgusta it is quite clear that the advent of theoretical lexicography had been positioned within the broader linguistic framework. This would demand that lexicographers take cognisance of developments in linguistic theory and that the data presented in a dictionary should result from a sound linguistic analysis. In the words of Mugdan (1984: 239): "Von einem Wörterbuch ist zweifellos zu fordern, daß es auf einer fundierten linguistischen Analyse beruhen sollte."

The publication of Zgusta's book heralded a new approach towards lexicography. The first four chapters of this book are not primarily concerned with lexicography but rather with linguistics, focusing on topics like lexical meaning, formal variation of words, combinations of words and variation in language. By including chapters on the formal variation of words and variation in language Zgusta gave a clear signal that linguistic influence does not only, or even primarily, run along the lines of formal grammar but the dictionary, as Fritz Ponelis puts it, as the display-window of language needs to reflect real language usage and not only the language of the ideal speaker-hearer. In this regard lexicography constituted a form of opposition to the ideas of the Transformational Generative Grammar and could rather be seen as a forerunner of some of the ideas of sociolinguistics. Zgusta (1989) yet again focuses on the role of dictionaries in the development of the standard and in reflecting linguistic change. This emphasises a change from a prescriptive to a descriptive approach in lexicography.

Given the scepticism among some linguists regarding the position of lexicography as a subdomain of linguistics this effort by Zgusta gave a clear signal that a sound lexicographic theory utilises sound linguistic principles. Zgusta's book, however, went further than this. Already in the introductory chapter Zgusta indicates the bivalent approach needed by a lexicographer when he says

that the lexicographer is doing scientific work but publishes it for users whose pursuits are always more practical (Zgusta 1971:16). Once again one has to negotiate the very real distinction between the theoretical lexicographer and theoretical lexicography on the one hand and the practical lexicographer and the lexicographic practice on the other hand. Important in this statement by Zgusta is the fact that lexicography may not be regarded as a theory merely for the sake of theory. The broad and inclusive domain of lexicography has to be regarded as eventually directed primarily at the process of dictionary compilation. Theoretical lexicographers devise theories aimed at enhancing the efforts of the practical lexicographer in his/her process of dictionary compilation. A dictionary can, among other things, rightfully be regarded as the display-window of linguistics. The people looking at these display-windows are not trained linguists but rather the average members of the relevant speech community.

By referring to the fact that a dictionary is prepared for users whose pursuits may be more practical Zgusta introduced a point of view which would become a driving force in the lexicographic research of the nineties, i.e. the user-perspective. Lexicographic theory would contribute to models which would allow practical lexicographers to compile dictionaries aimed at a well-identified target user group, taking cognisance of their specific needs and reference skills. Yet again, lexicography is not working on an abstract level but provides in the real needs of real users. In a much later publication Zgusta (1988:vi) says that lexicography is one of the view areas in which linguistic activity has an immediate impact on many people. Lexicographic theory has to negotiate this issue in a very real way.

### Within a linguistic fold

Zgusta's book heralded a period which saw lexicography moving into the linguistic fold. Unfortunately some lexicographers did little to ensure an optimal utilisation of this situation and rather tried to maintain a theory-free practice. Even the title of a popular text book on lexicography, i.e. Landau (1989), does not help the cause of lexicography very much but plays into the hands of sceptics by referring to the "art and craft of lexicography". One of the reasons why lexicographic practice still partially eschewed a stronger theoretical linguistic influence could be found in the commercial success of dictionaries. As long as it sells there is no need to change it. That this approach diminishes the linguistic authority of dictionaries did not convince enough publishers to give serious attention to a stronger linguistic and theoretical basis for their dictionaries.

Since 1971 varying degrees of proximity have prevailed between lexicography and linguistics with different theories and schools of thought in linguistics having a lesser or a bigger influence on both lexicographic theory and the lexicographic practice. Different linguistic theories, cf. Geeraerts (1984, 1986) and Gouws (1989), had an influence on e.g. the explanation of meaning in monolingual dictionaries. Different approaches to lexical semantics, the way in which the influence of structural linguistics led to a distinction between semantic and encyclopaedic data and the much more lenient approach following from cognitive linguistics had an impact on the contents of the lexicographic definition.

The varying influence of linguistics on lexicography as seen in general dictionaries has primarily been noticeable in the nature and extent of the presentation of semantic data. General language dictionaries, both monolingual and bilingual ones, have displayed a strong semantic bias, cf. Gouws (1996), and this has been to the detriment of other data categories. Burkhanov (1998: 136) states a widely accepted belief that lexicographic practice belongs to the domain of applied linguistics whereas metalexicography forms part of theoretical linguistics. He indicates that this belief has led to the assumption that linguistic semantics should provide the theory for lexicography. This would virtually equal linguistic semantics and metalexicography.

Variation was also noticeable in the nature, extent and treatment of other data types in general dictionaries. Data types like pronunciation, morphology, etymology and even syntax have been presented and treated in a fairly consistent way. Although the outer texts of a dictionary displaying a frame structure, cf. Kammerer and Wiegand (1998), Gouws (2001, 2002), often include a mini-grammar or even a brief explanation of some of the productive rules of word-formation or syntactic constructions, the articles in the central list still display an insufficient account of syntactic and morphological data. However, in line with the lexicographer's assignment to record real language and not to set its style, cf. Sledd and Ebbitt (1962: 92), modern-day lexicography has emphasised the importance of examples as an integral part of the treatment of a given word. Fox (1997: 137) says that the use of examples forms an integral part of the learning of a word. These examples have to come from a corpus and have to represent real language (Fox 1997: 138). The use of examples plays an important role in dictionaries and the influence of sociolinguistics has made a definite impact on the way in which lexicography deals with this type of entry.

During the seventies and eighties theoretical lexicography was performed and studied largely within a linguistic context. Many publications in the field of

metalexigraphy focused on linguistic aspects of dictionaries resulting from a situation where many researchers working in the field of metalexigraphy were linguists by training and attached to university departments of linguistics or languages.

### The Wiegand era

Metalexigraphy in the eighties and nineties was dominated by the work of the German scholar Herbert Ernst Wiegand. In his early work he already signalled the importance of the formulation of a general theory of lexicography, cf. Wiegand (1983, 1983a, 1984). Wiegand (1984:13–15) argues that lexicography is neither a branch of applied linguistics nor a branch of lexicology and it is by no means theoretically determined by lexicology alone. He regards linguistic lexicography as a scientific practice aimed at producing reference works on language, whereas the field of metalexigraphy is constituted by the four components, i.e. the history of lexicography, a general theory of lexicography, research on dictionary use and the criticism of dictionaries. This approach of Wiegand's to the lexicographic practice is ascertained and confirmed in Wiegand (1998:62, 254). Wiegand (1989:251) maintains that lexicography is a practice, aimed at the production of dictionaries in order to initiate another practice, i.e. the cultural practice of dictionary use. Wiegand (1998:256) also confirms the status of theoretical lexicography, he uses the term *dictionary research* as a scientific research area and a discipline with a clearly identifiable academic existence and maintains that dictionary research can be divided into four research areas, i.e. research in dictionary use, critical, historical and systematic dictionary research.

Although he recognises the importance of linguistics for lexicography Wiegand has initiated an approach that lexicography has to be regarded as a discipline which is influenced among other things by linguistics but not to such a degree that it should be regarded as a subdiscipline of linguistics. Linguistics has language as its study object. This also applies to lexicology and consequently lexicology still has to be regarded as a branch or subdiscipline of linguistics. Practical lexicography is aimed at the process of dictionary-making whereas theoretical lexicography deals with dictionary research, cf. Hartmann and James (1998), Wiegand (1984, 1998) and Hausmann and Wiegand (1989). Although linguistics is an important influence in lexicography, the object of lexicography is not language but dictionaries. Consequently lexicography cannot be regarded as a branch of linguistics, although it does overlap with various subdisciplines from the field of linguistics.

In his prolific portfolio of publications Wiegand has focused dictionary research not only on the contents of dictionaries and dictionary articles but also on the structure of dictionaries. Since Wiegand (1983b) numerous publications of his have dealt with wide-ranging issues regarding the structure of dictionaries. They confirm his approach that metalexigraphy is no branch of linguistics. By analysing and discussing the structure of dictionaries Wiegand has added a component to his successful attempts at formulating a general theory of lexicography that emphasises the formal features of dictionaries. The Wiegand era has been characterised by the identification of all possible components of dictionary articles and by a meticulous description of their specific structure and function. In this process Wiegand has also made numerous suggestions in order to improve the quality of the lexicographic practice and to ensure that the genuine purpose of a specific dictionary can be achieved. Wiegand's description of the structure of dictionaries is not tantamount to formulating a theoretical model and imposing it on lexicographic practice. Wiegand rather took a critical look at existing dictionaries to identify and describe their structural features. He has moved from practice to theory so that the theory could be applied to enhance the practice.

Although lexicography is not regarded as a subdiscipline of linguistics the strong link between linguistics and lexicography, both theoretical lexicography and the practice of the compilation of especially language dictionaries, may never be ignored. Consequently, Wiegand was instrumental in seeing to it that lexicography also featured as a topic in the authoritative HSK series (*Handbücher zur Sprach- und Kommunikationswissenschaft / Handbooks of linguistics and Communication Science*) resulting in the impressive state-of-the-art three volume *Wörterbücher. Dictionaries. Dictionnaires. An International Encyclopedia of Lexicography* (Hausmann et al. 1989–1991). This work focuses on a number of relevant topics in lexicography, e.g. dictionaries and their public, dictionaries and their users, the history and theory of lexicography, components and structures of dictionaries, problems of description in the general monolingual dictionary, dictionary types, dictionaries dealing with language varieties, procedures in lexicographical work, lexicography of individual languages and the theory of bilingual and multilingual lexicography. Lexicology does not feature in the volumes on lexicography. A separate volume has been planned for lexicology and this process is currently underway.

One of the noticeable features of developments in theoretical lexicography during the Wiegand era is the strong bias towards the needs and the reference skills of the target users of dictionaries. In this regard the attention given by Wiegand to the structure of dictionaries played no mean role. His research re-

garding the access structure of a dictionary, i.e. the search route a user follows to reach the desired data, and the detailed discussion of aspects like the data distribution structure, the micro-architecture and the different search zones in dictionary articles, cf. Bergenholtz, Tarp and Wiegand (1999), as well as the use of integrated and unintegrated outer texts, helping to constitute the frame structure of a dictionary, cf. Kammerer and Wiegand (1998); Gouws (2001), places the focus yet again on the user-perspective, so prevalent in modern-day metalexigraphy. Wiegand's contribution to the development of a general theory of lexicography culminated in Wiegand (1998), the first volume of a comprehensive account of some of the most salient aspects of his research in the field of lexicography. The major part of this first volume focuses on research in dictionary use, including an exposition of the methodology of usage research and different types of dictionary consultation situations. The second volume will cover topics like systematic, historical and critical dictionary research.

The focus on the structure of dictionaries during the Wiegand era emphasised the fact that as containers of knowledge, cf. McArthur (1986), both the contents and the form must be regarded as extremely important. From a purely linguistic perspective little interest exists in the structure of dictionary articles or the use of front and back matter texts and inserted inner texts. Neither does the layout of a dictionary fill mainstream linguists with excitement. The metalexigrapher, however, may not ignore the significance of these formal properties of a dictionary. Too often in the past dictionaries have not achieved an optimal transfer of data due to an insufficient presentation and a less than satisfactory form to accommodate the good contents. During the last decade the emphasis which Wiegand placed on the structure of dictionaries has found another realisation, as pursued in publications like Bergenholtz (1995) and Almind and Bergenholtz (2002), i.e. a focus on problems relating to dictionary layout.

Almind and Bergenholtz (2002:261) indicate that layout is not a cosmetic issue but rather contributes to the access structure of a dictionary. A good layout enhances both the outer and the inner access structure. The choice of various typographical and non-typographical structural indicators is employed to ensure an optimal retrieval of information. Yet again the theory of lexicography goes further than purely linguistic procedures.

### Specialised lexicography

Wiegand's arguments, cf. Wiegand (1984, 1989, 1998), that linguistics is only one of many disciplines influencing lexicography is illustrated by the attention

in theoretical lexicography to the development of special-field lexicography, cf. Bergenholtz and Tarp (1995). Bergenholtz and Tarp make a distinction between language for general purposes (LGP) and language for special purposes (LSP). General dictionaries primarily deal with LGP, although some LSP items will also be included and treated in these dictionaries. Specialised dictionaries treat the various special fields of the lexicon. The compilation of LSP dictionaries presupposes collaboration between the lexicographer and the subject expert. The influence from the relevant subject field will determine the nature of the specific dictionary.

In the development of dictionaries for special purposes, theoretical lexicographers have been involved in devising models for a number of special field dictionaries. In this regard the work done by Henning Bergenholtz and Sven Tarp at the Center for Lexicography at the Aarhus School of Business needs to be mentioned. Their theoretical publications, cf. among others Bergenholtz and Tarp (1995), Tarp (2000), have played an important role in making practical lexicographers aware of salient theoretical issues.

The theoretical models devised by metalexicographers have been applied successfully in lexicographic practice. Lexicography should also benefit from this expertise. At present a project is underway to compile a special-purpose dictionary with lexicography as its object: the *Wörterbuch zur Lexikographie und Wörterbuchforschung / Dictionary of Lexicography and Dictionary Research*. An important contribution of this dictionary will not only be the explanation and standardisation of about three thousand lexicographic terms, but one of the front matter texts of this dictionary will be a systematic introduction to the field of lexicography and dictionary research, cf. Wiegand (2003). This will enable the target user to ascertain a brief overview of the object field of this dictionary. Theory is once more put into practice.

### Dictionary functions

The development of lexicography shows some interesting themes appearing, disappearing and, sometimes, reappearing. One such a theme which is particularly relevant in modern-day lexicographic thought is that of dictionary functions. The notion of functions is nothing new – it had already been introduced in 1940 by the Russian linguist Scerba. Following the suggestions made in Scerba (1940) some theoreticians in the field of dictionary research hold the meaning that for any given language pair at least four and perhaps even eight bilingual dictionaries have to be compiled to meet the diverse needs of the users coming from both language groups. According to them provision has

to be made for separate dictionaries aimed at the active and passive use by source and target language users respectively, cf. Kromann et al. (1984, 1984a). The active/passive principle focuses on the function of dictionaries in text production and text reception respectively, cf. Hausmann (1977, 1986). Certain applications of the active/passive principle imply that four different functions can be identified for each member of a language pair and that each one of these functions should be dealt with in a separate dictionary. Even the most sophisticated system cannot work with four to eight dictionaries per language pair. From a user-perspective it also is an unattainable objective.

Lexicographic theory may not be formulated at the cost of a successful lexicographic practice. Therefore Wiegand (1996a:XV) emphasises the fact that the formulation of a theory for bilingual dictionaries, and it also applies to monolingual dictionaries, may never be isolated from lexicographic practice. Consequently Wiegand (1996:2) pleads for the accommodation of different functions within one dictionary and even one dictionary article. He argues convincingly in favour of the compilation of only one polyfunctional bilingual dictionary for any given language pair.

During the last few years dictionary functions once again came to the fore in lexicographic research, cf. Tarp (1994, 2000, 2002, 2002a, 2002b), Bergenholtz and Tarp (2002), Wiegand (2001) and Tarp and Gouws (2003).

Modern-day lexicographic theory has an underlying assumption that dictionaries are utility products. Consequently Tarp (2002:67) argues that the methodology for planning a dictionary should make a typology of potential users, user situations and problems that might arise for each type of user in each type of user situation. The characteristics of the users need to be determined and eventually the relation between the needs of each type of user in each type of user situation and the data included in a dictionary to satisfy these needs constitutes the basis for the theory of lexicographic functions. According to Tarp (2002:70) a lexicographic function represents the assistance that a dictionary provides to a particular type of user to cover the needs of that user in a specific user situation. He distinguishes between knowledge-orientated and communication-orientated functions. Communication-orientated functions will assist the reception and the production of texts in the native and foreign language. The focus on dictionary functions emphasises the user-directed approach in modern lexicographic theory very strongly.

## Where are we now and where are we going?

The answer to the question *Where do we come from?* has brought us to the position where we are now and has already commented on the present state of affairs. Lexicology, as a subdiscipline of linguistics, is still competing strongly in the linguistic fold. Lexicography, as an independent discipline, still benefits from a strong linguistic influence, albeit that the focus in lexicographic research has shifted to the structure and the functions of dictionaries. Although I have made no reference to it, modern-day lexicography actively participates in utilising the electronic media. Already in Zgusta (1971: 353) this was foreseen when Zgusta said that one of the most important decisions a lexicographer will make is whether or not to use automatic data-processing machines. Improving and expanding the use of electronic corpora and theoretical issues regarding electronic dictionaries will be major challenges for metalexigraphers of the present and the near future. This is a vibrant research field in lexicography where once again different disciplines can combine to produce knowledge that can benefit them all. Lexicographic research should also focus on models for dictionaries directed at specific target user groups, cf. Dolezal and McCreary (1999) identifying the work done in the field of pedagogical lexicography. The interface between metalexigraphy and lexicographic practice must be utilised to stimulate further scientific research and to enhance the quality of dictionaries and improve the cultural activity of dictionary use.

## In conclusion

Lexicography and lexicology are well-established with and within their own respective domains and deserve to be on the programme of this XVII Congress of Linguists. Within the field of linguistics lexicology has a central position, especially in the study of lexical semantics. Lexicography is served by professional associations like The Dictionary Society of North America, The Lexicographical Society of India and the LEX-family: EURALEX, AUSTRALEX, AFRILEX and ASIALEX, but still lexicographers need to participate in a conference with a strong linguistic basis because the linguistic component of lexicography may never be eschewed. This interactive relation adds to the pleasure of being involved in lexicography and ascertains what Hulbert (1955) has said: "I know of no more enjoyable intellectual activity than working on a dictionary".

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# Pragmatics

## State of the art

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### Introduction

Pragmatics, as we know it today, is a sprawling subject, which has branched out in every direction since its terminological origins in the 1930's (Morris 1938). Here we can review only certain central themes in pragmatics, and even then, only incompletely. We have chosen: part I: speech acts and implicature, part II: conversational and discourse analysis, and sociopragmatics.

### Speech acts

Speech acts are acts performed in or by speaking (or more generally, in or by using language). As such, speech acts lie at the intersection of the theory of action and the theory of language, and historically the developers of speech act theory have tried to relate these two aspects of this topic. Contemporary speech act theory divides roughly into two traditions: rules (and norms) vs. intentions (and inference).

### Rules and norms

Modern speech act theory dates from the publication of Austin (1962), although important and interesting (albeit fragmentary) work was done by Frege (see Harnish 2001) and Wittgenstein (see Stenius 1967). Austin noted that there were many uses of language which had the superficial appearance of fact-stating, but were really something different: 'I do', 'I quit', 'I promise I will be

there'. These are 'performatives' and their job in the language is to be used to do something (and are governed by "felicity conditions") not just to say something (and be governed by "truth conditions"). He soon came to believe, in the face of examples such as 'I state that stocks slumped', that the distinction between saying and doing could not be rigidly maintained (but see Recanati 1987), and that every sentential utterance has a performative aspect. Thus he moved from the special theory of performatives (vs. constatives) to the general theory of speech acts: 'locutionary' acts are acts of "saying something in the full normal sense" i.e. with a certain sense and reference. 'Perlocutionary' acts are acts of affecting the hearer's thought or action in some way (persuading). Austin only characterized 'illocutionary' acts, the third and most important category, by example (stating, questioning, commanding, promising etc.) and by citing certain of their general features, such as being governed by conventions beyond those of grammar, sense and reference, and being capable of being made explicit by a performative prefix. He developed a preliminary taxonomy of illocutionary acts that has served as the basis for subsequent taxonomies in philosophy: Verdictives, Exercitives, Commissives, Expositives, Behabitives.

Austin's ideas were developed in philosophy primarily by Searle (1969), where Austin's doctrine of infelicities became integrated with Wittgenstein's notion of language games, and Frege's distinctions between force, sense, reference and predication. Searle, following Frege, factored illocutionary acts with content into an illocutionary force (F) and a propositional content (P). Searle proposed that the illocutionary force component of a (nondefective) illocutionary act can be analyzed into four kinds of conditions. Sentences of a natural language, he thinks, can be analyzed into a device which indicates the illocutionary force of the sentence (IFID) and a device which indicates the (propositional) content of the sentence (PCID). The ("constitutive") rules for the use of these devices are derived from the above conditions. Searle (1979) criticized Austin's taxonomy of illocutionary acts for being unprincipled and failing to distinguish illocutionary acts from illocutionary verbs, and he went on to offer his own influential alternative taxonomy of illocutionary acts using twelve dimensions of difference between illocutionary acts (see Wierzbicka 1987 for a survey of illocutionary verbs): Assertives, Directives, Commissives, Expressives, and Declarations. Building on Grice, Searle (1979) also systematically investigated indirect and nonliteral uses of language. Searle and Vanderveken (1985) modified Searle's earlier taxonomy of illocutionary acts to include just seven components of illocutionary force: illocutionary point or purpose, degree of strength of illocutionary point, mode of achievement, propositional content, preparatory conditions, sincerity conditions, and degree of strength of sincerity

ity conditions. Every illocutionary force is defined by setting a value for each of these parameters. They went on to investigate aspects of illocutionary logic based on these changes (see also Vanderveken 1990). This framework has been applied to a variety of phenomena in Vanderveken and Kubo (2002). Alston (2000) criticized details of Searle's analysis of illocutionary acts, and proposed a modification and extension based on the normative notions of 'taking responsibility' for a condition being satisfied, having a "normative effect" and illocutionary rules. He also proposed a taxonomy of illocutionary acts combining Austin and Searle: Exercitives, Commissives, Directives, Expressives and Assertives.

Austin's ideas were developed in linguistics within both the 'generative semantics' framework and the 'interpretive semantics' framework. Katz and Postal (1964) argued, on the basis of linguistic data, for the Fregean idea that information concerning (illocutionary) force is coded into the mood structure of sentences. They proposed that this take the form of abstract morphemes, with semantic interpretation, which trigger syntactic transformations and related lexical and phonological changes characteristic of these moods. Ross (1970) argued, in effect, that the marker for declaratives is actually a higher embedding clause in the deepest structure of the sentence, clauses which typically remain unspoken and unwritten, and have the form of Austin's performative prefixes (e.g. 'I assert to you that'). Similar proposals were made for interrogatives and imperatives (Sadock 1974), but the 'higher performative' analysis of force had serious problems (Gazdar 1979) and was abandoned in the 1980's when generative semantics was abandoned, though much of the data remains unaccounted for to this date. Katz (1977) proposed to incorporate force into the alternative 'interpretive semantics' framework. Like Frege, Katz proposed that sentences contain speech act information (what Katz calls propositional 'type') and propositional content. The propositional content is divided into referential information and predicative information (which Katz calls the 'condition'). When the type of the sentence is assertive, it converts the condition into a truth condition, thus correctly connecting assertion with truth. When the type of the sentence is requestive, it converts the condition into a compliance condition, thereby correctly connecting requesting with compliance. Katz proposed a provisional alternative categorization of illocutionary forces as encoded into the meaning of sentences. These include: Requestives, Advisives, Expressives, Permissives, Obligatives, Expositives and Stipulatives.

Austin's (and Searle's) ideas were developed and modified in computer science as "plan-based" reasoning (see Cohen & Perrault 1979; Perrault & Allen 1980), and in cross-cultural pragmatics through the Cross Cultural Speech Act

Recognition Project (see Blum-Kulka et al. 1989). In psycholinguistics the focus has been on the role of literal and direct messages in the recovery of nonliteral and indirect messages. Original work, due to Clark and colleagues (see Clark 1992) suggested that literal and direct messages were computed on line before nonliteral or indirect. But later work (see Gibbs 1994) suggested that when context is allowed to play its role, the literal and direct message can be bypassed.

### Expressed attitudes: Intentions and inference

Grice (1989) envisaged a unified theory of meaning, saying, speech acts and communication, but like Austin, did not live to complete it himself. Grice's program for meaning and speech acts was modified and developed in one direction by Schiffer (1972) (see also Holdcroft 1978), who, first argued for an alternative analysis of speaker-meaning, then, worked out an analysis of convention, and connected it to speaker-meaning giving an analysis of the nature of the conventional meaning of a linguistic expression. Finally, building on his characterization of speaker-meaning, as well as ideas of Strawson (1964), he proposed a taxonomy of illocutionary acts which divided them into an 'assertive' class, and an 'imperative' class. Within each of these classes, three jointly exclusive and exhaustive subclasses were defined in terms of the form of the belief or desire, and the reasons being offered to form that belief or desire. In another direction, Grice's program was developed by Bach and Harnish (1979), who extended and schematized the inference pattern behind conversational implicatures and extended it to speech acts in general, and illocutionary acts in particular, which were analyzed in terms of expressed attitudes and their successful recognition in communication. They developed inferential strategies for direct, literal, nonliteral and indirect communication along with presumptions which drive these inferences and a conception of "standardization" which allowed some forms to be used nonliterally or indirectly by "short-circuited inferences" (see also Morgan 1978). They proposed a taxonomy which divided illocutionary acts into two major categories, conventional (acts performed by satisfying a convention and communicative (acts performed by having a reflexive intention recognized, then subdivided each into subcategories conventional acts: Effectives and Verdictives; communicative acts: Constatives, Directives, Commissives, and Acknowledgments. Harnish (1994) connects this theory to the study of mood as grammaticalized illocutionary force potential: a mood is a conventional pairing of form and force (see also Sadock & Zwicky 1985). One consequence of this conception is that performatives threaten to expand the class of moods into the thousands because each performative sentence, such as

'I promise to be there' will be a distinct form, with a distinct force. One way to avoid this is to make performative utterances 'indirect' in some manner, as declarations (Searle 1989), or as statements (Bach & Harnish 1979, 1992) – see Grewendorf and Meggle (2002) for more recent discussion.

## Reference

Ever since Frege's (1892) pioneering study of reference, three sorts of devices have been the focus of research: definite descriptions, proper names and deictics (indexicals and demonstratives), and each kind of device has revealed a pragmatic dimension. According to Donnellan (1966, but see Neale 1990), descriptions can be used referentially as well as attributively. Recanati (1993) and Perry (2001) argue that successful use of proper names require exploiting context-sensitive naming conventions, and according to Kaplan (1989) and Perry (2001) deictic elements depend for their success on contextual facts and demonstrating intentions – Anderson and Keenan (1985) review many of the cross linguistic facts. Searle (1969) formulated constitutive rules for reference, and Schwarz (1979) characterized reference in Gricean terms. Fretheim and Gundel (1996) review issues relating linguistic structure and processing, and Clark (1992) reviews important psychological work on descriptions and deixis.

## Critique

The idea that speech acts are useful categories of analysis for naturally occurring speech, discourse and conversation, has come under serious criticism (Levinson 1981; Schegloff 1988). The basic complaint is twofold: speech act categories are too coarse and rigid to capture the fluid context dependence of most conversational moves. Second, there is no principled pairing of speech acts with individual sentences. The alternative proposal is that naturally occurring speech must be analyzed at a more micro-level, using a different vocabulary from the macro-level of speech act theory. Speech act theorists in reply (Searle 1992) think these inadequacies are not essential to the speech act perspective, which was not initially intended to be exhaustive. Geis (1995) attempts to reconcile speech act theory with conversational analysis, as well as the work in computational speech acts.

## Implicature

Grice (1989) analyzed a speaker's meaning (to communicate) something in terms of intending to produce some effect in a hearer by means of the recognition of the intention to produce that very effect. Grice briefly discussed how these two central categories of meaning and speech acts might be related to sentential mood (declarative, imperative, interrogative), but his main interest was in analyzing the 'total signification of an utterance', and here he drew the influential distinction between what is 'said' (related to Austin's 'locutionary' act and Searle's 'propositional' act) and what is conventionally and conversationally 'implicated' (particularized and generalized). Conversational implicatures turn on talk exchanges being 'cooperative' ventures between speaker and hearer, where cooperation often amounts to conforming to various 'maxims of conversation' such as:

QUANTITY Be informative.

1. Make your contribution as informative as is required (for the current purposes of the exchange).
2. Do not make your contribution more informative than is required.

QUALITY Try to make your contribution one that is true.

1. Do not say what you believe to be false.
2. Do not say what you lack adequate evidence for.

RELATION Be relevant.

MANNER Be perspicuous.

1. Avoid obscurity of expression.
2. Avoid ambiguity.
3. Be brief (avoid unnecessary prolixity).
4. Be orderly.

Speakers conversationally implicate something (Q) in saying something (P) by 'flouting' a maxim i.e. by inducing the hearer to reason as follows: the speaker is obeying the maxims, but in order for this to be true something must be being communicated that is not being said (P), and the likely candidate for this is Q. Davis (1998) critically discusses the explanatory adequacy of Grice's theory of implicature and proposes a convention-based approach. Others have questioned Grice's suggestion that what is meant divides exhaustively into what is said and what is implicated. An utterance of 'I've had breakfast' typically communicates that I've had breakfast TODAY, but that information is does not seem to be a part of the semantics of the sentence, nor does it seem to be in-

ferred by any flouting of the maxims. So where does it come from? And what is its status? Sperber and Wilson (1986) and Recanati (1993) opt for including it in an expanded conception of what is said (and an expanded role for pragmatics in determining it), whereas Bach (1994) opts for a narrower Gricean conception of what is said. All agree that mechanisms of ‘completion’ and ‘expansion’ are required to supplement Grice’s account. Grice’s theory of generalized conversational implicature has recently been elaborated by Levinson (2000), and a ‘relevance’ account of implicature has recently been elaborated by Carston (2002). The psychological status of implicature has been studied by Gibbs and Moise (1977) (see also Nicolle & Clark 1999), and by Bezuidenhout and Cutting (2002). A related phenomena, called “unarticulated constituents”, see Perry (2001), is illustrated by sentences such as ‘It is raining’. The location of the rain required for a complete, truth evaluable proposition to be expressed, is understood to be where the speaker is located, though there seems to be no word, such as ‘here’, in the sentence to indicate this. Stanley (2000) argues that such information is in fact signalled by syntactic constituents of the sentence, though they be unspoken, and Recanati (2001) contests this.

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# Phonological dialectics

## A short history of generative phonology

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### 1. Introduction

The organization and content of this article is as follows. In Section 2, the largest and main section of this article, I discuss the development of generative phonology. My theme is that the focus of attention has been shifting between representational and derivational issues. I will discuss the derivational debates in the seventies (rule ordering and rule formats), the representational explosion in the eighties (multilinearity), and the derivational denial in the nineties. In Section 3, I conclude that it is now time to turn back to representational concerns.

### 2. Three decades of generative phonology

#### 2.1 The general form of a phonological theory

Phonological theorizing (like many other human activities) seems to follow a dialectic pattern such that the attention and creativity of those involved shift back and forth between what are the two major aspects of any phonological theory: *representations* and *derivations*:

$$(1) \quad F(\text{Input}) = \text{output}$$

The function  $F$  forms the derivational aspect of the theory, whereas both input and output form representational levels, which as such require a basic vocabulary and a combinatorial system.

Anderson's (1985) discussion of phonology in the twentieth century also views this broader development as one that involves a focus on theories of rules and theories of representations which he construes as resulting from a focus on *grammar* (as a mental construct) or on language (as a collection of sentences), respectively. Perhaps today, that opposition would be construed as one between I(nternal) language and E(xternal) language. I most certainly refer to Anderson's insightful book for a broader historical perspective (which covers generative phonology up to the early eighties); also see Fisher-Jørgensen (1975). However, as I will show, within the development of generative phonology, which as a whole is a theory of I-language or the *internal phonological system*, one can see a dialectic shift between a focus on this internal system as a *procedural rule system* and a *declarative representational system*. Both systems consist of a set of 'statements', typically called rules and constraints, respectively, although use of the term 'rule' (or statements that contain arrows) does not in itself imply a commitment to a procedural model (cf. Mohanan 2000). The important distinction lies in reference to intermediate levels (levels between the input and output) and the concomitant use of extrinsic rule ordering.

The phonological theory developed in the early days of generative phonology, culminating in Chomsky and Halle's (1968) *The Sound Pattern of English* (SPE) focused mostly on the derivational aspect. With respect to the representational assumption, SPE's theory was deliberately minimal. There is, of course, nothing wrong with minimalism if it simply refers to the attempt to get away with the fewest number of primitives and mechanisms. Hence, Chomsky and Halle tried to get away with a view of representations that involved a linear arrangement of phonological segments, characterized as unordered, unstructured sets of a finite set of binary-valued features, a minimal view indeed. It must be said that Chomsky and Halle devote considerable attention to the development of their feature system, as well as to the expression of the markedness of feature values. These are largely representational issues, which shows that my characterization of the successive stages of generative phonology is somewhat 'idealized'. In SPE, the function F was, on the other hand, highly complex, although, on the other hand, also minimal. The basic unit of F was the *phonological rule*, an operation that could change properties of the input. For simplicity's sake, let us say that the changes did not affect the vocabulary or combinatorial system as such (although it was argued that certain *plus* values (like for the feature [stress]) could be changed into n-ary values). The simplicity of the function F lay in the fact that there was only one type of mechanism in it, viz. the phonological rule, which was essentially a transformational rule, like the so-called transformations in syntactic theory. The complexity

of the function resulted from two factors. Firstly, since few restrictions were imposed on the *rule format*, the rules could get quite complicated, especially since various notational conventions allowed collapsing seemingly independent rules (*landscape* printing or unfolding pages were not uncommon in those days). Secondly, since rules could be extrinsically ordered and no restrictions were imposed on the distance between input and output, derivations could get quite long.

It can be said that the explanatory goal of SPE was to relate as many surface forms as possible, where ‘relating’ means ‘deriving from the same input form’. Hence, with rules that could *do* anything and input forms that could *be* anything, only ‘poverty of imagination’ stood in the way of deriving *paternal* (minus suffix) and *father* from the same input source (cf. Lightner 1972). The absence of a morphological theory that could place *semantic* limits on the notion of relatedness stimulated the creative quest for ‘common’ sources. Clearly, with so much freedom, chances to arrive at real explanatory accounts diminished in inverse proportion to the depth of the derivations that were proudly proposed.

## 2.2 Derivational issues predominate the seventies

It did not take long before articulated criticism and alternatives were launched. Indeed, the seventies showed lively debates concerning the *derivational side* of the SPE-model. Both the freedom of rule format and the absence of restrictions on the derivational distance between input and output came under attack. The derivational distance could be tackled in two ways (together or independently). On the one hand, one might propose constraints on the distance between input and output. From the outset it had been suggested that this distance had to be *as small as possible*; this was called ‘the naturalness condition’ (Postal 1968), but that in itself embodied no specific limitation. More tangible restrictions involved the idea that the input needed to be identical to some of its outputs, or at least be composed of segments that occur in its outputs. Such proposals were essentially attempts to formulate the ‘naturalness condition’ in more precise terms (cf. Kiparsky 1968, 1973; Vennemann 1971, 1974). The second, perhaps more direct, approach toward reducing the input/output distance was to eliminate the notion of extrinsic rule ordering, or to reduce ordering to cases that would follow from general principles (Koutsoudas, Noll, & Sanders 1974).

The problem with rules was, of course, that no one knew what a ‘possible rule’ was. Do rules have to be ‘(phonetically) natural’ and if so, what does that mean? And if not, what sets the limit? In the absence of knowing, or agreeing

on answers to these questions, one could at least try to exclude certain types of rules. One approach was to compare the structural description and change of the rules to the output of the whole derivation and require that the output does not contain positive exceptions (apparent overapplication) or negative exceptions (apparent underapplication). By excluding such situations (involving what came to be called *opacity*; cf. Kiparsky 1973), in other words by requiring that rules express 'true generalizations' (cf. Hooper 1976), extrinsic rule ordering was effectively rendered unnecessary.

Thus, several alternatives to the SPE-model arose, all differing in terms of the kinds of restrictions that were imposed on the derivational side of the overall theory. The most far-reaching rejection of SPE were, to my mind, *Natural Generative Phonology* (Hooper 1976; cf. van der Hulst 1978) and Stampe's *Natural Phonology* (1973), both of which restricted phonology proper to automatic processes.

Then, morphology kicked in and one idea that came up was to formalize an apparent distinction between 'inner' (Class I) and 'outer' (Class II) affixes or word formation processes (cf. Siegel 1974; Allen 1978). The empirical basis for this idea did not remain undisputed (cf. Aronoff 1976), but it caught on and influenced phonologists to cut up the phonological derivation accordingly, leading to the so-called *Lexical Phonology* model (cf. Kiparsky 1982). This model offered, in part, a solution to a problem that many critics of the SPE-model had advanced concerning its treatment of phonological alternations that do not reflect 'surface generalizations' but rather co-occur with specific (classes of) morphological affixes or processes. With its 'one rule format' minimalism, SPE had lumped together rules that accounted for surface generalizations (sometimes called 'automatic processes', whether allophonic or neutralizing) and rules that accounted for so-called *morphophonological* alternations, here referring to alternations that are limited to specific morphological contexts (cf. Dressler 1978 for many examples; Spencer 1998 for an overview).

Approaches that aimed at eliminating extrinsic rule ordering or its result, opacity, had effectively kicked morphophonology out of phonology, leaving an account of the relevant alternations to the lexicon (in case no 'real' morphology was involved) or to the morphological component (cf. Vennemann 1974; Hudson 1974; Hooper 1976; Strauss 1982). Recognizing a separate class of morphophonological rules (sometimes also called *morpho-lexical rules*, Anderson 1976, or *allomorphy rules*, Aronoff 1974) did not always result from a rigid ban on extrinsic rule order or opacity. Some researchers simply felt that recognition of such a class was called for, irrespective of the exact workings of the remaining part of the phonological component (cf. Anderson 1976). Ef-

fectively, proposals to cut up the derivation into a morphophonological part, a 'phonological' and perhaps even a 'phonetic' part help to shape the overall theory into the direction that was then also taken by the Lexical Phonology model (cf. *supra*) with its distinctions between level I and level II phonology of the lexical phonology, and the distinction between lexical and post-lexical phonology. A substantial part of the former cases of extrinsic *rule* ordering were taken care of by the extrinsic ordering of (*sub*)*components*.

All the above developments were largely driven by the desire to arrive at better accounts of the observed data (especially alternations) and not necessarily by a concern with 'psychological reality'. It had not escaped people's attention that SPE-type accounts using extrinsic rule ordering had a tendency to reflect the *historical development* of languages. Heuristically, it actually paid to consider the historical phonology of languages (if known) before embarking on a synchronic analysis, although it was also understood that the synchronic account could not be motivated on the basis of historical arguments. However, the analysis was ultimately an attempt to tackle the logical problem of language acquisition and not to reconstruct the historical development. The input and derivation had to be justified on internal grounds plus whatever principles and mechanisms UG was making available to the child (which, presumably, did not include introductions to the historical development of the language). Thus, phonological analysis was supposed to be 'psychologically real', i.e. there was supposed to be some sort of relationship between the elements of the analysis and what was actually happening in the mind of (idealized!) native speakers.

This mentalistic side of the SPE-model was also criticized in its own right, quite independently of the kinds of formal considerations that we discussed above. Linell (1978) offers both an overview of and a contribution to this line of work. Overall, *experimental* work offered little confirmation of the 'history-recapitulating' aspects of SPE-analyses that the more concrete approaches (such as *Natural Generative Phonology*) were also seeking to dismantle. In other words, experimental research and concerns with the restrictiveness of theory were going in the same direction.

### 2.3 Representational issues predominate the eighties

Meanwhile, 'back at the fort' (i.e. MIT), things were not standing still. Proponents of the SPE-approach, having channeled some of the discussions concerning rule ordering and depth of derivations into the development of lexical phonology (Kiparsky 1982), had shifted their attention to the **representational** properties of the SPE-model. As of the mid-late seventies and continuing dur-

ing a good deal of the eighties, an explosion of new ideas concerning various aspects of phonological representations started dominating the phonological scene (cf. van der Hulst & Smith 1982a). The incentive for some of these developments came from the rejection of SPE's ban on syllable structure (Vennemann 1971; Fudge 1969), as well as from pre-SPE models that had argued for parallel or syntagmatic organization in phonological representations, alongside vertical, syntagmatic organization (cf. Firth's *Prosodic Analysis*, Firth 1948; Harris's long components, Harris 1948). In some cases isolated individuals had independently developed similar ideas (cf. Rischel 1972 concerning the representation of stress).

The minimalist idea of a phonological representation being a strict linear arrangement of unordered feature bundles gave way to a rich view of representations in which segments sprouted into hierarchically arranged tiers and association lines, leaving 'a pair of empty brackets' (represented as Cs and Vs or Xs on a skeletal tier) as a trace of the linear segment string. The skeletal units not only functioned as anchor points for features on their tiers, but also themselves formed the starting point of a hierarchical organization, starting with syllabic (and subsyllabic) groupings, and resulting (via feet and groupings of feet) into various higher level categories such as 'prosodic words', 'prosodic phrases', 'intonation phrases' and so on (cf. Nespor & Vogel 1986). Representations became (very) multilinear; cf. van der Hulst and Smith (1982b) for an early overview of all these developments.

Even though all these developments concerned the representational aspect of the theory, there were potential intrinsic trade-off relations between the derivational and the representational aspects. The enrichment of the representational side of the theory had, or was hoped to have, an effect on the derivational side. It was expected that rich representations might allow for the reduction of rules to elementary operations that insert or delete association lines. However, even such 'elementary operations' can be extrinsically ordered or be responsible for dramatic changes if not properly restricted. In addition, adding subsegmental hierarchical structure and adding rules that construct suprasegmental hierarchical organization lead to novel possibilities for the creation of intricate derivations in which rules that manipulate the subsegmental feature structure may precede or follow the addition of, for example, syllable structure.

With respect to the 'features', now organized in a hierarchical structure (cf. Clements 1985), an independent issue (already raised in Sanders 1972) arose concerning the question of whether all of the features are binary-valued. Leaving aside isolated proposals to invoke multivalued features, the question of

*unary* (or *monovalent*) features drew some attention. Since SPE's chapter nine it was, of course, known that there are significant recurrent asymmetries between the two values of most, if not all, features. Certain values are universally more 'expected', 'more natural', or 'more frequent' (either across the board or in specific phonological environments) and Chomsky and Halle had tried to capture this in their theory of markedness. They replaced the 'favored' values by 'u' (for *unmarked*) that were not supposed to 'count' for or contribute to the cost of rules and representations. Markedness theory was superseded by a re-emergence of underspecification theory, with the significant extra idea to leave 'unmarked' values simply unspecified (Kiparsky 1982). Chomsky and Halle had rejected the mechanism of underspecification of redundant feature values because of technical problems, notably the emergence of 'ternary power'. (This problem disappears when extrinsic ordering is rejected, as shown in Ringen 1977, but this, as we have seen, was not accepted at MIT.) Briefly, Kiparsky solved the ternarity issue by imposing that for each feature only one value can be left unspecified. This idea of 'radical underspecification' (not only leaving out redundant but also unmarked values) made all features, at least in the input representation, virtually single-valued and this effect 'bled' any serious consideration of true single-valued features in 'mainstream' generative phonology.

My goal, as in the preceding section, has not been to offer a detailed and motivated account of the developments and proposals, but simply to show that, indeed, there was this period in the history of generative phonology, of intense focus on the structure of phonological representations. However, I wish to make one additional point before we turn to the nineties.

In discussing the representational developments, I have limited my reference to what could be called 'mainstream' generative phonology, i.e., proposals and developments that stem from North American institutions, mainly MIT. However, 'meanwhile, back in the old world', very similar and interesting proposals had been developed concerning phonological representations that were (and still are) completely ignored in the 'mainstream'. I am referring here to the model of *Dependency Phonology* (developed by John Anderson and various other phonologists, mainly in Great Britain). This work (that goes back to the early seventies!) culminated in Anderson and Ewen's (1987) *Principles of Dependency Phonology* (PDP). I regard the PDP-theory as the most principled and fullest alternative to the SPE-theory, and it is in many respects just as programmatic and unconstrained. The important insights that we referred to above as being characteristic of mainstream phonology in the eighties are all there. We find subsegmental, hierarchical organization of phonological features (pre-

ceding similar ‘mainstream’ ideas by a decade), a truly radical expression of markedness in terms of single-valued features (called components), hierarchical organization of segments into syllabic constituents and higher structure, and, perhaps most importantly, a consistent recognition of the importance of asymmetrical relations between phonological units. The latter relations are expressed in terms of the notions *head* and *dependent*, which occur at all layers of the phonological representation. Independently of PDP, headedness had also found its way into several aspects of the ‘mainstream’ models. For example, the metrical theory of stress and its extension into ‘prosodic phonology’ was founded on the idea of heads and dependents, obscured in the earliest versions by the use of S and W-labels (for heads and dependent, respectively); cf. Hayes (1995). In the theory of ‘feature geometry’, headedness was used in the guise of ‘arrows’ that identified major articulators (cf. Sagey 1986) or features that occupied the root node (McCarthy 1988), or vowels being ‘heads’ of syllables (sometimes in the context of head-based theories of the syllable; cf. Levin 1985). However, there was never a general claim concerning the omnipresence and potential of this notion. This was only so in Dependency Phonology, which therefore had that name. The PDP-model was also founded on the idea of there being a *Structural Analogy* between the various components of the grammar (Anderson 1992). According to this idea, structures in the various components of grammar are expected to be parallel, modulo the effects of differences that result from starting out with different primitives. Mainstream generative phonology, on the other hand, proceeded on the assumption that phonology is different, and that, hence, significant parallels between different components are not to be expected (cf. Bromberger & Halle 1989); cf. van der Hulst (2000, to appear) for further discussion of these points. I will return to the PDP-model at the end of this article.

The reasons why Dependency Phonology generated so little interest and research are largely a result of a perhaps more empirically-driven research mentality in North America, as opposed to a more conceptual attitude in Great Britain and perhaps Europe at large. Other (sociological) factors (involving fame and fashion) play a role too, but a consideration of these would take us too far afield.

## 2.4 Derivational issues predominate the nineties

We now expect, following the dialectic model, that the next shift in attention will bring us back to derivational issues. Indeed, this is exactly what we see and it all happens right at the beginning of the nineties. The nineties are dom-

inated by the approach called *Optimality Theory* (OT), a ‘non-derivational’, *constraint-based* approach to phonology. At first sight, it may seem odd to introduce a *non-derivational* theory as the main player in a decade that is supposed to focus on derivational issues. However, in accordance with the diagram in (1), OT is a theory about the function F, and in that sense it concerns the derivational side of the theory. The above reference to ‘non-derivational’ as a property of OT refers to the fact that OT does not recognize or need so-called *intermediate levels* as a consequence of there being no extrinsically ordered rules or subcomponents (such as a lexical and post-lexical component).

#### 2.4.1 *Constraints in phonology*

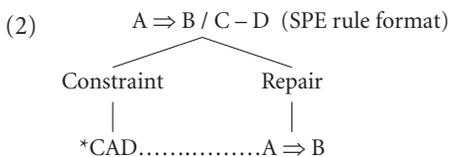
The history of constraints in phonology does not start with OT. Constraint-based phonologies (or proposals moving in that direction) have been around in phonology (and linguistics) for a long time. Shibatani (1973), Sommerstein (1974), and Clayton (1976) offer early proposals to use ‘output constraints’. In general, all proposals to abandon extrinsic ordering, going back to Koutsoudas, Sanders and Noll (1974), Vennemann (1971, 1974), Ringen (1977), and Hooper (1976) lead to phonologies without intermediate levels. In other cases, even though several levels are adopted, mapping between these takes place in one step; cf. Goldsmith (1993), Lakoff (1993), among others. Prince and Smolensky (1993) also mention a long list of researchers that have anticipated their OT constraint-based approach. For an historical perspective on constraints in phonology see Paradis and Prunet (1993) and Bird (1995). OT instantiates a particular version of this approach with the specific property that, in this model, constraints are violable (or ‘soft’). This softness of constraints results from the possibility of imposing an extrinsic ordering on the constraints, allowing them to be violated in the output if more highly ranked constraint enforces this.

In a striking way, OT, while being derivational in the most minimal sense, resembles the champion of derivationalism, viz. the SPE model. Both theories specify the function F as a set of extrinsically ordered statements. There are additional parallels between both models (cf. van der Hulst & Ritter 2000b). Both models employ one *type* of statement (rules or constraints, respectively), which means that both models are ‘holistic’. OT, like SPE sees no need to formally distinguish ‘morphophonological’, ‘phonological’ and ‘phonetic’ phenomena. It might, furthermore, be argued that OT attributes to the (idealized) native speaker an account of the data that may very well be plausible from the viewpoint of historical developments. When introducing the concept of historical changes to students, most linguists will make reference to the fact

that all aspects of languages are under constant pressure to change in the direction of ‘preferred patterns’. However, language is a system that must meet various requirements. Its grammar must be learnable and its representations are interfaces to other modules (involving production and perception on the phonological end and conceptual structures on the semantic end). We all, at some point or other, make reference to the ‘ongoing battle’ between ‘production’ and ‘perception’, implying that certain sound changes are motivated by ‘articulatory ease’ while others facilitate ‘ease of perception’. And we then add that what is good for one may be bad for the other. In addition, the outcome of both ‘forces’ may pose problems for learnability by causing allomorphy, and thus increasing the distance between input and output. Hence, there is *potential* conflict between various sound changes and *inherent* conflicts between sound changes and learnability. The latter force, then, may cause various forms of analogy, undoing ‘the point’ of sound changes. Any language, at any given point in time, is thus the result of this ongoing battle in which certain universal tendencies temporarily take precedence over others. It seems obvious that OT, like SPE, but in a different way, translates a plausible diachronic scenario into a synchronic account of how native speakers capture the regularities and irregularities of their language (cf. Boersma 1998). This does not mean that OT is wrong as a viable model of a speaker’s competence. It could be on the right track and I return to the issue of OT psychological plausibility below.

#### 2.4.2 *Constraint-and repair approaches*

Before we look at the way in which OT works in somewhat more detail, I will discuss another constraint-based approach that predated OT, the so-called *constraints-and-repairs* (CR) model. The idea of a constraints-and-repairs approach has been put forward in, among others, Hockett (1955), Stewart (1983), Singh (1987), Calabrese (1986, 1988, 1995, to appear), Paradis (1988) and Paradis and Béland (2002); cf. La Charité and Paradis (1993) for an overview of several constraint-based theories. In comparison to the rule-based approach advanced in Chomsky and Halle (1968), one might say that the CR-approach breaks up the ‘rules’ that were used in classical generative phonology, into two parts:



Once these two ingredients are teased apart, it becomes possible to say that one constraint may be ‘served’ by a variety of repairs that act in different ways on violations of that constraint, a situation referred to by Kisseberth (1970) as a ‘conspiracy’. Conspiracies cannot be dealt with in a satisfactory manner in a rule-based approach without the loss of generalization, since several rules would necessarily repeat the statement of the relevant constraint. For example, if the constraint states that two adjacent obstruents cannot disagree in voicing, and such a situation arises due to morpheme concatenations, different morphological contexts may trigger different repairs. In Dutch, for example, the second obstruent will agree with the first if the latter is part of the past tense suffix (in which case it is a dental stop) (3a), or in other cases (such as compounds or prefixes) if the second obstruent is a fricative (3b). If the second obstruent is a stop (and not part of the past suffix), the first obstruent will be the chameleon (3c):

- (3) a. klop + de ⇒ klopte (to knock, sing. past tense)  
       straf + de ⇒ strafte (to punish, sing. past tense)  
       b. op + zeggen ⇒ opzeggen (to cancel [a subscription], or to recite)  
       af + zeggen ⇒ afzeggen (to cancel [a date])  
       c. op + doen ⇒ obdoen (to put on)  
       af + doen ⇒ avdoen (to take off)

Thus (3a) and (3b) show *progressive assimilations*, while (3c) shows *regressive assimilation*; these assimilations are the ‘repairs’. All three repairs serve the same constraint. The resulting analysis (offered here for expository purposes only in a simplified form) could look like this:

- (4) a. Constraint: \* [±voice] [∓voice]  
       b. Repairs: i. [–continuant ] & past tense ⇒ [–voice]  
               ii. [+continuant] ⇒ [–voice]  
               iii. [ ] ⇒ [+voice]

The separation of constraints and repairs can also be motivated on *cross-linguistic* grounds. A specific constraint, let us say one that bars a sequence of two vowels, may in one language trigger deletion of the left-hand vowel, while in another language the right-hand vowel will fail to show up. In a third language, epenthesis (of some consonant) might be used to remove the violation.

Repairs, then, are primarily said to come into action when a combination of morphemes, which may be well-formed from the viewpoint of the morphological (categorical) and semantic requirements, violates a phonolog-

ical constraint. This leaves open the possibility that violations are not repaired, in which case the morpheme combination would simply be blocked. For example, in English, the comparative suffix *-er* cannot be added to polysyllabic adjectives (except those bisyllabic adjectives that end in *-ow* or *-y*):

(5) \*importanter

There is no repair and the form is blocked.

The use of repairs need not be limited to accounting for polymorphemic words. Monomorphemic words also must be wellformed and it is, in fact, likely that many constraints are valid for both classes of words. With reference to monomorphemic inputs, however, we do not, at first sight, expect any repairing since one would assume that such items, being stored in the lexicon, are wellformed to begin with. Why clutter the lexicon with items that are not wellformed? However, things are not that simple. Firstly, if wellformedness constraints refer to 'words' (i.e. free occurring items), why would bound forms such as affixes or (bound) roots have to comply with these constraints? In fact, often they do not. Secondly, to account for certain cases of allomorphy, we may set up underlying forms that, as such, cannot occur as free forms. Notoriously, this is so in any account of final devoicing in languages such as Dutch, where the voiced allomorph must be taken as basic. This latter case shows that repairs may apply in the absence of morpheme concatenation, because of the way that the underlying form is (or must be) chosen. If 'abstract' analyses are permitted requiring (absolute neutralization), more instances of strictly morpheme-internal repairs are called for; cf. Calabrese (to appear) for discussion.

In any event, perhaps wellformedness constraints do not need to apply to lexical forms as such; this is precisely what various authors have argued (cf. Paradis & Prunet 1993; cf. also Paradis 1993; Booij 1999 on this issue). OT also adopts the view-point that lexical forms are not restricted; this is referred to as 'freedom of the base'. The general idea is that since lexical entries can only manifest themselves as part of wellformed words, no special precautions are necessary for these entries.

Thus, repairs can only be seen in case violations of constraints are created in morphological concatenations or because of certain choices concerning the underlying forms, choices that are also driven by accounts of allomorphy. But morphological analysis induces violations to only a subset of all constraints. For example, English does not have any morphology that causes illformed syllable-initial clusters, but, surely, there are constraints on such clusters, which, some would say, arise as inductive generalizations over the set of

all words. (We can also think of their emergence in the grammar as a result of parameter setting.) However, a certain area of phonology, namely *loan word adaptation*, reveals not only that such constraints really exist, but it also makes us realize that grammars must have access to repair strategies that play no role in the adjustment of morphologically induced constraint violations. It is now evident why constraint-based approaches have shown great interest in the field of loan word adaptations (cf. Paradis & LaCharité 1997 for an overview; cf. also Paradis & Béland 2002).

A question that needs to be addressed is whether there are cases where several repairs apply to the same input, and whether in such cases extrinsic ordering is required. I refer the reader to Calabrese (to appear) who discusses cases of this sort. In the model proposed by Paradis (cf. 1988), it is shown that constraints and repairs may be restricted to certain domains, which may create the impression that violations are tolerated to persist simply because certain combinations arise across the boundaries that delimit the scope of certain constraints and repairs. A detailed discussion of such issues is beyond the scope of this article.

### 2.4.3 *Constraint-only approaches*

2.4.3.1 *Declarative Phonology*. I start this section with a brief introduction of a constraint-based approach, called *Declarative Phonology* (Scobbie 1997; Bird 1990, 1995; and Coleman 1992, 1995a). This approach views the (phonological) grammar as consisting of constraints only; no repair rules are allowed, based on the principle that structure-changing operations cannot be allowed in the grammar. Constraints are not extrinsically ordered and thus not violable or 'soft'. As a consequence, every constraint must be met in the entire output (which is effectively equivalent to the requirement in *Natural Generative Phonology* that all phonological rules must be 'true generalizations of all the surface forms'). Ogden (1999) combines the declarative model with a Firthian approach to phonological representations, while Coleman (1995b) integrates the lexical/post-lexical distinction. Declarative Phonology does not claim that phonological constraints are universal or innate, and consequently lexical entries are seen as constraints themselves. The surface forms result from the *unification* of constraints. Thus 'applying' a process to a form is achieved by unifying the relevant lexical entries with the constraint that expresses surface-true processes or generalizations. To accommodate certain types of phonological and (I suppose, all) morphophonological alternations, lexical forms must be provided with choices (disjunctions). Declarative Phonology strikes me as a restrictive, sound and promising approach which therefore deserves

more attention and exploration even if it perhaps relies on representational assumptions that are too strict or too ‘phonetically-driven’. More on that below.

**2.4.3.2 Optimality Theory.** I now turn to OT. In what way does this model differ from constraint-and-repair models? We have just seen that for any given input that violates some constraint, a variety of possible repairs is in principle available (either within a single language or cross-linguistically). In the constraint-and-repair approach, one needs to specify which repair applies in the appropriate circumstances. This is a parametric choice, assuming that UG makes the set of choices available to the child. One might, although this has never been done in principle-and-parameter approaches, formalize the choice of parameter values in terms of ranking of the choices, putting the choice that applies to the language at issue at the top of the list. Suppose now that all possible repairs of an input that violates some constraint are simply freely made available (in the form of a so-called candidate set) and that the correct output is selected by choosing the one that conforms to the repair that is at the top of the list of possible repairs. We then arrive at an approach to repairs that is almost the one found in OT, but not quite yet.

Before we get to that, let me mention that approaches that select the correct output from a set of viable candidates have been proposed now and then (cf. Lapointe & Feinstein 1982). Such models, essentially prefiguring the gist of OT, interestingly never gained much support (sometimes being accused of being ‘global’, i.e. using look-ahead mechanisms.).

In OT, repair rules are disguised as ‘anti-repair’ constraints. Instead of rules that either delete the first vowel, or the second, or insert a consonant (all to repair a constraint against vowel clusters), OT has *constraints* that *forbid* the deletion of a left-hand vowel or right-hand vowel, or the insertion of a consonant. Such ‘constraints’, however, are very different from the constraints that trigger the ‘repairs’ in the first place. The latter (the *real* wellformedness constraint, I would say, which OT calls ‘*markedness constraints*’) make reference to the structure and content of output representations. The former (called ‘*faithfulness constraints*’), on the other hand, *compare* two levels of representation, the input and the output. In order to know that some segment has been inserted, deleted or altered in the output it is, after all, necessary to look at the input form. So how do faithfulness constraints and markedness constraints work together in OT to select the correct output?

To answer this, we first need to account for the situation in which there is no repair at all. How can a form that has a vowel cluster surface at all? (OT ignores the option that such a form is simply rejected, maintaining that there

always is an optimal output and thus there are no such things as *absolute ungrammaticality*; I ignore that point here.) A possible answer is that a vowel cluster can surface in case the language simply lacks the constraint against vowel clusters. OT does not follow this course. It assumes that all markedness constraints (and all faithfulness constraints) are part of the UG contribution to every grammar; they are innate. After all, with the mechanism of ranking already in place, we can simply say that constraints that are violated rank below all possible faithfulness constraints that refer to possible repairs of this constraint:

- (6) don't delete V1, don't delete V2, don't insert C >> \*VV

In (6), it is said that it is worse to violate any of the anti-repair constraints than to violate the wellformedness constraint that forbids vowel clusters. Hence, the language allows vowel clusters. The faithfulness constraints do not even have to be ordered among themselves. When repair does apply, the markedness constraint can be located just above the faithfulness constraint that can be violated:

- (7) a. don't delete V1, don't delete V2,\*VV >> don't insert C  
 b. don't delete V1, don't insert C,\*VV >> don't delete V2  
 c. don't insert C, don't delete V2,\*VV >> don't delete V1

(7a) applies when the language has consonant insertion, (7b) when it has deletion of the right-hand vowel, and (7c) when it has deletion of the left-hand vowel.

This aspect of OT (i.e. the relative ranking of faithfulness and markedness constraint) recapitulates the conflict between learnability and phonological processes. Faithfulness constraints serve learnability by militating against any difference between input and output. Markedness constraints serve ease of production and perception at the cost of allowing processes that change the input. OT also recapitulates potential conflicts between different phonological processes. My favorite example comes from an account of iterative stress systems that can be found in two basic varieties, i.e. quantity-sensitive systems (cf. Hayes 1995). In non-OT parametric approaches this choice is represented as a parametric option. As said before, parameter choices *could* be represented in terms of ranking the two values. Effectively, this is exactly what happens in OT, but the ranking is, in some cases, claimed to involve two independent markedness options that are in conflict. In the case of iterative footing one might argue that there is an inherent conflict between a regular binary (thus rhythmic) grouping, which turns every other syllable into a rhythmic beat,

and the acknowledgment of intrinsic weight differences between syllables. Such weight differences militate against placing ‘heavy syllables’ in non-beat positions. Given, then, that there are two potentially conflicting constraints, both language types can simply be seen as resulting from different orderings:

- (8) a. Regularity (i.e. have a regular binary grouping)  
 b. Weight (i.e. place heavy syllables in beat positions)
- Quantity-sensitive: Weight >> Regularity  
 Quantity-insensitive: Regularity >> Weight

The parametric approach ‘shortcuts’ the prioritization of the two independent constraints by positing one (binary) constraint. Once the parameter is set, it functions effectively as a constraint and the difference with OT is that ‘the other constraint’ (i.e. the other value of the parameter) is simply not there. (It would be an error to conclude that no grammar can display evidence of both settings; this is certainly possible in case both settings apply to different domains; cf. van der Hulst & Ritter 2002 for discussion.)

The OT-model (first introduced in Prince & Smolensky 1993; McCarthy & Prince 1993; cf. Kager 1999; McCarthy 2002 for introductions) has been enormously popular (both among phonologists and non-phonologists), so much so that agendas for conferences and job vacancies changed almost overnight. Its popularity was due, aside from intrinsic properties of the model, to widespread marketing and distribution of the seminal manuscripts, and subsequent practically unmonitored posting of papers on an internet site that sends out messages of new arrivals to all who register. In addition, the model was introduced with rigorous (or at least impressive) formal underpinnings and an almost obligatory set of notational conventions. At the same time its essential design was almost ridiculously simple (constraint and ordering, that’s it). All this created enormous homogeneity among the fast-growing number of applications of the theories to both old and new data.

But there are certain problems. As might be expected, these problems have led to a number of variants of the model, while having little effect on the enthusiasm for the general idea of constraint ranking. Here, I cannot discuss variants of OT in any detail; cf. Ritter (2000) for a collection of critical articles.

The most interesting problem concerns *opacity*. OT predicts that it does not exist. The effects of counterfeeding and counterbleeding rule application (creating the effects of overapplication and underapplication, respectively) that required the extrinsic ordering of rules in SPE, cannot be replicated in OT. SPE could postulate a rule A that could be violated in the output because some later

rule B had introduced potential new inputs for A (counterfeeding), or a rule A whose output would be modulated by later rules so that it would seem that it that rule A failed to apply in the relevant cases (counterbleeding). It was felt that such rules, despite being opaque, express *significant generalizations*.

Let us imagine that in the case of Dutch, involving the voicing assimilation facts, a further rule enters the grammar that deletes vowels in certain cases, creating clusters of voiced and voiceless obstruents (this is actually possible in fast speech):

- (9) tabak  $\Rightarrow$  tbak ‘tobacco’

Given an OT grammar that selects [db] as the optimal output for inputs that contain /tb/ there is just no way to get [tbak] as the optimal candidate, given the free availability of other candidates (such as [dbak]) that do not contain the violation. (With enriched representations in which, for example, an empty vowel slot remains between *t* and *b* the output form [t-bak] would not violate the voicing constraint since *t* and *b* would not form a cluster.) Hence such an approach would allow certain types of opacity; cf. van der Hulst and Ritter (2000a) or Goldrick and Smolensky (1999).

This prediction of OT, one might argue, fits nicely with the widespread suspicion that arose in the seventies against opacity and the mechanisms that made it possible (rule ordering) and as such it could have been seen as a welcome result. However, the architects of OT had never been suspicious of opacity as such and they never had quarrels with rule ordering in the seventies or (eighties). OT was not developed to arrive at more ‘concrete’ grammars and its consequence for opacity thus came as somewhat of a shock and disappointment. In this sense OT, or rather its proponents again view phonology very much in the spirit of the SPE-model. They are not prepared to give up on the ‘significant generalizations’ that were embodied in the opaque SPE-rules. There is by now a variety of (to my mind compromising) approaches to ‘solve’ the opacity problem (cf. McCarthy 1999, 2002). Among the more interesting ones are those that reintroduce the extrinsic ordering of phonological subcomponents, for example Kiparsky (2000), who combines OT with the lexical/post-lexical division, thus allowing the post-lexical constraint system to introduce opacity with respect to the lexical system; in the Dutch example above, the vowel deletion process in (9) could be seen as belonging to the post-lexical phonology where the voicing constraint occupies a different place in the ranking. Another approach (advocated in Goldrick & Smolensky 1999) seeks to understand opacity effects in terms of a richer notion of representations (cf. supra, and the discussion in van der Hulst & Ritter 2000a who compare

this idea to an approach called Government Phonology; Kaye, Lowenstamm, & Vergnaud 1985, 1990; Kaye 1990, 1995; Harris 1994; Ritter 1995; van der Hulst & Ritter 1999, 2002).

But OT faces other problems as well. It is hypothesized that all constraints are universal. This may appear reasonable for those phenomena that involve 'natural' processes (cf. Stampe 1973 who also argued that such processes are innate and have to be unlearned if not applicable in the language that is being learned.) However, many phonological alternations are morphophonological and thus limited to specific environments which can sometimes be quite arbitrary or 'crazy'. It is simply ridiculous to claim that quite arbitrary phoneme substitutions (which result from specific historical scenarios involving 'telescoping' of several processes) manage to make their way into the human genome. Calabrese (to appear) argues that such cases simply reflect learned rules in which there often is no generality in the configuration that is targeted, nor in the change that is made.

The innateness claim is not only totally unlikely when it comes to morphophonological effects, it is also untestable. Any given constraint (needed for a set of quirky facts in some language) can be tucked away low in the constraint hierarchy of other languages so that its effect will never emerge. This problem of unfalsifiability, however, is not devastating for OT. There is no deep reason why OT could not embrace the concept of learned constraints. In fact, Hayes (1996) proposes that *all* constraints are learned, although he especially focuses on constraints that express natural processes. He proposes that such constraints are derived from intrinsic phonetic tendencies (or language-specific 'exaggerations' of these) in the process of language acquisition.

OT also faces psycholinguistic problems. According to the theory, optimal outputs are selected from an infinite set of candidate outputs, rather than from a small set of 'reasonable' alternatives. It is not clear how this translates into a realistic model of (even an idealized) native speaker's competence. Selecting an optimal candidate from infinite sets of all possible structures is a way of looking at the problem of characterizing wellformedness, but it does not seem viable as a model of what occurs in people's finite minds.

This brings me, finally, to a criticism of current OT that does not regard the theory as such, but rather the practice of it. Let us assume that the problem of dealing with morphophonological alternations can be dealt with in a satisfactory way, either by following Calabrese's proposal to allow learned rules, or by allowing learned constraints (both of which make reference to specific morphological contexts). Let us also accept that it is possible to translate parametric choices between repair rules into ranking of anti-repair (faithfulness) con-

straints and translating all other parametric choices that regard wellformedness ('markedness') of phonological representations into ranking of these choices. Finally, let us realize that so-called 'principles' (expressing properties that all languages have) can also be seen as constraints, albeit that they have to be universally 'top-ranked'. In short, let us admit that it is *possible* to view the grammar as a set of ranked constraints. What, then, is the (or my) problem with the practice of OT? The problem is, in my view, a serious neglect of representational issues. Following the dialectic pattern of phonological theory, it seems that the nineties must be the decade for paying attention to the **content** of wellformedness constraints, i.e. to *the phonological representations that they are supposed to characterize*. OT is often referred to as a theory of constraint *ranking*. As such, it can be applied to anything (ranging from language to religion and everything in between). The interest of OT as a phonological (or linguistic theory) lies, however, in the content of its constraints. So, one might ask: what is a possible constraint? I don't believe that the question is fair. Rather, it seems to me that we need to realize that the set of wellformedness (or 'markedness') constraints IS, as a whole, a theory of phonological representations. This means that the constraint set (whatever the exact form of constraints) must form *a tightly organized and coherent set of statements*, based on a finite set of primes and a finite set of principles, regulating the set of complex expressions that can be formed from those primitives. It would seem obvious that the set of primitives and combinatorial constraints must be kept as minimal as possible so that the set of possible phonological systems (or grammars) is kept small. After all, unless something has changed lately, it would seem that a theory's explanatory value is still inversely related to its restricted make-up. Theories that employ constraint ranking have to be extra careful in this respect since the addition of every constraint causes an exponential increase in the number of possible grammars; for  $n$  constraints, adding one, multiplies the set of possible grammars by  $n+1$ . (I am ignoring here that the set of possible grammars is further enlarged by allowing equal ranking of constraints, as well as a possibly infinite array of 'conjoined constraints'.) Compare this to parametric theories in which the addition of one parameter multiplies the set of grammars by two. One would think that this fact cautions OT proponents in proposing new constraints. It does not look that way. In defense of the exponential growth of possible grammars, proponents of OT argue that many of the OT grammars allowed by any set of constraints 'converge' by allowing the same language. But should this be a license for increasing the set of constraints without caution (since they are innate anyway) and without any attempt to

construct the constraint set with reference to a coherent view of phonological representations?

To control the explosion of constraints, OT has argued that constraints need to be ‘grounded’, which is another word for ‘natural’ and in practice means that there must be a phonetic basis for the constraint. But little progress can be seen in developing such phonetic bases, despite the fact that more and more phonologists have returned to studying their phonetic text books, while formulating constraints with reference to phonetic notions involving formant frequencies and all sorts of gradient properties (timing, among others). The appeal to phonetics is, I suppose, stimulated by the idea that constraints must make reference to the output. It turns out, however, that the output is no less an elusive notion than the input. Where the permitted degree of abstractness of the latter can by no means be determined on a priori grounds, the same is true for the former. Is the output a ‘phonological’ level of representation (I would say: yes), or it is a ‘phonetic’ representation (stated in terms of motoric-articulatory instructions or perceptual-acoustic primitives)? Or is it perhaps the case that the distinction between phonology and phonetics is illusory to begin with? The latter view has become increasingly popular both in OT-circles and in *Laboratory Phonology* circles. There appears to be much confusion on this issue, but the idea that phonological output representations form a cognitive-symbolic level of representation, related to, but distinct from an also cognitive phonetic level (or levels, if production and perception are kept apart) becomes increasingly unpopular.

In sum, notwithstanding some insightful and interesting results, we must conclude that OT faces serious problems, mainly due to a lack of discipline regarding the formulation of ‘markedness’ constraints. It is not possible to construct a theory of phonological representations by randomly formulating constraints that make reference to all sorts of undefined phonological and phonetic concepts. OT, while focussing on the derivational aspect of phonology (following the dialectic path) cannot proceed fruitfully without starting to pay serious attention to constructing a theory of phonological representations (which is what the set of ‘markedness constraints’ should be).

Without this discipline, OT is ‘easy’ and all problems can be solved during a 9-to-5 working day (and that is not necessarily a good thing although our families will be happy). To demonstrate that OT is ‘easy’ and to encourage its users to side-step real solutions, I conclude with an example (that I made up to illustrate the point more clearly and to not point my finger at a specific analysis). Suppose one looks at a language in which vowels reduce in metrically weak positions, with one exception: final vowels never reduce. Here’s an ‘analysis’:

## (10) NonFinalReduce &gt;&gt; WeakReduce

This strikes me as the kind of analysis that is based on a logical mistake due to which *what needs to be explained* becomes the *explanation* (*Modus Explanando Explanans*).

### 3. Conclusions

There is, I believe, no intrinsic problem with viewing the choices that languages make in terms of ranking mechanisms instead of parameter setting (although the two remain notational variants until significant differences can be shown to exist). The crucial point I am making here is *not* that constraint ranking is bad per se. In certain cases, it has an appeal and it therefore may prove to have a place in phonology (perhaps mostly in the post-lexical phonology). The point is rather that OT does not offer a serious candidate for a theory of phonology because its constraints comprise no view, let alone a coherent view on the representations that they are suppose to define.

I believe that there are serious representational theories ‘on the market’. Many were developed during the eighties and I would like to draw specific attention (reflecting my own bias) to theories such as Dependency Phonology and, more recently, Government Phonology, which elaborates on and refines central concepts of Dependency Phonology. Government Phonology is, effectively, a constraint-based model, sharing properties with Declarative Phonology in allowing only hard constraints, although proposals for some ranking have been made (cf. Polgárdi 1998). In van der Hulst (in prep.) I provide a detailed overview of Dependency and Government phonology (cf. also Scheer to appear). Currently, proponents of both approaches are small in numbers, which means that much work remains to be done. But the ideas are, to my mind, interesting and powerful, deserving more attention. It is perhaps also possible to benefit from the formalisms such as those advanced in Declarative Phonology circles, given that these lend themselves to the formulation of constraints that refer to somewhat more abstract representations (restricted in terms of universal principles and parameters) than are usually adopted in Declarative Phonology analyses. Let us hope, in any event, that the present decade is well-behaved in terms of our dialectic schema and that phonologists *will* start focussing on representational issues which would allow them to develop constraint-based approaches (possibly with a bit of ranking) that are truly explanatory. It is time.

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# Types of languages and the simple pattern of the core of language

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## 1. Introduction

I would like to present and substantiate several points which may be of crucial importance to the development of theoretical linguistics, although they have been largely neglected in the “mainstream” trends. After several decades of investigations and discussions first in the context of V. Skalička’s typology and then in that of the Functional Generative Description, I am convinced that they are significant:

1. The *physical character* of natural languages, which use phonetic means, imposes *strict limitations* on the relationships between meaning and expression. The limitations are decisive for the ways in which grammatical values are conveyed. Basically, grammatical values can only be expressed by (a) morphemes (b) alternations, and (c) the order of lexical items in a sentence. These properties constitute the fundamental background to the types of languages. A characterization of the types is discussed in Section 3.1 below, after a brief examination of the nature of general concepts used in connection with language typology in Section 2.
2. One of the main results of the history of typological thought is the transition from overestimation of Indo-European (old or modern) languages to the recognition that typological change does not constitute a line of “progress” and that language types are *not* directly connected to the semantic *richness* of languages (see Section 3.2 below).
3. The interactive nature of language is reflected in the sentence structure by the opposition of topic (T) and focus (F), i.e. by the relation of *aboutness*, of a “psychological” predicate F and its argument T as the background to

the meaning (i.e. of the underlying structure) of the sentence, with T being the linguistic counterpart of “given” and F that of “new” information. As a general rule, a sentence cannot be interpreted on the basis of a predicate structure of a shape similar to  $R(a_1, a_2, \dots, a_n)$ , with arguments  $a_i$  in the positions of subject, objects, etc., but only as  $F(T)$ , with its negative counterpart corresponding to  $\neg F(T)$ , i.e. adding an operator (negative, positive), and working with typed lambda calculus. The T-F articulation, the analysis of which is beyond the scope of this paper, has been characterized as one of the basic aspects of sentence syntax in Sgall et al. (1986), Hajičová et al. (1998).

4. Distinguishing between (underlying) *sentence structure* and the *morphemic* means of its expression in the sense of paragraph 1, it is possible to regard the core of the language system as patterned in a relatively simple way (using a dependency-based grammar and an underlying order of T preceding F); see Section 4. In this light, the pattern of the core of language comes close to what is usually supposed to belong to the general human *mental abilities*; the large and complex periphery of language can be described as consisting of contextually restricted specific deviations from the core (see Section 5).

## 2. General concepts in typology

### 2.1 Terms and notions

It is crucial for our discussion to distinguish between (a) different meanings of an ambiguous term, and (b) different approaches to a single object of study. While in case (a) it is relevant to ask about the terminological appropriateness of the different uses of the term, in case (b) the question is which of the approaches is most suitable in the case of the given object. Thus, one should distinguish:

- i. whether the term ‘typology’ is used in the sense of a theory of *language types*, or without being connected with a notion of type; in the present contribution, language types are involved, so that attention is not devoted to partial typologies, such as those oriented towards the semantics of quantification (as with B. H. Partee), incorporation (E. Bach), or other more or less specific layers (as e.g. W. Croft’s or T. Shopen’s writings on syntax), or towards areal typology,<sup>1</sup>

- ii. whether different uses of the term ‘language typology’ concern a single object or not, i.e. whether the investigations aim at an analysis of languages covering their basic properties; this question certainly is not simple, since the use of the term *language type* varies from one author to another in many aspects, some of which are discussed below.

In any case, these distinctions (cf. Sgall 1971) are relevant to a delimitation of different aims, each of which is certainly interesting and important in its own right.

## 2.2 The concept of type of language

A methodological basis for typology may be found in Hempel and Oppenheim (1936), who use *comparative concepts* and characterize objects as closer to or further away from a given point (or extreme).<sup>2</sup> The notion of type of language was developed in a similar orientation in the classical *Prague Linguistic Circle*, in which Skalička (1935, 1979) characterized type of language as a collection (cluster) of properties intrinsically connected by probability implications of the form: “if a language has property A, then it probably also has property B”, i.e.  $P(A,B)$ . The fundamental nature of these *probability implications* was had already been specified by G. von der Gabelentz (1894: 5f.), according to whom “...die Erscheinung A trifft mit so und so großer Wahrscheinlichkeit mit B, C, D usw. zusammen, selten mit E, nie mit F” Skalička assumes that probability implication P is a symmetric relation: if  $P(A,B)$  holds for a pair A,B, then  $P(B,A)$  also holds.

From the understanding of the notion of type as based on probability, it follows that the types are ideal extremes not fully attainable by existing languages; the languages come closer or less close to one (or more) of the existing types, with properties of different types co-occurring in the structure of every existing language. Typology, based on clusters of properties, thus differs from a *classification*, since the latter can be based on a single property and distinguishes between classes on a yes/no basis, i.e. strict classification is based on a partition of a set. Let us just recall that language typology and research into language universals – known esp. from H. Seiler – study similar classes of phenomena, although they analyze problems of different kind.

### 3. Types of languages

#### 3.1 Language types as based on the means of expression of grammatical values

If the concept of type of language is understood along the lines discussed in Section 2 above, then it appears appropriate – instead of Skalička’s “mutual” favorability of the typologically relevant properties – to work with their favorability as an *asymmetric* binary relation, i.e., from  $P(A,B)$  neither  $P(B,A)$ , nor  $\text{non-}P(B,A)$  follows. With this approach, Praguian typology has been brought to a stage at which it is not necessary to work with lists of typologically significant language properties as Skalička did. It is now possible to identify a *single property* as favorable to all the other features characteristic of a certain type (see Sgall 1995 and the writings quoted there).

Such a fundamental property may be seen in the way of expression of grammatical values, understood in a broader sense, as including the formation of lexical units, i.e. derivation of words, their composition, borrowing from other languages, or creation of lexical units composed of more than one word. While lexical values are conveyed by strings of phonemes (lexical morphs, roots) in all languages, grammatical values have different means of expression, the repertoire of which is limited by the conditions given by the phonic nature of language.

It is then possible to work with three instances of the fundamental property as giving rise to five types, thus arriving at the types which were specified in the 1930s by V. Skalička on the basis of the tradition of typological research (having culminated in the works of F. N. Finck and of E. Sapir) and of his extensive knowledge of many languages from all parts of the world:<sup>3</sup>

- a. The grammatical values are also expressed by morphemes, which themselves are rendered by morphs (strings of phonemes), and these can have one of two different forms:
  - a1. they resemble the strings conveying lexical meanings – both kinds of morphs are often monosyllabic, and their word order positions vary at least within certain grammatically fixed boundaries; this is the *analytic* type, in which grammatical values are conveyed by function words; this basic property is favorable to (i) an abundance of dependent (embedded) verb clauses with conjunctions, (ii) the absence both of case endings and of agreement, which is favorable to grammaticalized word order (i.e. to the main syntactic functions expressed by positions of the words, e.g. with the order SVO required by grammar); the word order cannot then be used on a large scale

to distinguish topic and focus; this is favorable to the presence of articles (since definiteness has much in common with givenness and indefiniteness with introducing new discourse referents); the absence of affixal derivation is favorable to lexical conversion (e.g. the type *stone wall* in English) and to numerous loan words; many words are unmotivated, non-derived, and may then well be monosyllabic, which is favorable to a large number of vowels, being useful to distinguish the short words; English, French, but also e.g. Hawaiian are Skalička's examples of languages coming close to the extreme of this type, although each of them is far from reaching such an extreme;<sup>4</sup>

- a2. or the grammatical and derivational morphs differ from lexical morphs by being attached to them as affixes, each of which expresses a value of a single category; this is the type of *agglutination*, with a high number of morphemic cases (although the subject is expressed merely by the bare lexical morph) and an unclear boundary between them and adverbs derived from nouns (i.e. between grammatical morphemics and lexical derivation), with many deverbal nominals (nouns of action, of actor, of artefact, adjectival participles, etc.), long word forms, and (since in such word forms the differences between all the phonemes are not necessary to distinguish the forms) with phonemic reductions such as vowel harmony; languages close to this type are e.g. Turkish, Hungarian, Finnish, Georgian, Basque, Eskimo, Armenian.
- b. The grammatical values are expressed by modifications of lexical morphs, i.e. by *alternations*, which occur either
  - b1. at the end of the lexical morphs, as is the case with the *inflectional* (fusional) type: a single ending is present with every word form, with no clear boundary line between the ending (i.e. alternation) and the stem, so that we face an alternation of the end of the lexical morph, rather than an affix (e.g. Lat. *anima, animae, animis*); the single ending expresses a set of cumulated functions (case, number and gender, or person, number, tense, mood and diathesis, etc.); the case of the subject (nominative) has its own endings; the endings exhibit a high degree of synonymy and of ambiguity; agreement abounds (a verb agrees with its subject, an adjective with its head noun); a high degree of "free" word order is present, which (together with the sentence prosody) conveys the topic-focus articulation; a dependent (embedded) verb often has the form of an infinitive without a preposition; inflectional conversion is frequent (i.e. derivation has the form of subsuming the derived word into a class other than the deriva-

- tional basis (Lat. *anima* – *animus*); inflection prevails in Latin and other Old Indo-European languages, in Russian, Czech, etc.;
- b2. or in the inner part of words – *introflexion*: phonemes (especially vowels) occurring within individual lexical morphs serve to express word formation and morphemics; this basic property is favorable to further features similar to those of inflection; in Semitic languages this property is combined with agglutination.
- c. Grammatical values are expressed just by the order of lexical morphs – *polysynthesis*: the boundary between lexical and grammatical units is unclear; there are many compound words (Vietnamese, written Chinese, Yoruba, Thai).<sup>5</sup>

Even from this very short survey it may be seen that the five language types do not concern only the morphemic shape of word forms. On the contrary, the strength of the different types in individual languages is relevant to many layers of the language systems and to many features of communicative activities. Also *syntactic* properties come into play, such as the form of a dependent verb or of subject and object, the presence of agreement, or the presence of a copula vs. that of an agglutinative affix with the predicate nominal (as in Turk languages, see Giger & Vykypěl 2001). In *phonemics*, e.g. the number of vowels (and the presence or absence of their harmony) is involved; cf. also Plank (1998), who, quoting G. Fenk-Oczlon and A. Fenk, writes that agglutination is favorable to simple syllable structure.

Thus, Comrie's (1981:78) requirement, according to which language types should cover most different layers of language, appears to be met to a relatively high degree by this approach to typology. In any case, a fully holistic typology seems to be excluded; it is an empirical fact that the clusters of typologically relevant properties have never been found to be strong enough to cover the whole of a real language, rather just as theoretical, idealized constructs (cf. Note 4, as for the disadvantages which would concern the "complete" types).

The advantages of implication laws stated by Holenstein (1985) are preserved in the probabilistic approach. This concerns above all the possibility to identify a *hierarchy* among the properties concerned, which is lost if only a prototype and its periphery are distinguished (and also if one works with symmetrical 'favorability', which made Skalička formulate lists of properties of individual types, rather than adduce their fundamental properties). The basis of this hierarchy is anchored in the opposition of lexicon and grammar, i.e. in a fundamental opposition present in every natural language (although the boundary line between these two domains is not clear-cut, especially with re-

spect to the opposition of cases and denominal adverbs, to that of verbs and deverbal adjectives, or to certain pronouns as opposed to personal endings.

The fact that the implication laws underlying typology are based on probability makes it necessary to work with a *quantitative* evaluation. It is important to base this evaluation on values that are of fundamental importance to the language types, rather than on those that are easily accessible for counting. Thus, it is not sufficient to concentrate on the length of sentences or of word forms. Instead of this, e.g. a procedure allowing for the identification of the degrees of inflection may take into account phenomena decisive for this type, such as the cumulation of functions in the ending, or the synonymy and the ambiguity of endings. Using such a procedure (see Sgall 1983), a characterization of Czech declension resulted in the following values: 58 morphemes, 116 endings, 26 values of morphological categories, 34 morphs; three relevant indices can then be assigned the following values:

index of cumulation of functions –  $58:26 = 2.23$ ,  
 index of synonymy of endings –  $116:58 = 2$ ,  
 index of ambiguity of endings –  $116:34 = 3.41$ .

This method was applied (with a certain simplification) for a comparison of Slavic languages by I. I. Revzin and his colleagues, see Volockaja et al. (1963). Also further studies devoted to a typological comparison of languages on the basis of Skalička's view, which check and enrich this approach, confirm that every language contains properties of different types, which is a consequence of the probabilistic character of Praguian typology, and one of its basic ingredients.<sup>6</sup>

If the 'natural morphology' approach by W. Mayerthaler and others (now see Dressler 2003) is taken into account, it is possible to see at least a certain degree of naturalness (especially of iconicity) in some of such *combinations* of different types (cf. esp. Popela 1991, 1999; Nau 2001; Giger & Vykypl 2001; Sgall 1988). Thus, agglutinative features in word derivation often occur in languages of other types thanks to the prototypical situation in which a semantically specific derivation base is combined with affixes having more general meanings (e.g. diminutives, feminine nouns derived from masculines, or J. Kuryłowicz's 'syntactic derivation' of switching the parts of speech). On the other hand, inflectional morphemics (even with its irregularities) seems relatively appropriate for a short way of expressing the most frequent kinds of word forms, i.e. especially for the case forms that express subject and objects (or Actor, Objective, Addressee, etc.). The other (adverbial) cases are often connected with prepositions, i.e. analytic function words, in inflectional languages as well,

and such means seem appropriate to express adverbial and attributive relations between autosemantic lexical words; they connect two such words, whereas a derivational affix just expresses a semantic adaptation of a single word.<sup>7</sup>

Many other phenomena have features of two or more different types, such as the ergative sentence structure, or suppletive forms. Often even individual word forms exhibit properties of more than one type, e.g. those of analysis and of inflection in combinations of prepositions and articles such as French *du, des, au*, etc., German *am, vom, zum*, not to mention inflected analytic function words such as articles distinguishing gender and number in the just cited languages (Schwegler 1990: 148) or of the affinity between function words and affixes (Plungian 2001), both of which represent grammatical means having the form of morphs, i.e. belong to the class of types specified as (a) in Section 3.1 above.

The importance of typological studies for understanding issues of *diachrony* has been known for more than a hundred years. Gabelentz (1901: 255–258) characterized the typological development of languages as a spiral going from “isolation” (or, in Skalička’s terminology, polysynthesis) to agglutination, from there to inflection, then to new “isolation” (or analysis). This movement, which differs from a trend of enriching the lexicon and the grammar, can be understood as a more or less regular rotation of types, rather than as a route for the “progress” of language. The existence of exceptions to Gabelentz’s spiral was pointed out in Skalička’s (1941) observations on the declension in the eastern branches of Indo-European languages, which develop from inflection “back” to agglutination. Perhaps this is due to the rapid development of civilization leading to higher regularity, and to different internal conditions for such external factors: if in the epoch of rapid external changes inflectional endings were more or less weakened in a language, this might have been decisive for the growth of regularity to reduce either the endings themselves, substituting them by prepositions, or the number of different paradigms and other inflectional intricacies.<sup>8</sup> The typological development of the Indo-European languages was properly enriched by Vennemann’s (1974) analysis of the topic-focus articulation.

The Prague image of the types of languages may be understood on the one hand as a result of having its roots in the history of linguistics, to which we now turn, and, on the other hand, as offering certain highly important insights into the nature of language (see Section 4 below).

### 3.2 Three main lessons from the history of typology

Only an extremely brief comment on certain points of the development of linguistic typology can be presented here, based on more detailed studies (summarized in Sgall 1995) and intended to help delimit the position of Prague typology among the trends in studying types of languages.

As Ramat (1995) recalls, in the 18th century G. Girard distinguished between “analogous” and “transpositive” languages; this may be compared to the difference between languages with a fixed SVO word order and those with a higher degree of “free” word order (and with rich sets of inflectional endings). This forerunner of typology, and also his successor A. Smith, understood the language types as given once and for all, recognizing a possible change of type only in cases of “mixing” of types. Similarly, in A. W. Schlegel’s approach one of the basic ideas was the absence of a change of type. The view of language types as rigid categories connected to some psychological values of ethnic groups was more or less clearly discarded by Humboldt (1836),<sup>9</sup> but emerged again, especially with H. Steinthal’s ethnopsychological view of Indo-European languages as superior. Another turn towards a sober analysis of linguistic structures themselves, including the interplay of typologically different properties within a single language, can be found in the adaptation of H. Steinthal’s work by Misteli (1893). Also the works of Max Müller, A. F. Pott, A. Hovelacque, W. D. Whitney and other linguists of those epochs are highly illustrative from this point of view. Later, F. N. Finck changed his attitude along these lines between his two books (1899, 1910).

A similar change can be seen in the reasoning on *diachronic* typology – from A. Schleicher’s “Sprachaufbau-” and “Sprachzerstörungsperiode” with the Proto-Indo-European in between as an ideal stage, exhibiting all the richness of morphemics, and Jespersen’s (1894) “progress in language” (with Modern English as superior to other languages) to G. von der Gabelentz’s “Spirallauf”, mentioned in Section 3.1 above.<sup>10</sup> Gabelentz’s expectations concerning what now could be called a *holistic* typology can be characterized by his (1901:481) requirement that our knowledge of any relevant property of a language should immediately lead to a specification of its other properties, similarly as in biology knowing a leaf we know the properties of a tree (see Plank 1991).<sup>11</sup>

E. Sapir’s approach was fully freed from the old prejudices, but lacked systematic attention to the interconnections between individual linguistic properties. Such interconnections were studied in Prague especially in Mathesius’ (1928) ‘linguistic characterology’ and in connection with Jakobson’s (1929,

1958) revealing idea of implication laws. Jakobson pointed out the goal-oriented, *teleological* nature of language (now see Leška 1986, 1987; Toman 1995:141; Sériot 1999).<sup>12</sup> Within this linguistic context Skalička (who understood laws of the form of strict implications to be important to phonology, rather than to grammar in the narrow sense) specified his five types as based on probabilistic affinities and constituting a relatively highly holistic characterization of languages. The just quoted formulation by G. von der Gabelentz may find at least its partial parallels in Prague typology: e.g., if we know that a given language typically expresses a syntactic connection of two nouns by (i) a preposition, (ii) an ending, (iii) an affix, or (iv) a compound, then we can predict that this language will exhibit other properties of the relevant type, too, i.e. (i) analysis (having embedded clauses introduced by conjunctions, an infinitive with a preposition, articles, many auxiliaries and monosyllabic words, etc.), (ii) inflection (with a complex set of personal and case endings, a prepositionless infinitive, agreement, a high degree of “free” word order, and so on), (iii) agglutination, or (iv) polysynthesis.

The predictive power of Praguian typology is certainly lower than ideal, due to the probabilistic nature of this approach. In another respect, the predictive power depends on the scope of the clusters of properties which constitute the types. The clusters are larger with the Praguian approach than with the word-order based typology of Greenberg and others (see esp. Mallinson & Blake 1981), which has brought many highly valuable insights not only into the word order, but also into the general conditions of the order of morphemes within words in most different languages. Let us just remark that the differences in the degrees of “free” word order might be assigned much more importance for the characteristics of language structure than is done here. The most frequent word order in Latin (Czech, Russian, etc.) is SVO, as is the case in English, French, and also in Chinese; however, it does not seem to be optimal to regard all these languages as belonging to the same type. Greenberg (1995) opens a way to a much more general view of typology, especially to that of the change of type.

In another sense, search for a holistic typology may be seen in Coseriu’s (1980, 1983) approach, which stresses the integrational character of typology; however, diachronic transitions between types are not abrupt and thus phenomena of different types are present in a single language, as Coseriu (1980:169) admits, being aware of the difference between type and class (p. 167). The large set of writings by Coseriu and his followers certainly is the richest source of European typological thinking in general and especially of Romance linguistics. However, a question still unanswered is the degree of the

explanatory and predictive power of his integrational typology, which understands a type primarily as being characteristic of a single language as the basic patterning of its functional layer. A comparison with Skalička's approach has been presented by Geckeler (1988), see also Dezsó (2000) and Kretz (in prep.).

From regarding the types of languages as rigid categories connected to assumed psychological values of ethnic groups, the development of research has led to a sober analysis of linguistic structures themselves, bringing to the foreground especially the following points:

- a. Large-scale differences between the structures of languages are determined by relationships displaying certain degrees of probability, which constitute *clusters* of properties, language *types*.
- b. The image of language types should not be blurred by their assumed connections with some intrinsic psychological values of languages or with their semantic richness. As we know from Skalička and his predecessors, the types are basically *semantically equivalent*. The experience corroborates the view that although translating between English and Czech (or Japanese, Chinese, Arabic, and so on) certainly is a more complex task than translating between English and French, still the typological differences only make the translation process less easy rather than impossible.
- c. The typological differences concern relationships between (*underlying*) syntax and (*morphemic*) surface, as was stated by Ramat (1985:20); more specifically, they are based on the way of expression of grammatical values (and of word formation), see Section 3.1 above.

## 4. Typology and the nature of language

### 4.1 Typology as a challenge for theoretical linguistics

A theoretical descriptive framework should allow us to describe a language as preferring a certain type. Close connections between parameters, especially those concerning the relationships between (underlying) sentence structure and morphemics, should be reflected. Stratificational models distinguish the defining functions of transducing automata (suitable e.g. for movement rules) from matrices of locally conditioned modifications (such as the choice of morphs); the latter are connected with a 'lower cost'. Perhaps different descriptive frameworks can correspond to different language types, with properties of other types being marked, 'more expensive'. The teleonomic explanations

of the favorability among properties should be analyzed from the viewpoints appropriate to goal-directed systems.

#### 4.2 Fundamental oppositions within the language system

If (as just mentioned), the differences between the types of languages are determined by relationships between sentence structure itself and its means of expression, i.e. between the levels of Curry's (1962) *tectogrammatcs* and *phenogrammatcs*, then it is important for understanding the nature of language to be aware that these relationships belong to the essential properties of its structure. An analysis of the opposition of these two levels has been carried out in the descriptive framework of Functional Generative Description, FGD, see Sgall et al. (1986), Panevová (1994), Hajičová et al. (1998). FGD works (along with phonemics and phonetics) with two sets of sentence representations, viz. with the tectogrammatical (underlying syntactic) level and with a level of morphemics. In the tectogrammatical representations (TRs), the lexical occurrences proper (autosemantic) are represented by nodes in dependency trees (or, with the inclusion of the relations of coordination and apposition, in multi-dimensional networks with a basically simple patterning).<sup>13</sup> The tectogrammatical correlates of function words have the form of indices of the lexical node labels, i.e. of syntactic and morphological symbols representing values of functors (Actor, Objective, Addressee, Manner, Locative, Cause, etc.) and of grammatemes (Plural, Preterite, Imperative, Comparative, etc.), respectively. While one of the dimensions of the tree (schematically indicated as "top-down") corresponds to syntactic dependency, the other dimension (left-to-right) serves to represent the underlying word order (the scale of communicative dynamism in the terminology of the topic-focus-articulation theory). On the other hand, a morphemic sentence representation has a single dimension (left-to-right, surface word order); it is a string of more or less closely connected symbols, i.e. morphemes.<sup>14</sup>

What we face here is the opposition between (underlying) syntax and (morphemic) surface. It appears to be appropriate not to continue working with an intervening level of "surface syntax" in the theoretical description of language (see Sgall 1992). Thus, essentially, we are coming back to the classical opposition between *syntax* and *morphemics*. Similar to the opposition between *grammar* and *lexicon*, this is a pair of old concepts, richly discussed and shown as a cornerstone of plausible hypotheses. Both these oppositions have served for many centuries of linguistic research, giving ground for a modular understanding of language, although their boundary lines are blurred by "gray

zones” of transitions, of intermediate phenomena (between morphemics and word formation, syntax and analytic morphemics, idiomatics, and so on).

### 4.3 The simple pattern of the core of language and its vast periphery

In the Prague School, the dichotomy of the center and the periphery of the language system has always been treated as one of the most important oppositions. Its analysis has been founded on R. Jakobson’s concept of *markedness*, the hermeneutic and theoretical roles of which are stressed esp. by Battistella (1995).<sup>15</sup> It is impossible to overlook the numerous aspects this concept in its different forms and sectors shares with *prototype* theory. What is especially relevant for us is the relationship between the relatively simple pattern of the unmarked phenomena, determining the *core* of language, which can be captured as based on a *simple pattern*, coming close to systems that may be understood as innate for independent reasons (propositional calculus). This view is made possible if the unmarked layer of sentence structure is accounted for by dependency trees with complex node labels, and unmarked, regular relations between syntax and morphemics are seen as prototypical.

Even if, along with different relations of syntactic dependency, coordination and the topic-focus articulation are also taken into account as aspects of sentence structure (of the TRs), the patterning of sentences can be described as (multi-dimensional) tree-like objects that may be univocally represented just by *bracketed strings* of symbols, each of which comprises a lexical component and indices for the values of grammatememes and functors, cf. e.g. the (simplified) representation (1′) for the sentence (1):

- (1) Jim and Jane’s son, who were present there, belong to the BEST specialists.  
 (1′) ((*Jim* ((*Jane*)<sub>Appurt</sub> *son*<sub>Sing.Def</sub>))<sub>Conj</sub> (<sub>Restr</sub> (*who*<sub>Plur</sub>)<sub>Act</sub> (*there*)<sub>Loc</sub> *be*<sub>Pret.Decl.Imperf</sub> (<sub>Obj</sub> *present*)))<sub>Act</sub> *belong*<sub>Pres.Decl.Imperf</sub> (<sub>Dir</sub> *specialist*<sub>Plur.Def</sub> (<sub>Restr</sub> *good*<sub>Superl</sub>))

Note: Every dependent item or collocation is enclosed in its pair of parentheses, the indices of which, i.e. functors, denote (i) either a dependency relation with its index attached to that parenthesis that is oriented towards its head: Appurt(enance, broader than Possession), Restr(ictive Adjunct), Act(or), Obj(ective), Loc(ative), Dir(ectional), etc., (ii) or a coordination construction with its symbol attached to the right parenthesis: Conj(unction), Disj(unction), etc. The indices by the lexical item (indicated only by its orthographic form, which has to be substituted by a symbol for lexical meaning), i.e.

grammatemes, correspond to the morphological values: Sing(ular), Def(inite), Pret(erite), Decl(arative), Imperf(ective), Superl(ative), etc. The items written to the left of their heads are contextually bound (in topic, in the prototypical case), those to the right of their heads are non-bound (in focus).

The transition between TRs and the *surface* (morphemic) forms of sentences can be handled by a set of rules (including movements) that does not surpass the generative power of one or two (subsequent) pushdown transducers, so that the whole description of language is not much stronger than context-free (cf. Plátek & Sgall 1978). A highly significant task then is to specify different forms of such devices appropriate to languages preferring one of the types (i.e. connected with a lower cost for the chosen type).<sup>16</sup>

Non-prototypical, marked phenomena in language are responsible for the existence of a vast and complex *periphery* of its system. Three layers of marked, secondary phenomena may be distinguished:

- a. marked members of grammatical, semantically relevant oppositions within the language core, such as the morphological values of Plural, Preterite, Imperative, or the contextually bound items, and so on;
- b. peripheral phenomena in the TRs, which constitute the marked layers of underlying sentence structure, e.g. coordination and apposition, or marked positions of focusing operators (see Hajičová et al. 1998), i.e. phenomena which require a more complex set of rules (or of descriptive devices) for specifying the set of TRs;
- c. contextually restricted relations between TRs and morphemic (or phonemic and phonetic) representations of sentences, which constitute a very large domain, ranging from ambiguous and synonymous items in the lexicon and in morphemics (with the sets of inflectional paradigms, their irregularities, etc.) to instances of surface word order not corresponding directly to the scale of communicative dynamism.

The *core* of language with its relatively simple structure is substantial for the child's acquisition of language; on the other hand, the complex, large *periphery* can be mastered by children step by step, with the specific, contextually restricted deviations and exceptions internalized one after the other, on the basis of analogy. Also a theoretical description capturing the core of language by relatively weak means (equivalent to a context-free grammar) should perhaps be accompanied by models of the non-prototypical subdomains and exceptional phenomena, based on lists of items relevant to the contextual restrictions of the marked points. Such a description may perhaps be based on an alternative

mathematical approach working with the concept of a collection or semi-set, in which the set membership is not fully delimited, see e.g. Vopěnka (1989).

Such a description should reflect the presence of two fundamental tendencies:

- a. the *consistency* of the *type* that is basic to the language described and the properties of which should be connected with low cost in the description of the relation between the underlying and the morphemic levels of this language (the low cost might also correspond to a high degree of communicative efficiency), and
- b. *naturalness* in the sense mentioned in Section 3.1 above, which might underlie a mechanism that would restrict the extreme type.<sup>17</sup>

Thus, the concept of *markedness* may play an important role also in connection with typology: within the structure of a language, the consistency of a given type can be understood as the unmarked, prototypical case, and one of the main tasks is to specify mechanisms appropriate to the description of the co-existing properties of other types, i.e. deviations, marked cases.

If the methodological requirements brought in with the Chomskyan revolution, i.e. explicitness in linguistic thought, not just in description, are to be connected with the main results of European functional and structural sources, then a simple pattern of the core of language can be gained: a set of TRs in the form of dependency trees, the possibility of a linearization of which (and of multi-dimensional networks including coordination) documents its fundamental perspicuity, see (1') above. A formal description of this core may then start from a view of prototypical relationships between the TRs and the morphemic strings. Only an extremely simplified scheme may be presented here as a starting point:

tectogramatical word form:  $\text{root}, (d_1, \dots, d_n), g_1, \dots, g_m, c$   
 d – derivational suffix (prefixes are to be handled similarly)  
 g – grammateme value (value of a morphological category)  
 c – functor value (syntactic relation)  
 n, m – natural numbers

*Agglutination* in its extreme form is based on a one-to-one relation between the TR and the means of its expression (morphemes), cf. e.g. Turkish *ev-ler-i-miz-in* 'to our houses' with the root *ev-*,  $d_1$  *-ler-* 'Plur.',  $d_2$  *-im-* '1st Pers. Possess.',  $d_3$  *-iz-* '1st Pers. Plur.',  $c$  *-in* 'Dative'.

*Analysis*: function words correspond to the functors and grammatemes, and no suffixes are present, cf. e.g. *E. to a house of mine*.

*Inflection*: the functors and grammatemes are expressed by a single ending (more precisely, by an alternation of the stem at its end, or, with *introflexion*, in the middle of it), e.g. Czech *matka* ‘mother.Nom.Sing’, *matce* ‘mother.Dat.Sing’, *matek* ‘mother.Gen.Plur’.

*Polysynthesis*: the most frequent functors are expressed by the order of bare word roots (e.g. in a pattern such as SVO or SOV), the expression means of other functors and of the grammatemes belong to other types or are of an intermediate character (words having a rather general meaning may be ambiguous, used also in grammatical functions, e.g. an equivalent of the verb ‘give’ in the function of Dative).

Many questions of different kinds remain open, one being how a description of the relationships between TRs and morphemics can best account for the two fundamental tendencies specified above as (a) and (b), i.e. the *consistency* of a type (connected with a low cost in the descriptive framework to be chosen) and its *limitations* (connected with naturalness and/or economy).

## 5. Conclusion: Typology helps towards an understanding of the nature of language

We have seen that, if Skalička’s specification of the language types is adapted to the asymmetric view of favorability, then the Prague typology of languages discloses an image of the fundamentals of types as anchored in the way of expression of grammatical values. This image requires a systematic distinction between the level of sentence structure and that of morphemics; it may be assumed that within the *core* of language the relations between units of these two levels are unmarked or prototypical. The pattern of the core is *relatively simple*, coming close to that of the propositional calculus and of other systems which for independent reasons may be viewed as being generally accessible to humans, i.e. determined by their innate properties. This may be useful in explaining the *easiness of acquisition* of language, analyzing the child’s language acquisition as founded on the interactivity of language in communication (cf. Schnelle 1981), rather than on complex innate mechanisms specific to the language faculty.

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## Notes

1. Highly important studies on causation, resultative, iterative and other constructions, as well as on general issues of typology, have been presented by the group led by A. Xolodovič, as summarized by Nedjalkov and Litvinov (1995). Also J. Nichols' characterization of language structures with syntactic relations marked either on the head or on the dependent word is of great interest, although often both kinds of marking co-occur, e.g., in many languages, the agreement of the verb with its subject is accompanied by the agreement of the adjective with its head noun and/or by case marking; cf. also Stolz and Urdze (2001).
2. If Ineichen (1991:2) claims that such a comparative approach is of advantage in medicine or psychology, rather than in linguistics, then it may be recalled that intermediate grey zones blurring the boundary lines between subdomains (and classes) of phenomena are certainly present in natural languages, and are crucial to their description (cf. e.g. Lehmann 1987 on preponderant classes and residues; also Sgall 2002).
3. Other layers of means of expression could be looked for in the domain of intonation and prosody, but these can hardly serve as something more than features accompanying the main means of expression (in the lexicon, as e.g. in Chinese, or in grammar, as e.g. in the case of interrogative intonation in several languages). Also a type (b3), with inflectional alternations at the beginning of word stems would probably lack the necessary minimum of perspicuity.
4. The extreme of the analytic type would contain no endings or affixes and no compounds, so that the lexicon of such a language would be fully scattered, without productive derivation means, cf. *sir* vs. *lady*; there would be no word class boundaries, i.e. an unlimited conversion such as that of *stone wall* vs. *wall stone*. An extremely agglutinative language would have only a single word basis, from which all lexical units would be derived by long strings of affixes. An extremely inflectional language would have a specific paradigm for every noun, adjective and verb, so that it would be no exaggeration to characterize the language, as Skalička did jokingly in the case of Old Greek, as a nice language, the learning of which requires no tedious memorizing of vocabulary, since having learned its grammar you know its lexicon.
5. Skalička rejected the older use of the term 'isolating', which was sometimes connected with an evaluation of such languages as displaying only poor grammatical patterns. Therefore, he used the term 'polysynthetic' and spoke about the isolating type of English or French, which has no affixes or endings, preferring function words, isolated from their lexical words, and which favors unmotivated lexical units, i.e. a dispersed lexicon, cf. Note 4.

In M. Giger's introductory remarks to Skalička (2002) more data can be found concerning Skalička's writings on the notion of incorporation, as well as on Finno-Ugric languages.

6. See esp. the analyses of Japanese (and of general linguistic issues concerning variation, communicative competence, and so on) by Neustupný (1978), of Slavic languages by Ďurovič (1973), more specifically on the West Slavic domain by Weiss (1983), Lotko (1997), and Giger (1998); Czech is compared to Russian by Popela (1988), to Baltic languages by Giger and Vykypěl (2001), and the interplay of different typological properties in West-European languages is examined by Čermák (1978), Uhlíř (1969, 1988) and Geckeler (2001).

7. This concerns also the fact that Semitic languages are predominantly agglutinative, although they exhibit a larger amount of introflexion than that present in other languages (Rubba 2001), as well as the well known agglutinative features in Latin conjugation (see e.g. Bossong 2001, who, without offering a new solution to the issue of language type, comments on Skalička's views in a rather superficial way, seeing indiscriminate, generalizing ("pauschalisierende") classifications in an approach that in fact works with properties of different types as combined in individual languages).

8. In his spiral, Gabelentz does not distinguish between the types he called analytic and isolating; on p. 257 he says that English seems to rush towards a pure isolating system ("...dem rein isolierenden Systeme zuzueilen scheint"). It may be assumed that while the analytic type occupies the position between inflection and agglutination in the spiral, the isolating type (called 'polysynthetic' by Skalička, owing to its way of word formation) stays apart, perhaps being suitable as a starting point to the whole development, never to be reached again. However, Gabelentz only speaks of the spiral development itself, rather than of a fixed state of its origin, not presenting any possibility of characterizing individual languages as more and less "developed" (positioned further away or closer to such a state). A further question would concern the (im)possibility of investigating how many times a language has been through a part of the rotating spiral (cf. the "secondary" agglutinative forms that include affixes developed from pronouns in some of the Romance languages, e.g. Italian *dámelo*). Even in the case of Indo-European languages we do not know much about their prehistoric development, although before the (partially and hypothetically) reconstructed shape of their common source, there may have been a number of such cycles. Even less can be said about the stages that may have preceded the known systems of languages of other families, most of which nowadays are agglutinative or analytic.

9. Kurzová (1993) and Hoskovec (1999–2002) do not understand the oldest known (and reconstructed) shape of Indo-European languages as a direct witness of agglutination, and speak only of a stage of 'derivational inflection' as preceding that of 'paradigmatic inflection', the latter subtype corresponding to Skalička's concept of inflection and the former to a stage in which the word forms in the sentence were less strictly grammaticalized (with a smaller range of agreement and of determination of case by the governing lexeme, lacking a clear boundary between lexical derivation and morphemics). It remains to be discussed whether this stage can be interpreted as a transition from an even older agglutinative system to (paradigmatic) inflection.

10. The beginnings of typology are described by Dezső (1999); many new insights on different approaches are offered by Kretz (in prep.).

11. A view similar to that underlying Gabelentz's spiral, as well as its synchronic counterpart, was characterized by Lehmann (1985) as a scale of grammaticalization, which comes close to the Prague typology in several respects. Another branch of grammaticalization can be seen in changes such as those mentioned in Note 9. Also, cases of degrammaticalization have been registered, see Kim (2001).
12. As for the question of terminology raised by Plank (1991), it still seems possible to believe, along with Ineichen (1991: 1) and others, that Gabelentz wanted to speak of typology, although the editors used the mistaken label "hypology" in the title of his posthumous paper, as well as (perhaps without effective checking) in the table of contents.
13. In Romance studies, this nature of language is discussed e.g. by Schwegler (1990: 177–183), quoting A. Martinet and H. Geisler.
14. Dependency-based syntax, known in European linguistics since the 1830 (thanks to K. F. Becker) and systematically elaborated by L. Tesnière in the context of functional structuralism, is one of the sources of Fillmore's Case Grammar. It differs from the descriptivist constituency and we prefer its structural trees to those of the minimalism approach, since (along with other advantages) they make it possible without any complications to distinguish between "to the left of" and "above" in the sentence pattern. FGD works with projective trees, which correspond to structures with continuous constituents.
15. Thanks to the substantial progress of computational linguistics (i.e. of the use of both structural and statistical methods in the elaboration of semi-automatic linguistic procedures), a descriptive framework can now be checked as soon as it is implemented and used not only in morphemic ("part-of-speech") corpus tagging, but also in syntactically annotating a large corpus. FGD is being checked now in this sense in the three-level annotation of texts from the Czech National Corpus, see Hajičová (2002).
16. One of the domains in which the concept of markedness has been used as a cornerstone, is research into child language acquisition, see Anderson (2000) and the writings quoted there, especially in analyzing the relationships between parts of speech, naming, reference, syntactic dependency, and predicate-argument structures. However, as long as intonation (prosody) plays a marginal role in child-language research and the predicate-argument relation is not understood as primarily serving the topic-focus articulation of the sentence, the nature of basic linguistic structures and of their role in child language acquisition cannot be properly recognized.
17. Note that in the issues of word formation also the tectogrammatical structure of words depends on typological properties.
18. In a somewhat broader sense naturalness can also be seen in syntax, esp. if issues of economy are taken into account. Thus, it may be understood as suitable for a language system to include much of grammatical information in lexical entries, especially the valency frames.

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# Attempt at the revival of Warrungu (Australia)

## Its cultural and scientific significance\*

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### 1. Introduction

A large number of people – community members and linguists among others – have responded to the crisis of language endangerment and they have been conducting a wide range of activities, which include those listed below:

- a. language documentation and language description;
- b. language revitalization;
  - b-i. language maintenance;
  - b-ii. language revival, and;
- c. fund raising, lobbying, awareness raising, etc.

(See Himmelman 1998 and Lehmann 2001 for the concepts of ‘language documentation’ and ‘language description’.)

The main aim of the present paper is to report an attempt at the revival of the Warrungu language of North Queensland, Australia – a case that seems to be unique among language revitalization.

Many linguists have tended to consider languages merely as the object of their academic curiosity and to ignore or neglect the interests and wishes of the people concerned. In this paper I will try to emphasize the community’s perspective, often citing community members’ statements verbatim. Partly due to this, this paper may sound too personal and too emotional to some readers. However, its personal and emotional overtone is unavoidable. Furthermore,

this overtone will give a clear idea of what it is like to be involved in a language revitalization movement and also what drives certain people to these activities.

I am very much in agreement with the spirit of Fishman's (1991:33) advice to 'RLS ers' (i.e. activists of Reversing Language Shift): "RLSers need not feel embarrassed, defensive or unsure of themselves for engaging in or advocating such efforts", and also with that of Spolsky's (1991: 137) statement: "dispassionate scholarship... easily serves to justify the absence of concern for the people whose fate is being studied".

The format of this paper is as follows. Section 2 briefly considers some of the general issues that concern language revitalization. They are repeated from portions of Chapters 10 and 11 of Tsunoda (forthcoming), to which the reader is referred to for further details. Section 3 outlines the Warrungu revival movement, while Section 4 explores its cultural and scientific significance, followed by Conclusion in Section 5.

## 2. Language revitalization: Some general issues

### 2.1 Definition of language revitalization

Language revitalization' may be characterized as 'restoration of vitality to a language that has lost or is losing this attribute' (cf. Spolsky 1995: 178). It is divided as follows.

- a. Language maintenance: this concerns languages that are endangered, but still alive.
- b. Language revival: this has to do with extinct languages.

### 2.2 Motivation for language revitalization

Devoted efforts are being carried out, in many parts of the world, to maintain endangered languages, and to revive extinct ones (Tsunoda 2001:8352). See, for instance, Maher (1995) and Sawai (1998) on Ainu of Japan; Amery (2000), Bell (1982), Hartman and Henderson (1994), Hudson and McConvell (1984), McKay (1996), Nathan (1996), Schmidt (1990), and Walsh and Yallop (1993) on Australian Aboriginal languages; Jeanette King (2001), Nicholson (1990), and Spolsky (1991, 1995) on Maori; Kendall King (2001) on Quichua of Ecuador; Cantoni (1996b), Hinton (1994), Reyhner (1997), and Reyhner et al. (1999) on languages of North America; Shoji and Janhunen (1997) on

language of Russia; Huss (1999) on languages of North Scandinavia; Maguire (1991) on Irish; and Jones (1998) on Welsh.

Why do community members want to keep their language? In general, retention of their language – and culture – does not help them, for instance, in the way of economic prosperity (although it may create employment, say, in tourism and education, e.g. as language teachers – where such opportunities exist). The answer is spiritual rather than material. Thus, Littlebear (1999: 1) (a Cheyenne person, the USA) states:

But why save our languages since they now seem to have no political, economic, or global relevance? That they seem not to have this relevance is exactly the reason why we should save our languages because it is the spiritual relevance that is deeply embedded in our own languages that is important. The embeddedness of this spirituality is what makes them relevant to us American Indians.

That is, the value of traditional languages is in their integrative, rather than instrumental or pragmatic, function (cf. Dorian 1978:608; Rouchdy 1989:96; Spolsky 1995:179). They have an instrumental function only to the extent that they are useful for purposes such as employment, say, as language teachers. But, more importantly, they have an integrative function in that they play an important role in maintaining the groups' identity, as an 'ethnic marker' (Edwards 1984:289).

In addition, according to the belief of Aboriginal Australians and Native Americans, their language is a gift from their ancestral beings, and it is their sacred duty to maintain it. (See 4.2-d for further details.)

### 2.3 Can languages be revitalized?

Is it possible to revitalize languages? There is no unequivocal answer to this question. The answer depends on (i) the definition of language revitalization, and (ii) the aim of a given revitalization program. We shall consider (i) and (ii) regarding language revival, rather than language maintenance. Language revival is the more directly relevant to the main theme of the present paper: attempt at the revival of Warrungu.

#### [1] *Definition of language revival*

We need to consider at least two factors: (i) definition of language death, and (ii) intactness of the structure.

- a. Definition of language death. Different people have different definitions of language death. Thus, according to one view, a language is dead when it is no longer used as the vehicle of communication in the community. In this view, revival of a language means restoring it to the state where it is again used as the means of communication. This is an extremely difficult task to achieve. According to another view, a language is alive as long as there is at least one speaker left – even if he/she is not fluent in it. In this view, a language is revived if at least one speaker emerges, irrespective of the degree of his/her proficiency in it. There are other views, e.g. the view that a language is alive if place names in the language survive. In this view, language revival is an easy job; mere replacement of a few place names with the indigenous ones will result in the revival of the language.
- b. Intactness of the language structure. If language revival means restoring a structurally intact language to the way it was spoken by its traditional, fluent speakers, then this task is formidable. In contrast, if language revival means use of any variant of the traditional language, or even a simplified form thereof, then the task is much easier.

## [2] *Aim of language revitalization*

Some people may have modest aims, while others may be more ambitious. Some of the people I interviewed in North Queensland stated that they wish to use their ancestral language (i) to say “Hello” when they meet on the street, (ii) to start a meeting, and (iii) to have road signs such as “Welcome to the country of . . .”. There are also people who seem to be happy if they can sprinkle a few Aboriginal words in their English sentences (see Schmidt 1990: 35). These aims are fairly easy to achieve. On the other hand, there are people who are not content with modest aims. They wish to speak their language – if not fluently like their ancestors. This aim is more difficult to achieve.

To sum up, the answer to the question as to whether a language can be revitalized depends the definition and aim of language revitalization.

The generally adopted – even when not explicitly stated – view seems to be that language revitalization means maintaining or restoring a language to such a state that it is spoken by a reasonable number of people, reasonably fluently, and in a reasonably intact form. See Amery (1994: 147), Schmidt (1990: 106) and Tovey et al. (1989: 33). (This characterization leaves unanswered the question of what is meant by ‘reasonable’.)

The best-known example of language revival is that of Hebrew. The language had died as the medium of everyday conversation more than 2,000 years ago, but it was revived with considerable effort in the late nineteenth and early

twentieth century; see Fishman (1991:289–336), Mesthrie (1994:1989), and Spolsky (1995). Another, well-known instance concerns Irish; it was revived in Belfast, Northern Ireland, in the middle of the English-speaking community (Maguire 1991). Irish is still spoken in a number of areas in Ireland, and it is not extinct. Therefore, the effort to revitalize Irish is an instance of language maintenance, and not language revival. However, Irish had not been spoken in Belfast, and as far as Belfast is concerned, this case is an instance of language revival, rather than language maintenance.

In the case of Hebrew, the revival has produced a large number of fluent speakers, and the language is used as the medium of daily communication in Israel. The revival of Irish in Belfast has produced a fair number of fluent speakers, but its use seems to be confined to the school environments and devoted families.

#### 2.4 Any ingredients for success?

What are ingredients for the success of language revitalization activities? No definitive answer seems to be known as yet. First of all, the people concerned have to be determined, committed and dedicated to the cause, and prepared to make a sacrifice, if necessary (Tsunoda 2001:8532; cf. also Amery 1994:147). Otherwise, their revitalization program will have no chance of success. However, these ingredients alone are not sufficient. Broadly speaking, language revitalization calls for a social environment that will facilitate such activities. Language revitalization does not concern language exclusively. It “forms part of a much broader movement towards reestablishing societies on a human scale...” (Fettes 1997:315) (see also Fishman 1991:18; Thieberger 1990:352). It requires “social justice” (Thieberger 1990:348, 352).

Unfortunately, however, these activities are beset with a multiplicity of problems, e.g. (i) dispersion of the population, (ii) inadequate funding, (iii) shortage of learning materials, and shortage of human resources (Amery 1994:147; Dorian 1992:145; Hinton 1994:222–224; Jones 1998:35; Schmidt 1990), (iv) pressing social problems and needs, which demand time and energy that are then unavailable for language revitalization (Schmidt 1990:10, 101), (v) dialects and standardization, and (vi) certain types of language attitude.

Looking at the other side of the same coin, remedies for these problems will enhance the success of language revitalization activities. However, it is very difficult to suggest effective remedies for them. For example, the government’s support (Nagy 2000:155; Tsunoda 2001:8352; Wurm 1998:192) will be helpful, and it may be provided in the form of the recognition of human rights

and financial support, among others. However, government support is not easy to obtain.

The present paper in the main deals with those problems which have to do with issues such as language attitude and – more broadly – identity. The problems concerning language attitude include the following, most of which are closely interrelated. (Issues of identity will be looked at in 4.2-a.)

- a. Often young people are not interested in their traditional language, and they are not motivated to learn it (Hudson 1994:163; Hudson & McConvell 1984:36–37), and some are even embarrassed or ashamed about it (Hudson 1994:163; Huss 1999:106).
- b. Community members are conscious of vocabulary, but they are generally unaware of the existence of morphological or syntactic structure (Amery 1994:141; Dorian 1995:132). Consequently, language lessons are often confined to the teaching of isolated vocabulary (Cantoni 1996a:vii; Greymorning 1999:11; Kendall King 2001:168–169; Schmidt 1990:88).
- c. Often the language to be revitalized is morphologically complex and consequently it is very difficult to learn for people whose first language is, say, English. See Dorian (1992:146), McConvell (1991:154), and Schmidt (1990:100, 109) on Australian Aboriginal languages, and Dorian (1994:492) on Irish. Learners may find the complexity of the language intimidating, and “trying to learn [the language – TT] can be a very demoralizing experience” (Amery 1994:147).
- d. In view of the problem mentioned in (c), proposals have been put forward to teach a modified form of the language (Powell 1973; Thieberger 2002:325). However, such an approach is opposed by ‘purists’ of the communities.
- e. The older generation may prefer not to teach the language to the younger generation at all, rather than to have it ‘corrupted’ by the latter. They may prefer to take it to the grave. See Dorian (1995:133), Schmidt (1990:20), Wurm (1998:197, 209), and Yamamoto (1998:220). Schmidt (1990:20) refers to this as “gemstone effect”.

These problems will be considered in 4.1, 4.4, and 4.6. They may appear insignificant to those readers who are not familiar with language revitalization activities. However, they are crucial issues for successful execution thereof.

## 2.5 Methods of language revitalization

What are effective methods of language revitalization? Again, there seems to be no definitive answer. Various methods have been employed, e.g. immersion method (e.g. Jeanette King 2001; Nicholson 1990 on Maori of New Zealand); master-apprentice method (Hinton 1994:231) for Californian languages; telephone method (Taff 1997) for Deg Xinag of Alaska; radio method (Maher 1995) for Ainu of Japan; and language adoption method (Fesl 1982) for Bandjalang of Australia (see 4.2-d for Bandjalang). The circumstance varies from one revitalization program to another, and a given program needs a method that will suit its own peculiarities. Thus, the immersion method, which needs an environment in which learners will only hear and speak the language, requires a fair number of speakers, and it will not be applicable to languages that have very few remaining speakers. In contrast, for such languages the master-apprentice method, in which a small group of fluent speakers (i.e. masters) and learners (i.e. apprentices) isolate themselves from, say, the English-speaking society in order to transmit the language, may be used. And so on.

Thus far, we have looked at some of the general issues that concern language revitalization. We turn now to the attempt at the revival of the Warrungu language of Australia, exploring its cultural and scientific significance.

## 3. Attempt at the revival of Warrungu

### 3.1 Fieldwork in the early 1970s: “I’m the last one to speak Warrungu”

From 1971 to 1974, when I was an M.A. student at Monash University, Melbourne, I conducted fieldwork in and around Townsville, North Queensland. My main focus was on the Warrungu language. Peter Sutton, who was an M.A. student at Macquarie University, Sydney, was carrying out research in the same area, his main focus being on Gugu-Badhun. Sutton and I recorded data from the last speakers of these languages. They passed away, and the languages became extinct.

In the case of languages that seem to have no or little chance of survival, the last speakers often wish, and indeed make every effort, to have their language recorded. This heartfelt desire and commitment were best expressed by the late Alf Palmer, the last fluent speaker of Warrungu, who used to say to me: “I’m the last one to speak Warrungu. When I die, this language will die. I’ll

teach you everything I know, so put it down properly". Indeed, Alf Palmer made admirable efforts to teach me everything he knew.

In retrospect, it was Alf Palmer who taught me the importance of documenting endangered languages. His was perhaps one of the earliest responses to the crisis of language endangerment. It was made twenty years before the publication of works such as Hale et al. (1992) and Robins and Uhlenbeck (1991), which are possibly the first works that urged linguists to pay serious attention to language endangerment. Note also that this response was made by a community member (whose language was facing extinction). This shows that the concern for endangered languages and the realization of the importance of their documentation are neither novel nor invented by the linguistic academia. (I owe this observation to David Nathan, p.c.)

### 3.2 The language revival movement starts

More than a quarter century later, a few groups of people there, including Warrungu and Gugu-Badhun people, started a movement to revive their ancestral languages, and they approached Peter Sutton and me, requesting us to assist their activities.

I was first approached in April 1998. In March 2000, I visited Townsville (where many Warrungu and Gugu-Badhun people live nowadays), after 26 years since my last visit of 1974 to the area. The visit was followed by another visit in March 2001.

Like many other groups, Warrungu and Gugu-Badhun people no longer live in their respective traditional territories, and they are scattered over a wide area, which includes the city of Townsville. The dispersion of the population is one of the many obstacles to language revitalization movements, as noted in 2.4.

Now, many of the people involved in the language revival activities turned out to be grandchildren of the last speakers whom Peter Sutton and I had recorded in the early 1970s. The visits were like visiting old friends or relations. Although I had never met them previously (except for Rachel Cummins, who is Alf Palmer's granddaughter), they already knew about Peter Sutton and me, and about our work. Some of them had obtained copies of relevant field tapes from the Australian Institute of Aboriginal and Torres Strait Islander Studies, Canberra (with which I lodge all my field tapes), and they even knew my voice.

### 3.3 Warrungu had a unique phenomenon called syntactic ergativity

In March 2000 and March 2001, I had preliminary discussions with a number of people. One day in March 2001, I was having a discussion with Rachel Cummins (who is the central figure in the Warrungu revival movement) and her husband, John Cummins (who is not a Warrungu person, but a Gugu-Yalanji person). Rachel and John have five daughters: Meaghan, Tyrelle, Knomi, Tahlia, and Mheelin. They were listening to our discussion. I described one aspect of the Warrungu language roughly as follows.

Warrungu had a phenomenon that linguists call syntactic ergativity.<sup>1</sup> This phenomenon is unique among the world's languages. It mainly occurs in Australia, in Queensland, and in fact, in North Queensland. It occurs in Warrungu, and also Warrgamay, Girramay, Jirrbal, Mamu, Yidiny, and Djabugay. Because this phenomenon is unique among the world's languages, it is a very important part of the cultural heritage – not only for the people of this area, but also for the entire humankind.

Upon hearing this, Knomi, one of the five daughters, said, “I’m curious”. Later, in June 2001, Rachel said to me in one of her e-mail messages, “You inspired Tahlia”, and also “Tahlia is eager to go to Japan to study with you”. (Tahlia was 17 at that time.) I was truly honoured to hear all this. Unfortunately, however, it is not feasible for Tahlia to come to Japan for study. She does not know the Japanese language. It is difficult to obtain a scholarship. The alternative is for me to go over to Australia and teach the Warrungu language.

### 3.4 Warrungu lessons start

In January and February 2002, I prepared some materials for Warrungu lessons, and in March 2002, I visited Townsville, accompanied by my wife (Mie Tsunoda), for the first round of Warrungu lessons. The duration of the lessons was one week, from the 17th (Sunday) to the 24th (Sunday). A brief outline of the lessons is given below.

Two of Rachel and John Cummins’ daughters, namely, Tahlia (then 17 years old) and Mheelin (then 15 years old), attended the classes, and so did Mie. Rachel was able to attend the class only twice or three times. The other three daughters would have liked, but were unable, to attend the classes, due to other commitments.

17th (Sunday). Tahlia, Mheelin, Rachel, and Mie attended the class. At the beginning, I stated, “We are witnessing a very historic moment – the first les-

son of the Warrungu language revival". Everyone knew the significance of the occasion.

I played one of the tapes I had recorded in the early 1970s. It contains stories narrated by the late Alf Palmer in Warrungu. Needless to say, Rachel has many fond memories of her grandfather. But Tahlia and Mheelin were born after their great-grandfather had passed away. They had never met him, and never heard his voice. We were listening to Alf Palmer's stories. I looked around, and noticed that Tahlia's eyes were filled with tears. So were Mheelin's eyes. And Rachel's eyes and Mie's eyes, too.

After listening to the tape, I explained the writing system (that Rachel had approved in March 2001, and that is employed in the present paper). We practiced its use by writing and pronouncing a large number of Warrungu words. Rachel, Tahlia, and Mheelin recognized some of the words, such as *bama* 'man', *gaja* 'head', and *jina* 'foot'. In the list of words for practice, I had included the names of some of the family members. Rachel knew a few of them, e.g. *jimbilnggay* 'Alf Palmer', and *jabinu* 'Roderick Palmer' (Alf Palmer's grandson and Rachel's cousin). The inclusion of these names was a success; it captured these Warrungu people's great interest.

18th (Monday). We practiced expressions such as greetings, 'Thank you', farewell, and self-introduction ('My name is ...'). We then tried to work out how much knowledge of the Warrungu language had been retained. All of Rachel, Tahlia and Mheelin had some knowledge of Warrungu. For example, Mheelin (15 years old) knew at least 4 Warrungu words and perhaps a dozen more words from other languages. Rachel knew many more words.

19th (Tuesday). In the morning, I described the research conducted on Warrungu and available Warrungu materials (mainly the data I had recorded). We also had a discussion as to the motivation and goal of learning Warrungu. In the afternoon, we turned our attention to grammar. First, simple sentences. I explained the concept of 'S', 'A', and 'O' (cf. Note 1), and compared the case-marking in English and that in Warrungu. It is interestingly to note that the ergative-absolutive case-marking system and the ergative suffix of Warrungu did not seem to pose much difficulty.

20th (Wednesday). We embarked on the study of syntactic accusativity and ergativity. First, regarding syntactic accusativity of English, I described the conjunction reduction (i.e. coreferential deletion in coordination) that does not involve passivization. An example is (1).

- (1) a. *The man (S) went.*  
 b. *The man (A) drank water (O).*  
 c. *The man (S) went and [A] drank water (O).* (S=[A])  
 (The square bracket indicates deletion.)

Then, concerning syntactic ergativity of Warrungu, I described the conjunction reduction that does not involve antipassivization. An example is (2).

- (2) a. *bama-Ø yani-n.*  
 man-ABS (S) go-NONFUT<sup>2</sup>  
 ‘The man (S) went.’  
 b. *warrngu-nggu bama-Ø nyaga-n.*  
 woman-ERG (A) man-ABS (O) see-NONFUT  
 ‘The woman (A) saw the man (O).’  
 c. *bama-Ø yani-n*  
 man-ABS (S) go-NONFUT  
*warrngu-nggu [O] nyaga-n.*  
 woman-ERG (A) see-NONFUT  
 ‘The man (S) went and the woman (A) saw the man (O).’ (S=[O])

For each of English and Warrungu, I gave the students some exercises.

21st (Thursday). We proceeded with the study of syntactic accusativity and ergativity. I described the deletion in English that involves passivization. An example is (3).

- (3) a. *The man (S) went.*  
 b. *The woman (A) saw the man (O).*  
 c. *The man (S) went and [S] was seen by the woman.* (S=[S])

Then, I described the deletion in Warrungu that involves antipassivization. An example is (4).

- (4) a. *bama-Ø yani-n.*  
 man-ABS (S) go-NONFUT  
 ‘The man (S) went.’  
 b. *bama-nggu gamu-Ø bija-n.*  
 man-ERG (A) water-ABS (O) drink-NONFUT  
 ‘The man (A) drank water (O).’  
 c. *bama-Ø yani-n*  
 man-ABS (S) go-NONFUT  
 [S] *gamu-nggu bija-gali-n.*  
 water-ERG drink-ANTI-NONFUT  
 ‘The man (S) went and the same man (S) drank water.’<sup>3</sup> (S=[S])

In the Warrungu lessons I use the terms ‘passivization’ and ‘antipassivization’, without which it is impossible to discuss these phenomena.

22nd (Friday). We had some exercises on syntactic accusativity and ergativity. An example of exercises for English is (5):

- (5) Combine the following two sentences, and delete the second occurrence of the same word. Use passivization.
- a. *The kangaroo (S) went.*
  - b. *The dog (A) bit the kangaroo (O).*

An example of exercises for Warrungu is (6):

- (6) Change the second sentence into an antipassive sentence, combine the first sentence and the antipassive sentence, and delete the second S.
- a. *warrngu-Ø nyina-n.*  
 woman-ABS (S) sit-NONFUT  
 ‘The woman (S) sat down.’
  - b. *warrngu-nggu manyja-Ø muja-n.*  
 woman-ERG (A) food-ABS (O) eat-NONFUT  
 ‘The woman (A) ate food (O).’

23rd (Saturday). We had more practice with the writing system. I dictated a large number of Warrungu words for the students to write down.

24th (Sunday). We had additional exercises with the kind of syntactic ergativity that involves antipassivization. This concluded the first round of Warrungu lessons.

In August 2002, the second round of lessons were conducted, from the 12th (Monday) to the 17th (Saturday), which were attended by Rachel, Tahlia and Mheelin Cummins and also Mie Tsunoda. About a half dozen more people – most of them adults – had been planning to attend the classes, but they were forced to withdraw as an unexpected and urgent land rights meeting coincided with the lessons; the issue of land rights is a crucial one for Aboriginal Australians.

The August lessons provided further exercises on simple sentences and syntactic ergativity, and introduced new expressions such as ‘Let’s . . .’ and ‘Yes, let’s . . .’. Prior to the August lessons, David Nathan prepared a multi-media CD of the stories that we had listened to on the 17th March 2002. During the August lessons, using the CD, I explained points of grammar and vocabulary, and we practiced pronouncing the sentences.

I planned to visit Townsville in March 2003 to continue Warrungu lessons. However, I was forced to cancel the plan due to the war in Iraq.

I mentioned above that (in March 2002) three of Rachel and John's daughters were unable to attend the classes, due to other commitments. And that (in August 2002) about a half dozen people were forced to withdraw as a land rights meeting coincided with the lessons. These are instances of pressing social problems and needs that hinder language revitalization activities, mentioned in 2.4.

Recall that on the 17th March (Sunday) Alf Palmer's descendants' eyes were filled with tears when they were listening to his stories. These tears indicate how much their ancestral language means to them, and this in turn shows that these tears are the very reason why we should be engaged in the activities to combat the crisis of language endangerment. Linguists have put forward a number of reasons for the importance of concern about this crisis, and apparently their most favourite reason is the value of linguistic diversity (Hale 1992; Krauss 1992: 8). However, the communities' wishes, which were manifested by these tears, are just as important and compelling a reason for this concern as the value of linguistic diversity (Tsunoda forthcoming, Chapter 10).

Having outlined aspects of the Warrungu language revival movement, we shall explore its cultural and scientific significance.

#### 4. Cultural and scientific significance of the Warrungu revival movement

##### 4.1 Syntactic ergativity as a rare and precious gem

During the lesson on syntactic ergativity on the 22nd March 2002, I emphasized that syntactic ergativity is a rare and precious gem, saying to Tahlia and Mheelin: "Your great-grandfather handed it to me. I have been keeping it for 30 years. Now I am passing it on to you". Upon hearing this, Rachel said, "This is like a time capsule".

After the lesson, we returned to the Cummins' home. I said to John Cummins, Rachel's husband: "We studied syntactic ergativity today. Tahlia and Mheelin did really well. They now know what syntactic ergativity is. They have a gem in their head". John was very happy and proud of his daughters' achievement, and his eyes were wet with tears.

We saw in 2.4-a that often young people are not interested in their ancestral languages. In the case of Warrungu, as seen in 3.3, the knowledge of the existence of a unique phenomenon in their ancestral language inspired a few young Warrungu people and enhanced their interest (and also pride) in it.

In 2.4-e, we saw what Schmidt (1990:20) termed ‘gemstone effect’: some members of the older generation prefer to take the language to the grave, rather than to teach it to the younger generation. In the case of Warrungu revival, the gem – i.e. syntactic ergativity – yielded the opposite effect. It inspired some young Warrungu people to the extent that, in 2002, Tahlia Cummins was enrolled at the Centre for Australian Languages and Linguistics, Batchelor Institute of Indigenous Tertiary Education, and she is studying for a Diploma of Arts (Language Studies), with a view to working on her ancestral language, in order to revive it.

The above shows that Alf Palmer’s dedicated efforts to have his language documented have proved to be truly worthwhile. What he sowed 30 years ago is now beginning to be harvested by his descendants.

#### 4.2 Learning of an ancestral language as a second language in language revival<sup>4</sup>

For Rachel, Tahlia and Mheelin Cummins, English is the first language (i.e. L1), while Warrungu is a second language (i.e. L2). Learning of an ancestral language as an L2 in language revival is different in several respects from what may be considered typical cases of L2 learning, e.g. learning of Japanese by American students of English descent who came over to Japan. The differences include the following.

- a. The aim of learning an ancestral language has generally to do with identity, including cultural continuity. That is, the aim is mainly spiritual (Littlebear 1991:1), ideological (Spolsky 1991:139), and integrative (Dorian 1978:608), as seen in 2.2. In contrast, this is unlikely to be true of typical cases of L2 learning.

Rachel Cummins described one of the reasons for learning her ancestral language as follows: “Language is important for identity, to know who I am”.

As seen above, on the 19th March 2002, we had a discussion about the motivation and goal of learning Warrungu. Tahlia stated her aim and motivation as follows: “To learn my family’s language and background, so when I have my kids I can teach them and tell them some stories. Hopefully this will be carried on by my kids and their kids to keep the language alive and strong”. Mheelin’s aim and motivation were very similar to Tahlia’s, but she added that for her to learn and revive her ancestral language “will make my parents and Granddad [i.e. her great-grandfather, Alf Palmer – TT] proud

of me". These statements show the value placed on cultural continuity by these young community members, who are only teenagers.

- b. Some, though not all, of community members believe in 'gene-assisted language acquisition advantages': the belief that "if your ancestors once spoke a particular language you will have an easier time learning that language . . . than other people would" (Dorian 1995: 131). See also Nathan (1999: 2) and Yamamoto (1998: 222).<sup>5</sup> It is relevant to note in this connection that some (or many?) Aboriginal Australians believe that their languages are not dead, but are merely 'sleeping' (Amery 2000: 41; Nathan 1999: 2). In typical cases of L2 learning, by contrast, there is no basis for such a belief.
- c. Some learners may have heard the language as a child. Thus, Rachel Cummins heard Alf Palmer, her grandfather, speak Warrungu. In addition, they may have some previous knowledge (Amery 1994: 140; Spolsky 1991: 139) – albeit rudimentary – of the language. For instance, as noted in 3.4, Rachel, Tahlia, and Mheelin already knew some Warrungu vocabulary. (However, I have never heard any of them speak Warrungu words in their English.) These situations are different from typical cases of L2 learning. Thus, an American student of English descent who is learning Japanese is unlikely to have heard his/her grandparents speak Japanese. Chutatip Yumitani (p.c.) points out that the issue of (c) raises an interesting question as to how much of the language a person was exposed to when he/she was young affects his/her acquisition of that language later in life. This is an important issue, and needs to be investigated. Note that the possession of previous knowledge of the language is different from the belief in the gene-assisted language acquisition advantages.
- d. According to the belief of Aboriginal Australians, a given group's language is a gift from Dreamings or Ancestral Beings (Sutton 1991: 50), and consequently they 'own' the language (Amery 1994: 140, 2000: 44; McKay 1996: 101; Nathan 1996: 26, 1999: 1) – even if they no longer speak it. According to cultural protocols, permission may be required for people of other groups to learn it (Amery 2000: 44; McKay 1996: 101). Fesl (1982) describes an example: a group of Aboriginal Australians of Victoria had to obtain permission to learn the language of Bandjalang people of New South Wales. (This is an instance of the language adoption method, mentioned in 2.5.) In contrast, no such permission is required for, say, American students to learn Japanese.

The belief in language as a gift from ancestral beings is observed in North America as well. Stories of how North American languages were given by Creators are found in Hinton (1994). Aboriginal Australians (Hudson &

McConvell 1984:37) and Native Americans (Yamamoto 2001:339) share the view that, since their language is a gift from their ancestral beings, it is sacred duty to maintain the language. In contrast, such a view is unlikely to be held by typical L2 learners.

- e. The language to be revived is a so-called ‘minority language’ in every (?) case, while on the other hand in typical cases of L2 learning the language to be learned is generally a so-called ‘major language’, e.g. English and Japanese.
- f. Language revival is often beset with problems such as shortage or lack of teaching materials and human resources (as noted in 2.4; Warrungu is no exception), but this does not seem to be the case with typical cases of L2 learning.
- g. In typical cases of L2 learning the target language to be learned is taken for granted, i.e. the ‘standard’ dialect, e.g. the Tokyo dialect of Japanese. In contrast, in the case of language revival, the target language is not always taken for granted. Naturally, learners wish to learn the variety or dialect that their ancestors spoke and they are generally reluctant to learn another variety or dialect. However, not all dialects were adequately documented, and some of the dialects may not have been documented at all. This makes it extremely difficult to decide what to teach to whom. (I owe the observation in (g) to Hiroshi Nakagawa, p.c.) This is an example of the problem that surrounds dialects and standardization, mentioned in 2.4.

Most of these differences are cultural. In view of these differences and with emphasis on the cultural differences, I have adopted the term ‘re-learning’ (rather than just ‘learning’) to refer to learning of an ancestral language as an L2 in language revival. (Huss 1999: 112 uses the term ‘re-learning’, and Wurm 1998: 201, 203, 205 ‘re-learn’ and ‘relearn’, in the context of language revitalization.) David Nathan (p.c.) suggested the term ‘re-acquisition’, his motivation being that “acquisition also evokes the important idea of re-taking ownership (and also, perhaps, that the language will be acquired as an L1 by succeeding generations, although the latter is clearly unlikely)”<sup>6</sup>

Spolsky (1991: 138) suggests that “it is worth looking at language revitalization in the context of a general theory of second language learning”. Indeed, research on L2 acquisition deals with language maintenance (e.g. Ellis 1994: 224, 229) and language revival (e.g. Baker 1993: 51). However, Spolsky, Ellis, and Baker examine only the social aspects of L2 in language revitalization. I suggest that it is worth investigating its linguistic and pedagogical aspects as well –

more specifically, these aspects of language revival. Examples of such attempts are given in 4.3 through 4.7.

Learning of an ancestral language as an L2 in language revival is in a few respects similar to learning an ancestral language by migrants' descendants. Thus, in the case of the latter as well, the primary aim concerns identity (Nobuya Itagaki, p.c.), and the belief in gene-assisted language-acquisition advantages is prevalent (Dorian 1995: 131).

There are, however, a few differences. Thus, one difference is that in the case of migrants' descendants (e.g. Americans of Japanese descent) there is a homeland to visit to learn the language, whereas in the case of language revival, such as Warrungu, returning to the traditional territory would not provide the people with an opportunity to be exposed to the language (Joseph Blythe, p.c.). Another difference is that, while learning of the ancestral language of a given group of migrants (e.g. the Japanese language in the USA) is open to other people as well, in the case of language revival this opportunity may be limited to members of that group, as seen above – as far as Australian Aboriginal languages are concerned. A third difference is that in the case of migrants' descendants there does not seem to be shortage of teaching materials or human resources, while on the other hand this shortage is frequently encountered in language revival.

#### 4.3 Ergativity in language acquisition

As emphasized above, and in 4.1 in particular, one of the unique aspects of the attempt at Warrungu language revival concerns the acquisition of syntactic ergativity. We shall consider this issue in a broader context of language acquisition, in terms of (i) morphological and syntactic ergativity, and (ii) L1 and L2 acquisition.

- a. Acquisition of morphological ergativity in L1. There were many instances in the past, e.g. Warrungu, and there are also many instances currently, e.g. Allen (1996) on Inuktitut (Quebec), Bavin (1992) on Warlpiri (Central Australia), and Imedadze and Tuite (1992) on Georgian.
- b. Acquisition of morphological ergativity in L2. There appears to be no account or analysis, although there seem to be a few attempts. Thus, R. M. W. Dixon taught Yidiny (North Queensland) to Yidiny people from 1972 to 1973, and Robina Cosser taught it in 1987 (Dixon 1972–1973; Cosser 1987).<sup>7</sup> The Warrungu lessons constitute another example.

- c. Acquisition of syntactic ergativity in L1. Warrungu and a small number of languages of North Queensland used to provide instances of this (as seen in 3.3), but there appears to be no account or analysis. As mentioned in (a), there are many studies of L1 acquisition of morphological ergativity, but the languages investigated do not seem to have syntactic ergativity.<sup>8</sup>
- d. Acquisition of syntactic ergativity in L2. The Warrungu lessons appear to be the very first attempt. As mentioned in 3.3, Yidiny had syntactic ergativity; see Dixon (1977). But Dixon's and Cosser's Yidiny teaching materials do not deal with it.

The situation described above is summarized in Table 1.

**Table 1.** Acquisition of ergativity

	L1 acquisition	L2 acquisition
morphological ergativity	Warrungu, etc. of the past, Inuktitut, Warlpiri, Georgian, etc.	Yidiny of the 1970s, Warrungu of 2002
syntactic ergativity	Warrungu, etc. of the past	Warrungu of 2002

Schmidt (1985) recorded the loss of ergativity – in particular, syntactic ergativity – in dying Dyirbal and made an invaluable contribution to historical linguistics.<sup>9</sup> It will be interesting to investigate how ergativity – in particular, syntactic ergativity – behaves in a process that appears to be the opposite of language loss, that is, language acquisition: L1 and L2 acquisition.

Ergativity seems to be unexplored in L2 research. Works on L2 such as Baker (1993), Ellis (1994), and Mitchell and Myles (1998) do not deal with acquisition of ergativity in L2, let alone that of syntactic ergativity. As mentioned above, the Warrungu lessons appear to be the very first attempt at L2 acquisition of syntactic ergativity. If this process can be fully documented, this will make an important contribution to L2 acquisition research. One possible contribution concerns the debate as to whether language acquisition and language death are mirror images; this will be discussed in 4.7.

As seen above, regarding L1 acquisition there is already a large literature on morphological ergativity, but there appears to be none on syntactic ergativity. It is therefore particularly interesting to see – if syntactic ergativity is acquired by the current learners of Warrungu proficiently enough – whether it will be acquired in the L1 of the next generation.

#### 4.4 Is syntactic ergativity difficult to learn?

Prior to the March 2002 lessons, I had entertained the following two opposing hypotheses: (i) syntactic ergativity would be difficult to learn, and (ii) it would not be difficult to learn.

Hypothesis 1: Syntactic ergativity would be difficult to learn. There are at least two reasons for this.

Reason 1: Typological markedness (Ellis 1994:421). Regarding L2 acquisition, Ellis (1994:426; cf. also 421, 423) notes: “Learners seem to find it easier to acquire typologically unmarked structures than typologically marked structures”.<sup>10</sup> (See also Olshtain 1989:153.) As emphasized in 3.3, syntactic ergativity is unique among the world’s languages, that is, it is typologically marked. Consequently, it would be difficult to learn.

Reason 2: Language distance (i.e. linguistic difference) between the native language and the target language (Ellis 1994:315, 327). Ellis (1994:335, cf. also 306, 323) notes: “learners find it easier to learn an L2 that is similar to their own language”. Syntactic ergativity of Warrungu is very different from syntactic accusativity of English, and, in this respect there is a large language distance between them. Consequently, syntactic ergativity would be difficult to learn – for people whose first language is English.<sup>11</sup>

Hypothesis 2: Syntactic ergativity would not be difficult to learn. Until about 100 or 150 years ago, every Warrungu person was using syntactic ergativity fluently (in their L1). No doubt people – whether people of the past or people of the present – have the same degree of language acquisition abilities, and this applies to Warrungu people as well.<sup>12</sup> Therefore, provided that learners receive adequate instruction and that they are motivated, and so on, syntactic ergativity (in L2) would not be difficult to learn.

The result of the March 2002 lessons yielded support to Hypothesis 2. That is, syntactic ergativity was not all that difficult to learn. Although, for the first one or two sets of exercises, each of Tahlia and Mheelin wrote an incorrect answer, this seems to be not because they did not comprehend syntactic ergativity, but because my instruction about the exercises was not clear enough. After I gave some advice as to how to use antipassivization and how to delete the second occurrence of the coreferential noun, Tahlia and Mheelin each wrote a correct answer for every one of the remaining sets of exercises. It was clear that they had a full comprehension of the mechanism of syntactic ergativity. (Also, they thoroughly enjoyed the exercises. After completing all the exercises, Mheelin said, “I want more exercise!”.)

It was only on the fourth day (22 March 2002) after the lessons on Warrungu grammar started (19 March 2002) that Tahlia and Mheelin wrote correct answers to the exercises on syntactic ergativity. In view of this, the teaching of syntactic ergativity in these lessons may be considered a success.

The outcome of L2 acquisition depends on a large number of factors, such as the following (Nobuya Itagaki, Chikako Matsuura, and Yeong Kwong Leong Samuel (p.c.)) (see Ellis 1994; Mitchell & Myles 1998):

- a. affective factors: language attitude, motivation, situational anxiety, self-confidence, personality;
- b. intelligence, language aptitude, and;
- c. learning style, teaching method.

Almost all of these conditions were fulfilled to an unsurpassable extent. For example, first, the students were highly motivated (see 3.3), and so was the teacher. In addition, the fact that the number of the students was small (at most four) was an additional advantageous factor (Chikako Matsuura, p.c.). Second, it is clear that Tahlia and Mheelin are blessed with a high degree of intelligence and language aptitude. Third, syntactic ergativity and syntactic accusativity are mirror images (see Note 11), and I emphasized this during the lessons, constantly comparing the syntactic ergativity of Warrungu with the syntactic accusativity of English. This may have facilitated the comprehension of syntactic ergativity.

The above indicates that a complex phenomenon may not be all that difficult to learn, provided that relevant conditions are fulfilled.

#### 4.5 What has been achieved in the Warrungu revival movement?

Is the Warrungu language movement successful? Is it possible to say that Warrungu has been revived? There is no clear-cut answer to these questions. As seen in 2.3, the answer depends on (i) the definition of language revival, and (ii) aim of this movement.

Rachel Cummins expressed one of the aims of their movement as follows: “I want 50% of the [Warrungu – TT] children to speak the language”. This goal has not been achieved, and according to this aim the movement is not a success thus far.

Then, what has been achieved so far? We shall examine the proficiency that has been acquired. In this connection, Rod Ellis (p.c.) points out that it is necessary to distinguish between “*implicit* (i.e. intuitive and unconscious) and *explicit* (i.e. metalingual and conscious) L2 knowledge” (Ellis 1994: 31).

On the 17th March 2002, when we were practicing the word *bija-Ø* ‘drink-IMPERA’, Rachel uttered the following sentence spontaneously.

- (7) *bija-Ø*                      *gamu-Ø*.  
       drink-IMPERA water-ABS  
       My translation: ‘Drink water!’

(This is the first word or sentence spontaneously uttered by a Warrungu person that I have ever heard since 1974, when I worked with Alf Palmer.) This suggests that Rachel is now beginning to acquire an implicit knowledge of Warrungu.

After the March 2002 lessons, Rachel said on a few occasions that Tahlia and Mheelin started teasing each other and joking to each other in Warrungu. This is the beginning of spontaneous use of Warrungu, although I have not observed it myself.

As for syntactic ergativity, it seems clear that Rachel, Tahlia and Mheelin have acquired an explicit knowledge of this phenomenon, though not implicit knowledge thereof as yet. They could write correct answers for the exercises involving syntactic ergativity, but they did not reach (and still now have not reached) the stage where they speak it fluently.

In 2.3, we saw that according to one view a given language is alive as long as there is at least one speaker – even if he/she is not fluent in it. In this view, Warrungu has been revived – although this view is unlikely to be shared by many people.

#### 4.6 A modified language for easy learning?

As seen in 2.4-c, the complexity of the language to be revitalized may be intimidating, and even demoralizing to some learners. Solutions have been proposed to tackle this problem.

It is relevant to note here that research on L2 acquisition looks at the effect of simplified input (Hatch 1983) and that of modified input (Ellis 1994: 247–267) on language learning. Hatch (1983:84) states that simplified input may promote second language learning.

In the case of language revitalization, proposals such as the following have been put forward, as mentioned in 2.4-d.

[1] Amery (2000:215) reports: Powell (1973), “working with Quileute in the north-west of the United States, advocates the development of what he refers to as an artificial pidgin formed by the incorporation of Quileute words, one by one, into English sentence structure”. (See also Amery 1994:141.)

Amery (2000:216) notes that a similar result is occurring in the revival of the Ngarindjeri language of South Australia; the emerging language is a kind of re-lexified English.

[2] Thieberger (2002:325) puts forward a proposal that is much less drastic than Powell's, i.e. to "develop a form of language which might bear great resemblance to a hypothesised, once-spoken language, but which is nevertheless a new form of the language".<sup>13</sup>

Both Powell's and Thieberger's proposals recommend the use of a modified (as well as simplified) input. However, these approaches have pitfalls, and they are opposed by 'purists' of the communities.

First, such a language is "often considered 'non-authentic'" (Ellis 1994:257). Wurm (1998:196) argues that this view is based on linguists' "scientific purism". However, this 'purism' is shared by some community members. As seen in 2.4-e, some members of the older generation prefer to take the language to the grave, rather than to have it corrupted by the younger generation. Also, Rachel Cummins stated, "I'm too much of a purist", and she explicitly rejected the idea of teaching a modified language. In her words, such a method is "an insult to . . . our old people and our heritage". (To be precise, Rachel Cummins said to me, "this will be an insult to the time you put into the programme and to our old people and our heritage".) Similarly, according to Thieberger (2002: 312), Eve Fesl, who is the first Aboriginal Australian to be awarded a Ph.D. degree in linguistics, criticizes programs that show "no appreciation of the complexity of the traditional language". It is relevant to note that, as Hudson and McConvell (1984: 38) report from the Kimberley, Western Australia, "Many [Aboriginal Australians – TT] are proud of the complexity of their languages".

Second, there is a danger of the modified language being 'fossilized', and an attempt to "unlearn" (Ellis 1994:34, 299) deviant forms may take a long time or may never succeed. (I owe this observation to Mie Tsunoda, p.c.) (Fossilization refers to "the persistence of a non-standard form in interlanguage" (Ellis 1994:205) or "persistent errors" (Ellis 1994:353, 409), and it is a phenomenon in which "a learner's L2 seems to 'freeze', or become stuck, at some more or less deviant stage" (Mitchell & Myles 1998:13).) If a modified form of the language, which is often considered non-authentic, is taught from the beginning, the future generation may regret it – just as the current generation regrets the loss of their ancestral language.

Third, the complexity of a given language may turn out to be not all that difficult to learn – provided that relevant conditions are fulfilled, as can be seen from the account of the learning of syntactic ergativity given in 4.4.

Fourth, there are community members who wish to learn the complexity of the language and who are prepared to tackle it. Thus, Rachel Cummins admitted, “There is no magic” [i.e. no easy way to learn a language – TT]. According to Marianne Mithun (e-mail of 5th February 2003), Mohawk people of North America seem to be interested in, and eager to learn, the morphological details of their traditional language.

Fifth, one of the aims of learning an ancestral language is to appreciate its beauty, e.g. complexity and uniqueness. In view of this, teaching a modified language may be considered “defeatism” (Fishman 1991: 14), and also it may discourage those people who are eager to learn these attributes of the language (Nobuya Itagaki, p.c.).

If syntactic ergativity had been excluded from the Warrungu lessons on the grounds that it might pose difficulty, Alf Palmer’s descendants would not have been able to appreciate the complexity and beauty of their ancestral language, especially the unique phenomenon of syntactic ergativity. In order to appreciate these attributes of Warrungu, learning of syntactic ergativity is a minimum prerequisite. Furthermore, syntactic ergativity (created by means of antipassivization) is so pervasive in Warrungu that it is impossible to speak the ‘real’ Warrungu language without any command of it.<sup>14</sup> A simplified or modified form of the language would not have been satisfactory for people like Rachel Cummins.

The above indicates that the complexity of a given language should not be sacrificed for the sake of easy learning. Needless to say, feasibility of language revitalization activities varies from case to case (David Nathan, p.c.) and learning of the complexity of their ancestral language may not be always successful. Nonetheless, defeatism should be avoided, and an attempt should be made to learn and appreciate the complexity of the language.<sup>15</sup>

As we noted in 2.4-b, community members are conscious of vocabulary, but often they are unaware of the existence of morphological and syntactic structure of the language, and consequently language lessons tend to be confined to the teaching of isolated vocabulary. However, Rachel Cummins is critical of such an unbalanced approach; she stated, “Language comprises a whole”. It seems that her awareness of the wholeness of a language was enhanced (if not created) by the exposure to syntactic ergativity. In this respect as well, the inclusion of this phenomenon in the Warrungu lessons was useful.

## 4.7 Are language death and language acquisition mirror images?

### 4.7.1 *Introductory notes*

It has been claimed that language death and language acquisition are mirror images, i.e., what is learnt later is lost earlier, e.g. Cook (1989) and Voegelin and Voegelin (1977), both of which are concerned with L1 acquisition. (Similarly, Andersen 1989:385 states that L2 acquisition and language death are very similar, although he does not state that they are mirror images.) This claim is widely known and has been reported by a number of works, such as Campbell (1994:1965), Craig (1997:260), Dorian (1999:102), Gal (1989:314), Hill (1983:259), Mithun (1989:255), and Schmidt (1985:220). However, it has been criticized on the grounds that it has not been substantiated by empirical data or that the mirror-image relationship is non-existent or limited (Campbell 1994:1965). See, for instance, Craig (1997:260), Dorian (1999:102), and Schmidt (1985:220) regarding L1, and Craig (1997:260), Dorian (1983), and Olshtain (1989:164) concerning L2. We shall consider this issue in terms of syntactic ergativity.

### 4.7.2 *Syntactic ergativity in coordination and subordination*

In Warrungu and many other Australian Aboriginal languages, roughly speaking, coordination is formed by juxtaposition of clauses, without any conjunction (cf. Dixon 1980:458). Examples from Warrungu include (2c) and (4c). (4c) is repeated below, as (8). Regarding subordination, a very common and possibly almost pan-Australian type is purposive subordination, in which the verb in the subordinate clause has the purposive suffix and indicates purpose ('in order that') or consequence ('...,... so that') (cf. Dixon 1980:458). Examples of purposive subordination from Warrungu include (9) and (10).

- (8) *bama-Ø      yani-n*  
 man-ABS (S) go-NONFUT  
 [S] *gamu-nggu bija-gali-n.*  
 water-ERG drink-ANTI-NONFUT  
 'The man (S) went and [S] drank water.'
- (9) *bama-Ø      yani-n*  
 man-ABS (S) go-NONFUT  
 [S] *gamu-nggu bija-gali-yal.*  
 water-ERG drink-ANTI-PURP  
 (i) Purpose: 'The man (S) went in order to drink water.'  
 (ii) Consequence: 'The man (S) went and [S] drank water.'

- (10) *bama-Ø      yani-n*  
 man-ABS (S) go-NONFUT  
 [S] *gamu-wu    bija-gali-yal.*  
           water-DAT drink-ANTI-PURP  
 (i) Purpose: ‘The man (S) went in order to drink water.’  
 (ii) Consequence: ‘The man (S) went and [S] drank water.’

Note that in the ‘consequence’ reading, (9) and (10) (subordination) have much the same meaning as that of (8) (coordination).<sup>16</sup>

It is important to point out that syntactic ergativity involves a higher degree of morphological complexity in subordination than in coordination – in two respects in Warrungu and in one respect in Dyirbal, as shown below.

- a. Verbal morphology. In both Warrungu and Dyirbal, purposive subordination generally involves two different verbal suffixes: the main-clause verb typically has the nonfuture suffix, and the purposive-clause verb has the purposive suffix. In contrast, coordination typically has a sequence of nonfuture forms.
- b. Case-marking. In Warrungu, if the patient NP of an antipassive purposive clause is inanimate, it can alternate between ERG and DAT; see *gamu-nggu* ‘water-ERG’ in (9) and *gamu-wu* ‘water-DAT’ in (10). (Animate and human nouns and pronouns do not exhibit such an alternation.) In contrast, in coordination an inanimate patient NP occurs in ERG consistently, e.g. (8). In Dyirbal, every patient NP in an antipassive purposive clause appears to occur in DAT only (Dixon 1972:69).

The situation is shown below.

- (11) Syntactic ergativity in coordination of Warrungu and Dyirbal:  
 ... Verb-NONFUT ... inanimate-ERG Verb-ANTI-NONFUT    e.g. (8)
- (12) Syntactic ergativity in subordination of Warrungu:  
 ... Verb-NONFUT ... inanimate-ERG Verb-ANTI-PURP    e.g. (9)  
 ... Verb-NONFUT ... inanimate-DAT Verb-ANTI-PURP    e.g. (10)
- (13) Syntactic ergativity in subordination of Dyirbal:  
 ... Verb-NONFUT ... inanimate-DAT Verb-ANTI-PURP

#### 4.7.3 *Intra-linguistic distribution of syntactic ergativity in terms of coordination and subordination*

Silverstein (1976:163) proposes “a hierarchy of clause-clause logical relations”, which sets up a scale of the tightness of clause-linkage, on which syntactic erga-

tivity (created by means of antipassivization) is more likely to be used where the linkage is tighter, e.g. in purposive subordination, than where the linkage is looser, e.g. in coordination (see also Foley & Van Valin 1984:264–303).

Relevant Australian Aboriginal languages exhibit (or, to be more precise, exhibited) intra-linguistic distribution of syntactic ergativity in terms of subordination and coordination as shown in Figure 1 (Tsunoda 1988a:43, 1988b:619). Syntactic ergativity occurs only in subordination in Warrgamay, Djabugay, Yalarnnga, Kalkatungu, and Bandjalang, but it extends to coordination in Warrungu, Dyirbal, and Yidiny. (The sources of information are cited in Tsunoda 1988a.) The following languages are from outside the North Queensland area in question and were not mentioned in 3.3: Yalarnnga, Kalkatungu, and Bandjalang.

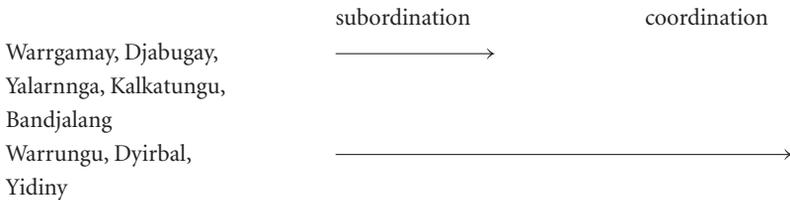


Figure 1. Intra-linguistic distribution of syntactic ergativity

#### 4.7.4 *Rise and fall of syntactic ergativity*

The distribution shown in Figure 1 suggests the following two predictions regarding diachronic changes of syntactic ergativity in a given language.

Prediction 1: It will appear in subordination first, and then in coordination.

Prediction 2: It will disappear in coordination first, and then in subordination.

Regarding Prediction 1, there appears to be no pertinent data. There are a large number of works that deal with the emergence of morphological ergativity, e.g., Comrie (1978:370–374), Dixon (1994:187–192), but none of them appears to deal with the rise of syntactic ergativity.

Regarding Prediction 2, there is relevant data, and it is provided by a study of language death. Schmidt's (1985:124) study of dying Dyirbal shows that syntactic ergativity began to disappear in coordination before it did in subordination – exactly as predicted.

There is one intriguing aspect to this result. As seen in 4.7.2, syntactic ergativity involves a – slightly – higher degree of morphological complexity in subordination than in coordination. In view of this, it is intriguing that syn-

tactic ergativity in Dyirbal began to be lost in coordination first, and then in subordination, rather than the other way round. This needs further investigation.<sup>17</sup>

#### 4.7.5 *Language death and L2 acquisition: Mirror images?*

Now, according to the mirror-image claim, syntactic ergativity would be expected to be acquired in subordination first, and then in coordination. We shall look at this issue.

One of the central questions in research on language acquisition seems to be the relative order or sequence (Ellis 1994:20, 73) of acquisition. That is, in the case of Warrungu revival, this would focus on the relative order or sequence of the acquisition of syntactic ergativity in coordination as against that in subordination. However, since the Warrungu lessons are conducted in a classroom setting (rather than a naturalistic setting; see Ellis 1994:214, 563), the interest will concern not the relative **order** or **sequence** of their acquisition, but the relative degree of **difficulty** or **ease** (Ellis 1994:300, 308) of their acquisition, the **rate** (Ellis 1994:303), i.e. relative speed, of their acquisition, or perhaps the relative degree of **accuracy** (Ellis 1994:269–270, 617) in their use.<sup>18</sup>

Regarding this issue, research on L2 acquisition makes predictions that are mutually contradictory.

Prediction 1: Typological markedness (Ellis 1994:421). Figure 1 shows that syntactic ergativity is crosslinguistically more common in subordination than in coordination – as far as these Australian Aboriginal languages are concerned. Therefore, it will be acquired in subordination first, and then in coordination.

Prediction 2: Structural complexity (Ellis 1994:270). Since syntactic ergativity exhibits a – slightly – higher degree of structural complexity in subordination than in coordination, it will be acquired in coordination first, and then in subordination, i.e. in the order opposite to that in Prediction 1.<sup>19</sup>

In the Warrungu lessons conducted in March and August 2002, I introduced syntactic ergativity in coordination only, not in subordination. This is because, in view of the – slightly – higher degree of morphological complexity of subordination, I had hypothesised that this method would facilitate teaching and also learning, and that introduction of syntactic ergativity in subordination, rather than in coordination, might create confusion in the learners.

In December 2002 in Tokyo, I administered tests on syntactic ergativity of Warrungu (comparing it with syntactic accusativity of English) to about 40 people, most of whom are Japanese students of linguistics and who had attended at least one lecture on syntactic ergativity (and syntactic accusativity). The tests included the question cited in (6) (and also that in (5)).

The result was that virtually every subject wrote correct answers for both coordination and subordination. That is, as far as these tests are concerned, there seems to be no difference in terms of difficulty of learning. Although structural complexity would be expected to correlate with learning difficulty, this result suggests that such a slight difference in morphological complexity does not produce a difference in learning difficulty.

However, one of the subjects, who is a professor of linguistics, commented that the questions involving subordination were more difficult than those involving coordination. This suggests that, in acquisition of syntactic ergativity in L2, some learners may find subordination more difficult than coordination.<sup>20</sup>

It still remains to be seen in which of the two environments syntactic ergativity is learned more easily (and earlier). This is important not only for L2 research, but also for the purpose of preparing effective teaching materials.

Also, it will be interesting to investigate this issue in L1 acquisition as well. It will be particularly interesting to see whether it is acquired first in subordination or coordination – if syntactic ergativity is acquired by the current learners of Warrungu proficiently enough and if it is acquired in the L1 of the next generation.<sup>21</sup>

If syntactic ergativity is ever to be acquired in the L1 of the future generation, Warrungu will be justifiably said to have been revived. This is because (i) the language, with syntactic ergativity retained, will be ‘authentic’, and (ii) it will be spoken in the home, since it is acquired in L1.

To conclude, although 4.7 has not provided any conclusive evidence, it has shown that L2 acquisition in language revival raises interesting questions, and it has suggested new directions of research.

## 5. Conclusion

This paper presented an interim report on the Warrungu language revival movement, and attempted to explore its cultural and scientific significance.

Regarding the general subject of language endangerment and language revitalization, this paper showed the importance of documenting endangered languages; it is impossible to revive a language if there is no documentation thereof. It also pointed out the importance of documenting language revitalization activities (Hiroshi Nakagawa, p.c.). This is important for the communities as well as the science. (Examples of such documentation include Amery 2000 on Kaurna of South Australia, and Maguire 1991 on Irish in Belfast.)

With respect to the Warrungu revival movement, this paper showed that it is unique regarding the people and the language.

As for the people, Alf Palmer was exceptional in intelligence, which was manifested in his early realization of the importance of documenting his linguistic heritage and also in his foresight as to its future; it looks as if he envisaged that his descendants would attempt to revive the language. This attribute of his has been handed down the generations, as can be seen, for instance, in Rachel, Tahlia and Mheelin Cummins' dedication to the language.

Concerning the language, it had a unique phenomenon of syntactic ergativity, and this has played an important role in tackling some of the problems prevalent in language revitalization activities. Thus, this uniqueness has inspired some young Warrungu people to the extent that Tahlia is now studying linguistics, with a view to working on the revival of her ancestral language. Furthermore, the issue of re-learning of Warrungu – and its syntactic ergativity in particular – has presented interesting issues to investigate in the field of language acquisition research.

Since the case of the Warrungu revival movement is unique, in terms of the people and the language, what I stated about them in this paper may not necessarily be generalizable to other cases of language revitalization efforts. Nonetheless, I wish to emphasize again, in accordance with Fishman's spirit, that defeatism should be avoided. Syntactic ergativity is not the only gem, and no doubt each language has its own gem. I wish to urge the people concerned to look for their own gem.

As is almost always the case with language revitalization efforts, the Warrungu movement is beset with numerous problems. For example, the people are scattered in a wide area (as seen in 3.2). The movement suffers not from inadequate funding, but from lack of funding. And so on. Nonetheless, the people's spirits are high.

Fortunately, the School of Indigenous Australian Studies, James Cook University in Townsville has offered generous assistance, and the lessons of August 2002 were conducted in this school. Rachel Cummins is hoping to have the Warrungu language introduced as a subject of the university, incorporating a one-week camp at a place called Kirrama (Aboriginal name: *bajubala*), which is located on the eastern border of the traditional Warrungu territory. This plan is only "a dream" (Rachel Cummins, p.c.) at this stage. But if it is ever realized, it will help to furnish the Warrungu language with a status equal to that of languages such as English – the status that has been denied to Warrungu and other Australian Aboriginal languages since the colonization started.

## Notes

\* I dedicate this paper to the memory of the late Mr. Alf Palmer (Warrungu name: Jibilnggay), who taught me the importance of documenting endangered languages as early as 30 years ago. I am grateful to Rachel Cummins (Alf Palmer's granddaughter) for supporting my presentation of this paper and also for approving the way I cite Alf Palmer and his family in this paper. Words of thanks are also due to the following people, who provided invaluable comments on earlier versions of this paper: Joseph Blythe, Nancy Dorian, Fuyuki Ebata, Jeannie Herbert, Yuki Inaida, Nobuya Itagaki, Norikazu Kogura, Yasuhiro Kojima, Christian Lehmann, William McGregor, Chikako Matsuura, Hiroshi Nakagawa, David Nathan, Motoki Nomaci, Hiroki Nomoto, Kazuyuki Nomura, Elite Olshtain, Ayako Sakamoto, Tetsuya Sano, Daisuke Sasai, Masayoshi Shibatani, Peter Sutton, Miki Takahashi, Nicholas Thieberger, Mie Tsunoda, Douglas Whalen, Yeong Kwong Leong Samuel, and Chutatip Yumitani. All the remaining shortcomings are my responsibility.

1. Ergativity is a phenomenon in which the S ('intransitive subject') and the O ('transitive object') are treated alike, in distinction from the A ('transitive subject') (i.e. S/O, A). It is in contrast with accusativity, in which the S and the A are grouped together, as opposed to the O (i.e. S/A, O). Ergativity and accusativity may be manifested at the level of simple sentences, i.e. morphological ergativity and accusativity, respectively, and also at the level of complex sentences, i.e. syntactic ergativity and accusativity. Detailed accounts of ergativity and accusativity are given in Comrie (1978) and Dixon (1994). Accounts of syntactic ergativity of Warrungu are in Tsunoda (1998a, 1988b).

2. The abbreviations employed are the following: A, so-called transitive subject; ABS, absolutive; ANTI, antipassive; DAT, dative; ERG, ergative; IMPERA, imperative; NONFUT, nonfuture; O, so-called intransitive object; p.c., personal communication; PURP, purposive; and, S, so-called intransitive subject.

3. The ERG NP that indicates the patient, and not the agent, of an antipassive construction (e.g. *gamu-nggu* 'water-ERG' in (4c)), does not have the function of the A, despite its ergative marking.

4. Amery (1994: 140) suggests that it may be more appropriate to refer to such languages as 'heritage languages', and not as 'second languages', in order to distinguish them from 'foreign languages'. Fishman (1991: 362) uses the term 'heritage languages' to refer to languages that have undergone the process of language shift, in order to connote their past – rather than their present – currency as languages of everyday life.

5. Such a belief creates the risk of swift disillusionment when a given language revitalization does not produce prompt results (Dorian 1995: 131; cf. also Nathan 1999: 2).

6. Ellis (1994: 14) notes that some researchers distinguish between 'acquisition' (the subconscious process of 'picking up' a language through exposure) and 'learning' (the conscious process of studying a language). However, Ellis (1994: 14) himself uses these terms interchangeably, and his practice is adopted in the present paper.

7. I am grateful to Bruce Rigsby for providing the information cited above and photocopies of Dixon (1972–1973) and Cosser (1987).

8. K'iche' Maya appears to have syntactic ergativity in the formation of relative clauses (Pye 1992: 229–230, 234–235), but Pye (1992) does not investigate its acquisition.
9. Jirrbal, Girramay and Mamu are collectively called 'Dyirbal' by Dixon (1972: 23). In the following, we shall use the label 'Dyirbal'.
10. Ellis (1994: 427) adds: "It is clear, however, that typological markedness alone cannot account for L2 acquisition".
11. Syntactic ergativity-cum-antipassivization and syntactic accusativity-cum-passivization are mirror images (Michael Silverstein, p.c.). That is, the difference between them is systematic, and not random. It is possible that a systematic difference is easier to learn than a random difference. If that is the case, syntactic ergativity may not be so difficult for English speakers to learn as Reason 2 predicts.
12. Note that I am not referring to the belief in 'gene-assisted language acquisition advantages' (mentioned in 4.2-b). Warrungu people do not seem to subscribe to this belief.
13. Nicholas Thieberger (p.c.) informs me that his proposal does not rule out learning of the complexity of the language but that it is concerned with "a timeline in which learners situate themselves between the old language and their current language, . . . , to allow that as they learn they can move the language they are learning back along the timeline towards the more complex version of the old language".
14. In my text counts (Tsunoda 1988a: 32), roughly speaking, the use of antipassivization to bring forth syntactic ergativity in Warrungu is approximately four times as frequent as that of passivization to create syntactic accusativity in English.
15. It will be interesting to see – if the Warrungu language is revived at all – whether its syntactic ergativity will be retained intact (no doubt, due to Warrungu people's recognition that this unique feature is their important heritage) or it will go through changes as is often the case in language revitalization (see, for instance, Dalton et al. 1995 on Gurindji of Australia, Dorian 1992: 144, 1994: 481–484 on Tiwi of Australia, and Jones 1998: 141 on Welsh). (I owe this observation to Miki Takahashi, p.c.)
16. According to Foley and Van Valin (1984: 240–243), Olson (1981) recognizes three types of clause-linkage in terms of two criteria: (i) embedded versus non-embedded, and (ii) dependent versus independent:
- a. coordination: –embedded, –dependent
  - b. subordination: +embedded, +dependent
  - c. cosubordination: –embedded, +dependent

In Warrungu and, no doubt, in many other Australian languages as well, the purposive clause in purposive subordination is best considered non-embedded and dependent. That is, the purposive subordination is an instance of cosubordination, according to Olsen's classification. In this respect, the purposive 'subordination' of Warrungu may be different from certain types of subordination of English, which are considered embedded. This in turn points to the need to distinguish cosubordination from subordination (and also from coordination) in research on language acquisition.

17. Three factors may be relevant: tightness of clause-linkage, 'chunk', and frequency.

As seen in 4.7.2, clause-linkage is tighter in subordination than in coordination. In view of this, Schmidt's (1985) study suggests that a tighter structure will be lost later than a looser structure. (I owe this observation to Ayako Sakamoto, p.c.).

Chikako Matsuura (p.c.) suggests that purposive subordination may be retained as a 'chunk' (i.e. an unanalysed whole or a formula – Mitchell & Myles 1998:12).

A cursory examination of the first two texts in Dixon (1972:368–387) shows that in Dyirbal the occurrence of syntactic ergativity is approximately twice as frequent in subordination as in coordination. This, combined with Schmidt's (1985) study, suggests that a more frequently used structure survives longer than a less frequently used structure – irrespective of the relative degree of their complexity. (I owe this observation to Yasuhiro Kojima, p.c.) In Warrungu texts as well (Tsunoda 1988a:12, 16), this phenomenon is more than twice as frequent in subordination as in coordination.

It may well be the case that these three factors are related, that is, tighter structures tend to be retained as chunks and they are used the more frequently. They may jointly account for the longer survival of syntactic ergativity in subordination.

18. If the Warrungu re-learning were conducted in a naturalistic setting, input frequency (Ellis 1994:269–270, 423, 427, 704) would be another relevant factor.

19. It is interesting to note that here the criterion of typological markedness and that of structural complexity are contradictory.

20. The tests conducted were written tests. If oral tests are conducted, and also if syntactic ergativity could be observed in natural settings of L2, there might emerge some difference in terms of accuracy and 'latency' (Ellis 1994:376). I suspect that subordination would trigger a larger number of errors than coordination, and also that subordination would take a longer time to produce a correct sentence.

21. Regarding L1, Clark and Clark (1977:337) note that two kinds of complexity correlate with the order acquisition: formal complexity (or perhaps structural complexity) and cognitive complexity (or perhaps semantic complexity). The following quotes are relevant. (i) Regarding formal complexity: "It is reasonable to suppose that the more complex a linguistic device is, the longer children will take to learn it" (Clark & Clark 1977:338). (ii) Concerning cognitive complexity: "The simpler an idea is, the earlier children are able to map it onto language and so talk about it. More complex ideas take much longer to get mapped onto language" (Clark & Clark 1977:337). With respect to semantic complexity: "The simpler the meaning of a term, the earlier it seems to be acquired by the child" (Clark 1979:156). Masayoshi Shibatani (p.c.) suggests that subordination in question involves more complex meanings (e.g. purpose) than coordination (e.g. 'and'), and consequently this might create some difference in acquisition.

The above leads to the speculation that syntactic ergativity of Warrungu will be learned earlier (and more easily) in coordination than in subordination. If this proves to be the case, it will constitute an instance where language death and L1 acquisition are not mirror images.

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# The future of creolistics

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## 1. The beginnings of creolistics as a discipline

From the point of view of the historian of linguistics the emergence of creolistics as a linguistic subdiscipline is a classic example of the institutionalization of a new discipline and its subsequent incorporation into mainstream linguistics. Individual studies of pidgin and creole languages go back to the 19th century, but it is generally held that the real beginnings of this field are marked by two conferences organized in Jamaica, the small conference at Mona in 1959 attended by 13 scholars (Le Page 1961) and the much larger conference organized by Dell Hymes in 1968, which was attended by some 50 scholars (Hymes 1971a). The proceedings of these conferences breathe an atmosphere of community sense. The scholars attending them experienced the feeling that they had finally come into their own, that they belonged to a special field in its own right with its own rules. From then on, the label of ‘creolistics’ began to denote a real discipline. This “consciousness of participation in a common field” (Hymes 1971b:4) was enhanced by newsletters, special journals, such as the *Journal of Pidgin and Creole Languages*, founded in 1986 and organizations whose members came to regard themselves as ‘creolists’.

Such a new community needs an action plan. In his introduction to the 1968 proceedings Hymes (1971b:9) mentions four components that in his view should be integrated in the theoretical part of creolistics:

- the universal tendencies to adapt speech and varieties of a language by simplification in some circumstances, expansion in others
- the occurrence of these tendencies in situations of language contact

- the conditions, linguistic and social, under which forms of speech so adapted and influenced become and remain independent of the norm of any contributing tradition
- the subsequent histories of languages so formed.

These components are not directly translatable into directions for further research, but several strands of later research may be distinguished: the debate about the emergence and acquisition of pidgins and creoles; the study of language contact; the study of variation in creole communities; and the issue of language policy and the status of creoles in society. These were the topics discussed by creolists in the first decades after the Jamaican conferences. The discourse of these early debates was marked by two opposite tendencies: on the one hand, the tendency to emphasize the common features in pidgins and creoles as objects of research, and on the other, the tendency to underscore the difference with other disciplines.

## 2. Common features in creoles and pidgins

The emphasis on the common features of pidgins and creoles served as a link between the scholars participating in the conferences, who hitherto had been working mainly in isolation in their own field without being aware of the work of others. There was a preference for ‘binding’ theories, connecting the various fields in which the participants were working. One of the ways in which this common element could be emphasized was that of finding a common historical origin for all pidgins and creoles, a topic that Hymes does not mention in his program but that was one of the hottest issues in early creole studies. In the first years after the conference theories of monogenesis, put forward by scholars like Thompson (1961), Stewart (1968), Hancock (1977), and Whinnom (1977), remained popular, although controversial. According to one theory all existing creoles and pidgins ultimately went back to a Mediterranean trade jargon, Sabir. Successive publications on pidgin and creole linguistics showed less and less interest in this theory, at least in its strong form. From the beginning the main argument brought forward against it was that any theory of common origin leaves unexplained the typological similarity between the classic creoles and pidgins and those non-Indo-European pidgins and creoles that emerged without any connection with the historical Mediterranean trade jargon (DeCamp 1971a: 24; Traugott 1977a: 76; for a recent appraisal see Holm 2000: 44–49).

The uniqueness of the object of research could also be expressed by stressing the common nature of the linguistic varieties in question rather than their common origin. Features found in all pidgins and creoles were regarded, not as evidence of a common origin, but as a result of universal properties of the human linguistic faculty. The early interest in universal/typological features on the part of creolists explains why creole language came to be regarded as a test-case for universals theories and why creolists became involved in the wider-ranging debate about universal grammar (UG).

Yet, universal grammar was not the only ally in the search for common features in pidgins and creoles. This search was also linked to the discussion about the process itself in which pidgins/creoles are acquired. The conditions of this process, for instance the fact that it took place in an unmonitored environment, came to be seen as an explanation of the common ('universal') features of the linguistic varieties called pidgins and creoles. This had an unexpected side-effect. Since these conditions had to be fairly general in order to cover all classic instances of pidgins and creoles, they could apply elsewhere as well. It is a common phenomenon in the history of science that the success of a new discipline attracts outsiders. After the success of the two Jamaican conferences many scholars from other language groups discovered the appeal of the new discipline and applied for membership with their own linguistic varieties, thus threatening the exclusiveness of the community. In the years to follow it became fashionable for a while to apply the terms of 'creole' and 'pidgin' to almost any language or language family outside the classic cases that constituted the core of the new discipline. Thus, for instance, attempts were made to demonstrate the pidginizing/creolizing origin of the Romance languages from Vulgar Latin, of modern Indian languages from Sanskrit (Houben 1996), or of modern Arabic dialects from Classical Arabic (Versteegh 1984). Even the language that had served as the lexifier for most of the classic pidgins and creoles, English, did not escape the claim that it had undergone a process of pidginization/creolization in its own past (Bailey & Maroldt 1977; Milroy 1984, and see the extensive criticism of this view in Thomason & Kaufman 1988:263–342).

Understandably, in order to keep a grip on things, the scholars belonging to the community of creolistics needed a strategy to emphasize the special character of the object of research of creole studies. One of the purposes of the ensuing terminological debates was to delimit the field in order to counter this diffusion. The labels 'pidgin' and 'creole' began to be seen as problematic, as notions that should be guarded against misappropriation. At the second Mona conference there still reigned a certain liberalism, an atmosphere of 'the more the merrier', so that anyone bringing in new candidates for pidginhood or cre-

olicity was welcomed. The proceedings of the conference include, for instance, papers about *Tây Bòy* (the Pidgin French of Vietnam), about creolized varieties of Swahili, about Mbugu, and about Chinook Jargon. Yet, already at the Mona conference there were voices warning against a too liberal interpretation of the notions of 'creole' and 'pidgin'. In the controversy between Hall (1962) and Whinnom (1968), for instance, the central issue was whether or not any makeshift language being used in a contact situation might be termed a 'pidgin', as Hall claimed. Whinnom took the strict position that this label could only be used when two non-native communities use a target language as their means of communication. This latter point of view seems to have prevailed in creolists' circles. Generally speaking, most publications attempted to restrict the notions of 'pidgin' and 'creole' and reserve them as much as possible for the original set of linguistic varieties that had started the entire enterprise.

There were two ways in which such a restriction could be achieved. One way was to focus on the historical circumstances of the processes that had led to the emergence of the classic pidgins and creoles and define these as necessary conditions for the restructuring that had taken place. In the case of pidgins, this implied that a colonial/slavetrading context came to be seen as a necessary condition, in the case of creoles a plantocracy. Taking these facts as their point of departure, some scholars limited the domain of research to precisely those varieties that had emerged in these historical circumstances, excluding all other restructured varieties. The underlying hypothesis was that there was something unique in these circumstances and that this unique element led to pidginization and creolization. When this component was lacking, it was believed that no pidgins or creoles could result. This effectively prevented the incorporation of languages like the Romance languages but it had the disadvantage of being far too exclusive: even among the classic pidgins and creoles there are some that did not emerge in the context of slave trade or develop in a plantocracy, such as the Melanesian English pidgins and creoles. Besides, it was hard to see how historical circumstances in themselves could be determining factors in linguistic structure.

The second way of restricting the set of pidgins and creoles that 'belonged' to the domain of the newly established discipline was to concentrate on the typological features, rather than the historical circumstances (e.g., DeCamp 1971a:25). The discussion about a typological test to determine which linguistic varieties are the real pidgins and creoles started at the first Jamaican conference and has gone on till the present day, but does not seem to have led anywhere (cf. Givón 1977:19–21). McWhorter (1999) claims to have identified several such features, for instance, the absence of tone in creoles, but as we

learn more about restructured varieties, counterexamples seem to turn up for all of them. Tone, for instance, has now been identified as a feature in the Arabic creole Ki-Nubi, in which it is used to denote the morphological categories of infinitive and gerund (Wellens 2003).

Even more damaging to the search for a litmus test of the pidginhood or creolicity of any given variety is an underlying methodological flaw, threatening its validity: there is no way to establish a control group. Most researchers do not go along with Hall's (1966) and Bailey's (1973) view that every language may have a pidgin/creole background somewhere in its history. Yet, most scholars would agree that it is methodologically impossible to establish beyond any doubt that those languages that are regarded as 'normal' do not contain features of creolization from a past in which they were creolized by other speakers. From the most recent literature it appears therefore that most scholars would concur with DeGraff's (2001b: 11) conclusion that "such a constellation of creole features appears chimeric", echoing Hall's statement (1966: 123) that "there are no structural criteria which, in themselves, will identify a creole as such, in the absence of historical evidence". Nevertheless, the struggle with the labels continues. In more recent literature the central issue seems to have become the question of whether or not a language may be called a creole only when it has gone through a pidginizing stage, in effect the same point that Whinnom (1968) had raised. Jourdan (1991) has shown that most of these debates run into a reifying quagmire.

The discussion about universal features also played a role in yet another debate, the controversy about the role of the base language of the pidgin/creole speakers. This debate divided – and perhaps still divides – the scholarly community into two camps: those who believed in the possibility to explain pidgin/creole features with the structure of the substratal languages and those who vehemently rejected this possibility. The proceedings of the 1985 Amsterdam workshop on substrate influences in creole languages (Muysken & Smith 1986) reflect very well the heated atmosphere of the debate as it was conducted in the early eighties, with scholars like Bickerton attacking the trend to see a substrate lurking behind every creole phenomenon, while others just as passionately tried to defend this position (Versteegh 1991). Theories explaining features in creole languages on the basis of the structure of the substrate languages are still popular (e.g., Lefebvre 1998) and the debate between universal theories of pidginization and theories of interference from the substrate language is far from resolved, in spite of Muysken's remark in the introduction to the proceedings of the Amsterdam workshop (1986: 11) that the issue is "potentially resolvable". Even the alternative of looking for an explanation of all or most

features of pidgins and creoles in the structure of the target language, which in early creole studies was the prevalent approach (Hall 1966), has not gone out of fashion completely. Both substratist and superstratist theories about the emergence of creoles still abound (DeGraff 2001:6–8), even though they have lost some of their relevance (see the appraisal by Holm 2000:61–64).

The general debate about the definition of pidgins and creoles continues to suffer from two major problems: the methodologically fatal absence of a control group; and the fact that there is little attention for those linguistic contact varieties that emerged in a setting that was different from the standard colonial one. One way of solving this is to deny that such processes could take place without the intervention of Europeans, as Samarin (1986, 1990–1991) does for Chinook Jargon, Kituba and Lingala. But this ‘solution’ cannot take care of all cases of restructuring all over the world. The problem does not go away, either, if we simply reject the validity of the label of ‘pidgins/creole’s for such varieties as Mufwene (2001) does for quite different reasons. He rejects the label of ‘creole’ for any restructured varieties of languages outside the group of the classic creoles, which he believes originated in a process of basilectalization in the plantation communities (Mufwene 2001:138). In his view varieties such as Afro-American English were called ‘creoles’ for racial reasons, hence his claim that the label of ‘creole’ should be restricted to the classic cases. Yet, it cannot be denied that the restructuring that took place in some of the non-Indo-European pidgins and creoles, such as Chinook Jargon, Ki-Nubi or Motu, is strikingly similar to what happened in the case of the classic creoles, even though the socio-cultural circumstances were quite different. It seems somewhat arbitrary to exclude them from the category of creoles and pidgins.

The problem of the insufficiency of the database is a persisting one. The concentration on the classic pidgins and creoles in the Atlantic region, the Indian Ocean and the Melanesian area, i.e., those pidgins and creoles that are connected with a colonial lexifier, essentially Portuguese, Spanish, French, English and Dutch, has serious consequences for the representativeness of the database on which many of the conclusions of creole studies are based. This undoubtedly affects the validity of general conclusions in these studies. The debate about universal features suffers from a lack of comparative data and even though the strict monogenetic theories seem to have been laid to rest, there still is a tendency to deny the similarities between the processes taking place in Indo-European pidgins and creoles and those in non-Indo-European ones. Reinecke’s warning (Valdman 1977b:ix) that only a few non-Indo-European pidgins and creoles have been studied so far does not seem to have been

headed by later creolists. In one of the most recent edited volumes the editor, DeGraff (2001b: 36) still has to apologize for the lack of information about non-Indo-European pidgins and creoles in his volume.

### 3. Language acquisition and pidginization/creolization

The problem of new candidates for membership of the community of creoles/creolists was not the only one impinging on its exclusivity. Another threat came from studies that looked at these processes from the angle of the language learning process involved. Pidginization and creolization were usually defined as processes of language acquisition in circumstances in which the input became structurally different from the output. This definition forced creolists to demarcate between phenomena of general L2 acquisition and pidginization, as well as between general L1 acquisition and creolization. DeGraff (2001b: 4), for instance, mentions in the case of Haiti the presence of “both restructured and/or reduced second-language versions of French and substrate-influenced early pidgins with French-based lexicons”, implying that these are different varieties or processes. The question remains whether it is possible or desirable to differentiate in a principled way between the two. It seems that in creolist literature there is a persisting idea that there is a difference between ‘normal’ L2 learners who are able to absorb completely the second language they are learning without any interference from their substratal language, and pidgin learners who cannot help but bring in the structure of their first language. The approach advocated, for instance, by Andersen (1983), in which these processes are viewed as manifestations of the same acquisition processes on a continuum, does not seem to have caught on and the consensus of most creolists seems to be that there is a marked difference between the acquisition process in ‘normal’ and in ‘handicapped’ circumstances.

Presumably this emphasis on the special character of the pidginization as against ‘normal’ L2 acquisition, and creolization as against ‘normal’ L1 acquisition has something to do with a shift in attitude towards the role of the new learner. Whereas earlier pidgin and creole studies emphasized the role of the speaker of the target language, there is a growing tendency in creole studies to concentrate on the role of the learner and to downplay the role of the speaker. No doubt this is a reaction to the initial tendency (Hymes 1971b: 3) to regard the speaker of the lexifier as the initiator, who gave the indigenous peoples or the slave laborers a bastard language. This way of looking at the new speakers represented them as hapless victims of a colonial process that was thrust upon

them while they themselves were unable to do anything on their own. This attitude was undoubtedly connected with a racist attitude still rampant at the time the proceedings of the Jamaica conference appeared (cf. also Mufwene 2001). As a result, notions such as 'facilitation' and 'foreigner talk' seem to have more or less disappeared from creolist studies and terms such as 'impoverished' or 'reduced' versions of the superstrate language have become more or less politically incorrect.

But when we look at studies of contact languages outside the immediate colonial context, we find that there is no compunction against using such terms and emphasizing the role of the native speakers who provide the new learners with a version of their own language that both groups perceive as easier to learn (although sometimes the new learners seem to be under the mistaken impression that what they are learning is the actual language of their hosts). Presumably, in such cases the lack of inequality between the two parties makes it unnecessary to downplay the role of the native speakers. Studies such as Dutton (1985) on the speech strategies of the Motu in New Guinea put the interaction between native speakers and foreign learners in a new perspective. On their trading trips when visiting other communities the Motu were forced to communicate in a simplified version of the language their hosts offered them, but when at home they themselves provided a simplified version of their own language to their visitors. Both in the instances discussed by Dutton and in other situations of heterolingual interaction such as the Amerindian jargons (Drechsel 1997), the interlanguage used between the parties or between third parties does not seem to have carried any stigma of the kind usually associated with pidgins and creoles. The comparison with such non-Indo-European restructured varieties is a very healthy and welcome addition to the database.

This also means that in some cases it might be advisable to acknowledge that we are really dealing with something that might be called an impoverished version of a reduced version (Traugott 1977:74–75). The objections to such notions are to a certain extent valid, and it is understandable that they have largely disappeared from creolists' discourse. Nevertheless, a certain degree of facilitation is always involved in interlingual interaction. Thus, for instance, the Motu mentioned in the previous paragraph tailored their language to the linguistic competence of their interlocutors: speakers of Austronesian languages, related to Motu, were provided by a version of Motu in which certain features, e.g. the use of bound pronouns, were maintained, whereas speakers of non-Austronesian languages received a much simpler version of the language, in which bound morphemes were replaced by free morphemes (Dutton 1985). Another example is that of classroom L2 acquisition in which teachers delib-

erately address their students in a simplified version of the language, the only difference being that in most cases this classroom language is grammatical although simpler, whereas in the case of improvised interaction grammaticality does not seem to be requested.

It is hard to see why there should on principle be a distinction between such cases and those of slave traders addressing their slaves. There is no doubt a difference between completely makeshift linguistic communication in accidental situations, as in the case of tourists (Hall 1962, 1966), and conventionalized means of communication. Yet, in the striking example of the Ngarluma pidgin (Dench 1998), which was used by Australian aborigines to communicate with shipwrecked sailors, the basic mechanisms would seem to have been the same as those used in accepted cases of pidginization. Of course, according to Whinnom's (1971) theory of secondary and tertiary hybridization, such cases as the Ngarluma pidgin would not count as pidginization anyway, since they represented strictly bilateral linguistic interaction, whereas in order to be counted as 'true' pidgins, they should have involved interaction between several non-native heterolingual communities.

The problem of categorizing restructured varieties gets worse when such situations are cast into a conceptual model in which *languages* are said to interact rather than speakers. This may be a metaphorical image, but it is one that has profoundly affected the thinking about what creoles and pidgins *are* as opposed to the acquisitional processes that form the basis for the restructuring. The distinction between E-creole and I-creole urged by DeGraff (2001:9) helps to keep the metaphor in its place, but does not help in discussions about the emergence of pidgins. Regrettably, initiatives to use communication theory in studying pidgin structure, as in Cassidy's (1971) contribution to the second Jamaican conference (Traugott 1977a: 85–86), have never become popular. Such an approach might have helped in keeping the focus on both the learner and the speaker of the 'superstrate', and in strengthening the connection between creole studies and the study of foreigner talk and simplified registers, as proposed by Ferguson (1977) and others.

With the shift of focus from the speaker to the learner, and subsequently to the language itself as an autonomous structure, another aspect disappeared almost completely from the field of research. At the second Jamaican conference Hymes (1971b:6) mentioned the special contribution the study of language variation in creole communities could make to sociolinguistics. In the same volume DeCamp (1971a:29) singles out his own and others' work in the study of language variation, in connection with what is called here possibly for the first time a 'post-creole continuum'. This work introduced the use of scalo-

grams and implicational tables to linguistic fieldwork, but somehow it does not seem to have caught on. Traugott (1977a:87–88) points out that a theory of diffusion of linguistic change is essential for the development of new paradigms of acquisition and change that aim to go beyond the homogeneity model of transformational/generativist grammar. Muysken (1981:vii) calls the high degree of variation a challenge to the generativist paradigm, but looks for a solution in the development of more adequate models within that paradigm. In fact, a model for the study of variation in creole communities, using implicational hierarchies and a dynamic wave model of linguistic change, had already been proposed by Bailey (1973). This approach would be able to address issues of emergence as well but does not seem to have caught on, either.

#### 4. Creole studies and general linguistics

The self-organization of the new discipline of creole studies and its emergence as a separate discipline was the work of the small group of scholars who organized the first conferences. What they had in common – apart from their special interest in pidgins and creoles – was their opposition to the growing tide of linguistics as the science of language that studies a homogeneous structure, the idealized language of an idealized speech community. Just like any other new discipline, creolistics had to struggle with problems of domain, terminology, definition etc. The question is what the position of creolistics was within the larger field of general linguistics. At first, the predominant framework of generativist/transformational grammar, with its claims regarding the structure of universal grammar, drew many creolists who were interested in the universal features of pidgins and creoles, to general linguistics. For them, the study of creole and pidgin languages constituted a challenge because they believed that the structure of these languages might serve as a test case for the validity of the framework of universal grammar. Consequently, within creole studies the kind of universals studied were the Chomskyan ones, rather than the Greenbergian ones. The predominance of generativist grammar also put an end to the original interest in variation grammar, given the predilection of transformational/generativist grammar for studying language as an idealized construct within a speech community presumed to be homogeneous.

The love affair between generative linguistics and creole studies was short-lived, which is not to say that linguists have stopped analyzing creole structure in a universal grammar framework (see for instance, Lightfoot 2001; Rizzi 2001). Generally speaking, however, there has been a shift towards non-

homogeneous linguistic phenomena, from code-switching and variation to language contact. Ironically, this renewed interest constitutes a threat for creolistics as a discipline from another direction. The upsurge in interest for diffuse contact phenomena has made room again for the non-Indo-European examples of linguistic contact that had become almost completely forgotten. The shift of focus away from the classic examples of pidgins and creoles towards a more 'global' view of contact phenomena also had the advantage of bringing back the speaker who facilitates the understanding and production of the non-native learner.

From the perspective of contact linguistics the difference between pidginization/creolization and ordinary linguistic change is much smaller than some creolists used to believe. Thomason and Kaufman (1988:192–194) maintain the difference between the two, chiefly because in their definition pidgins usually emerge in multilingual settings, but they also stress the similarity between them. From a completely different perspective Lightfoot (2001) reaches the same conclusion that creolization and language change are different aspects of the same phenomenon. A combination of contact linguistics with a theory of diffusion of linguistic change, as advocated already by Traugott (1977a:85–86), seems to hold a lot of promise for future research. So does the possibility of integrating variationist studies in the study of contact phenomena, which all too often are treated as unidirectional and homogeneous over the entire community rather than as diffuse and variegated.

Contact linguistics is not the only manifestation of the growing interest in non-homogeneous language use. The realization that language is not an organism but an individual set of habits as Holm (2000:2) puts it, had already come earlier within sociolinguistics. But an all-out attack on the idealization of speaker and speech that underlies generativist grammar (Salmon 1997) was still lacking, and only in the last two decades has this idea begun to strike root in mainstream linguistics. At an even deeper level, the philosophical assumptions of generativist linguistics such as the human monopoly on language have increasingly come under attack (Savage-Rumbaugh et al. 1998), with wide-ranging consequences for the scope of linguistic research.

In a recent study about sign language and the gestural origins of speech (Armstrong et al. 1995) one entire section is dedicated to creole and pidgin languages. Obviously, topics like the origin of speech, once officially declared taboo by the *Société Linguistique de Paris* (Stam 1976:255), have become a legitimate subject for research. Hardly any introduction into linguistics nowadays leaves out a discussion of how language originated. But even more interesting in this book is the combination of results from creole studies with

a completely different field, that of sign language (also very well represented in DeGraff 2001). The best example of such a combination of topics remains Bickerton's (1990) sweeping account of the nature of speech, in which results from the study of pidgins and creoles, language disorders, child's language, animal language and paleontology are combined into one consistent view of what pidginization is all about. His specific solution, the assumption of a biomechanism that is responsible for the development of speech and ultimately for the kinds of phenomenon that we find in pidginization and creolization, has not been universally accepted (see the discussion in DeGraff 2001:49–157). Certainly, there have been interesting developments in a different direction, linking the language learning mechanism directly with neuronal structures, for instance in Lortz (1999). But the trail-blazing audacity with which Bickerton combined phenomena that used to be studied in isolation should still be counted a lasting achievement.

Creole studies have indeed taken their rightful place in contemporary linguistic thinking, but ironically, with success and recognition has come a loss of autonomy. Creole studies have been incorporated in larger fields. They are no longer the exclusive property of a group of enthusiastic creolists within their own community but have become a common place topic on which all linguists feel qualified to talk. Perhaps the time has come to ask whether 'any sane creology' (Givón 1979) as an autonomous discipline has outlived its usefulness.

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# How the study of endangered languages will revolutionize linguistics

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Haskins Laboratories and the Endangered Language Fund

The field of linguistics is heading into a time a great change. The fundamental elements assumed by the field are undergoing radical revision, fueled by two factors: the computerization of language material, especially from endangered languages, and the more detailed study of endangered languages themselves. The improvement in our ability to document languages is occurring even as the languages cease to be spoken, increasing the urgency of the documentation process. Although documentation is essential to revival efforts and thus of great use to the native communities, this paper focuses on the dramatic influence such language material will have on the field of linguistics.

## 1. Introduction

Linguistics is at an exciting stage of development, poised to make more dramatic discoveries than at any time in the past 40 years. Unlike the conceptual advances that were largely responsible for the Chomskyan revolution (e.g., Harris 1993), the changes today are fueled by changes in the nature of the data and the technology that deals with it. The most fundamental elements of the field of linguistics are being revised, and the study of endangered languages will be at the center of this new revolution.

Let me hasten to add that the study of endangered languages is not new to linguistics – far from it. Many languages are now only known through the recording efforts of earlier linguists. Indeed, it is through these archives that the languages can be called “sleeping” rather than dead – various Native American groups have used this term, as outlined in Hinton (2001). Even at the formation of the Linguistic Society of America, Leonard Bloomfield noted the state of the “American Indian languages, which are disappearing forever, more

rapidly than they can be recorded, what with the almost total lack of funds and organization” (Bloomfield 1925:4). In the 75 years since then, several things have happened: The rate of language death has increased, but the means of recording languages has expanded dramatically. The rate at which languages are “going to sleep” is higher, but the availability of materials from which we might awaken them has increased as well. Oddly, it took another 65 years for organizations devoted to stopping language loss to appear, and the money has begun to flow in sizable amounts only in the last few years. The damage done to societies when they are forced to give up their language have been detailed elsewhere (Crystal 2000; Grenoble & Whaley 1998; Hinton & Hale 2001), so I will not go over the details here. Suffice it to say that language loss in the modern world seldom reflects a voluntary choice on the part of the language community.

We have already heard from Tasaku Tsunoda (this volume) about the effective use of linguistic materials, collected for purposes of analysis, but now the only record of a language. The fact that we can revive a version of a language that is satisfying to the heritage group (that is, those descended from the original speakers) is a tribute to the level of accomplishment we have attained in the field of linguistics. The use of material by native speakers and their descendants is an extremely important aspect of dealing with the modern world in an ethical manner. Although the ethical use of language is a complicated topic, it is one that is receiving a much needed upsurge in attention (e.g., Hinton & Hale 2001; Maffi 2000, 2001; Whalen 2001).

### 1.1 Focus on the benefits to linguistics

My focus today, though, is the effect of the study of endangered languages on the field of linguistics itself. Thousands of languages are in danger of disappearing without, in many cases, much of a record left behind. The fact that they are ripe for study is no guarantee that recording them would revolutionize the field of linguistics. The study of vanishing species does not necessarily hold the same promise for biology, for example. If the causes of speciation and genetic inheritance are the same in the extant species as in the endangered species, then biology might have only incremental lessons to learn before the species disappear. It is certainly true that species are being examined for unusual chemical products that they might produce, especially ones with medicinal value to humans. But the fundamentals of biology seem secure. Linguistics, on the other hand, has had some success at describing its subject matter, but there is no

grammar that is a complete and satisfactory one. The lessons we learn from endangered languages will change that within the coming decades.

The concept of language documentation has also been a rather recent addition to the discussion in linguistics. Christian Lehmann (personal communication), writes, "The primary purpose of language documentation is to represent the language for those who do not have direct access to the language itself." Thus the text collection that has proceeded throughout the history of our field is only a part of documentation. No text fully represents a language, and texts must always be supplemented with analysis if the language is to be known in anything like its full form. Documentation is not critical for the "safe" languages, the ones that will continue to be spoken, yet that is where most of the effort is concentrated. For example, in the excellent collection of material at the Linguistic Data Consortium ([www ldc.upenn.edu](http://www ldc.upenn.edu)), over half of the data sets are for English. Among the rest, only Ngomba is a small language, the next smallest being Czech and Farsi. This situation needs to change, and there are indications that it will.

## 1.2 Two main aspects of the coming changes

There are actually two parts in the realm of changes to linguistics that will take place, and only the second is truly unique to endangered languages. I will get to that later, but let me first discuss an aspect of language analysis that is becoming common to all language study and that will have revolutionary impact: the computerization of speech. Although this in itself is not new, the effects have yet to be felt. The Association for Computers and the Humanities, for example, was founded in 1978, to promote the use of computing in fields like linguistics. There are many fine programs that do such interesting tasks as constructing syntactic trees, watching the descent of daughter forms from proto forms, making dictionary entries accessible electronically, etc. But the impact has been limited so far. What needed to happen was to combine the power of the computer itself with the universal access associated with the internet. Now that such a union has occurred, there still need to be some changes before the revolutionary impact can be seen.

Having a computerized text is somewhat more useful than having a transcription, but it is not revolutionary. Being able to search through the text and have a program perform statistics on the distribution of various linguistic features is quite useful and can lead to insights that would not be feasible otherwise. The study of contested authorship, the frequency of occurrence of phonemes and words, and the acoustic analysis of the speech signal would all

be forbiddingly difficult without a tool like the computer. But the way that digitized language has been shared to date is much more like the longest tradition of linguistic field work, in which notes about the utterances reside with a single researcher and are available only to those who physically travel to where the notes are and receive permission to use them.

What is beginning to happen now is completely different, namely, original linguistic material is being made available to the broadest possible audience by putting it on the internet. There are four reasons this will change the way linguistics is done:

1. Linguists will be working from the same data rather than individual data sets.
2. The texts can be searched easily in many ways.
3. The actual sounds will be available rather than just transcriptions or further idealizations.
4. Work will progress on a unified ontology for linguistics.

The first two issues are quite dependent on the technological advances provided by the internet. It might seem that the fourth, the development of a single set of descriptive terms for linguistic structures, could happen regardless of changes in technology, but I will try to show that this is not the case.

2.

### 2.1 Access to data

The sharing of data is critical to advancing the field. Imagine the state that the Neo-Grammarians would have been in had they not all had access to the same data. Imagine that Bopp had the Rig Veda and Whitney had the Atharva Veda, and they were the only ones that could see them. We would never have had the explosion of progress in comparative analysis that we did see. Instead, the texts were widely available and widely used. Granted, they were not best-sellers even then, but libraries throughout the world could provide the raw material for linguistic analysis. If anyone felt that a citation was incorrect, she could look it up. It seems uncontroversial that this is a good way of doing science.

But compare that with the situation that we are in now. For common languages, we have a wealth of material along with native speaker intuitions. But for looking at more unusual languages, we are at a stage where essentially one or two linguists have access to the raw data, and everyone else gets snippets of

their analysis. Even in the best case, in which a linguist has deposited recordings with an accessible archive like the American Philosophical Society or Canada's Museum of Civilization, access is limited to those that can make a trip to those locations. Further, the amount of work that is needed to extract material from a tape recording that is deposited in an archive is extremely large. In the best case, there is a transcription of the entire tape to start from, but aligning that transcription with the tape itself is a technological challenge. In many cases, there is no transcription but only a translation, which requires even more expertise on the part of the would-be user. It appears to be even more common for large sets of materials to end up in archives with no accompanying interpretation at all. At this point, one must almost be a native speaker in order to make use of the material.

In order to make the basic facts of a wide range of languages available to the broadest range of linguists, we need the internet. Publishing texts is an improvement over the more typical state of affairs, but print publishers are increasingly reluctant to produce such volumes. They are typically not profitable and end up with limited distribution. Having a published version that very few people have access to is only a slight step ahead of not having a published version at all. With the internet, the initial costs are all that have to be considered. Once a text is put on the web, it can be accessed once or hundreds of times, and the resulting costs are essentially the same. This allows for wide dissemination without excessive cost.

Incidentally, this version of publishing is increasingly useful for native speaker and heritage communities as well. More and more groups are gaining access to the internet, often before they have many other services. In some cases, they have internet access even in refugee camps, allowing access that would not be possible with printed material.

## 2.2 Searching the data

The second important aspect of putting language material on the internet is the searching capabilities. These are very useful for the kinds of research that linguists do, and they allow research that would be all but impossible with printed texts. Imagine searching through texts for every occurrence of the phoneme /p/, and only deciding at the end of the search that you also needed every instance of /b/. Hundreds of hours of work, going over the same material, would ensue. With an on-line search, much of the work is done even before the search is attempted. Words must be tagged separately, and each phoneme in a fully marked-up text will already be accessible. The additional work needed to find

and count different classes of phonemes is then relatively trivial. Certainly the actual searching is done by a machine that does not really mind being asked to do the same thing over and over, with slight variations. After all, it's just more electrons flowing through as far as the computer is concerned. For the linguist, it is the difference between having results and having none. Electronic publishing of texts and other linguistic material, therefore, will bring us back to the stage we were at a hundred years ago, when any interested linguist could go to the original source material if she so desired.

### 2.3 Access to sound files

The third aspect of electronic publishing, however, is even more revolutionary: the inclusion of sound files. As linguists, we know that orthographies record as much of a language as native speakers need to easily reconstruct an utterance, and thus are of varying use for linguistic analysis. We also know that transcriptions, even the most narrow ones, are an idealization of the signal. Yet we have been limited in our reanalysis of other researchers' materials to just these compromises. Having a sound file available with the text is a way to overcome these limitations to a large extent. Recordings themselves are something of a compromise, since visual information is lost and aspects of the recording situation can affect the apparent linguistic sounds that are recorded. As a simple example, if the recording situation was in someone's kitchen, it may be impossible to tell whether a certain syllable contained a click or was accompanied by a can being opened in the background. However, the amount of phonetic detail that is captured is several orders of magnitude larger than that in the transcription. This information can now be shared at the click of a button. An excellent example is the work on tone in Bamileke Dschang (Bird 1999), with a web version available at <http://www ldc.upenn.edu/sb/fieldwork/>. Here, the rather subtle differences between the tones can be heard and other, related forms can be found as well. As the linked texts become more common, even more examples of the tone contrasts would become available to linguists all over the world.

Phonology and phonetics certainly stand to gain from the inclusion of sound files. It is often the case that a linguist will question data in published reports on a language that she knows well. With the sound file included, it is possible for any linguist to make a judgment. Caution is certainly necessary; it is usually the case that subtle aspects of an utterance will elude the untrained ear. Yet it is also the case that four ears are better than two, at least as a corollary to the saying, "Two heads are better than one." One listener will often detect

something that another listener will miss, and sometimes being unfamiliar with the language is a positive benefit. It should certainly be possible, in any case, to convince listeners that the expert's analysis is correct – the subtleties should ultimately be perceivable.

Syntax, though, may benefit even more than phonology and phonetics. Our current transcription systems are rather schematic, as already mentioned, but they are typically ignored altogether for intonation. The use of intonation to signal syntactic relations is one of the hallmarks of language, yet most syntactic analysis proceeds without regard to intonation. This is largely a practical decision, though it is sometimes raised to the status of a theoretical one (e.g., Martinet 1960). What is quite clear is that certain sentences that are thought to be ambiguous when examined in written form are perfectly unambiguous when spoken. As a headline, “British left waffles on Falkland Islands” (Lederer 1989:63) is ambiguous. When spoken, it would usually take a fairly uncooperative listener to take the wrong meaning. This is a simplification as well, since there are intonations that are rather ambiguous for this sentence, but we have not yet explored how frequent such constructions are nor how often speakers use them in favor of an unambiguous intonation. The data has, for the most part, been unavailable. With immediate access to spoken versions, we will be able to assess the intonation more directly. The concentrated attention of large numbers of syntacticians may also lead to a more useful narrow transcription system for intonation.

It is worth mentioning here that we will probably come to expect that linguistic examples in published papers will have spoken versions available on the web. This will be of great assistance to those who are reading the articles in evaluating the data. Often times, the reader is not a native speaker of the language in question, and so may be unaware of the kind of interpretation it would be given. Even for those who speak the language, dialect differences often are not apparent in the written form, or even the transcription. Ultimately, this will allow a more complete description of the language data to inform the theoretical debates.

## 2.4 A unified ontology

The fourth improvement in linguistic analysis that will be due to putting material on the internet is the development and use of a unified linguistic ontology. An ontology in the modern, nonphilosophical sense, is a description of objects and their relationships to each other. While the description of ontology might seem like an atheoretical task, it actually goes to the heart of a discipline, es-

pecially one like linguistics in which the “objects” are all mental categories and thus difficult to find evidence for. The use of a unified ontology will be felt both because the machines require it and because it is the right time to tackle this problem. The current state of linguistic theory contains an extreme contradiction in what we think about our subject matter. On the one hand, we assume that all languages are basically the same. Every child seems to be able to learn any language (though I will return to this later), and the basic building blocks seem to be the same for all languages. However, all languages are different. This is true first in the trivial sense that if they were not different, we would not call them separate languages, but it is also true that they differ in ways that are not easy to describe as just being minor variations of universal categories. Yet how can this be, if all languages are the same? It is always possible to take an extremely general view of language and say that the really important part is only a single aspect such as recursion (Hauser et al. 2002), but when the grammars are written, this level of analysis is insufficient to capture the patterns of language that are found. The descriptive vocabulary in particular seems to fail us as we go from one language family to another (and sometimes from one language to another).

Let’s take an example from a universal in human languages, the treatment of aspect. Every language has some way of expressing the relationship of events to a time-line. Sasse (2002) lists three areas of general agreement on this issue: aspect is a matter of boundaries; it is governed both by the grammar and the interaction of the grammar and the lexicon; and explicit theoretical descriptions of syntax, the lexicon and their interaction are needed for an understanding of aspect. Sasse then states, “Within these general confines, there are contradictory positions on almost any of the basic issues: there are considerable differences in the various underlying models of ‘time logic’; there are divergent views on what constitutes ‘atoms’ of an aspect theory (i.e., the granularity of distinctions); there is disagreement on the interpretation of the different semantic effects observed (truth-conditional values, pragmatic implications, universal semantic categories, etc.)” (2002:202). Before discussing more specific issues, it is startling to see that such a universal feature of language has not allowed for a universal and agreed upon analysis. This tells us that simply being a universal part of language is no guarantee that a unified description is obvious and adequate. The disagreements about the terminology come about not because the temporal domain is so complex but because the interacting domains that different languages take to be relevant are numerous and idiosyncratic. Sasse concludes that much has been accomplished, but that individual languages must be examined to confirm or refute our cur-

rent theories. The internet will make that more feasible, since examples that have been labeled with one category or another can be easily compared and the underlying data examined by all interested researchers.

Sasse also recommends an “open-minded research strategy” (2002:266), which appears to be at odds with the notion of creating a single ontology for linguistics. However, the two must go together. If we do not attempt a unified ontology, then it will be impossible to make use of the rich source of data that the internet is beginning to provide. Only when the disagreements about labels can be argued on the basis of data rather than definitions can we begin to have a true science of language. To take an analogy with chemistry, we have not yet discovered the periodic table of linguistics. We do not yet know what evidence it would take to establish a new “element,” let’s say, a new morphological category. But surely if we do not attempt to restrict the elements, then we will never know which ones are truly needed. The chemical analogy goes further, in that elements interact with other chemicals differently in different ionization states. Once these states are recognized, the fact that elements behave differently in different environments is predictable rather than a cause for disagreement. We are only beginning to find such a description for linguistics.

Using a single ontology will require changes in areal grammatical terms, and this will cause disruption in the short term that should be justified in the long term. Consider the “obviative” marking in the Algonquian languages. This complex ranking of third person cases is a hallmark of all the Algonquian languages (Bloomfield 1962; Frantz 1966; Hockett 1966). Yet is it a category that does not exist in any other language family? Should we expect to find that the existence of entire morphological categories is a feature of being a language family? Recent work by Aissen (1997) indicates that we should expect other languages to have processes similar enough to obviation to deserve being classified as such. She examines two languages unrelated to Algonquian – Tzotzil, a Mayan language, and Chamorro, a Western Austronesian language – and finds that similar processes are at work there. It is only through the unwillingness to take a language family label as the ultimate best description that allows us to uncover similar patterns, and presumably similar solutions, in other languages. Although Aissen’s work shows that this kind of effort can be accomplished without the impetus of a unified ontology, the ontology will spur many more researchers on to this important kind of work.

One of the most extensive attempts at a linguistic ontology is the “General Ontology for Linguistic Description” (GOLD) being developed by Terry Langendoen, Scott Farrar, and William D. Lewis. They are working under the EMELD project, funded by the U.S. National Science Foundation

([www.emeld.org](http://www.emeld.org)). This five year project is devoted to solidifying the techniques and metadata needed for presenting endangered language material on the web. It is under the direction of Helen Aristar Dry and Anthony Aristar, whom most of you will recognize as the editors of the Linguist List. Not having been satisfied at creating one of the most useful tools the discipline has known for keeping its members in touch with each other, they have embarked on this project to make the raw data of the field available to everyone. As part of that effort, a unified ontology is being proposed (Farrar et al. 2002). This draft proposal is just the kind of effort that needs to occur so that the field can make use of the language material on the internet and make progress in its own task of describing languages. It will not be possible for this ontology to satisfy everyone, but the notion of simply having multiple ontologies with translations between them would lead us back to the same kind of regionalism that has given us the current state of affairs in linguistics. It will be much more rewarding to see what evidence guides the arguments over definitions and scope of the terms in the ontology. We can expect this debate to last a considerable time – I would expect at least a decade – but I also expect that there will be greater consensus afterwards than the field has ever known.

There are two aspects to ontology, as Farrar et al. point out in their paper: The first is the definition of each narrow sense grammatical concept and its possible instances, such as *DurativeAspect* and *InstrumentalCase*. This will involve laying out the meaning of all the terms in use with a thoroughness seldom attempted. The second is to provide the basis of the analysis of the open-class vocabulary of human languages, which clearly differ in the range of meanings that can be associated with them. Both goals have to be pursued at the same time because the distinction between features for open and closed classes is not clear for any one particular language and can be controversial when unrelated languages are compared. As Farrar et al. (2002: 18) point out, “the features involved in the analysis of the closed class of ‘handling verbs’ in Athapascan languages are very much like those needed to describe open class vocabulary items in other languages.” Even knowing how the grammatical and lexical features can distribute themselves across languages is something that is not realistically possible now, but will become so as the internet data come into being.

## 2.5 Summary of effects of internet access to data

The computerization of language data, then, will soon bring us to an unprecedented state of facility for analyzing language. The availability of the raw data

will force a higher standard of descriptive adequacy than we have known for spoken material (as opposed to written). The kinds of searches that can be done will be extraordinarily more powerful than the ones that are available without computerization. The use of sound files for exchanging data will allow better phonological descriptions, ones that take the actual realization of phonemes into account, and will further change the way we do syntax, by making it easier to evaluate and incorporate intonation into the description. And we will be forced to attempt a unified ontology, in order to make the data as useful as possible. This last concern leads us directly to the changes in linguistics that will be more specifically related to endangered languages.

### 3. Results from endangered languages

The greatest diversity in language features occurs in the languages spoken by the smallest percentage of the world's population. Every language is full of wonders and infinite nuances. Even English, a language of which I am rather fond, has not yielded up all its secrets to the hundreds of linguists who have been poring over it for the past 50 years and beyond. Yet the most extreme cases of language differentiation are surely to be found in the highly divergent language families, many of which are faced with extinction as an entire family, not just a member language or two. Most of the Australian languages, for example, are moribund. Many of them, however, are undergoing revitalization efforts, and some of these may succeed. But entire lines of native L1 learning of these languages has ceased, making it virtually impossible to do new linguistic research on some of the most unusual language families.

This pattern is repeating itself throughout the world, and linguists have been strangely passive about it. Despite the explicit statement I quoted earlier from Bloomfield, the field as a whole developed in such a way that there was little attention paid to the issue of language disappearance, or, more accurately, languages going to sleep. The Australian linguistics community was energized in the early 1980s to begin a systematic recording of all the Australian languages, and this project succeeded with a thoroughness that has not been replicated elsewhere. The issue of endangered languages was on the plenary-session level of the agenda of the XV International Congress of Linguists, 1992 (Crochetière et al. 1993). In the U.S., the publication of a group of articles in the journal *Language* (Hale et al. 1992) was the first sign of a collective awareness of the issue within the discipline of linguistics. Soon after that, various groups arose specifically devoted to the preservation of endan-

gered languages. The Linguistic Society of America created the Committee on Endangered Languages and their Preservation (CELP). The National Science Foundation in the U.S. began specifically soliciting projects on endangered languages. In Japan, Osahito Miyaoka obtained funding from the Grant-in-Aide for Scientific Research on Priority Areas of the Japanese Education Ministry (Monbu-kagaku-sho ), and for three years, he headed a large scale project with over 150 linguists to document endangered languages of the Pacific Rim (2000–2002) <[www.Elpr.bun.Kyoto-u.ac.jp](http://www.Elpr.bun.Kyoto-u.ac.jp)>. Joanna Hess created the Institute for the Preservation of the Original Languages of the Americas, or IPOLA, which has since changed its name to the Indigenous Languages Institute (ILI). The group Terralingua was formed to highlight the interaction between ecosystem and language. In England, the Foundation for Endangered Languages was started by Nick Ostler, and in Germany, Hans-Jürgen Sasse started the Gesellschaft für bedrohte Sprachen. And I started the Endangered Language Fund in the U.S. These groups have similar aims with different emphases, but they are all dedicated to making the most of our linguistic heritage.

Since those early additions to the preservation effort, we have had two substantial infusions of research money. The first was an initiative by the Volkswagen Stiftung in Germany, which promoted the comprehensive description of endangered languages by interdisciplinary teams of researchers. That project is ongoing, and has contributed a great deal to the work on the topic, both in terms of promoting work in the field and in the use of computerization to share the results. Even more recently, the Lisbet Rausing Charitable Fund has initiated a program of grants to support the documentation of endangered languages, and has appointed the School of Oriental & African Studies, London University [SOAS] to administer the scheme. This well-funded program will support a great many projects throughout the world, and the material will be archived at SOAS indefinitely. Here again, we can expect great things to come from this program. In addition, other foundations are beginning to take notice of the issue. The concern does not arise from primarily linguistic concerns but rather for issues of human rights and social cohesion. Still, I think that linguists can be proud that our efforts are resulting in greater awareness of the impact of language on the welfare of various human populations, especially the smaller groups. UNESCO's Intangible Cultural Heritage Unit is renewing its efforts in supporting endangered language communities, and a group of linguists has been working with UNESCO to formulate guidelines and recommendations on maintaining linguistic diversity. A proposal "Language Vitality and Endangerment" is being prepared for the international meeting in March, 2003, by a group that include Matthias Brenzinger, Arienne Dwyner,

Colette Grinevald, Michael Krauss, Osahito Miyaoka, Osamu Sakiyama, Rieks Smeets, Ofelia Zepeda, and Akira Yamamoto. Yamamoto is attempting to mobilize as many linguists as possible by using internet networks in preparing this document.

The focus of this paper, as I said before, is on the other side of this two-way street, the way in which the field of linguistics will change with the study of endangered languages. Clearly, nothing will happen if the languages are not studied, and the support for their study is a necessary component of this picture. The researchers need to be in place for anything to happen as well. And the results have to be made accessible, as I discussed in the first half of this paper. Now I want to discuss some of the ways that endangered languages are going to contribute that would not be served by unendangered languages:

1. The time-scale at which we can understand linguistic change will be greatly expanded if we have data from the languages and families that are going to sleep.
2. Claims for universals and what is learnable will be more justifiable with the more unusual data.
3. The effects of writing, and the limitations of studying only languages with writing systems, can be determined.
4. The linguistic ontology (coming full circle) will be more complete.

None of these benefits can be had once the languages are gone. And literally hundreds of languages will go to sleep during the professional career of those starting out in this year of 2003.

### 3.1 Increasing the time depth of comparative work

Comparative data have given us the most solid results in the realm of linguistics. The proposed genetic relationships based on correspondences among cognates have withstood the test of time, and the techniques have hardly been improved upon (Hock & Joseph 1996). To tackle greater time depths, we need to have a larger set of relations to be established among farther flung families. This has so far eluded us, and it may never happen. But the use of material from distantly related families is the only hope we have for dealing with a greater time scale than is currently possible. The data that will be required is primarily lexical, but the semantic changes that can be used to establish cognates need to be delimited better than they currently are in order to avoid overgeneralizations. Again, we may never achieve the time depth that we would like, but the data for even trying is disappearing.

### 3.2 Tests for the essential character of the language faculty

Second, the nature of the language faculty and how it allows for first language acquisition are hotly contested, and there is much more concrete evidence that is necessary. If learning a language is changing settings of a universal set of parameters (e.g., Culicover 1997), we need to know just how broad a range of settings is possible. If learning is only pattern detection, as proposed by neural network theory (e.g., Elman et al. 1996), we need to see if there are in fact patterns that may not be truly learnable. None of this will critically require studying acquisition of the endangered language, though that would, of course, be useful. (The status of most endangered languages as not being learned from the cradle means that the study of acquisition in those languages is not typically possible.) But just knowing what the end-result grammars are like will help us understand what it is that infants must derive as they acquire their language. These issues are widely debated, but there may be a way of putting the question that could help, one that helps us envision something outside the language faculty.

What would it take for a language to be unlearnable by some portion of the human population? One case might be phonological, based on some peculiarity of anatomy. Let's say that some group developed a flap connected to an air sac in one cheek. This might allow for a quick pop to be released any time during a speech sound, even in the middle of a vowel. Humans with the current anatomy would be unable to produce these sounds, and might not even be able to perceive them correctly. In particular, it might be impossible for them to tell when the pops occurred within the speech stream and thus to decide what order the phonemes occurred in. Some unpublished research that Alvin Liberman and I performed to look at timing in speech suggests that this is plausible. Listeners were unable to report where within a syllable a nonspeech click occurred, even when it was exactly in the center of the syllable. They most often reported it at the beginning or the end of the syllable, no matter what its real location was. The ordering of the speech sounds is easy for us because we have a specialization to deal with these sounds (Liberman & Whalen 2000), not because we are generally good listeners.

So if one subspecies of humans developed both the ability to produce these cheek pops and the ability to locate them relative to the total syllable, it might make it impossible for other groups to fully master that language. Of course, one would assume that many aspects of the language would be in common with existing languages, but these new sounds would probably be common in the language. Languages that invest in unusual consonant types appear to rely

on them heavily (Hombert & Maddieson 1998). For example, English has the relatively rare dental fricatives theta and edh, but they are used in many of the most common words of the language. The clicks of the Khoisan languages are not relegated to an occasional word but rather appear throughout the vocabulary. The extremely rare voiceless lateral affricate (occurring in 5 of the 317 languages in Maddieson 1984) is used heavily in Nahuatl. For example, 2985 of the 8306 entries of Amith's online Nahuatl dictionary contain this segment (Amith 1999). We can expect, then, that once this unusual capability was available, it would be heavily used in the language, and the language would be, in a very real sense, unlearnable by other humans.

It may be that brain evolution (where small changes can have large effects) is even faster than evolution in external anatomy, and thus we might see changes there first. Imagine that a language – let's call it Octavian – developed so that grammaticality required exactly one member of eight sets of morphemes to be uttered each day. Now, this certainly exceeds the definition of grammar as relating only to sentence-level phenomena, but the rules of anaphora and coherence clearly require that extension in any event. So, let's say there are eight classes of morphemes, each with real semantic content, but which are mutually exclusive on any particular day. They would also have to occur in order, requiring a great deal of planning to get it right. While this scheme seems fairly outlandish, I think it is fairly easy to see how it might arise. Imagine that there is an extremely strict religion that requires eight prayers during the day. Each prayer should be selected from some set of prayers, as dictated by season or circumstance. Over the years – or centuries, if need be – it is not hard to imagine that the prayers might be reduced to simple words or even bound morphemes, much in the way that “God be with you” ends up becoming “bye.” It is also not hard to see how someone who did not have sufficient long-term memory to keep track of these prayers might be excluded from the gene pool. Even if this religion did not strictly punish people for getting it wrong, it still would tend to mark those individuals who got it wrong as “profane” or even just “unlucky.” Again, over the years, such pressures could lead to the exclusion of the memory-challenged from the gene pool.

So, picture the difficulties faced by the “normal” language learner who is thrust into the “Octavian” environment. While the Octavians have a sort of memory buffer with eight slots that makes the day's duty seem natural, the “normal” learner is baffled. She can't understand why people keep asking her at bed time if she doesn't have anything else to say. All of her sentences are grammatical, but she goes to bed with an asterisk hanging over her head, punctuating yet another ungrammatical day. Now in this case, it should be possible

to learn what the eight morphological categories are, and what their (daily) distribution has to be. But would a non-Octavian be able to learn this pattern without explicit instruction? Currently, there seems to be no pattern of language (other than the auxiliary system of reading) that requires explicit instruction. This is one of the great strengths of the language faculty, since every normally developing human is a fluent native speaker, but far fewer are good analyzers and teachers. So it might just be impossible for a non-Octavian to learn this daily structure naturally. This would then be another way in which a language might develop so that it would not be learnable by the general population.

There are no doubt other ways in which languages might evolve so that they could not be learned. It is a topic that has not received much attention. Indeed, even the assertion that all children can learn any language is more an article of faith than an established fact. We have only recently seen something like a natural experiment of this, in the long-distance adoption practices of the modern world. The universality of language does seem to hold up well under this test, but are there subtleties that are missed? The languages that have large enough groups of speakers to support this long-range adoption of infants may simply have lost anything that might be unique. If a genetic change affected a small group and *their* language was not learnable, we would never know that fact if it was their children that were adopted into a majority group. So the natural experiment is incomplete, as are all natural experiments. To the extent that it is possible to extend that experiment, the time is now, while we have over 6,000 languages to work with. The languages that are most vulnerable are exactly those that are the most likely to have diverged significantly.

If there is already a language out there that is not fully learnable by the majority of the human species, we may not have the tools to discover it. An examination of the discussion of slips of the tongue provides a good analogy here. It has often been claimed that slips are segmental and mostly phonotactically coherent (e.g., Fromkin 1971). Yet when the data consist of transcriptions that are done in a nearly exclusively segmental system like the IPA, it is hardly surprising that segments seem to be the proper level of analysis. More recent studies that have looked at muscle activation (Mowrey & MacKay 1990) or movements of the articulators (Pouplier & Goldstein 2002) find consistent sub-segmental effects that are often inaudible. Only when we look at the signal with the right tool do we see the unusual. We may already be missing features of languages that have evolved beyond the normal ability.

### 3.3 Effects of literacy

The third area that endangered languages can uniquely inform us on are the effects of literacy. Most of the endangered languages have never had an orthography designed for use by native speakers, and many of those that have had such orthographies devised do not make extensive use of them (e.g., Bielenberg 1999). There are many reasons that such a lack of use can occur. There may not be enough technical support for printing, paper, computers, etc. The community may not feel that there is enough need to invest the time in learning how to use an orthography. Dialect differences may result in “orthography wars” which make the simplest writing a politically sensitive act. And some groups simply feel that writing destroys the spirit of the language. It is the scientific roots of this last feeling that I would like to explore here.

Writing changes a language, and some languages resist writing. These are not self-evident facts, or, at least, I hope they are not self-evident, because it took me a long time to accept them. Writing is parasitic on speech, as we know (e.g., Coe 1992). There is no language community in which writing has replaced speech or sign as the primary means of communication. Children do not acquire reading and writing as their first language. Yet this does not mean that writing must necessarily leave the language system intact when it begins its parasitic existence. (Parasites are typically bad for the host organism, but I do not mean to imply that here; I am very happy to be literate, and I am glad that you are able to read my words as well.) Learning to read changes the way words are stored in the mental lexicon. When English speakers make auditory judgments on rhyming words, they are affected by the spelling even though it is never presented in the experimental setup (Seidenberg & Tanenhaus 1979). Learning to read also affects which part of the brain processes phonologically legal nonwords (Castro-Caldas et al. 1998). Writing makes a language more conservative and makes older forms available on a regular basis, both enriching and complicating the language environment. So, the notion that a language should not be written because writing changes, and often fossilizes, a language is one that has some support. Only when we examine the trade-offs involved in adopting writing as a major part of the language community can we determine whether a literacy campaign should be started.

Some languages are resistant to writing as well, and it could easily be that they would change dramatically if they were written on a regular basis. Most of the time, the sound system is not a major obstacle to literacy. The 95 consonants and 24 monophthongal vowels of !Xu~ (Maddieson 1984) are a challenge, though an apparently successful orthography was devised (Lloyd

1911). Yet some instances are problematic. Tarahumara is legendary for its phonetic variability (Copeland 1994). Literacy has not gained much ground within the native community, but some speakers have become literate in their second language, Spanish. Amazingly, these speakers then lose their variability in their native Tarahumara, even though they are not writing it (Copeland, personal communication). Literacy can have large effects, probably the more so as the language is atypical.

The syntax of a language can apparently change as well. Kalmar (1985) reports a case in which Inuktitut apparently created a new class of subordinate clauses due to increased literacy in the community. Although many of the speakers were bilingual in English, it did not appear that the new construction was simply a borrowing. Rather, tendencies within the language appear to have been heightened by the use of writing and subsequently entered the spoken language.

It is quite possible that languages change in other ways as they make greater use of literacy and incorporating a description of those changes will entail a major change in our conception of language. We are at a convergence of trends that would allow us to gain a better understanding of just what the increase in literacy means for the use of the language as a whole. Some of the currently endangered languages that will survive will probably do so by making greater use of literacy, which is a seemingly indispensable tool in dealing with the ubiquitous presence of the global economy. Linguists today will therefore be able to study this process in a way that may never be available again. It would be especially useful if several different languages entering into similar literacy programs were studied at once, allowing for better generalizations across the necessarily divergent starting conditions. A study of what it is that is difficult about writing a language like Tarahumara could allow for predictions to be made for specific languages, which would be testable within a generation of readers. It is so seldom that linguists are able to make predictions about language change that this would be an extremely appealing opportunity for research. And, again, to make a bit of a statement about the other side of this two-way street, it would allow the linguist to make recommendations for the literacy program.

### 3.4 Diverse inputs to the ontology

The fourth feature that endangered languages offer linguistics is their typological diversity and its effect on the ontology. We can take any three languages and divide up their grammatical categories so that a coherent story can be told. If

the three languages all happen to come from a literate tradition, we might expect that their grammars would already be molded in some similar ways. It is only when we add in the features of the typologically distant and unusual languages that our theories really get a workout. The task of collecting the relevant data has been so large that only limited, though impressive, progress has been made on these issues. If we are ever to be able to listen to what these endangered languages have to tell us, now is the time. They are not going to be talking for that much longer. And they will only be sleeping rather than vanished if we use the tools of our trade to help communities preserve their languages.

The task is large, and it would benefit from efficiency gains in the way we do linguistics, but there has been very little in the way of such gains so far. The use of the internet will be one clear case. The ultimate tool will be the one that will take a recording of a text, do automatic speech recognition on it, return a transcription, perform a morphological mark-up, and then give a literal and free translation. I regret to say that this ideal will not come about before most of the languages spoken today have fallen silent. Anyone who simply records texts with no accompanying translation, in the hopes that they will be automatically translated by a machine of the future, may be passing that hope on to her grandchildren's grandchildren. Texts must be translated now, as they are collected, and the more mark-up that can be accomplished today, with the assistance of native speakers, the more useful that text will be for all time and for heritage learners as well as linguists.

The one real gain in the speed of analysis has been the training of native speakers as linguists. This has been successfully done in all parts of the world, and it results in much more progress for both sides of the equation than would be possible without that training (Hardman 1985). Other groups, such as the American Indian Language Development Institute and the Oklahoma Native Language Association, are also training native linguists. We can also hope that as our field begins its current revolution, the tools will be easier to teach (being based on more accurate and extensive data) and easier to apply in understandable and consistent ways.

#### 4. Conclusion

In short, we are poised to see a revolution caused by an unprecedented level of access to the raw materials of our discipline, using tools that have only recently become available. The very act of creating the tools to access these data is part of the revolution itself, as epitomized by the search for a unified linguistic on-

tology. The most important part of this revolution is that the elements of the linguistic periodic table (to continue the analogy to chemistry) will become clearer, and the standards of evidence for proposing or discarding these elements will become clearer as well. The minority languages of the world have a tremendous amount to offer linguistics in this regard. Again, let me emphasize that I have only been talking about one side of a two-way street in the paper. Linguistics has much to offer the native communities as well, and linguists are much more aware of the ethical ways of obtaining, analyzing and sharing their results than they have ever been. There have been many cases in which later generations have felt the loss of their language and want to waken it from its sleeping state, and they have benefited from the linguist's efforts at preserving it. We can be sure that this will happen repeatedly in the coming decades.

We have reached a stage in the study of language at which it is no longer ethical for a linguist not to consider working on an endangered language. This is not to say that all linguists must work on an endangered language: There are many valid reasons why a linguist might decide that a nonendangered language is the most appropriate one to study. But not to ask the question is insupportable. However, it is not just a moral issue: The vanguard of the revolution will be those who study endangered languages. It is always a heady decision to join a revolution, but the rewards of a successful one are great. And we are witnessing the beginning of a successful revolution in linguistics.

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# Index

## A

- Abkhaz 6, 11, 20  
Abney 175  
Aboriginal language 268, 272, 283, 290,  
292, 293, 295  
ABS – absolutive 24  
absolutive 296  
acoustic 157, 323  
acquisition 172, 176, 281, 293, 296, 306,  
314  
actor 249, 254, 255  
Addressee 249, 254  
Adegbija 84  
Adelaar, W. 117  
adjective 131, 228, 247, 249, 259  
adverb 247, 249  
adverbial 3, 12, 249, 250  
affix 3, 12, 35, 40, 220, 228, 247–250,  
252, 259, 260  
affixation 36  
affricate 133, 335  
Afrikaans 85  
Afro-American 310  
agent 296  
    nominal 36, 38  
Ager 69–72, 76, 79, 84  
agglutination 247, 248, 250, 252, 257,  
260  
agglutinative 248, 249, 259, 260  
Aho 163  
Aikhenvald 1, 3, 4, 7, 8, 11, 12, 18, 21,  
117  
Ainu 268, 273  
Aissen 329  
Akh 19  
Al-Kasimi 187  
Alaska 129  
Albesano 170  
Alexandersson 173  
Algonquian 329  
Allegrini 102  
Allen 163, 166, 167, 173, 209, 220, 283  
allomorph 228  
allomorphy 32, 126, 220, 226, 228  
allophone 149  
allophonic 150, 220  
allophony 126  
Almind 198  
Alston 209  
Altaic 110, 117  
alternation 94, 95, 147, 148, 229, 243,  
247, 259, 291  
Amazonia 9, 21, 118  
ambiguity 175, 212, 247, 249  
ambiguous 244, 258, 327  
Amerind 117  
Amerindian jargon 312  
Amery 268, 270–272, 281, 287, 294, 296  
Amharic 53  
Amith 335  
analogy 224, 256  
anaphora 166, 179, 335  
ancestral 279, 280, 282, 283, 288, 289,  
295  
Anchieta 146  
Andersen 130, 131, 290, 311  
Anderson 12, 33, 36–40, 42, 43, 211,  
218, 220, 223, 224, 261  
Andrzejewski 67  
animal language 316  
animate 291  
annotation 102, 103, 157, 158, 261  
antipassive 278, 291, 296  
antipassivization 277, 278, 285, 289,  
292, 297  
Aoki 12

- Apache 9, 10, 18  
APPL – applicative 24  
apposition 256  
Appurtenance 255  
APSG 165, 173  
Apter 81  
Arabic 124, 127, 253, 307, 309  
Arawá 4  
Arawak 1  
Archi 6, 12  
areal 110, 111, 117, 121, 244  
argot 163  
argument 32, 92, 94, 95, 102, 150, 243,  
244, 261  
Aristar 330  
Aristar Dry 330  
Armenian 6, 247  
Armstrong 315  
Armstrong-Warwick 177  
Aronoff 34, 43, 220  
articulation 133, 149, 244, 247, 250,  
254, 255, 261  
articulator 224, 336  
artificial intelligence 163, 164, 167  
aspect 3, 4, 22, 328, 330  
~, durative 330  
~, incomplete 25  
~, obligatory 1  
aspirated 127, 133  
assertive 208–210  
assimilation 126, 227  
ASSUM – assumed 24  
assumed 8, 9, 11, 25  
Athabaskan 122, 129  
Athapaskan languages 6, 330  
Atharva Veda 324  
ATN 165  
attitude 4, 78, 126, 147, 210  
Atwood 55  
Augmented Phrase Structure Grammar  
165  
Austin 159, 207–210, 212  
Australian 112  
language 12, 297, 331  
Austriac 117  
Austronesian 110, 122, 312, 329  
autosemantic 201, 250  
AUX – auxiliary 24, 252  
Aymara 8  
**B**  
Bach, E. 50, 54, 57, 58, 210, 211, 213,  
244  
Bahasa Malaysia 70, 85  
Bailey 307, 309, 314  
Baker 37, 38, 41, 282, 284  
Baker, Mark 146  
Baldauf 63, 70, 77–80, 82  
Balkans 110  
Baltic 110, 260  
Balto-Finnic 7  
Bamgbose, A. 62, 65, 67, 68, 76, 81, 84  
Bamileke Dschang 326  
Bandjalang 273, 292  
Bantu 122–123, 131  
Bar-Hillel 50  
Barnes 1, 24  
Baron 72  
Bartolini 101  
base 34–36, 38, 42, 167  
Bashir 5  
Basque 122, 247  
bastard language 311  
Bates 165  
Battistella 255  
Bauer 76  
Bavin 283  
Beard 43  
Becker 261  
Beeferman 177  
behabitive 208  
Bel 94  
Béland 226, 229  
Belgian-French 93  
Bell 268  
Bellwood 122  
Benedict, Ruth 146  
Berg, van den 6  
Bergenholtz 198–200  
Bergman 7, 24  
Bezuidenhout 213  
Bhat 11  
Bickerton 309, 316  
Bierman 170

- bilabial trill 149  
 bilingual 100, 171, 195, 199, 200, 338  
 bilingualism 70, 71, 82, 85  
 Bird 225, 229, 340  
 Blake 252  
 Blommaert 81, 84  
 Bloomfield 321, 331  
 Blum-Kulka 210  
 Blythe 283, 296  
 Boas 1, 51, 146–148, 155  
 Boersma 226  
 Booij, G. 228  
 Bopp, F. 324  
 Bora 16  
 Bora-Witoto 16  
 Borgman 24  
 borrowing 110, 111, 115, 120, 121, 126,  
     127, 246  
 Bossong 260  
 Bougainville 132  
 boundary 247, 248, 254  
 Brazilian Portuguese 3  
 break 152  
 Brent 177  
 Brenzinger, M. 332  
 Bresnan 165, 166, 174  
 Breton 42  
 Brill 177  
 British English 168  
 Bromberger 224  
 Brown Corpus 168  
 Buckley 171  
 Burkhanov 195  
 Burnaby 64–66  
 Busa 96, 168  
 Bybee 115  
 Byrd 167
- C**
- Cadbury 144  
 Caddoan 8, 10  
 Calabrese 226, 228, 229, 234, 238  
 Californian languages 273  
 Calvet 63, 64  
 Calzolari, N. 89, 91, 93, 96, 172  
 Campbell 7, 24, 111, 114–118, 120,  
     122–124, 290  
 Cantoni 268, 272  
 Carberry 167  
 Cardie 177  
 Carpenter 174  
 Carstairs-McCarthy 34  
 Carston 213  
 case 32, 111, 116, 131, 247, 329  
     ending 246, 252  
     suffix 116  
 Cassidy 313  
 Catalan 93  
 caucasian 5–7  
 Cause 254  
 CELP 332  
 Čermák 259, 260  
 CF grammar 176  
 Chafe 4  
 Chamorro 329  
 change 111–116, 119–121, 124–127,  
     131, 133, 148, 252, 314, 331  
 ~, lexical 127, 209  
 channel 151–153  
 Chappell 24  
 Charniak 177  
 cheek pop 334  
 Chelliah 6, 21  
 Cheng 58  
 Chibchan 11  
 Chierchia 58  
 child language 113, 151, 261, 316  
 Chinese 124, 248, 252, 253, 259  
 Chinook 308, 310  
 Chirikba 6, 11  
 Chomsky, N. 48–50, 53, 54, 57, 143,  
     147, 154, 163, 165, 180, 218, 223, 226  
 chunk 101, 175, 297, 298  
 Chutatip Yumitani 281  
 CL – classifier 24  
 Clark 210, 211, 213, 298  
 Classical Arabic 307  
 classification 49, 171, 245, 260  
 clause 3, 4, 11–14, 23, 33, 41, 209, 252,  
     291  
 ~, purposive 297  
 ~, relative 297  
 ~, subordinate 41, 290, 338  
 ~, verb 246

- Clayton 225  
Clemenceau 176  
Clements 222  
click 131, 133, 334, 335  
clitic 3, 25, 32, 39–42, 44  
CLWG 92  
Clyne 72  
CMPL – completive aspect 24  
COBUILD 168, 173  
code-switching 315  
codification 74, 80  
Cohen 167, 209  
coherence 141, 142, 144, 145, 149, 160,  
179, 335  
Coleman 229  
Collins 168  
collocation 171, 255  
Colombian Spanish 3, 7  
colonial 69, 73, 308, 310  
Committee on Endangered Languages  
332  
communicative isolation 128–129, 210,  
260  
comparative 109, 115–118, 228, 245,  
259, 324, 333  
    linguistics 109  
    method 117, 119  
competence 260  
complement 99  
complexity 96, 132, 133, 289, 296, 298  
compound 227, 248 252, 259  
computerization 321, 323, 330–332  
Comrie 21, 24, 248, 292, 296  
concept 55, 75, 95, 96, 243  
conjunction 34, 175, 246, 252, 255,  
276–277, 290  
connotation 23  
consonant 127, 132, 133, 148, 150, 152,  
153, 227, 230, 231, 233, 335, 337  
constative 208, 210  
constituent 12–14, 35, 39, 261  
constraint 42, 92, 153, 225–232,  
234–237  
CONT – continuous 24  
contact 121, 127, 132  
    language 127, 312  
    linguistics 315  
continuant 153, 227  
continuum 63, 90, 98, 100  
convergence 119, 121, 125  
conversion, lexical 247  
Cook 290  
Cooper 66, 71, 72, 76, 77, 79, 81–84  
coordination 255, 256, 291–294, 297,  
298  
Copeland 338  
copula 248  
Corbett, Greville 159  
Corbin 34  
Core 173  
corpus 89–93, 97–103, 167–170, 172,  
173, 177, 178, 181, 195, 261  
correlate 254  
Coseriu 252  
Cosser 283, 284, 296  
Coulthard 180  
Coupland 125  
CR-approach 226  
Craig 290  
creole 78, 305–316  
creolization 307–309, 311, 315, 316  
Cristea 178, 179  
Croft 244  
Crystal 72  
Csató 6  
Csató-Johanson 24  
culture 148, 149, 153, 154, 192  
Cummins 274, 275, 279, 286, 288, 289,  
295  
Cupeño 12  
Curnow 6  
Curry 254  
CUST – customary 24  
Cutting 213  
Czech 248, 252, 253, 258, 260, 323  
Czech National Corpus 261
- D**  
Daelemans 177  
Dagan 171, 177  
Dahl 166  
Dalton 297  
DAMSL 173  
dancing 152

- Danish 93, 131  
 Dardic 5  
 Das Gupta 66, 75, 82  
 Dasher 115  
 DAT 291  
 data 156–158, 198, 200  
 database 154, 156, 157, 167, 170, 180, 310, 312  
 Dative 257, 258, 296  
 Davis 212  
 DEC – declarative 24, 229, 237  
 DeCamp 306, 308, 313  
 Décsy 6  
 Dedenbach-Salazar Sáenz 22  
 DEF – definite 24  
 DeGraff 309–311, 313, 316  
 Deg Xinag 273  
 deictics 211  
 deixis 211  
 DeLancey 3, 6, 18  
 deletion 115, 227, 230, 231, 277  
 Demers 58  
 Demirdache 58  
 DEM:ANIM – demonstrative animate 25  
 Dench 12, 313  
 Dene-Caucasian 117  
 Dennett 120  
 denominal 249  
 dental 227, 335  
 Derbyshire 10  
 derivation 38, 40, 93, 217, 219–222, 246, 247  
 ~, lexical 247, 260  
 description 49, 52, 211, 257, 259, 328, 329, 331, 338  
 deverbal 247, 249  
 Dewey, John 146  
 Dezsö 253, 260  
 Diakonof 119  
 dialect 24, 71, 113, 114, 128–131, 156, 271, 282  
 ~, open 131  
 Dialogos 170  
 dialogue 163, 166, 167, 170, 173, 178, 179, 180  
 system 167, 170  
 Dickinson 8, 16  
 dictionary 93, 99, 167, 172, 177, 187, 189–191, 193–201  
 diffusion 111, 120, 121, 127, 314, 315  
 diminutive 249  
 diphthong 125  
 DIR.EV – direct evidential 25  
 directive 208–210  
 disambiguation 98, 177  
 discourse 20, 21, 154, 167, 170, 173, 178–180, 207, 211, 247  
 disjunction 229, 255  
 distribution 118, 123, 173, 336  
 divergence 99, 120, 121, 125  
 diversification 121–123, 127  
 Dixon, Bob 4–8, 11–13, 18, 19, 21, 24, 110, 111, 117, 119–121, 130, 283, 284, 290, 292, 296–298  
 Djabugay 275, 292  
 Dobrushina 5  
 Dolezal 201  
 Donabédian 6  
 Donnacha 78–80, 84  
 Donnellan 211  
 Dorian, Nancy 269, 271, 272, 280, 281, 283, 290, 296, 297  
 Drechsel 312  
 DRESS lexical set 112  
 Dressler 220, 249  
 Drosdowski 192  
 DRT model 166  
 du – dual 25  
 Dua 85  
 DUKE 170  
 Durie 111  
 Ďurovič 260  
 Dutch 93, 227, 228, 233, 310  
 Dutton 312  
 DVD 156, 157  
 Dwyner 332  
 Dyrbal 284, 291–293, 297, 298  
  
 E  
 Eagles 91–93, 96  
 Early 165  
 Ebata 296  
 Ebbitt 195

- Eckert 125, 126, 130  
ecosystem 74, 77, 78, 332  
e-creole 313  
Edwards 269  
Egerod 19  
elaboration 74, 75, 80, 126  
e-language 53, 54  
elision 126  
Ellis 282, 284–288, 293, 296–298  
Ellsworth 72  
embedded 247, 252, 297  
Embera 8, 11  
EMELD 157  
Emeneau 110  
EMP – emphatic 25  
empiricism 141, 142, 144, 145, 160  
enclitic 12  
encoding 91, 92, 94, 95  
endangered language 48, 148, 155, 159,  
268, 274, 294, 296, 321–323,  
330–334, 337–340  
ending 167, 246, 247, 249–252, 258, 259  
Enets 6  
Enga 7  
English 21, 72, 85, 93, 110, 115, 127,  
168, 171, 228, 247, 251–253, 259,  
260, 272, 276–278, 280–282,  
285–288, 293, 295, 297, 307, 310,  
323, 331, 335, 337, 338  
Epena Pedee 11  
epenthesis 227  
epistemic 3, 6, 10, 23  
equilibrium 119–121  
ERG 291  
ERG – ergative 25, 250, 276, 296  
ergativity 276, 283, 284, 286, 290, 292,  
296  
Eskimo 122, 247  
esoterogeny 126–128, 130  
Estonian 7  
ethnography 150, 153  
etymology 195  
Eurasia 119  
Europe 119, 123  
Eurotra 171  
EuroWordNet 96  
EV – evidential 25  
Evans 12, 123, 127  
Everett 142, 149–154  
evidential 3, 4, 10–25  
evidentiality 1–6, 8, 10–13, 17–24  
Ewen 223  
exc – exclusive 25  
exercitive 208, 209  
exoteric 127, 130  
exotero-geny 127, 128  
expositive 208, 209  
expression 50, 243, 244, 259, 276  
extinction 274, 331  
EYEW – eyewitness 4, 5, 8–11, 13, 19,  
21, 24, 25
- F
- f – feminine 25  
Fabb 34  
faculty of language 143  
farming-language 122, 123  
Faroese 131  
Farrar 329, 330, 340  
Farsi 323  
feasibility 66  
feature 33, 36, 48, 129, 153, 158, 174,  
197, 218, 222, 248, 310  
Federici 102  
Feinstein 230  
Fenk 248  
Fenk-Oczlon 248  
Ferguson 75, 313  
Fernandez-Vest 7  
Fesl 273, 281, 288  
Fettes 271  
FGD 254, 261  
field linguistics 157  
field research 141, 144, 147, 155, 159,  
160  
fieldwork 52, 141–150, 154, 155,  
159–161, 273, 314, 324  
file 167  
Fillmore 261  
Finck 246, 251  
Finnic language 131  
Finnish 93, 122, 168, 247  
Finno-Ugric language 260  
Firth 222

- Fisher-Jørgensen 218  
 Fishman 75, 79, 80, 82, 268, 271, 289,  
 295, 296  
 Floyd 8, 9, 21, 23  
 Fogelberg 122  
 Foley 292, 297  
 foreign language 200, 296  
 form 32–37, 42, 126, 247, 291  
 ~, inflected 93  
 Fortescue 12  
 fossilization 288  
 Foundation for Endangered Languages  
 332  
 Fowler 340  
 Fox 195  
 frame 94, 261  
 structure 198  
 framework 209  
 Frege 207–209, 211  
 French 93, 171, 247, 250, 252, 253, 259,  
 310, 311  
 frequency 97, 102, 115, 297  
 Fretheim 211  
 fricative 131, 227, 335  
 Friedman 165  
 Friedrich 119  
 FST technology 176  
 Fudge 222  
 FUG 174  
 Fuhrhop 34  
 function 151, 200, 247, 257  
 function word 246, 249, 250, 254, 259  
 Functional Unification Grammar 174  
 functor 255, 257, 258  
 fusion 127  
 FUT.NOM – future nominalization 25
- G**  
 Gabelentz, von der 245, 250–252, 260,  
 261  
 Gal 290  
 Garrett 120  
 Gazdar 166, 174, 209  
 Geckeler 253, 260  
 Geeraerts, D. 189–191, 195  
 Geis 211  
 Geisler 261  
 GEN – genitive 25  
 gender 12, 32, 66, 156, 247, 250  
 Genelex 93  
 Genelex Consortium 93  
 generalization 220, 227, 229, 233, 338  
 generative description 243, 254  
 grammar 42, 50, 147, 164  
 lexicon 95, 96  
 linguistics 181, 314  
 phonology 217, 218, 223, 224, 226  
 semantics 209  
 theory 31  
 genre 97  
 geography 119  
 Georgian 127, 247, 283  
 GER – gerund 25  
 German 93, 250  
 Germanic 110, 131  
 gerund 309  
 Gesellschaft für bedrohte Sprachen 332  
 Gibbs 210, 213  
 Giger 248, 249, 259, 260  
 Giles 125  
 Gill 70  
 Girard 251  
 Girramay 275, 297  
 Givón 308, 316  
 GL 96  
 GL-Simple 96  
 glottal stop 151, 152  
 glottalized 127, 133  
 Godoberi 5  
 GOLD 329  
 Goldrick 233  
 Goldsmith 225  
 Golla 24, 126, 129, 130  
 Gomez 5, 24  
 Gomez-Imbert 21  
 Gordon 111–113, 115, 124  
 Gorman 68, 75  
 Gouws, R. 190, 195, 198, 200  
 GPSG 166, 174  
 grammar 33, 42, 43, 50, 51, 57, 89, 92,  
 144, 147, 148, 153, 155, 164–166,  
 174, 176, 177, 180, 192, 193, 208,  
 218, 224, 229, 231–233, 235, 246,

- 248, 250, 252, 254, 256, 259, 276,  
278, 323, 328, 335, 339
- ~, case 261
- ~, categorial 57, 166
- ~, CF 176
- ~, functional unification 174
- ~, generative 42, 50, 147, 164, 193
- ~, lexical functional 174
- ~, OT 235
- ~, phrase structure 174
- ~, probabilistic context free 177
- ~, transformational 24, 164, 165, 193,  
314
- ~, variation 314
- ~, Warrungu 286
- grammateme 254–258
- grammatical 3, 20, 94, 114–117, 131,  
165, 246–248, 261, 330
- category 3, 12, 20, 23, 96, 338
- feature 330
- function 258
- gender 32
- morph 247
- morphemics 247
- pattern 98, 259
- property 192
- structure 24, 43
- theory 164, 166
- value 243, 246–248, 253, 258
- word 35
- grammaticality 335
- grammaticalization 7, 23, 109, 116, 261
- Great Basin 119
- Greek 93
- Greenberg 50, 117, 252
- Grewendorf 211
- Greymorning 272
- Grice 208, 210, 212, 213
- Grinevald 333
- Grinevold, Colette 159
- Grosz 165, 179
- Gruzdeva 11
- Guarani 146
- Guentchéva 4, 7
- Gugu-Badhun 273, 274
- Gugu-Yalanji 275
- Güldemann 117
- Gundel 211
- Gurindji 297
- Gusev 5, 8, 10, 11, 21
- guttural 149
- H**
- Haarmann 80, 82
- Haas 146
- Haensch 192
- Haiti 311
- Hajičová, Eva 244, 254, 256, 259, 261
- Hale 50, 52, 159, 274, 279
- Hall 308–310, 313
- Hall-Partee 166
- Halle 218, 223, 224, 226
- Hamel 84
- Hancock 306
- Hanks 192
- Hano Tewa 121
- Hansson 19
- Hardman 8, 23
- Hare 6
- Harnish 207, 210, 211
- Harrington 124
- Harris 49, 50, 57, 115, 116, 147, 222, 234
- Hartman 268
- Hartmann 191, 196
- Haruno 177
- Harwood 50
- Haspelmath 7
- Hatch 287
- Haugen 73–75, 80, 84
- Hausa 124
- Hausmann 196, 200
- Hawaiian 132, 247
- Hayes 166, 224, 231, 234
- HCL 154
- hearsay 8, 11, 13, 15, 17, 18, 25
- Hebrew 147, 270, 271
- Heine 110
- Hempel 245
- Henderson 268
- Herbert 296
- Herold 113, 114
- Herriman 64–66
- Herzog's Principle 113
- Hess 332

- heterolingual 312, 313  
 Hewitt 6  
 Hickey 113  
 Hidden Markov Model 169, 177  
 Hill 12, 121, 126, 128, 130, 290  
 Himalayas 118  
 Himmelmann 267  
 Hinton 268, 271, 273, 281, 321  
 Hipp 167  
 HISTLING list 109  
 historic present 21  
 historical dictionary 189, 190, 196, 198  
     linguistics 109, 110, 116, 181, 284  
     phonology 221  
 Hixkaryana 10  
 HLT 89, 94, 101, 103  
 HMM 177  
 Hock 117  
 Hockett 35, 50, 226  
 Holdcroft 210  
 Holenstein 248  
 Holm 306, 310, 315  
 homonymy 114  
 Hooper 220, 225  
 Hopi 121  
 Hoskovec 260  
 Houben 307  
 Hovelacque 251  
 HPSG 174  
 Hudson 47, 220, 268, 272, 281, 288  
 Hulbert 201  
 Hulst, H. van der 220, 222, 224, 225,  
     232–234, 237  
 hum speech 151  
 Humboldt 251  
 Hungarian 247  
 Huss 269, 272, 282  
 hybridization 313  
 Hymes 130, 151, 305, 306, 311, 313  
 hyperonymy 172  
 hyponymy 95, 96
- I**
- Icelandic 34, 42  
 iconicity 249  
 I-creole 313  
 identity 272, 280, 283  
 ideology 65  
 idiom 94, 98, 126, 127, 176  
 idiomatics 255  
 idiosyncrasy 132  
 IFID 208  
 IJAL 146  
 i-language 53, 54, 218  
 ILI 332  
 illocutionary 208–210  
 Imedadze 283  
 imperative 99, 150, 209, 212, 254, 256,  
     296  
     class 210  
     clause 11  
     marker 11  
 imperfective 256  
 Inaida 296  
 inanimate 291  
 INC – incomplete aspect 25  
 Inca 124, 127, 133  
 Indian language 21, 23, 307, 321  
 indigenous language 68  
 Indigenous Languages Institute 332  
 INDIR.COP – indirective copula 25  
 INDIR.PAST – indirective past 25  
 Indo-European 42, 110, 122, 123, 243,  
     250, 251, 260, 310  
 ~, Old 248  
 ~, Proto 251  
 Indonesian 132  
 induction 100  
 Ineichen 259, 261  
 inference 6, 8, 10, 14–18, 115, 166, 167,  
     169, 173, 207, 210  
 inferential 2, 3, 10, 14, 15, 17, 18, 24, 25,  
     173, 210  
 inferred 8–11, 14, 18, 21, 22, 24  
 infinitive 247, 252, 309  
 infix 33, 34, 42  
 inflection 32, 40, 248–250, 252, 258, 260  
 inflectional 40, 247  
     alternation 259  
     concept 55  
     conversion 247  
     ending 250, 251  
     language 249, 259  
     morphemics 249

morphology 127  
paradigm 256  
information retrieval 101, 171  
information technology 180  
INFR – inferred 25  
input 170, 219, 221, 223, 230, 231, 233,  
236, 287, 288, 311, 338  
insertion 230  
Institute for the Preservation of the  
Original Languages of the Americas 332  
InstrumentalCase 330  
Intangible Cultural Heritage Unit 332  
integrative 269  
interaction 97, 126, 147, 166, 313, 328  
interference 209, 311  
interlanguage 171, 312  
interlocutor 114, 126, 312  
International Journal of American  
internet 323–325, 329, 330, 339  
interrogative 11, 209, 212, 259  
intimacy 151  
intonation 157, 222, 259, 261, 327, 331  
INTR – intransitive 25  
intransitive 296  
INTRATERM.ASP – intraterminal aspect  
25  
introflexion 248, 258, 260  
introspection 98, 100  
Inuktitut 283, 338  
inversion 174  
IP – immediate past 25  
IPA 336  
IPOLA 332  
Iranian 4  
Irish 93, 269, 271, 272, 294  
ISA dimension 97  
ISI 173  
ISLE 91, 92, 103  
isolation 132, 133, 250  
Itagaki 283, 286, 289, 296  
Italian 93  
iterative 231, 259

## J

Jackendoff 37, 57  
Jacobsen 4, 8, 57  
Jacobson 12

Jacobson 24, 130, 251, 255  
James 141, 144, 145, 161, 191, 196  
Janhunen 116, 268  
Japanese 12, 171, 253, 260, 280–283  
Jaqi 8  
Jarawara 4, 5, 11, 13, 18, 19, 21  
jargon 306, 312  
Jelinek, Eloise 58, 177  
Jernudd 66, 75, 80, 82  
Jirrbal 275, 297  
Jochelson 21  
Johanson 4, 6, 7  
Jones 85, 269, 271, 297  
Joseph 117  
Joshi 165, 179  
Jourdan 309  
Jurafsky 115  
juxtaposition 290

## K

Kager 232  
Kalasha 5  
Kalkatungu 292  
Kalmar 338  
Kam 85  
Kamaiurá 15, 24  
Kammerer 195, 198  
Kamp 166, 178  
Kaplan 63, 70, 77–80, 82, 165, 166, 174,  
211  
Karttunen 176  
Kasami 165  
Katz 209  
Kaufman 110, 307, 315  
Kaurna 294  
Kay 171, 174  
Kaye 234  
Keenan 50, 211  
Kepser 174  
Keresan 122  
key-word 171, 172, 174  
Kham 7  
Khanty 6  
Khoisan 117, 131, 335  
Khowar 5  
Ki-Nubi 310  
Kibrik 6, 12

- Kiefer, F. 340  
 Kim 261  
 King, Jeanette 268, 273  
 King, Kendall 268, 272  
 Kiparsky 38, 219–221, 223, 233  
 Kisseberth 227  
 KIT class 112  
 Kituba 310  
 Kloss 66, 75  
 Knight 177  
 Kogura 296  
 Koisan 131  
 Kojima 296, 298  
 Koreguaje 10  
 Koskenniemi 168, 176  
 Koutsoudas 219, 225  
 Krauss 279, 333  
 Krauss, Michael 159  
 Kretz 253, 259, 260  
 Krifka 58  
 Kroeber 116  
 Kromann 200  
 Kronning 3  
 Kroskritis 121  
 Kubo 209  
 Künnap 6  
 Kuno 1966 165  
 Kurohashi 178  
 Kuryłowicz 249  
 Kurzová 260  
 Kuznetsova 6, 11  
 Kwasny 165
- L**
- L1 280, 282–285, 290, 294, 298  
   acquisition 284, 290, 294, 298, 311  
   learning 331  
 L2 280, 282–285, 288, 290, 294, 298  
   acquisition 282, 284–287, 290, 293,  
     294, 297, 311, 312  
   learner 282, 311  
   learning 280–282  
 Labov, W. 111–114, 124–126  
 LaCharité 226, 229  
 Ladakhi 11  
 Ladefoged 154, 340  
 LAGB 159  
 Lakoff 57, 225  
 lambda calculus 244  
 Landau, S. 194  
 Lang 7  
 Langendoen 329  
 language 53, 62, 68, 71, 118, 122, 127,  
   129, 130, 132, 133, 168, 218,  
   244, 255–258, 259, 288, 296,  
   312, 314, 328, 331, 332  
   acquisition 48, 71, 79, 113, 151,  
     221, 234, 256, 281–284, 290,  
     293, 295, 297, 311, 334  
   adoption 273, 281  
   allocation 62, 75, 78  
   amalgamation 77, 78  
   aptitude 286  
   attitude 79, 271, 272, 286  
   behavior 61, 76, 78, 82  
   boundary 123, 124  
   change 54, 77, 78, 116, 120, 124,  
     126, 315, 338  
   community 129, 337  
   contact 77, 78, 109–111, 120, 121,  
     131, 132, 305, 306, 315  
   cultivation 74, 82  
   death 77, 78, 269, 284, 290, 292,  
     293, 298, 322  
   disappearance 331  
   distribution 119, 124  
   endangerment 159, 267, 274, 279,  
     294  
   extinction 47  
   faculty 143, 258, 334, 336  
   family 110, 117–119, 122, 129, 130,  
     133, 328, 329, 331  
   learning 311, 316  
   loss 48, 284, 322  
   maintenance 85, 267–269, 271, 282  
   native 24, 142, 147, 200, 285  
   planning 61–65, 67, 69, 73–86  
   policy 63–73, 79, 85, 86, 306  
   reinforcement 74, 78, 79, 84  
   resource 89, 93, 104  
   revitalization 267–274, 279, 282,  
     287, 289, 294–297  
   revival 77, 78, 267–271, 274, 280,  
     282, 283, 286, 294

- revival movement 274
- right 62, 69, 85
- shift 77, 78, 85, 296
- spread 77–79, 85, 118, 123
- status 64, 78
- structure 104, 132, 252, 259
- system 248, 254, 261
- teaching 63, 79
- technology 164
- treatment 63, 64, 74, 76, 82, 83
- type 243–246, 248, 249, 251, 253, 258, 260
- typology 243, 245
- universals 245
- ~, ancestral 279–283, 288–290, 295, 309
- ~, animal 316
- ~, colonial 73
- ~, distant 129
- ~, emblematic 126, 127
- ~, esoteric 130
- ~, exoteric 130
- ~, external 218
- ~, extinct 268
- ~, faculty of 143
- ~, first 62, 71, 337
- ~, heritage 296
- ~, human 89, 90, 165, 328, 330
- ~, indigenous 68
- ~, individual 133, 328
- ~, inflectional 249, 259
- ~, national 62, 65, 68, 69, 77
- ~, native 24, 142, 147, 200, 285
- ~, natural 50, 74, 94, 163, 165–167, 170, 171, 173, 176, 178, 180, 208, 243, 248, 259
- ~, real 169, 248
- ~, regional 69
- ~, second 282, 287, 296, 338
- ~, source 200
- ~, target 171, 282, 285, 308, 310, 311
- ~, traditional 270, 272, 288, 289
- ~, type of 243–246, 254
- Lapointe 230
- LaPolla 8, 10, 14, 22, 25
- Laporte 176
- Lascarides 178
- Latin 124, 248, 252
  - conjugation 260
- ~, vulgar 307
- Lazard 21
- LE 91
- learnability 226, 231
- LeBlanc 120
- Leech 91
- Lefebvre 309
- Lehiste 7, 24
- Lehmann, Christian 259, 261, 267, 296, 323, 340
- lemmatization 97
- Lenci 94
- length 152
- leniting change 115
- Le Page 125, 305
- Lery, de 146
- Leška 252
- Levin 224
- Levinson 211, 213
- Levy 340
- Lewis 50, 329
- lexeme 260
- lexical 98
  - category 32, 58
  - change 127, 209
  - component 225, 255
  - conversion 247
  - derivation 247, 260
  - diffusion 111, 114, 115
  - entry 92, 228, 229, 261
  - form 228, 229
  - information 101, 102, 104
  - item 95, 96, 167, 172, 177, 243, 255
  - meaning 192, 193, 246, 255
  - model 90, 104
  - morph 246–248
  - phonology 33, 38, 220, 221
  - representation 96, 102, 103
  - resource 91, 93, 101–104
  - semantics 92, 96, 192, 195, 201
  - system 167, 192, 233
  - unit 192, 246, 248, 259
  - word 250
- Lexical-Functional Grammar 174
- Lexicalist Hypothesis 33

- lexicographic 191, 195, 200  
 definition 195  
 gloss 95  
 practice 187, 188, 190, 193–197,  
 199–201  
 research 200, 201  
 theory 193–195, 200
- lexicography 90, 97, 98, 104, 179,  
 187–199, 201
- lexicology 104, 187–192, 196, 197, 201
- lexicon 31, 58, 89–97, 99–105, 114, 115,  
 118, 126, 147, 189, 192, 220, 228,  
 248, 250, 254, 256, 259, 328
- ~, computational 89–101, 104, 168
- ~, mental 337
- ~, open 92
- lexifier 311
- Lezgian 7
- Lezxic 7
- LF 154
- LFG 166, 174
- LGP 199
- Lieberman 334
- Lightfoot 37, 115, 116, 314, 315
- Lightner 219
- lineage 118
- linearization 257
- Linell 221
- Lingala 310
- lingua franca 24, 118, 123, 127
- Linguist List 330
- linguistic analysis 171, 193, 324, 326,  
 327  
 area 110, 111, 119, 121  
 behaviour 95, 125, 130  
 change 116, 120, 125, 130, 131,  
 193, 314, 315, 333  
 competence 312  
 Data Consortium 323  
 description 51, 52  
 diffusion 121, 126  
 diversity 53, 118, 122, 279  
 ecosystem 77, 78  
 feature 169, 323  
 knowledge 99, 100, 154, 176, 177  
 Society of America 110, 332
- structure 130, 132, 169, 211, 251,  
 261, 324
- system 113, 129, 130
- theory 48, 50, 51, 144, 160, 190,  
 191, 193, 195, 328
- typology 251
- variety 307, 308
- linguistics 47, 49, 50, 51, 110, 111, 117,  
 119, 120, 146, 169, 174, 180, 181,  
 189–198, 201, 209, 225, 243, 250,  
 259, 321–323, 330, 331, 338–340
- ~, applied 190, 195, 196
- ~, cognitive 181, 190, 195
- ~, computational 163, 164, 167–172,  
 177–181, 261
- ~, contact 315
- ~, corpus 169
- ~, descriptive 50, 51
- ~, documentary 48
- ~, ecological 48
- ~, generative 181, 314, 315
- ~, historical 109, 110, 116, 189, 284
- ~, mainstream 305, 315
- ~, structural 147, 181, 195
- Linguistics Association 159
- literacy 62, 78, 337, 338
- Litmann 167
- Littlebear 269, 280
- Litvinov 259
- Livonian 7
- loan word 229, 247
- LOC – locative 25
- locative 254, 255
- locutionary act 208, 212
- Long Head Movement 40
- Loritz 316
- Lotko 260
- Lowe 9, 13
- Lowenstamm 234
- LR 89–91, 101, 103
- LRE 91, 171, 172
- LSP 199
- Lyons 57
- M
- m – masculine 25
- MacDonald 7

- machine learning 175, 177  
 machine translation 89, 101, 168, 171,  
 177, 178  
 Macías 72  
 Maddieson 132, 335  
 Maguire 269, 294  
 Maher 268, 273  
 Maidu 8, 11  
 Makah 12, 57  
 Malay 132  
 Malkiel 192  
 Mallinson 252  
 Malone 1, 24  
 Mamu 275, 297  
 Mandala 171  
 Mann 178  
 MANNER 212  
 Manner 254  
 Mansi 6  
 Mantell 144  
 Maori 132, 268, 273  
 mapping 93, 165, 166  
 MapTask 173  
 Maratsos 165  
 Marcu 173, 178  
 mark-up language 168, 339  
 markedness 218, 223, 224, 235, 255,  
 257, 261, 285, 293, 297, 298  
     constraint 230, 231, 236  
     theory 223  
 marker 1, 3, 6, 11, 13, 16, 269  
 Maroldt 307  
 Martha's Vineyard 125  
 Martinet 261  
 masculine 249  
 Maslova 11  
 Mathesius 251  
 Matisoff 117  
 Matsuura 286, 296, 298  
 Matthewson 58  
 Maya 297  
 Mayan 118, 122, 329  
 Mayerthaler 249  
 Mbugu 308  
 McArthur 187, 198  
 McCarthy 232, 233  
 McCawley 57  
 McClelland 177  
 McConvell 123, 124, 127, 268, 272, 282,  
 288  
 McCreary 201  
 McDonough 340  
 McGregor 296  
 McKay 268, 281  
 McLendon 8, 17, 21  
 McMahan 117  
 McNaught 91  
 McWhorter 308  
 meaning 3, 10, 12, 14, 18, 33, 94, 96, 98,  
 181, 195, 209, 210, 212, 243, 244,  
 249–258, 298  
 ~, compositional 33  
 ~, lexical 192, 193, 246, 255  
 ~, literal 167  
 ~, mirative 18  
 Meggle 211  
 Meithei 6, 21  
 mental lexicon 337  
 merger 112–114  
 Mesoamerica 118, 119  
 Mesthrie 271  
 metadata 156–158  
 metalexicographer 199, 201  
 metalexicography 188, 190, 195–198,  
 201  
 metathesis 35  
 methodology 157, 163, 198  
 metrical theory 224  
 Migliazza 24  
 migration 122, 128, 129  
 MILE 91–93, 103, 104  
 Miller 8, 127, 172  
 Milroy 111, 126, 307  
 minority language 62, 71, 77, 282, 340  
 MIR – mirative 3, 10, 18, 25  
 Mirror Principle 37  
 Misteli 251  
 MIT 223  
 Mitchell 284, 286, 288, 298  
 Mithun 289, 290  
 Mixe-Zoquean 122  
 Miyaoka 332, 333  
 MLM 92  
 modal verb 3

- modality 3, 11, 149, 151  
 modification 247, 253  
 modularity 180  
 Mohanan 218  
 Mohawk 41  
 Moise 213  
 Monachini 91  
 Monbu-kagaku-sho 332  
 Mongolian 127  
 monogenesis 306  
 monogenetic theory 310  
 monolingual 195  
 monolingualism 130  
 monomorphemic 228  
 monosyllabic 246, 247, 252  
 Monserrat 5, 11  
 Montague 50, 166  
 Montemagni 93  
 Montoya 146  
 mood 6, 11, 209, 210, 247  
 Mooney 177  
 Morgan 210  
 morph 35, 246, 247, 249, 250, 253  
 morpheme 1, 10, 11, 18, 31, 34–38, 176,  
     209, 227, 228, 243, 246, 249, 254,  
     312, 335  
 ~, bound 312, 335  
 morpheme-based 35, 36, 43  
 morphemic 261  
     case 247  
     level 257  
     mean 244  
     representation 254, 256  
     surface 253, 254  
 morphemics 247–249, 251, 253–256,  
     258, 260  
 morphological 33–37, 55, 93, 117, 167,  
     195, 220, 228, 254, 256, 296, 339  
     affix 220  
     analysis 168, 176, 180, 228  
     category 55, 249, 257, 309, 329, 336  
     complexity 34, 127, 291–294  
     constituent 33, 34  
     ergativity 283, 284, 292, 296  
     irregularity 127, 131  
     marker 37, 38, 40, 42  
     structure 33–36, 272, 289  
     theory 219  
     unit 93  
 morphology 31–43, 93, 127, 167, 176,  
     195, 220, 228  
 ~, derivational 40  
 ~, inflectional 127  
 ~, natural 249  
 morphophonological 220, 225, 234  
 morphophonology 220  
 morphosyntactic 12, 36, 93, 115, 121  
 morphosyntax 91, 93  
 morphotactics 32, 39  
 Morris 207  
 Mortensen 8  
 motivation 286  
 Motu 310, 312  
 Mufwene 310  
 Mugdan 193  
 Müller 251  
 multilingual 81, 90–92, 94, 97, 100, 172  
 multilingualism 82, 123, 129, 171  
 multiword 102  
 Munda 122  
 Muran 132  
 Muysken, Pieter 309, 314  
 MWE 98  
 Mýky 5, 11  
 Myles 284, 286, 288, 298
- N**  
 Nagao 178  
 Nagy 271  
 Nahuatl 124, 335  
 Nahuatl dictionary 335  
 Nakagawa 282, 294, 296  
 Nambiquara 9, 13  
 narrative 10, 17, 20–23  
 Nathan 268, 274, 278, 281, 282, 289, 296  
 native speaker 55, 89, 90, 98, 99, 149,  
     157, 225, 312, 322, 325–327, 336,  
     337, 339  
 naturalness 219, 249, 257, 258, 261  
 Nau 249  
 Neale 211  
 near synonym 126  
 Nedjalkov 259  
 NEG – negative 25

- negation 4, 11, 12, 19, 23  
 Nenets 6  
 Neo-Grammarians 111, 117, 324  
 neologism 71  
 Nespor 222  
 Nettle 120, 123, 124, 130–132  
 neural network 169, 177  
     network theory 334  
 Neustupný 63, 75, 76, 80, 260  
 New Guinea 118  
 New Zealand English 111, 112, 115  
 Newman 155  
 nf – nonfeminine 25  
 Ng 177  
 Nganasan 5, 8, 10, 11, 21  
 Ngarindjeri language 288  
 Ngarluma pidgin 313  
 Ngomba 323  
 Nichol 111  
 Nichols 4, 117–119, 259  
 Nicholson 268, 273  
 Nicole 213  
 Nikolaeva 6  
 Nilo-Saharan 117  
 Nivkh 11  
 NLP 94, 97  
 node label 174, 255  
 Noll 219, 225  
 NOM – nominative 25  
 Nomaci 296  
 nominal 13, 18, 33, 248  
 nominative 247  
 Nomoto 296  
 Nomura 296  
 non-Austronesian language 312  
 non-derivational 225  
 non-eyewitness term 3, 5, 6, 13, 15,  
     18–21, 24  
 non-Indo-European 306, 310–315  
 NONEYEW – non-eyewitness 25  
 nonliteral 208–210  
 NONVIS – nonvisual 25  
 nonvisual 8–11, 17, 21, 22, 25  
     sensory 17  
 Nootka 57, 148  
 Nordlinger 13  
 norm 74, 75, 125, 132  
 Norwegian 93, 131  
 Noss 65  
 Nostratic 110, 117  
 noun 13, 71, 165, 247, 249, 252, 259,  
     291  
     incorporation 39, 40, 146  
     phrase 12, 13  
 NP 291  
 nsg – nonsingular 25  
 Nuckolls 21  
 null hypothesis 58  
 number 1, 33, 247, 250  
 Nurse 110  
 Nuuchahnulth 57
- O**
- object 244, 248, 249, 296  
 objective 249, 254, 255  
 obstruent 133, 227, 233  
 Ogden 229  
 OLAC 157  
 Old Greek 259  
 Old Indo-European 248  
 Olsen 297  
 Olshtain 285, 290, 296  
 Olson 297  
 ONOM – onomatopoeic 25  
 ontology 92, 95, 97, 104, 324, 327–331,  
     338, 340  
 ~, linguistic 327, 329, 333  
 opacity 220, 232, 233  
 open class 330  
 Oppenheim 245  
 Optimality Theory 38, 42, 225, 230  
 ordering, extrinsic 220, 221, 223, 225,  
     232, 233  
 orthographic 89  
     form 255  
     reform 83  
     transcription 156  
 orthography 49, 63, 74, 83, 326, 337  
 Ostler 332, 340  
 OT 225, 226, 228, 230–236  
 OTA 157  
 overgeneralization 333  
 Owen 144

- Oxford Text Archive 157  
 Ozolins 72
- P**
- p – person 25  
 Pacific Northwest 118  
 Pacific Rim 332  
 paleontology 316  
 Palmer 273, 274, 276, 279, 280, 287,  
 289, 295, 296  
 Pama-Nyungan 119, 122, 123, 127  
 Panevová 254  
 Panoan 122  
 Papago 128  
 Papuan 7, 122, 132  
 paradigm 12, 163, 167, 180, 256, 314  
 paradigmatic 11, 12, 100, 260  
   inflection 260  
   opposition 12  
 Paradis 225, 226, 228, 229  
 Parks 8, 10  
 Parole 91–95, 97  
 parser 164, 170, 175, 178  
 parsing 101, 102, 163–165, 167,  
 173–175, 178, 179  
 Parsons 50  
 PART – particle 25  
 part of speech 93, 97, 173, 249, 261  
 Partee 48, 244  
 participle 7, 247  
 particle 3, 19, 24, 25  
 passive 174, 200  
 passivization 276–278, 297  
 past 11–13, 19, 25, 227, 296  
   suffix 227  
   tense 11–13, 227  
 ~, remote 13, 19, 25  
 Pastazo Quechua 21  
 patient 291, 296  
 Pawnee 8, 10  
 Payne 24  
 PCFGs 177  
 PCID 208  
 PDP 223, 224  
 Peckham 167, 170  
 perception 5, 226, 231  
 Pereira 176  
 performative 208–211  
 permissive 209  
 Perrault 167, 209  
 Perrot 7  
 Perry 211, 213  
 Peters 57  
 PF 154  
 pharynx 149  
 phenogrammatcs 254  
 Phillipson 70  
 philology 51, 52  
 phoneme 51, 149, 150, 153, 246–248,  
 323, 325, 326, 331, 334  
   contrast 111, 114  
   inventory 132  
   reduction 247  
   representation 256  
   substitution 234  
 phonemics 149, 248, 254  
 phonetic 51, 89, 111–114, 131, 150, 154,  
 156, 158, 167, 176, 225, 236,  
 256, 338  
   base 236  
   change 131  
   contrast 114  
   form 32, 39, 147  
   transcription 156  
   variability 338  
 phonetics 113, 149, 154, 254, 326, 327  
 phonological 33, 34, 51, 110, 112, 126,  
 127, 132, 150, 152, 154, 168,  
 218, 220, 221, 224, 225, 235, 236  
   alternation 220, 234  
   change 116, 209  
   complexity 127, 132  
   constraint 228, 229  
   description 331  
   modification 38  
   representation 222–224, 229, 235,  
   236  
   rule 218, 229  
   structure 34, 149  
   theory 149, 217, 218, 235  
 phonology 33, 34, 38, 42, 149, 151, 153,  
 218, 220, 221, 223–225, 229, 233,  
 234, 236, 237, 252, 326, 327  
 ~, declarative 229, 237

- ~, dependency 223, 224, 237  
 ~, generative 220, 221  
 ~, laboratory 236  
 ~, lexical 33, 38, 220, 221  
 ~, mainstream 223  
 ~, segmental 151, 153  
 phonotactics 150  
 phrase 32, 35, 40, 171, 176, 178, 222  
 Phrase Structure Grammar 174  
 pidgin 78, 123, 287, 305–316  
 pidginization 307–309, 311, 313, 315, 316  
 Pike, Kenneth 145  
 Pinkal 166  
 Pirahã 132, 149–153  
 Pirrelli 93  
 pl – plural 25  
 Plátek 256  
 Plank 248, 251, 261  
 Platt 190  
 Plungian 250  
 plural 254, 256  
 PNG format 157  
 Polanyi 178  
 Polgárdi 237  
 Pollard 174  
 polymorphemic 228  
 polysemy 98  
 polysyllabic 228  
 polysynthesis 248, 250, 252, 258  
 Pomo 8, 10, 17  
 Pomoan 8  
 Ponelis, F. 193  
 Popela 249, 260  
 Porter 78  
 portmanteau 35  
 Portuguese 93, 310  
 POS 167, 172, 173  
 Poser 117, 118, 123  
 Postal 209, 219  
 postposition 116  
 Pott 251  
 Powell 272, 287, 288  
 PRAAT 157  
 pragmatic 89, 114, 167, 173, 178, 211, 269, 328  
 pragmatics 115, 166, 179, 192, 207, 209, 213  
 pragmatism 141, 143, 144  
 Prague Linguistic Circle 245  
     School 187, 255  
     typology 246, 249, 251, 252, 258, 261  
 Prahalad 84  
 predicate 13, 57, 92, 95, 102, 243, 244, 248, 261  
 predication 19, 20, 208  
 prefix 167, 208, 209, 227, 257  
 preposition 175, 247, 249, 250, 252  
 presence 248  
 present 296  
 Preterite 254, 256  
 PREV.DIR.EV – previous direct evidence 25  
 primitive 235, 236  
 Prince 225, 232  
 PROG – progressive 25  
 pronominalization 94  
 pronoun 39, 66, 71, 165, 249, 260, 291  
 pronunciation 115, 124, 195  
 proper name 211  
 proper noun 97  
 propositional 167, 208, 209, 212, 255, 258  
 prosodic 33, 152, 158, 222, 224  
 prosody 151, 154, 259, 261  
 prototype theory 255  
 Prunet 225, 228  
 psycholinguistic 165, 210  
 Pueblo 129  
 punctuation 120, 121  
 purism 71, 288  
 purist 272, 288  
 purposive 11, 208, 290–292, 296–298  
 Pustejovsky 96  
 Pye 297
- Q**  
 Qiang 8, 10, 14, 21, 22  
 qualia 95, 96  
     structure 96  
 quantity-insensitive 231, 232  
 quantity-sensitive 231, 232

- Quechua 8, 9, 21–23, 124, 127, 133  
 query 171, 175  
 Quichua 268  
 QuickTime 157  
 Quileute 287  
 QUOT – quotative 25
- R**
- r-language 54  
 Rakhilina 3  
 Ramat 251, 253  
 Ramirez 15, 24  
 Rankin 117  
 Rannut 81  
 Ratnaparkhi 177  
 Raw 171  
 RDF 92, 104, 105  
 real-time 111  
 reanalysis 115, 116  
 REC.P – recent past 25  
 Recanati 208, 211, 213  
 reconstruction 110, 116  
 Reddy 166  
 reduction 115, 128  
 redundancy 12, 100  
 reference 208, 211, 261  
 regionalism 330  
 regularity 173, 232  
 Reinecke 310  
 Renfrew 122  
 REP – reported 25  
 repair 226–231  
 reportative 14, 21, 22  
 representation 165, 217, 218, 222  
 requestive 209  
 resource 95, 103, 172  
 resultative 259  
 retroflexed 149  
 Retuarā 8  
 Reuse, de 9, 18  
 Reversing Language Shift 268  
 revitalization 269, 271, 294, 331  
 revival 69, 269, 270, 274  
 Revzin 249  
 Reyhner 268  
 Reyle 166, 178  
 Ricento 72
- Richards 190  
 Rig Veda 324  
 Rigsby 296  
 Ringe 117  
 Ringen 223, 225  
 Rischel 222  
 Ritchie 57  
 Ritter 225, 232–234, 238  
 Rizzi 314  
 RLS 268  
 Robins 274  
 Roche 176  
 Romaine 130–132  
 Romance language 260, 261, 307, 308  
 Rombandeeva 6  
 root 246, 257  
 Ross 111, 126–128, 130, 209  
 Rotokas 132  
 Rouchdy 269  
 RP – remote past 25  
 RST 178  
 RSTool 173  
 Rubba 260  
 Rubin 75, 80  
 Ruimy 93  
 rule ordering 217, 218, 220, 221, 233  
 rule-based 177, 226, 227  
 Rumelhart 177  
 Russian 171, 248, 252, 260
- S**
- S – intransitive subject 25  
 Sabir 306  
 Sack 180  
 Sadock 209, 210  
 Sag 174  
 Sagey 224  
 Sahaptain 129  
 Sakamoto 296, 298  
 Sakiyama 333  
 Salish 58  
 Salmon 315  
 Salmons 117  
 Salton 171  
 Salzman 121  
 Samarin 310  
 Samoyedic 5, 6, 8, 11

- Samuel 286, 296  
sanction 64, 65, 72, 83  
Sanders 219, 222, 225  
Sandler 13  
Sands 117  
Sanfilippo 93, 96  
Sankoff 124  
Sano 296  
Sanskrit 307  
Sanuma 24  
Sapir 40, 41, 146–148, 155, 246, 251  
Sasai 296  
Sasse 328, 329, 332, 340  
Saussure 32  
Savage-Rumbaugh 315  
Sawai 268  
SCE 128  
Scerba 199  
Schegloff 180, 211  
Schieber 174  
Schiffer 210  
Schiffman 63, 64, 66  
Schilling-Estes 125  
Schlegel 251  
Schleicher 251  
Schlichter 25  
Schmidt 268, 270–272, 280, 284, 290,  
292, 298  
Schnelle 258  
School of Oriental and African Languages  
159  
School of Oriental and African Studies  
332  
Schwarz 211  
Schwegler 250, 261  
Scobbie 229  
Scotton 73  
Searle 167, 208, 209, 211, 212  
segmentation 49  
Seiler 245  
Seki 24  
Sekine 171  
selectional restriction 95  
Selkup 6, 11  
semantic 36, 51, 58, 89, 92, 94, 102, 104,  
114, 115, 165, 178, 181, 195,  
219, 328  
adaptation 250  
category 96, 328  
change 115, 116, 333  
class 96, 99  
clustering 98  
complexity 5, 298  
content 4, 36, 104, 335  
disambiguation 177  
extension 3, 9  
feature 92, 95  
frame 177  
information 89, 103, 104  
interpretation 114, 166, 179, 209  
lexicon 94, 97  
markup 98  
pattern 99  
relation 92, 95, 96  
representation 33, 44  
richness 243, 253  
rule 166  
structure 166  
type 94–96, 100  
unit 92, 95  
vocabulary 97  
web 92, 93, 103–105  
semantics 32, 42, 57, 91, 94, 100, 192,  
209, 212  
~, lexical 92, 96, 192, 195, 201  
Semitic 110, 248, 260  
sense 95, 208  
sensory 7, 8, 18  
sentence 36, 170, 178, 244, 249  
mood 212  
pattern 261  
prosody 247  
representation 254  
structure 31, 243, 244, 253–255  
sequence 293  
aspect 38  
Sériot 252  
sg – singular 25  
Sgall 244–246, 249, 251, 254, 256, 259  
SGML 179  
Shasta 8  
Sherpa 6  
Shibatani 225, 296, 298  
shift 113, 218, 268

- ~, chain 111–113
- Shipibo-Konibo 5, 8, 10, 11, 14, 15, 21, 23
- Shipley 8, 11
- Shohamy 62, 65
- Shoji 268
- Shopen 244
- Shorish 67
- Shoshoni 127
- SHRDLU 163
- Shuy 75
- sibilant 148
- Sidner 165
- Siegel 220
- sign language 315, 316
- Silberztein 176
- Silver 8
- Silverstein 291, 297
- Simple 91, 94–97, 103
- Sinclair 171, 180, 191
- Singh 226
- single word 34, 250
- Singular 256
- Skalička 243, 245–250, 252, 253, 258–260
- Slavic 249, 260
- Sledd 195
- slot 11, 12
- Smeets 333
- Smith 167, 222, 251, 309
- Smolensky 225, 232, 233
- SOAS 332
- Sochiapan Chinantec 11
- Société Linguistique de Paris 315
- sociolinguistics 61, 62, 124, 149, 180, 193, 195, 313, 315
- sociopragmatics 207
- Soderland 177
- Somali language 67
- Sommerstein 225
- Sondheimer 165
- Song 116
- sound 111, 149, 155, 334
  - change 111, 112, 114, 115, 126, 127, 226
  - structure 149
  - system 149, 337
- SOV 258
- Spanish 3, 7, 72, 93, 310, 338
- Sparkle 102
- SPE 218–221, 223, 225, 226, 232, 233
- speech 74, 115, 124, 125, 148–152, 211, 233, 305, 306, 315, 316, 323, 334, 337
  - act 167, 170, 178, 207–212
  - community 127, 130, 194
  - recognition 170, 177, 339
- Spencer 220
- Sperber 213
- spoken language 338
- Spolsky 62, 65, 268, 269, 271, 280–282
- stability 66
- Stahnke 72
- Stampe 220, 234
- standard 74, 91, 92, 104, 157, 168, 169, 172, 282, 331
  - English 70
- standardization 199, 210, 271, 282
- Stanford 120
- Stanley 213
- statistics 177, 179
- status planning 64, 75, 77, 79, 80, 82
- Stein 192
- Steinthall 251
- stem 34, 93, 247, 258
- Stenius 207
- Sterkenburg, P. van 190, 340
- Stewart 226, 306
- stipulative 209
- Stolcke 177
- Stolz 7, 259
- stop 127
- ~, glottal 151, 152
- Straits Salish 58
- STRANS – transitive 25
- Strauss 220
- Strawson 210
- strength 248
- stress 150, 151, 153, 157, 218, 224
- Strom 8
- structuralism 187
- Studdert-Kennedy 340
- Stump 36
- style 74, 286

- stylistics 192  
 sub-Saharan 110  
 subcategorization 93, 94, 99, 101, 102  
 subject 244, 247–249, 259, 296  
   field 199  
 SUBJECT-OBJECT label 174  
 subordination 290–294, 297, 298  
 substitution 35, 37  
 substrate 309  
 suffix 12, 19, 22, 116, 167, 228, 257, 276,  
   291  
 ~, derivational 12, 257  
 ~, verbal 291  
 SUNDIAL 170  
 superlative 256  
 superstrate 312, 313  
 suppletive 250  
 surface 256  
   form 219, 229, 256  
   generalization 220  
   structure 130  
   syntax 254  
   tree 165  
   word order 254  
 Sutton 273, 274, 281, 296  
 SVO 246, 251, 258  
 Swahili 68, 308  
 Swedish 93, 131  
 Sybesma 58  
 syllabic 222, 224  
 syllable 33, 34, 150–152, 224, 232, 334  
   boundary 152  
   structure 222, 248  
   weight 150–153  
 synonymy 95, 96, 172, 247, 249  
 syntactic 31–33, 36, 40, 51, 55, 89, 92,  
   94–96, 116, 175, 180, 195, 248,  
   249, 252, 254, 297  
   accusativity 276–278, 285, 286,  
   293, 297  
   analysis 175–177, 179, 180, 327  
   annotation 91  
   category 55–58  
   change 109, 115  
   constituent 213  
   construction 92, 195  
   dependency 254, 255, 261  
   displacement 39, 40  
   ergativity 275, 277–280, 283–289,  
   290–298  
   function 246  
   lexicon 93  
   parsing 97, 166, 179  
   pattern 99  
   relation 257, 327  
   structure 32, 272, 289  
   theory 40, 218  
   transformation 209  
   tree 174, 323  
 syntagmatic 100  
   organization 222  
 syntax 31–33, 35–37, 39–43, 58, 91, 93,  
   115, 195, 254, 255, 261, 327, 328,  
   331, 338
- T**  
 Tabouret-Keller 125  
 Taff 273  
 tag 178  
 tag-set 181  
 tagging 97, 157, 158, 167, 168, 173, 261  
 Tajik 67  
 Takahashi 296, 297  
 Tannen 180  
 Tanoan 121  
 Tarahumara 338  
 Tariana 1, 8–13, 19–24  
 Tarp 190, 198–200  
 Tatevosov 5  
 Tauli 74  
 Tauya 7  
 taxonomy 208–210  
 Tây Bòy 308  
 tectogrammatical 254, 257, 261  
 tectogrammatcs 254  
 template 95, 97  
 ~, semantic 97  
 tense 3, 4, 11, 12, 19, 33, 112, 247  
 Tepehuan 9  
 terminology 85, 104, 261  
 Terralingua 332  
 Tesnière 261  
 Thieberger 271, 272, 288, 296, 297  
 Thiesen 16

- Thomason 53, 110, 307, 315  
 Thompson 306  
 Thomson 178  
 Thurston 126, 127, 130  
 Tibeto-Burman 6, 7, 14, 19  
 Tiwi 297  
 ToBI labeling 158  
 Tohono O'odham 128  
 Tollefson 73  
 Toman 252  
 Tomita 165  
 tone 151, 152, 157, 308, 309, 326  
 tool 102, 103  
 topic 243, 247, 250, 254–256, 261  
 Tovey 270  
 TR 257  
 transcription 157, 323–327, 336, 339  
 transducer 176  
 transformation 165, 174  
 Transformational Generative Grammar 193  
 transformational grammar 24, 164, 165, 314  
 transition 176, 255, 256, 260  
 transitive 296  
 translation 63, 98, 171, 176, 253, 325, 330, 339  
 TRAP 112  
 Traugott 115, 306, 312–315  
 Traum 167  
 Travis 3  
 Tree-bank 175  
 Trésor de la Langue Française 168  
 trill, bilabial 149  
 TRs 254–258  
 Trudgill 113, 130–132  
 truncation 34  
 Tsafiki 7, 8, 10, 16  
 Tsez 21  
 Tsunoda 268, 271, 279, 292, 296–298, 340  
 Tucanoan 1, 9, 10, 21, 24  
 Tuite 283  
 Tungusic 122  
 tuning 97  
 Tupí-Guaraní 15, 24  
 Tupinambá 146  
 Turkic 6, 11, 127  
 Turkish 4, 124, 247, 257  
 Tuyuca 9  
 typological 23, 116, 117, 243, 246, 253, 285, 293, 297, 298, 307, 308, 338  
 typology 4, 65, 116, 200, 243–245, 248, 249, 251–253, 257–261  
 ~, diachronic 251  
 ~, holistic 248–252  
 Tzotzil 329
- U**  
 UG 153, 154, 221, 230, 231, 307  
 Ugric 6  
 Uhlenbeck 274  
 Uhlif 260  
 Ullmann 163  
 Umlaut 127  
 unambiguous 327  
 underspecification theory 223  
 UNESCO 332  
 ungrammaticality 231  
 uniqueness 289, 295  
 Universal Base 57  
 Universal Base Hypothesis 57  
 universal 48, 55, 307, 309, 328, 333  
   category 56, 328  
   characteristics 48  
   features 307, 309, 310, 314, 328  
   grammar 51–53, 55, 307, 314  
 Universal Networking Language 171  
 universality 58, 336  
 UNL 171  
 unmarked 10, 223, 255, 258, 285  
 unmotivated 247  
 unrounding 127  
 updating 97  
 Uralic 110, 122  
 Urdze 259  
 usage 100  
 user 194, 197–201  
   perspective 194, 198, 200  
   target 194, 197, 199, 201  
 Utas 4, 7  
 Uto-Aztecan 9, 118, 121, 122  
 uvular 133  
 uvular stop 127

- V
- Valdman 310
- Valenzuela 8, 14
- validation 98
- Valin 292, 297
- value 174, 223, 249, 254
- Vanderveken 208, 209
- Vansina 123
- variability 127, 131, 338
- variable 113, 115, 125
- variant 93, 113, 132
- variation 153, 193, 260, 306, 314, 315
- variety 70, 71, 113–115, 128, 131, 133, 231, 282, 305, 307–313
- Vaupés 1
- Vaupés area 21
- Veins Theory 179
- Vennemann 219, 220, 222, 225, 250
- verb 41, 208, 247, 248, 258, 259, 330
  - adjective 249
  - clause 246
  - second 32, 39, 41, 42, 127
  - structure 11
- verbal category 12, 18, 24, 95, 291
- Verbmobil 171, 173
- verdictive 208, 210
- Vergnaud 234
- verificational particle 10
- vernacular 124
- Versteegh 307, 309
- video 100, 156–158
- Vietnamese 248
- Vincent, Nigel 159
- violation 227–229, 233
- VIS – visual 25
- visibility 66
- visual 8, 9, 11, 21, 22
  - evidence 10, 15
  - evidential 9, 10, 14, 21, 22
  - perception 19
  - term 8
- vocabulary 71, 147, 211, 259, 272, 278, 281, 289, 328, 330, 335
  - expansion 74
  - research 189
  - selection 97
- ~, basic 96, 117, 217
- ~, sexist 71
- Voegelin 290
- Vogel 222
- voice 153
- voiced 228, 233
- voiceless 150, 233, 335
- voicing 153, 227, 233
- Volockaja 249
- Vopěnka 257
- Vossen 117
- vowel 34, 112, 127, 131, 132, 150, 151, 224, 230, 233, 236, 247, 248, 334
  - channel 152
  - cluster 230, 231
  - deletion 233
  - harmony 247
  - shift 112, 113
  - shifting 11, 124
- ~, left-hand 227, 230, 231
- ~, peripheral 112
- ~, right-hand 227, 230, 231
- ~, short front 111, 112
- Vries, de 75
- Vykypěl 248, 249, 260
- W
- Wackernagel 42
- Wahrig 191
- Wakashan 11, 12, 58
- Walker, Alastair 159
- Walsh 268
- Wanner 165
- Warlpiri 283
- Warrgamay 275, 292
- Warrungu 269, 273–287, 289–298
  - grammar 286
- Washo 8
- Watkins 116, 117, 120
- Watters 7
- wave theory 120
- Webb 73, 84
- Webber 178
- Weber 9, 190
- Weinreich 110
- Weinstein 165, 179
- Weiss 260
- Wellens 309

- wellformedness 231, 234, 235  
 Welsh 269, 297  
 Whalen 296  
 Whinnom 306, 308, 309  
 whistle speech 152  
 Whitney 251, 324  
 Wiegand 189, 190, 195–200  
 Wierzbicka 208  
 Willett 9, 19  
 Williams 64, 81  
 Willie 340  
 Wilson 91, 213  
 Winograd 163  
 Wintu 25  
 Wise 16  
 Wittgenstein 207, 208  
 Wong 85  
 Woods 165  
 word 33–39, 127, 132, 133, 176, 246, 259  
   field 190  
   form 33, 36, 40, 247–249, 257, 260  
   formation 33, 35, 36, 38, 167, 195,  
     220, 248, 253, 255, 260, 261  
   internal 31–39  
   meaning 96, 97  
   order 246, 247, 251, 252, 254, 256  
   structure 31, 32, 35, 36, 43  
 ~, complex 33, 35–39, 127, 132, 133,  
   176  
 ~, single 34, 250  
 WordNet 171, 172  
 Wright 176  
 Wurm, S. 271, 272, 282, 288
- X**  
 X-Bar theory 57  
 Xamatauteri 8, 15, 24  
 XML 179  
 Xolodovič 259  
 Xu 337
- Y**  
 Yaeger-Dror 115  
 Yalarnnga 292  
 Yallop 268  
 Yamamoto 272, 281, 282, 333, 340  
 Yanam 5, 24  
 Yanomami 5, 8, 24  
 yell speech 151  
 Yidiny 275, 283, 284, 292  
 Yoruba 83, 248  
 Yukaghir 5, 11, 21  
 Yumitani 296
- Z**  
 Zampolli, Antonio 89, 91, 93  
 Zelle 177  
 Zepeda 333  
 zero morph 35  
 Zgusta, L. 188, 191–194, 201  
 zone 119  
 Zulu 133  
 Zuni 122  
 Zwicky, Arnold 43, 210

