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INTERVIEW

"What's right about X-bar syntax is the X and the bar. What's wrong is everything else!"

WITH *JAMES McCAWLEY, UNIVERSITY OF CHICAGO*

by *Lisa Cheng & Rint Sybesma*

What do you think about frameworks and theories? Wouldn't it be much more efficient if everybody worked within the same framework, using the same theory?

As an epistemological anarchist in the tradition of Feyerabend, I am delighted to have multiple frameworks being used. Even if I regard a certain framework as completely crackpotted, that doesn't mean that it's not a resource that might be valuable to me. I share Feyerabend's idea that any framework is going to lead its adherents to come up with facts that quite likely you wouldn't come up with so easily in your own framework. So I regard the multiplicity of frameworks as highly desirable.

Working within a particular framework today doesn't imply that you have to work within the same framework tomorrow. It doesn't prevent you from stepping outside the framework. I teach courses that deal with frameworks other than the sort of very revisionist kind of transformational grammar that is represented in most of the stuff that I am doing myself. When I teach a course on Relational Grammar or Montague Grammar, I describe it as a tour, not a sight-seeing tour but a shopping tour. That way I hope to give people some idea of the kinds of facts that Montague grammarians and Relational grammarians are led to look at. I do comparisons and show how essentially the same thing is done in different frameworks. But I also point out cases where the variant way of looking at things has yielded interesting results. Take, for example, in Rela-

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GLOT INTERNATIONAL

Volume 3
ISSUE
5

Monthly Magazine for Linguists

May 1998

ISSN 1381–3439

Price single issue: Dfl 14,50

Glot International appears monthly, except June & July.

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Subscription information
Glot International appears every month, except in June and July. Subscriptions are for the printed or the electronic (pdf) version.

Prices for Volume 3 (1998):

Institutional rate		NLG	120.0
Individual rate			
pdf only		NLG	35.0
printed version		NLG	50.0
Postage			
surface mail	Netherlands	NLG	32.5
	other countries	NLG	37.5
air mail	Europe	NLG	42.5
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surface shipping in three batches (March—August—December)			
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Subscriptions at the institutional rate may be ordered from your bookseller or subscription agent, or directly from the publisher.
Prices exclude 6% VAT (EC only).
Orders must be prepaid.
Payments preferably by giro, credit card (American Express, Euro/Access/ Master, Visa) or international money order. Credit card orders must include the complete cardholder's address. There is an additional charge of Dfl 17,50 for payments by international bank cheque or bank transfer.

Publisher
Holland Academic Graphics
Julius Röntgenstraat 15 | 2551 KS The Hague | The Netherlands | phone +31 70 4480203 | fax +31 70 4480177 | e-mail: mail@hag.nl | http://www.hag.nl | bank acc. ABN AMRO 49 88 22 796 | postal giro 6261107 | VAT nr. NL 0588.61.798.B01

Advertisements
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Printing
Printed in The Netherlands

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Dissertations reviewed

Verner Egerland, *The syntax of Past Participles. A generative study of nonfinite constructions in Ancient and Modern Italian*. Lund University. 1996. Supervisors: Kjell-Åke Gunnarsson and M. Rita Manzini. 352pp. Lund University Press. [Études Romanes de Lund 53.] ISSN: 0347–0822. ISBN: 91–7966–368–0. No price given.

Jeannette Schaeffer, *Direct object scrambling in Dutch and Italian child language.* UCLA. March, 1997. Supervisors: Nina M. Hyams (chair), Hilda Koopman, Dominique Sportiche, Terry Au. 158pp. Available through: UCLA Working Papers in Linguistics: bulger@humnet.ucla.edu. Price: US$10.

Books reviewed

Diana Archangeli & Douglas Pulleyblank, *Grounded Phonology*. Cambridge, Mass., MIT Press. 1994. 502pp., with index. ISBN 0–262–01137–9. GBP 46.50.

Goodies

WinSAL-V, developed by Ingolf Franke. Trier, Germany: Media Enterprise, 1996. PC software on CD-ROM. CD ROM version 349DM, with video option 399DM + 30DM S & H (outside Germany). Discount for students. On-line ordering information: <http://www.media-enterprise.de/home_eng.htm> (English); <http://www.media-enterprise.de/home.htm> (German).

NEXT MONTH!

State-of-the-Article
Jonathan Bobaljik on Floating quantifiers

Guest column
Helen Dry on
The LINGUIST list: A personal history

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Yuko Yoshida's *On pitch accent phenomena in Standard Japanese*
reviewed by Nancy Ritter

Book reviews
Željko Bošković's *The syntax of nonfinite complementation. An economy approach*
reviewed by Marcel den Dikken

And Chris Sidney Tappan's Murder mystery
The Number of Death continues...
"He's not creepy, he just does not have the ability to distinguish important things from the unimportant details. I think you are betting —"

SCHWA IN PHONOLOGICAL THEORY

Marc van Oostendorp

The schwa is a special vowel: it is often the result of reduction or epenthesis. It is indispensible, yet, invisible for stress systems. In the following article, Marc van Oostendorp gives an overview of the different contexts in which we find schwa (and why) and of the different ways phonological theory has been trying to account for the distribution, characteristics and behaviour of this intriguing vowel.

1. Introduction

If a language has schwa in its vowel inventory, this segment usually plays special role in the phonology of the language. It can only occur in a simple type of syllable; or it is invisible for the stress system; or it is epenthetic; or it is the result of reduction; etc. Linguistic theory has to explain this special behaviour of schwa: why is it exactly this segment which behaves in exactly this way in so many languages? A lot of subtheories of phonology are at stake — syllable structure theory, stress theory, the theory of (sub)segmental representation, and of interaction between segments — and the questions surrounding schwa therefore will probably not be resolved until the perfect theory of phonology has been discovered.

On the other hand, schwa offers an excellent testcase for many theories of phonology. It therefore should come as no surprise that the vowel has been studied intensively within various branches of generative phonology. This article offers an overview of some of the ideas that have been proposed and some of the facts that have been discovered over the past thirty years. None of these analyses seems perfect in the sense that it can handle all the relevant facts; but together the different propsals give us a rather precise picture of the behaviour of schwa.

It is my aim in this paper to present in a more or less systematic way those facts about schwa which I consider to be most characteristic. I also briefly present some of the more influential theories surrounding this vowel within the generative paradigm.

I find it useful to distinguish pretheoretically between three types of schwa:

* *e-schwa;* this is the type of schwa that alternates with zero. The e in the name of this vowel refers to *epenthesis* because this is one of the possible sources of the alternation. It is of course also possible that this alternation is caused by deletion of schwa in certain environments, and in some (monostratal) theories one may deny the role of vowel epenthesis or deletion altogether and claim that the occurrence or non-occurrence of schwa in a certain position is a matter of phonetic interpretation (Charette 1991). In any case, if an alternation is observed, this counts as a diagnostic for E-schwa.
* *r-schwa;* this is the name for a schwa that alternates with a full vowel. The r stands for reduction, but again, it is also possible that the alternation is the result of a 'fortition' of schwa; and in principle, also nonderivational accounts are available.
* *s-schwa;* this is *stable schwa*, which is a rest category from a descriptive point of view: if there is no reason to call a schwa e-schwa or r-schwa, I call it s-schwa. S-schwa is usually already present in the underlying structure, but this is not a distinctive property of the vowel: also e-schwa can be underlyingly present (when the alternation is the result of deletion), and so can r-schwa (when the alternation is the result of fortition).

I do not think the taxonomy just presented should be awarded any theoretical status: a schwa may change its behaviour over the years, and indeed we also find cases where a vowel is e-schwa and r-schwa at the same time (as in the French paradigm *appeler* [ɑple~ɑpəle] 'to call' *j'appelle* [ʒɑpɛl] 'I call').

The classification is presented here because it helps to distinguish the various roles that schwa can play in the phonology of a language. Before we can study the behaviour of schwa in natural language, it may be useful to go into the question of how the segment should be represented phonologically.

2. The representation of schwa

Schwa is special not only from the point of view of phonological theory. We find that it is a special vowel also if we look at it from an articulatory point of view: it could be described as a 'targetless vowel' for which no inherent articulatory target has been specified, or as a vowel which targets a neutral vocalic position, 'the mean tongue-tract variable position for all the full vowels.' (Cf. Browman and Goldstein 1992).

From a standard autosegmental/metrical point of view, we have several options to represent the emptiness of schwa. We can divide these options roughly into two sets. On the one hand, we can award schwa the status of an empty prosodic position without (auto)segmental material (an empty mora, an empty nucleus, an empty V-slot in a CV-model). On the other hand, we can assign some minimal segmental specification to this vowel. In the feature geometric model of McCarthy (1988), for instance, major class features constitute the root node. Schwa can then be seen as a bare [–cons] root, or as a root node with an empty (vocalic) place node. It is hard to find good empirical tests that could distinguish between these options. Many things depend on the other theoretical choices we make *vis à vis* the structure of syllables and segments. In the remainder of this article, I will arbitrarily assume that schwa could be pictured phonologically as a bare root node, in (1):

(1)
[–cons]

Schwa can be literally seen as the maximally unmarked vowel: it is marked for being a vowel ([–cons]) but for nothing else. I believe that, in the ideal case, as many phonological properties as possible should be made to follow from the interaction of this fact and general principles of phonology. Ideally, no linguistic rule or constraint should specifically refer to schwa.

I will take the fact that many languages do not have a schwa in their inventory as an example. Within constraint-based phonology, we could analyse this fact as a consequence of a specific constraint against schwa:

(2)
*SCHWA: Don't have schwa

Yet it seems more reasonable to see this as a specific consequence of something more general. For instance the set of constraints that have been called FILL within Optimality Theory (Prince and Smolensky 1993):

(3)
FILL: Empty segments are disallowed.

In Van Oostendorp (1995) it is proposed that the behaviour of schwa can be understood in terms of a relation between segmental material and prosodic structure. If a segment has certain features, it should project to certain positions (say, the head of a syllable or a foot), and if a segment appears in the head position of a certain syllable or a foot, it should have certain (vocalic) features. This is regulated by the following general constraint scheme:

(4)
Where F=a (vocalic) feature, P=a prosodic position
PROJECT (F, P): If a vowel is [+F], it should appears in position P.
PROJECT (P, F): If a vowel appears in position P, it should be [+F].

Van Oostendorp (1995) shows that particular settings for the arguments F and P can explain the behaviour of many vowels in a number of languages. Schwa is a special case of this. Since schwa does not have any vocalic feature at all, it is restricted to a very small number of positions. In certain languages this number may be so limited, that it is null.

3. E-schwa

One of the most prominent characteristics of schwa is that it alternates with zero. As a matter of fact the term *shvarabakti vowel* was initially meant to refer to the epenthetic vowel in Tiberian Hebrew. Epenthesis — insertion of non-underlying material — is of course one possible source of the schwa-zero alternation that defines e-schwa. Another source is deletion of schwa.

If schwa is a very simple vowel, there are reasons to assume that this vowel is particularly eligible for both epenthesis and deletion. In the case of epenthesis, it is the most economical choice to make if we have to insert a vowel: no material needs to be epenthesized beyond the feature [–consonantal]. In the case of deletion, it is simpler to delete only the specification [–consonantal] than to delete this feature alongside others (which is needed in case of the deletion of a full vowel). It is thus predicted that if a language (i) allows schwa on the surface (of a given subphonology) and (ii) has a vowel-zero alternation (within that same subphonology), schwa will be among the alternating vowels. I have no evidence that would falsify this prediction; I know of no study which tries to systematically verify it either.

Assuming for the sake of simplicity that there can be no constraints on underlying forms (this is of course the standard assumption within Optimality Theory), we have evidence for deletion if there are certain phonologically well-defined contexts in which schwa does not surface. We have evidence for epenthesis on the other hand if there are phonologically well-defined contexts in which we *always* find a schwa. Interestingly, there are languages in which we find evidence both for schwa epenthesis and for schwa deletion: there are certain contexts in which schwa always occurs, there are other contexts in which schwa never occurs and there may even be contexts in which it is unpredictable whether we find a schwa or not.

In languages of this type, epenthesis and deletion often seem to be defined in complementary contexts. These languages are of particular interest to constraint-based theories of phonology, because they often involve some form of 'rule conspiracy': epenthesis and deletion seem to conspire to attain some absolutely well-formed (syllable) structure. A rather well-known instance of such a language is French. The distribution of schwa in French has been a topic of debate during virtually any period of time within the history of generative phonology (Anderson 1982, Basbøll 1981, 1988, Charette 1991, Dell 1973/1985, Durand 1976, 1986, 1990, Morin 1982, 1988, Noske 1982, 1988, 1992, Van Oostendorp 1995, Schane 1965, 1968, 1974, Selkirk 1978, 1982, Tranel 1981, 1987, 1994). The facts of French are rather complicated, and a lot of dialectal and probably even idiolectal variation is involved. Yet, the French facts are quite typical of the behaviour of e-schwa in natural language. Noske (1992) presents a useful classification of the facts of French e-schwa, which I will take as my guideline

here.

In the following discussion I distinguish between 'static' and 'dynamic' pieces of evidence. 'Static' evidence for a constraint (or rule) is distributional: we never find schwa in a certain position, therefore there should be a constraint or a morpheme structure condition against schwa in that position (or a rule deleting it there); we always find a schwa in a certain position, therefore there should be a constraint forcing it to be present (or a rule epenthesizing it). In order to be able to accept 'static' evidence for an output constraint, we have to assume some version of OT's 'Richness of the Base' (Prince and Smolensky 1993): there are no constraints on underlying representations, and everything can be input to the grammar.

'Dynamic' evidence for a rule or a constraint is based on alternations. A schwa in form A corresponds to a zero in exactly the same environment in the morphologically related form B. I take this to mean that there is a constraint against zero in A, or against schwa in B (or that there is an epenthesis rule applying to A or a deletion rule applying to B).

Type A: Prevocalic schwa deletion. The evidence for this type of process is that schwa cannot be followed by a full vowel. It is actually hard to find evidence for the rule in French, because we need a context where /xə#v/ surfaces as [X#V], and schwa is deleted at the end of a word or phrase in many cases anyway. The alternation of the masculine singular determiner *le* is sometimes cited as evidence for the rule. This clitic surfaces as /lə/ before a consonant-initial stem (*le camerade* 'the comrade' [lə̆kamrad]) and as /l/ before a vowel-initial stem (*l'ami* 'the friend' [lami]). The problem is, however, that a similar alternation applies to the feminine singular determiner *la* (*la camerade — l'amie*), but we have no reason to postulate a general rule of *a*-deletion before a full vowel.

The other dynamic pieces of evidence usually given are equally dependent on other assumptions. For instance, it is often assumed that the feminine singular suffix is a schwa (*autre* 'other, FEM.' /otR/ + /ə/ [otR]). The problem is that this schwa never surfaces at the end of a phrase, and in those environ ments in which it does surface (*autre femme* 'other woman' [otRəfam]), this may well be due to schwa epenthesis (alternation type D discussed below). Although the indirect phonological evidence, which is based on the process of liaison but which I cannot review here (cf. Encrevé 1988 and references cited there), is favourable to the hypothesis that the feminine suffix is schwa, there is no really strong phonetic evidence that the schwa is present at all, or that it is ever deleted specifically in the context before a vowel.

On the other hand, we do have evidence that the *constraint* against schwa in front of a vowel is operative in French; although schwa can be rather freely distributed over all positions in the French word, there are no words in which it immediately precedes a vowel (either a full vowel or schwa). French shares this property with many other languages, including Dutch, English, German and Indonesian. In Dutch we actually have some pieces of dynamic evidence: if the schwa-final word (*elite* [elitə]) is followed by a vowel-initial suffix (*-air* [ɛr]), the schwa gets deleted (*elitair* [elitɛr] 'snobbish') (Booij 1995).

I propose that this behaviour of schwa can be understood within Optimality Theory in the following way. As we have seen above, we need to posit a constraint against the occurrence of schwa. This constraint outranks most faithfulness constraints on schwa, but it is itself dominated by several well-formedness constraints on syllable structure. Schematically:

(5)
Well-formedness >> *SCHWA >> Faithfulness.

This ranking implies that schwa never surfaces unless it is required to do so by the syllable structure constraints. This in turn means that it is almost impossible to say whether we see deletion or epenthesis at work in a given case: schwa is deleted wherever it is not necessary because of *SCHWA and inserted wherever it is necessary because of high-ranking Well-formedness.

If an underlying schwa is both preceded and followed by a consonant, it needs to surface in order for the word to satisfy general constraints on syllable structure, in particular constraints against complex onsets (*[lkamrad]-[lə̆kamrad]). Yet if schwa is followed by a full vowel, there is no necessity for it to surface ([lami] is perfectly well-formed as far as syllable structure is concerned). This may be the reason behind the deletion of schwa.

A famous exception to the deletion of schwa before full vowels is formed by a class of words with a so-called *h aspiré*: a consonant position that was probably filled by a /h/ in earlier stages of the language but that is presently phonetically empty, although phonologically it still behaves as a full-fledged consonant:

(6)

mets le dessus	[mɛlədsy]	'put it on!'
retourne la	[rturnla]	'return it!'
mets le dehors	[mɛlədəɔr]	'put it outside!'
rehausse la	[rəosla]	'raise it again'

It is standardly assumed that this behaviour can be understood by assuming some more or less abstract consonant in this position. The schwa is therefore not adjacent to the full vowel in cases like these; hence, it is not subject to the deletion process.

Type B: Postvocalic schwa deletion. A similar line of reasoning may help to explain why schwa does not surface on the *righthand side* of a full vowel. Also in this context, there is no necessity for schwa to surface. The syllable structure of [ami] is actually better than the structure of hypothetical [amiə], because the latter has one extra onsetless syllable.

Again it is rather hard to find dynamic evidence for a deletion rule — the evidence that is cited by Noske 1993 for instance involves once again the feminine suffix which reputedly gets deleted after a full vowel: *risée* 'laughed at, FEM' /rise/+/ə/→[rise], but as I have already pointed out above, word-final schwa gets deleted in many other cases as well — yet there is some static evidence in that the relevant constraint is at work in the languages just mentioned: we don't find any (monomorphemic) words in which a schwa immediately follows a full vowel.

The question may now arise why not *all* vowels get deleted when they occur next to another vowel. I think that the reason for this is that deletion of schwa is less costly than deletion of other vowels, due to the fact that the schwa literally is a *substructure* of all the other vowels. If it is allowed to delete a full vowel in a certain context, this implies that it is allowed to delete a root node in that context; hence, also schwa will be deletable in that context.

Type C: schwa deletion in a 'two sided open syllable'. A schwa which occurs in an open syllable that is preceded by another open syllable may get deleted. In practice this means that an underlying schwa occurring in the context ...V$C__$C(C)V... can disappear on the surface. In contradistinction to the two previous cases, this type of alternation is either *optional* or otherwise subject to stylistic conditions.

The following relatively famous examples serve to illustrate this point.

(7)

Henri devrait partir	[ãridəvʀɛpaʀtiʀ]
'Henri would have to leave'	
	[ãridvʀɛpaʀtiʀ]
Jacques devrait partir	
'Jacques would have to leave'	[ʒakdəvʀɛpaʀtiʀ]

In this case it looks as if underlying schwa's should be deleted in principle, but deletion is blocked if it would result in an unwellformed cluster of consonants, in particular one which cannot form a reasonable syllable structure. Certain additional stipulations are necessary, however, since [dvr] is not otherwise a wellformed onset cluster of French, yet the syllabification that most speakers would assign to *Henri devrait partir* seems to be [ãri.dvʀɛ.paʀ.tiʀ]. This can be understood in several ways. It is possible for instance that there is still a 'trace' of schwa between [d] and [v]; this is the solution adopted in Charette (1991) modulo technical details. It is also possible that there is a distinction between two rounds of syllabification (e.g. lexical and postlexical); again modulo technical details, this seems the type of solution favoured by Noske (1993).

Type D: Schwa epenthesis in the environment CC]_[C. A schwa is (optionally) inserted between two words, if the first word ends in a cluster, and the second word starts with a consonant. In this case syllable structure seems the driving force (rather than a force blocking deletion); clusters of consonants are avoided also in this case:

(8)

un contact pénible	[œ̃kɔ̃takt(ə)penibl]
'a painful contact'	
un index formidable	[œ̃ɛ̃dɛks(ə)fɔRmidabl]
'a terrific index'	

Maybe we can understand these examples in the following way: complex codas are dispreferred in French. A schwa is needed to support the final consonant in a cluster. However, schwa can never surface at the end of a phrase in French, as we will see below. It also cannot appear (is deleted) before a vowel. The only case in which we therefore see the supporting schwa appear on the surface is phrase-internal before a consonant. The fact that we have epenthesis in these cases is therefore readily understood; the fact that the epenthetic vowel is schwa also is readily understood. Schwa is the simplest vowel of all; therefore it is more economical to insert this vowel than it is to insert any other.

Charette (1991) observes that within compounds, stress also seems to play a role in the behaviour of schwa. A schwa has to surface if the first component ends in a consonant cluster and the second is monosyllabic; if the second is polysyllabic, the schwa *cannot surface* (according to Charette, a similar pattern is found in Québec French in *phrases* as well).

(9)

port**e**-clefs	'keyring'
gard**e**-fou	'railing'
port**e**-manteau	'coat rack'
gard**e**-manger	'meat safe'

Since stress is on the final syllable of the compound, it does not seem unreasonable to suppose that it plays a role in these examples. The question is of course why schwa should surface when it is immediately followed by a stressed syllable. Charette's answer to this is that French feet are iambic, and that the unstressed syllable in the foot should be a schwa. These assumptions are not uncontroversial, but as far as I know alternatives to Charette's proposal are yet to be worked out.

Type E: Schwa deletion in phrase-initial syllables. The existence ot this property, mentioned by Noske (1993), seems subject to debate: not all speakers of French agree on the data. In any case, some speakers allow alternations such as the following:

(10)

revenez (demain)	[rəvəne]~[rvəne]	'demain'
te fais (pas de bil)	[təfɛ]~[tfɛ]	'don't worry'

Syllable onsets [rv] or [tf] are not normally allowed in French. It is possibke that the initial syllable of the word allows a more complex structure (as it does in Polish, cf. Rubach and Booij 1990).

According to Charette (1991) this type of deletion is restricted to bisyllabic words: *cheval* 'horse' may be pronounced as [šfal] , but *chevalier* 'knight' is [šəvalje]. In this case, it looks as if stressed syllables allow more complex onsets. On the other hand, the explanation given here seems to run counter to the explanation of *porte-clefs* vs. *porte-manteau* just given. In this case, French speakers do not try to build a 'perfect' iamb [šəval].

Type F: Schwa deletion in phrase-final syllables. This final type of 'alternation' is one of the most important ones: schwa never surfaces phrase-finally (except in Midi French, Durand 1986).

(11)

je vois l'autre	[ʒəvwalotʀ]	'I see the other'
voilà mon oncle	[vwalamõnõkl]	'there is my uncle'
la terre est plate	[latɛʀɛplat]	'the earth is flat'
la route est longue	[laʀutɛlõg]	'the road is long'

It is not immediately clear why schwa should be

deleted in final position. One possibility, suggested in Van Oostendorp (1995), is to extend the scope of the constraint FINALC to the phrase. The constraint would thus say that all phrases have to end in a consonant. Deletion of full vowels under this view would be blocked by the fact that this would involve deletion of too many features. Deletion of schwa, on the other hand, is less costly, and therefore, it is allowed in this particular configuration.

The solution of Government Phonology is technically a little bit different. According to Charette (1991), a domain-final empty nucleus is exceptionally licensed to remain empty in French. In any case, it seems as if something special needs to be said about the end of the phrase in French. No special theory is needed, however, in order to describe why it is exactly schwa that behaves as special in this particular spot.

4. R-schwa

Schwa not only alternates with zero in languages of the world: it also often alternates with full vowels. There is an instance of this in French: schwa alternates with [ɛ] in the following contexts in this language (Dell 1973/1985):

* when schwa is the final phonetically expressed segment of a word ([paqu[ɛ]ʈ])
* when schwa phonetically occurs in a word-internal or word-final closed syllable

It seems reasonable to assume that underlying schwa turns to [ɛ] in certain environments: namely when it is stressed or when it is in a closed syllable. This is not surprising; schwa disfavours stressed positions or closed syllables also in other languages, as we will see below. This thus seems to be an instance of *fortition* rather than reduction. We may of course wonder why schwa turns to [ɛ] rather than to some other vowel; a good theory of segmental structure should provide us with an answer to this question.

Reduction to schwa is well-known from the study of Germanic languages. A very simple theory of vowel reduction in English is presented in Chomsky and Halle (1968). The reduction rule in this work basically amounts to (12):

(12)
Unstressed vowels turn to schwa.

Syllable structure plays an important role in the analysis of E-schwa; in the case of R-schwa it is often stress that seems to trigger the alternation.

Within Optimality Theory, there are two possible ways to explain (12). In Alderete (1995) it is proposed that in principle all vowels turn to schwa. Schwa is the unmarked vowel and Alderete assumes that there is a constraint which requires all vowels in the word to be unmarked (I will call this constraint TURNSCHWA for the sake of simplicity, but it can of course be seen as an instance of the general constraint *STRUCTURE). On the other hand there are position-specific faithfulness constraints that specifically require *stressed* syllables to be faithful to the underlying structure. These position-specific constraints I will shorthand here as FAITHFULSTRESS.

An analysis for the English word *apron* would now run along the following lines.

(13)

/aprɔn/	FAITHFULSTRESS	TURNSCHWA	FAITHFUL
aprɔn		**!	
aprən		*	*
əprən	*!		**

Alderete (1996) claims that FAITHFULSTRESS constraints can also be put to use in analyses of phenomena other than reduction. For instance, he shows that the fact that epenthetic vowels usually avoid stressed positions (even if they are not schwa) can be understood in terms of this constraint. It is worse to epenthesize into a stressed position and violate *both* FAITHFULSTRESS *and* FAITHFUL, than to epenthesize into an unstressed position in which case only FAITHFUL will be violated.

Yet there are also problems connected to an approach based on position- specific faithfulness. We have already seen evidence for the fact that we need to posit a constraint against schwa (e.g. *SCHWA, (2)) within the universal inventory of constraints, if only because there are languages which do not have schwa. We then have a ranking FAITHFULSTRESS >> *SCHWA >> FAITHFUL within our factorial typology. This however gives us a result which is probably absurd: a language in which underlying schwa can only surface in a stressed position:

(14)

/əprən/	FAITHFULSTRESS	*SCHWA	FAITHFUL
ə́prən		**!	
ə́pren		*	*
épren	*!		**

More in general, a FAITHFULSTRESS account implies that there are more contrasts possible in stressed positions than in unstressed positions. While this is true in general, it is not true in the case of schwa. The prediction that there are languages in which we can *only* have schwa in a stressed position does not seem to be borne out.

An alternative is to make TURNSCHWA rather than faithfulness into a position-specific constraint, or a set of such constraints. We then get some form of projection constraints, as proposed in Van Oostendorp (1995):

(15) PROJECT-FOOT
a. Unstressed vowels turn into schwa
b. Schwa wants to be unstressed

These constraints fit into the general theory of Projection outlined in (4): (12b) says that a vowel in the head position of a foot needs to have some minimal feature structure; (12a) says that vowels with a certain feature structure desire to be in the head of a foot. If they are not, they turn into schwa. An analysis of English *apron* would run along the following lines.

(16)

/aprɔn/	PROJECT-FOOT (a)	PROJECT-FOOT (b)	FAITHFUL
áprɔn	*!		
áprən			*
əprən		*!	**

A language that only allows schwa in stressed positions is not part of the typology in this system: if we rank *SCHWA very highly, we get a language in which schwa does not appear at all; if we rank FAITHFUL very highly, we get a language in which schwa can in principle occur in any position.

Another advantage of this approach is it that it explains why there are languages in which underlying schwa's are just as stressless as the epenthetic ones or the ones which are the result of reduction. (Underlying schwas will be the topic of the next section.) There are also problems with this approach, however: most importantly, the fact that generally stressed positions do license more feature contrasts than unlicensed ones, and that epenthetic vowels are unstressable requires an explanation.

Regardless of which of the two lines of research will turn out to be most fruitful, the analysis of English vowel reduction should be more sophisticated. Burzio (1994) argues, quite convincingly in my view, that the traditional view on reduction in English (every unstressed vowel gets reduced to schwa) is too simple. It causes all sorts of problems, because it forces us to assign stress in an extremely complicated way and furthermore to introduce the formal device of *destressing rules* into the theory in order to undo some of the results of the stressing rules. Burzio (1994) proposes to simplify the stress rules. As a result of this, the conditions on reduction get more complicated. Burzio's main point is that reduction is generally blocked in unstressed closed syllables ending in an obstruent, but not in unstressed syllables ending in a sonorant; in this way, he can get rid of the so-called 'sonorant destressing' rules.

I find Burzio's arguments convincing because the difference between closed and open syllables, and the sonority of a following consonant also play a role in the behaviour of other types of schwa: we have already seen that schwa cannot occur in closed syllables in French, and I will give an example of S-schwa in Dutch below. It therefore seems more natural to say that unstressed vowels resist reduction because they occur in a closed syllable, than to say that they are stressed, and possibly later destressed. There is no independent evidence for the latter assumption.

Dutch

I will now turn to a language in which reduction is subject to even more factors than it is in English. This language is Dutch (the following discussion is based on Booij 1981, 1995, Kager 1989, Martin 1969, Van Oostendorp 1995). Factors that presumably play a role in Dutch vowel reduction are:

i. Frequency:
Vowels in more frequent words tend to get reduced more easily than those in non-frequent ones.
ii. Differences between sociolects and dialects, and differences betwee style levels
Certain dialects/sociolects/style levens show more reduction than others.

It is hard to see how factors (i) and (ii) could possibly be taken into account within generative grammar; presumably, this is the reason why they are usually ignored in the literature (but see Hinskens et al. (1997) for several recent attempts to integrate some of these concepts into phonological theory). We will not go into these factors either. Other factors can be analysed more easily.

iii. Syllable type:
Vowels in open syllables are easier to reduce than those in closed syllables; in some idiolects (e.g. the one of Booij 1981, and the one of the author of this article, but not in the one of Kager 1989) reduction in closed syllables is only possible if the following consonant is deleted.

(17)

/bɛnzinə/	[bəzínə] (*[bənzínə])	'petrol'
/kɑntor/	[kətór] (*[kəntor])	'office'

This is the same restriction as the one that Burzio (1994) claimed to exist for English. Yet in the literature Dutch there is less controversy that the purported restriction on open syllables belongs to the theory of reduction rather than to the theory of stress assignment. The reason for this is that reduction is optional in Dutch; apparently it is more attractive to say that reduction can optionally apply to invariably assigned metrical structures than it is to say that the stress rules themselves are optional.

I think that the restriction can be understood in the following way. A closed syllable is more complex than an open syllable. A minimal vowel is not sufficient to license such a complex structure. A projection constraint relating the quality of the vowel to the rhyme structure of the syllable in which that vowel occurs is therefore to be preferred.

iv. Stress position:
In Dutch — as in English and apparently any other language in which reduction is dependent on the position in the metrical structure — only unstressed vowels get reduced. As a matter of fact, we can distinguish between two types of unstressed vowel (Van Zonneveld 1985). Vowels in so-called 'semi-weak' positions — immediately following a stressed position — are easier to reduce than those in 'weak' positions.

(18)

/fonoloɤi/	formal style	[fɔ̀noloɤí]
	less formal style	[fɔ̀nəloɤí]
	informal style	[fɔ̀nələɤí]
	impossible	*[fɔ̀noləɤí]

Similar facts can be found in other languages as well (Burzio 1994:113). In English, *tat*[ə]*magouchi* seems preferable to *tataməgouchi*. Jacobs (1989) observes a similar patterning for syncope in the history of French:

(19)
Latin: similitudinem → simlitudinem → *Old French:* sembletume 'resemblance'

The precise way to describe the difference between the two types of unstressed position is dependent on our precise assumptions regarding metrical structure. If we assume, with Burzio (1994) that feet can be ternary, we should distinguish between 'foot-internal' and 'peripheral' unstressed syllables. If we work in a theory in which only binary feet are allowed, we should distinguish between weak syllables in a foot and syllables which are somehow left unfooted by the parsing algorithm.

Vowel reduction to low vowels in Russian (/stol-á/ — *stal-á*) only occurs in the syllable

directly preceding the stressed syllable (henceforth: the pretonic syllable henceforth). All vowels reduce to [ə] in unstressed, non pretonic syllables (the following facts have been taken from Alderete 1995).

(20)

Nom. Sg.	stól	slóv-o
Gen.	stal-á	slóv-a
Dat.	stal-ú	slóv-u
Instr.	stal-óm	slóv-om
Loc.	stal-é	slóv-e

v[ó]dəj	nom. pl.
v[a]d-á	nom. sing.
v[ə]davóz	'water carrier'

Here it seems that the position next to (on the lefthand side of) a stress is less likely to undergo full reduction than vowels in other positions.

v. Position in the word:
Vowels in absolute word-initial or absolute word-final position do not reduce. This observation is absolutely surface-true in Dutch. No matter what the style of speech is, we do not find reduction of the vowels that are absolutely word-initial or absolutely word-final. The /e/ is reduced quite easily in Dutch, for instance, but *not* if it is the first or the last segment of the word, as the following segments are intended to show:

(21)

/plezir/	[pləzír]	'fun'
/exal/	[egál] (*[əxal])	'even'
/tɔfe/	[tɔfe] (*[tɔ́fə])	'toffee'

We may think of this as some kind of alignment effect of phonological to morphological structure: the edges of output words should correspond exactly to their morphological specifications.

An alternative explanation for the word-initial vowels is that syllables consisting of only schwa is disallowed; a syllable needs some minimal specification (Cohen et al. 1959). An argument for this alternative may be that vowels next to /h/ also cannot be reduced:

(22)

/plezir/	[pləzír]	'fun'
/helas/	[helas] (*[həlas])	'alas'

We will see in the next section that also underlying schwas cannnot occur in absolutely word-initial position; this is true for many other languages as well, including German, Indonesian and French.

There is also an alternative explanation for the non-reduction at the right edge of the word. Some authors (e.g. Brink 1970) have claimed that Dutch schwa is always the result of reduction: words such as *mode* 'fashion' are underlyingly specified as [mode]; in that case reduction is obligatory in these environments and the word *toffee* [tɔfe] should somehow be marked as an exception (it is true that there is only a handful of words in Dutch ending in unstressed [e]).

vi. Vowel quality:
The quality of the underlying vowel plays a role in the choice between reduction and non-reduction in many languages. In English, for instance, high vowels are not as likely to undergo reduction as non-high vowels (Chomsky and Halle 1968). Mid vowels in Dutch are easier to reduce than high and low vowels; front vowels are easier to reduce than back vowels; rounded vowels are easier to reduce than unrounded ones. This is illustrated in the following table (from Kager 1989):

(23)

Reduces in	**Weak position**	**Semi-weak position**
/e/	formal	formal
/a/	semi-formal	semi-formal
/o/, /i/	semi-formal	informal
/u/, /y/	informal	excluded

Interestingly, Dressler (1973) found more or less the same hierarchy of segments for Breton vowel reduction to schwa. As far as I am aware, the structure of this hierarchy is still in need of an explanation.

5. S-schwa

R-schwa can be a non-underlying schwa which is the result of a reduction operation; e-schwa can be a non-underlying schwa which is the result of minimal vowel epenthesis. Both types of schwa can also be underlying; the alternation in the case of r-schwa can then be seen as a case of fortition. The alternation in the case of e-schwa can be analysed as a result of deletion. As a matter of logics, we also expect underlying schwas which do not alternate; these vowels I call s-schwa.

Even though s-schwas are stable in the sense that they occur both in the underlying and in the surface structure of a word, they still often display some special behaviour. The factors involved may be familiar by now: s-schwa does not occur in an unstressed position or in a syllable that is too complex.

We have seen that schwa in French cannot occur in a closed syllable. In Dutch, there are also several restrictions on the syllable shape in which a schwa can occur. For one thing, we do not find schwa in an onsetless syllable. The static evidence for this is that the vowel does not occur as the first segment of a word (*[əɤal]), and that it does not occur immediately after a vowel (*[hiət]). This restriction seems quite common in languages of the world: it can be found also in Indonesian, Slavic languages, and French. On the other hand, Dutch schwa also cannot occur in a syllable with a *complex* onset (*[papavrə]). I have found no other languages yet in which this restriction seems to hold (except maybe related Germanic languages such as German and certain dialects of Norwegian, Kristoffersen 1995).

The restrictions on schwa are not just restrictions on the onset; there are also restrictions on the rhyme of a schwa syllable. Other than full (short) vowels (*harp* 'id.' [hɑrp], *Belg* [bɛlx] 'Belgian', *ramp* [rɑmp] 'disaster'), schwa cannot be followed by two tautosyllabic consonants, except if the second consonant is coronal (*[adərp], *[katəlx], *[adəmp], *anders* [ɑndərs] 'different'). Marked coda's, just like marked onsets, are not allowed.

Even within the class of schwa syllables with a simple coda, we find a clear asymmetry. Schwa has a preference for syllables closed by a sonorant: words such as *hennep*, *tinnef*, etc. are very rare. It is not uncommon for languages to require that codas can only contain a sonorant. There is no evidence, however, that syllables headed by a full vowel are subject to this requirement in Dutch.

In general it seems that the requirements on schwa syllables are much stronger than those on syllables headed by other vowels. In Van Oostendorp (1995) I proposed that this is due to the theory of Projection in (4): schwa as an (almost) empty vowel does not have sufficient material to license all of the syllabic nodes that are necessary in order to get complex onsets or codas. Simple vowels are not allowed to project complex syllables.

Schwa in stress

In most languages that have (underlying) schwa, the vowel is left unstressed. Roughly speaking, this can happen in two ways: schwa can preferably end up in the weak position in a foot; this seems to happen in a language like Dutch, in which stress always falls on the penultimate syllable if the final syllable contains a schwa (*ballade* [bɑládə], *[bɑ́ladə], *[bɑladə́]); if the final syllable contains a full vowel, more (lexical) variation is possible (*pánama*, *pyáma*, *chocolá*). Schwa thus is not completely invisible for stress; under the standard assumption that Dutch feet are trochaic, we should say that schwa can only occur in the weak syllable of a foot.

Alternatively, schwa can behave as if it is completely invisible for the stress algorithm; an instance of this is Indonesian. In this language (Cohn 1989, Cohn and McCarthy 1994) primary stress is usually on the penultimate syllable of the word (a–b) secondary syllable is on the first syllable and on every odd syllable counting from the right-hand side of the word. An exception to this are words which contain a schwa. These can be best understood as if schwa were not present at all:

(24)

a.	cát	'print'
b.	cári	'search for'
c.	bicára	'speak'
d.	bìjaksána	'wise'
e.	kòntinuási	'continuation'
f.	òtobìográfi	'autobiography'
g.	àmerikànisási	'Americanization'
a′.	bərí	'give'
b′.	sətəláh	'after'
c′.	gámə̀lan	'Indonesian orchestra'
d′.	apártəmen	'appartment'
e′.	cərìtəra	'story'
f′.	pərəmpúan	'woman'
g′.	dìfərensiási	'differentiation'
h′.	dìvərsifikási	'diversification'

It is unclear, at least to me, how exactly these facts should be understood. We may understand why schwa cannot occur in the stressed position of a foot, but this does not clarify why it cannot occur in the unstressed position of a foot. Particularly intriguing are the cases in (g′) and (h′). There is no secondary stress on a full vowel in these cases: we do not find [dìfərénsiási]. According to Cohn (1989) and Cohn and McCarthy (1994) the reason for this is that stress only sees full vowels: a trochaic foot is therefore built over the first two full vowels of the word, i.e. the foot structure of this word is [(dìfəren)si(ási)]. It thus looks as if in this case schwa is not strong enough to project a grid mark at all, not even one which appears in the weak position of a foot.

Schwa remains unstressed also in most dialects of Yupik (Hayes 1995). Different dialects of Yupik choose different means to escape an unstressed schwa. Stress is generally on the second syllable of the word in Yupik; the stressed vowel is lengthened (/qayani/ — [qaya:ni]). If this syllable contains schwa, the vowel is deleted in the General Central dialect of Yupik, as if it wants to escape a stress position. Stress is then assigned to the first syllable of the word.

(25)

/qayapigkani/	[qayáápixkani]	'his own future kayak'
/qanəqa/	[qánqa]	'my mouth'

GCY schwa therefore should be classified as an e-schwa; it is however different from the cases of e-schwa we have seen above because the triggering factor for the alternation is not syllable structure, but rather stress. A pattern that is fairly similar to the one of GCY can be found in Alutiiq Yupik.

In other dialects of Yupik, schwa does get stressed, but this stress does not trigger lengthening, as it does on other vowels. Different dialects of Yupik may solve this problem in different ways. In the Norton Sound dialect, for instance, the consonant following the schwa gets lengthened: /atəpik/ — [atə́ppik]. In this way, the stressed syllable is still heavy, even though schwa needs not be lengthened in order to achieve this result. In the Central Siberian dialect of Yupik, on the other hand, nothing happens to the stressed schwa-headed syllable at all: /atəpik/ — [atə́pik].

6. Conclusion

Schwa is special in the sense that it has a more limited distribution than other vowels: for instance it can often only occur in a rather simple syllable type, and only in an unstressed position. Schwa is also special in the sense that it is the target of reduction and deletion; furthermore it is easier to delete schwa than it is to delete a full vowel. Phonology should be able to describe all of these facts.

In order to fully understand the behaviour of schwa, we need a fully developed theory of syllable structure, of metrical structure, of segmental structure, and of the way in which these different dimensions of phonological structure can interact. Inversely, while developing these subtheories, we sharpen our view of schwa. Most of the facts mentioned above could probably not have been raised without sufficient understanding of phonological structure, phonological derivation and phonological constraint interaction. Some of the facts mentioned here are still rather problematic for any phonological theory known to the author of this overview; chances are that new facts may be discovered while phonologists are developing their theories. I suspect that we will not have a satisfying theory of schwa untill we have a satisfying Theory of Everything.

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ACQUIRED WHINING

by *Neil Smith*

According to Woody Allen "sentence structure is innate but whining is acquired" (Sunshine 1993:190). The first proposition will be reasonably familiar to readers of *Glot International,* but I imagine that the second is less expected and, in part for that reason, mildly amusing. So what makes Woody funny? What makes **anyone** funny come to that? The literature on humour is notoriously unfunny, but a little theoretical insight is breaking into even this murky area, so it's worth trying to see how some of it works.

Everyone agrees that incongruity is a crucial element in most forms of humour, where incongruity ultimately reduces to contradiction. This raises two problems: first, there is nothing funny in asserting the contradiction that 'Sentence structure is innate and sentence structure is not innate'; second, how 'acquired whining' contrives to be contradictory is not immediately obvious. These problems suggest that the contradiction has to creep up on you unawares: it has to be **implicit** rather than **explicit**; and it has to be something you are led to construct indirectly for yourself: it cannot work without your involuntary collaboration.

In recent work exploiting Relevance Theory (Sperber & Wilson 1986/95), Carmen Curcó (Curcó Cobos 1997) has spelt out some of the issues involved in making these intuitive assumptions precise. She starts with the important point that what is essential is not the incongruity itself but the effects achieved by processing incongruity. Indeed, she makes a strong case for the claim that the analysis of humour requires no special theoretical apparatus which is not independently needed for the analysis of normal communication. Processing utterances is an inferential activity, carried out in conformity with the communicative principle of relevance, which gives you a guarantee that what is being said is going to have cognitive effects without putting you to gratuitous effort. In order to achieve such effects you are often constrained to provide an appropriate context on your own responsibility. If you ask whether *Husbands and wives* is worth seeing, and are told "all Woody Allen's films are great", you are forced to supply the implicated premise that the film is by Allen, because that is the only way the answer can be construed as relevant.

Consider in this context the following excerpt from a policy document put out by the Irish Ministry of Transport shortly after Ireland had joined the European Union.

For a two-year experimental period, beginning at midnight on 31st December, cars will drive on the **right** (as in the rest of Europe), instead of the **left** (as in the UK). If the experiment is a success, buses and lorries will thereafter also drive on the right.

As you read, you naturally interpret 'cars' as a generic term for vehicle, because this is the only rational meaning in a context where rules of the road are concerned and hence will be mutually manifest to speaker and hearer. That is you construct a scenario including the tacit assumption "All vehicles will drive on the right". When you get to the punch-line, you are forced to supply an implicated premise to the effect that 'cars' excludes other types of vehicle, hence that "Not all vehicles will drive on the right". The writer has deliberately not made this explicit but, by trading on the presumption that processing the utterance will be relevant, he gives you no option but to access this new, contradictory, premise: he has lured you into making incongruous, contradictory, assumptions. The point of the analysis is not to show **why** the particular example is funny, but (if it is funny) **how** the effect is, in part, achieved.

Incongruity can be disguised, and the nature of one kind of disguise can be revealed using another idea from Relevance. Part of the interpretive process involves treating something which should be in the 'foreground' as though it were in the 'background' (Sperber & Wilson 1986:202ff.; for discussion, see Smith 1989, ch. 14). A second example from Woody Allen (Sunshine 1993:255; cited in Curcó Cobos 1997:214) can be used to make the point):

There is no question that there is an unseen world. The problem is how far is it from midtown and how late is it open?

As you process any utterance, you make anticipatory hypotheses about what is coming next: whether a hypothesis is in the **foreground** depends on the syntactic structure of the sentence being uttered, the order in which the constituents are introduced and, crucially, whether it is relevant in its own right. What is in the **background** is then information that can be taken for granted. With the current example, it is obvious that the topic is the supernatural which presumably refers to a domain which is not located in space and time the way the physical world is. Accordingly, when you have got as far as "The problem is ..." you expect to hear next a putative solution to a 'problem' about the supernatural. What you get is an answer which presupposes something of incontestable relevance: it would overturn all our conceptions of the occult if it was true, but something which is presented as though it were just part of common background knowledge: that the unseen world is physically located near midtown.

It is **presented** this way, but the proposition being put forward is not actually being endorsed by the speaker. Another crucial strand in the manipulation of humorous effects depends on the notions of dissociation and metarepresentation. Intuitively Woody Allen is distancing himself from the claim that the unseen world is really accessible from midtown, and is indulging in a degree of (self-)mockery in the way he describes it. This dissociation is in turn dependent on the possibility of using a representation not just to **describe** a state of affairs, but to ***interpret*** (or metarepresent) another representation of some state of affairs. The classic case of such 'interpretive use' is found in irony. If I say "I do admire the president's honesty", it is likely to be clear that I am intending to ridicule anyone defending the proposition that the president is honest, (even if no-one actually has) and that I am simultaneously dissociating myself from such a proposition, perhaps with contempt.

Dan Sperber (1994) has emphasized the surprising complexity of metarepresentation in everyday conversation, arguing that speakers and hearers regularly entertain eighth and ninth order metarepresentations: If John says to Mary that there is an unseen world, then:

John intends — Mary to know — that John intends — Mary to believe — that John believes — that there is an unseen world.

Assuming that Mary is not so credulous, she will construct a representation of the kind:

John says — there is an unseen world, but I believe — there is not an unseen world — So John is mistaken.

If Mary believes that John doesn't really believe there is an unseen world, she will construct the more complex representation:

John says — there is an unseen world, but I believe — that John believes — that there is not an unseen world — So, John is lying.

It is a further step or two on the same progression before we get to the situation where the conclusion is that John is joking or being ironic:

Mary believes — that John believes — that Mary believes — that John believes — that there is not an unseen world — So, John may be joking (or demented).

Curcó Cobos argues that to get a humorous interpretation, a contradictory proposition must be embedded in a metarepresentation of at least fourth order. If she is right, it is not surprising that some poor people consistently miss the point of jokes and witticisms, and we are probably all painfully familiar with situations where we cannot really tell whether someone is joking or not. Some people, specifically those with autism, are in an even worse situation: they are inherently incapable of ever understanding jokes. Autists are customarily supposed to lack a "theory of mind", entailing that they cannot form metarepresentations, they cannot impute beliefs to others and, crucially, jokes are in principle beyond them. This leads to the incomprehension in the face of jokes and pretence that Ianthi Tsimpli and I described (Smith & Tsimpli 1996) in "Putting a banana in your ear" in an earlier issue of this journal.

To return at the end to Woody Allen, it is clear that the quotation I started with was inspired by his admiration for Chomsky (see also, "The Whore of Mensa" in Allen 1975). It is intriguing that the respect is mutual, and Chomsky cites Woody Allen in his most serious work with the same incongruous and slightly humorous effect. For example in *Rules and representations* (Chomsky 1980:117) he refers to the fact that "'The universe is constantly expanding' … the discovery that turned Woody Allen into a neurotic". I leave it to the reader to work out the correct depth of metarepresentational embedding that an appropriate understanding of this remark requires.

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THE SYNTAX OF PAST PARTICIPLES. A GENERATIVE STUDY OF NONFINITE CONSTRUCTIONS IN ANCIENT AND MODERN ITALIAN

by *Verner Egerland*
reviewed by *Yves D'Hulst*

Summary by the author

The thesis from which the below discussion has been extracted attempts to analyze participial syntax. The major aim is to reveal the functional structure that may be projected by a participial verb and to analyze the mechanisms of Accusative Case checking that are operative in the participial clause. A further aim is to work towards a maximally unified theory of Accusative checking in the participial clause and Nominative checking in the finite clause. For reasons of space, this presentation is limited to some ideas concerning the nodes of AgrO and Tense, leaving aside the discussion of Aspect nodes which however occupies an important part of the original thesis. The analysis is inspired by the minimalistic framework of Chomsky (1993) but diverges on some points from the developments put forth by Chomsky (1995), for instance in maintaining the assumption of Agreement nodes. The present analysis is mainly built on a comparison between different historical stages of Italian while modern languages are taken into consideration only to a limited extent. The reason for this is that certain interesting data attested in Old Italian do not appear to be reflected in modern languages, at least to my knowledge.

1. Word order and agreement in 14th century participial clauses

In Italian texts dating up to the mid 14th century, word order in the compound tense is optional, in the sense that the DP object is attested both to the right and to the left of the participial V:

(1)
... come che tu abbi perduti i tuoi denari ...
even if you have lost[masc.pl.] your money[masc.pl.]
(*Decameron*; II:5)

(2)
... co' denari avresti la persona perduta.
with your money you would have your life[fem.sing.] lost[fem.sing.]
(*Decameron*; II:5)

In the relevant period agreement on the participle is highly regular: the participle agrees with the object regardless of where the object appears in relation to the noun — to the right as in (1) or to the left as in (2). In many of the late 13th century texts agreement appears to have been almost exceptionless both in (1) and in (2). From the mid 14th century onwards, the modern word order (1) is established and the construction illustrated in (2) disappears, i.e. (2) disappears *with the meaning of a compound tense*. The word order of (2) may also correspond to a different construction, instantiating *have* of possession followed by a resultative small clause; as such, the word order of (2) is quite grammatical also in Modern Italian, although perhaps not with a predicate like *perdere* 'lose'. At the same time, the mid 14th century, cases where the participle shows agreement with the object, as in (1), become rare and in some texts from the late 14th century are only occasionally attested.

The same pattern is observed in participial absolute clauses (henceforth: PAC); until the mid 14th century, the internal argument of a transitive PAC is found both to the left and to the right of the verb:

(3)
... serrata la cella con la chiave, dirittamente se n'andò ...
locked[fem.sing.] the cell[fem.sing.] with the key, immediately he went away
(*Decameron*; I:4)

(4)
... il re di Francia, molte triegue fatte con gli alamanni, morì ...
the king of France, many truces[fem.pl.] made[fem.pl.] with the Germans, died
(*Decameron*; II: 8)

By the mid 14th century, the word order in (3) becomes established, Verb-Object, while that represented by (4), Object-Verb, begins to disappear from the texts. (3) corresponds to the word order of a transitive PAC in Modern Italian. At the same time, agreement on the participle is lost; the new word order and agreement patterns that are established from the late 14th century onwards are thus illustrated in (5) and (6):

(5) *compound tense:*
... e aver trovato la casa mia...
and having found[–Agr] my house[fem.sing.]
(*I Lucidi*; I:2)

(6) *absolute participle:*
... serrato le finestre se ne andarono...
closed[–Agr] the windows[fem.pl.] they went away
(*Cene*: Introd.)

The correct generalization appears to be that *rightward agreement with a DP object on the participle was obligatory as long as the DP object could appear also in a leftward position*. Participial agreement is lost when the position of the object is established to the right of the participle. These observations have some interesting consequences for the theory of syntax. Consider some of them:

1. Under the assumption that the rightward position of the object is a complement and the leftward one is a specifier, the data lend support to the Spec-head-agreement approach to participial agreement, outlined by Kayne (1989).

2. The fact that a shift in word order is attested in the compound tense and in the absolute participial construction at the same time suggests that the surface change was due to some change within the participial clause; indeed, it is unlikely that the shift from (1)/(2) to (5) had anything to do with, say, a change in Case assigning properties of the governing V *have*, given that a similar shift takes place from (3)/(4) to (6).

3. The fact that (2) and (4) are attested is a sign that Italian in the relevant period maintained SOV tendencies, although SVO is the predominant pattern. A natural way to account for this is to say that the Accusative feature was strong in the sense of Chomsky (1993) and required overt checking, hence, overt movement of DP(O) to SpecAgrO.

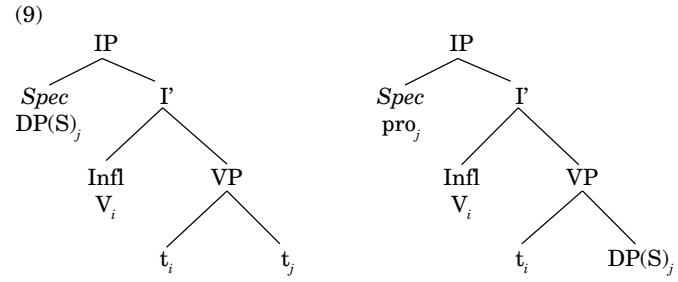
4. In a framework such as that of Chomsky (1993), it is not possible to assume, however, that movement of DP(O) was optional. In order to arrive at a stronger hypothesis, we may then assume that overt object shift as in (2) and (4) was obligatory in Old Italian. As for (1) and (3), I suggest that they should be accounted for on the basis on assumptions that have been common in the discussion on so called free subject inversion in the finite clause (cf. Rizzi 1982, and studies that follow his suggestions). As an example of this type of analysis, let us take a case of subject inversion with an ergative predicate as in (7) and (8):

(7)
Gianni viene
John comes

(8)
Viene Gianni
comes John 'John comes'

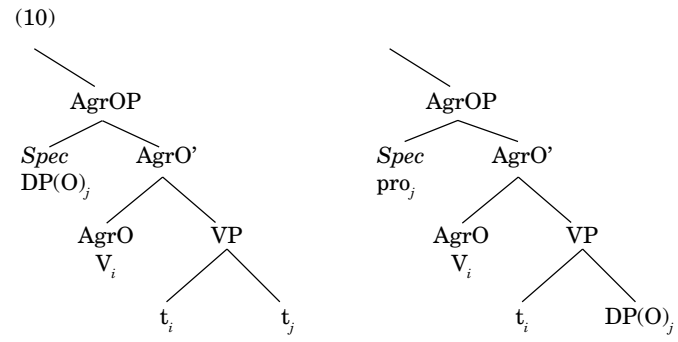
According to the classical analysis, the lexical subject in (8) is copied by an expletive element, *pro*, that occupies the specifier of Infl; cf. Rizzi (1982), (1986) and Chomsky (1986). Consider the structures in (9):

(9)



Let us suppose that this solution should be generalized to the case of Accusative checking in the participial clause. The structures corresponding to (1) and (2) in Old Italian are given in (10):

(10)



The advantage of this analysis is that it allows us to generalize a well-established approach to Nominative Case checking (and its relation to word order and agreement) in finite clauses to the equivalent phenomenon of Accusative Case checking (and its relation to word order and agreement) in participial clauses. The account diverges from Chomsky (1995) insofar as it maintains the assumption that Agreement nodes are projected in the syntax. Moreover, Chomsky (1995, 341) claims that SpecAgrS can host both an expletive and a DP, whereas the specifier of AgrO only hosts a DP. My account treats AgrO and AgrS as perfectly alike in this respect; in other words, expletive-associate dependencies can be both Nominative and Accusative.

This assumption also accounts for other word order variations in Old Italian absolute participle clauses. The modern PAC can host only one lexical argument; the argument carries Nominative if the verb is ergative and Accusative if the verb is transitive. In the ancient texts two lexical arguments in PAC are often attested:

(11) *Modern Italian:*
*Conosciuta Elena Gianni, ...
known Helen John, ...

(12) *14th century:*
ed Enea presa la lancia, con grande vigore uno si levò contro l'altro ...
and Enea taken the lance, with great force one stood up against the other ...
(*I fatti di Enea*; LVII)

Word order in the Old Italian PAC with two arguments could be VOS, SVO, and VSO; I have not attested *SOV, *OSV, or *OVS. The generalization that emerges is thus that *when there is a subject carrying Nominative, the verb must be to the left of the object*. These patterns are explained on the above hypothesis in terms of expletive chains. If the absolute participial clause can Case mark two lexical arguments, the conclusion in the minimalistic framework of Chomsky (1993) is that both AgrS and AgrO are projected in the absolute clause in the Old Italian PAC. If, then, pro may be licensed in both SpecAgrS and SpecAgrO, thus allowing the DP associates to remain in situ, the following possibilities are predicted:

1. VOS: DP(O) moves overtly to SpecAgrO; DP(S) stays in situ, being copied by an expletive pro in SpecAgrS; the verb raises overtly to AgrS.

2. SVO: DP(S) moves overtly to SpecAgrS; V raises to AgrS; DP(O) moves overtly to SpecAgrO or stays in situ, being copied by an expletive pro in SpecAgrO.

3. VSO: The verb raises overtly to SpecAgrS; DP(S) and DP(O) stay in situ, both being copied by expletive pros in SpecAgrS and SpecAgrO respectively.

If Nominative is to be checked in the clause, I assume that the verb must reach AgrS. It then follows that, if there is a Nominative argument to Case check, the verb must raise higher than the object that does not move beyond SpecAgrO.

2. The relevance of Tense for structural Case checking

The fact itself that only one lexical argument is allowed in the Modern Italian PAC, whereas the ancient construction could host two, is of course interesting. This difference is presumably related to another property of absolute participles: in Modern Italian, PAC cannot host negation. In the ancient texts, cases of negated absolute participles are attested:

(13) *Modern Italian (from Belletti 1990, 95):*
*Non arrivata Maria, Gianni tirò un sospiro di sollievo.
not arrived Mary, John was relieved

(14) *14th century:*
... , il papa non dimenticato lo sdegno preso contro alla parte bianca di Firenze,
... , the pope not forgotten the resentment against the white part of Florence,
non volle che soggiornasse e vernasse invano.
did not want him to stay and spend the winter in vain.
(*Cronica*; VIII: 49)

According to Zanuttini (1991) negation depends on the presence of Tense. Belletti (1990) has argued that the Modern Italian PAC lacks a Tense node. If by contrast in the Old Italian PAC such a node is present, the difference between (13) and (14) can be accounted for straightforwardly. A further assumption made by Belletti is that in the Modern Italian PAC the absence of Tense forces the verb to raise to Comp. Thus, for Belletti, V-raising to Comp is necessary for Case reasons, under the assumption that a tense feature is generated in Comp. Now, it is clear that in Old Italian the participial V did not raise as far as to Comp. This can be seen from the fact that, whereas a lexical argument cannot cooccur with a filled Comp in Modern Italian, this was indeed possible in Old Italian:

(15) *Modern Italian (from Belletti 1990, 99):*
*Benché partita Maria, ci divertimmo ugualmente.
although left M., we amused ourselves anyway

(16) *14th century:*
... dicono li soprascritti savi che, bene che quelli cotali uomini diventati animali, ...
the above mentioned learned say that, although those men become animals, ...
la mente dentro rimaneva loro umana, ...
their mind inside remained human
(*I fatti di Enea*: XXVII)

According to a line of thought in generative grammar, Tense is crucial not only for Nominative Case but also for Accusative Case (see, among others, Belletti 1990, Watanabe 1993, and Hoekstra & Roberts 1993). In the framework of Watanabe, the checking of structural Case against a functional head must be followed by overt raising of the same head to a higher functional head. For the participial clause, I assume the functional structure argued for by Kayne (1993):

(17)
... $[_{CP}$ C $[_{AgrSP}$ AgrS $[_{TP}$ T $[_{AgrOP}$ AgrO $[_{VP}$ V]]]]]

The checking of Accusative in AgrOP must be followed by raising of AgrO to T, and the checking of Nominative in AgrSP must be followed by raising of AgrS to C. Without entering into the details of the so called layered Case theory (see Watanabe 1993 and Chomsky 1995, 385 f.n. 49), it is clear that the comparison between absolute participles in Old and Modern Italian lends support to the general thesis. If in Modern Italian there is a Comp node but not a Tense node, it follows that only one structural Case can be checked; either Accusative, if the verb is transitive, or Nominative, if the verb is ergative. If in Old Italian both T and C are projected, it follows that structural Case in such a grammar can be checked on two arguments.

Finally, in Modern Italian the pattern illustrated in (6) above is replaced by (18):

(18)
Chiuse le finestre, se ne andarono.
closed[fem.pl.] the windows[fem.pl.], they went away

Thus, agreement on the participle again becomes obligatory although the lexical argument appears to the right of the participle. This is explained on the assumption that verb and object agree in AgrOP, after which the verb obligatorily raises beyond AgrOP to Comp. Belletti (1990) advances this hypothesis for the ergative PAC in Modern Italian. If, however, the solution is generalized to the transitive PAC (which has been suggested by Kayne 1989 and Cinque 1990), both transitive and ergative absolute participle constructions are derived from the same hypothesis.

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Review by Yves D'Hulst

1. Introduction

Egerland's dissertation gives a detailed and interesting survey of agreement and word order patterns in past participle constructions, both clausal and absolute, in medieval, renaissance and modern Italian. The comparative scope of the study is extended to other Romance languages (especially Spanish) and to Germanic (Scandinavian) languages. Egerland tackles virtually all aspects that one might think related to his main issue: the correlation between word order and agreement, the assignment and checking of case, expletives, affectedness, aspectual interpretation, adjectival phrases, possessive interpretation, functional structure, etc. One important topic is left out of the overall discussion: the possible causes of historical change, although some suggestive speculations are put forward.

In this review, I will concentrate on three issues. The first one relates to the data and is purely philological. The other two focus on the bulk of Egerland's dissertation: the interaction of case, word order and agreement in compound tenses and the syntax of participial absolute clauses.

2. The data

Because Egerland deals with older stages of Italian, his data are necessarily corpus-based. Throughout his dissertation, Egerland treats his data with philological care, meticulously basing his analyses only on examples that can be interpreted correctly without having to appeal to intonation or punctuation.

However, one critical philological note is in order concerning one of the central issues of this study: agreement of past participles. As the author points out, the quantitative data show that past participle agreement was the rule until the first half of the 14th century, independently of word order. Subsequently, agreement became more marginal whenever the object followed the participle. In both periods of time, there appears to be some variation among authors and exceptions are occasionally found.

The problem with this generalization, correct as it may be, is that a large amount of ancient Italian literature, which this corpus is built upon, did not reach this century straight-away. Frequently, the original autographs got lost and modern critical text-editions are based on copies dating from the 15th or 16th centuries. Considering that philological concern about text editions is a fairly recent attitude and that renaissance copyists had little reluctance manipulating the original texts at will, one might wonder whether the changes that Egerland has detected in the late 14th century texts actually took effect at that time. This doubt affects at least three out of five texts in Egerland's corpus for the relevant period

(*Il Pecorone*, *Il trecentonovelle* and *Il novelliere*): exactly the three texts where both OV order and agreement in VO construction score extremely low.

Of course, even if the linguistic change were to have occurred at a later stage, the fact remains that it occurred and that one has to account for it.

3. Case and agreement in compound tenses

Egerland's analysis of case checking in the three stages of Italian involves two parameters: one concerning the strength of the case feature on the noun and one concerning the **locus** of case checking. Generalizing, the first parameter governs movement of the object, the second governs agreement of the past participle.

To be more precise, a language where accusative case is strong, requires the object to move overtly to a position where the case feature can be checked. According to Egerland's study, medieval Italian is such a language, but neither Renaissance Italian, nor modern Italian are. as for the locus parameter, languages chose to check case in AgrOP or in a functional projection below AgrOP, namely Asp(ect)P. With the first setting, past participle agreement is obligatory whenever the object passes through SpecAgrOP. This is always the case for medieval Italian where the accusative case feature is strong. With a different setting for the strength parameter, one variety of renaissance Italian still triggers agreement if movement of the object is forced on independent grounds, namely in operator contexts (e.g. *wh*-interrogatives, relatives). If, on the other hand, the locus for case checking is lower down in the structure, past participle agreement is not triggered. This is the case for the other variety of renaissance Italian and for modern Italian as well. The different settings for the accusative case parameters are summarized in (1) and their effects in (2).

Egerland's analysis is certainly very convincing from a descriptive point of view and has a number of interesting consequences from a comparative point of view. Yet some theoretical issues still are in search for a satisfactory explanation.

3.1. Choice of parameters

From a comparative point of view, one expects the number of possible grammars to be coextensive with the number of settings of these two accusative case parameters. More specifically, one could imagine a language where Accusative is strong and the locus for checking is not AgrOP but AspP, i.e. a language with overt movement of the object, but no agreement of the past participle. As Egerland points out, none is to be found in the Romance area, but Germanic languages with scrambling of the object could very well instantiate this setting.

3.2. Case assigning property

On several occasions, Egerland notes that his analysis of accusative case closely resembles the traditional view on nominative case, a welcome result. One issue where nominative and accusative are argued to be alike is precisely the locus parameter. Just like nominative can be checked either in SpecTP or in SpecAgrSP, languages may choose to check case in SpecAspP or in SpecAgrOP. However, the parallelism breaks down in one respect: the choice for the checking of nominative case is between the specifier position of the projection with the case-assigning feature, T, and the specifier position of the agreement projection of T. For accusative case, things seem somewhat different: in the standard account, the category with the case-assigning feature is V and the choice for checking is between the specifier positions of two other categories, Asp and AgrO. The problem thus reduces to the following: is there a principled way to account for the possible checking sites of case? If both AgrO and Asp can check accusative case, why can't other (functional) categories as well?

A possible way to answer this question, would be to resort to head movement: case can be checked in the specifier position of whatever functional projection that hosts a case-assigning property, either inherently (T) or through movement (AgrS, AgrO, Asp). This hypothesis implies that the choice for accusative case checking in participle constructions is restricted to the specifier positions of the functional categories to which the participle is attracted, but not further. Under this view, it remains unclear, however, how one can prevent accusative to be checked higher up in the functional structure of non participial clauses.

There is, of course, an equally reasonable and less problematic alternative. In the literature, it has been suggested that past participles do not have a case assigning property (see among others Roberts 1987). One could imagine, then, that Asp is endowed with a case assigning feature supplementing the defective participial V. Under this view, the parallelism with nominative case would be fully restored and the possible checking sites for case would be restricted in a principled way: case is checked in the specifier position of the projection with the case-assigning feature (T, Asp) or in the specifier position of its agreement projection (AgrS, AgrO).

3.3. Null expletives

Finally, let us go back to the summary in (1) and the settings of medieval Italian: accusative needs to be checked overtly and the *locus* for checking is SpecAgrOP. With these settings, one expects, of course, the object always to move overtly to SpecAgrOP. Assuming Egerland's view that the past participle raises only as far as AgrO, one also expects the object always to precede the past participle, as in (3a). However, even in medieval Italian, the predominant pattern is the one where the object follows the past participle, as in (3b) (in Egerland's corpus there are 99 cases with VO order and 30 with OV).

(3)
a. ... co' denari avresti la persona perduta.
with your money you would have your life lost
(*Decameron* II:5)
b. ... come che tu abbi perduti i tuoi denari ...
even if you have lost your money
(*Decameron* II:5)

Egerland's account of the pattern in (3b) is similar to what has been proposed in the literature for subject inversion (see Rizzi 1982): case is licensed through a chain headed by an expletive. So once again, there appears to be a striking similarity between the properties of nominative and accusative DP's. The question, however, is whether expletives of the kind can still be maintained in the minimalist framework Egerland adopts. The hypothesis that case can be checked through an expletive in SpecAgr(O)P seems to completely undermine the idea that some features have to be checked in overt syntax. This is so because the expletive would only carry those features that need overt checking. In other words, languages could have the option to carry out in overt syntax what other languages do in LF: checking of features by moving only the features, not the lexical category they are associated to.

(1)

	Accusative case parameters	Medieval Italian	Renaissance Italian Type 1	Renaissance Italian Type 2	Modern Italian
a.	Strength	yes	no	no	no
b.	Locus of checking	AgrOP	AgrOP	AspP	AspP

(2)

	Agreement	Medieval Italian	Renaissance Italian Type 1	Renaissance Italian Type 2	Modern Italian
a.	Ordinary DP's	yes	no	no	no
b.	Operators	yes	yes	no	no

It seems worthwhile to consider the alternative where the DP overtly moves to the case-checking position (SpecAgr), both for subjects and objects (4a). Under this view the difference between OV and VO (or SV and VS) results from subsequent movement of either the DP, the verb, the XP containing the verb, or a combination of the preceding:

(4)
a. $[_{XP}$ Spec X $[_{YP}$ Spec Y $[_{AgrP}$ DP$_i$ V$_j$-Agr [... [(t$_i$) t$_j$ (t$_i$)]]]]]
b. $[_{XP}$ Spec X $[_{YP}$ Spec V$_j$-Agr $[_{AgrP}$ DP$_i$ t$_j$ [... [(t$_i$) t$_j$ (t$_i$)]]]]]
c. $[_{XP}$ Spec X $[_{YP}$ DP$_i$ V$_j$-Agr $[_{AgrP}$ t$_i$ t$_j$ [... [(t$_i$) t$_j$ (t$_i$)]]]]]
d. $[_{XP}$ $[_{AgrP}$ t$_i$ V$_j$-Agr [... [(t$_i$) t$_j$ (t$_i$)]]]$_k$ X $[_{YP}$ DP$_i$ Y t$_k$]]

The alternation between structures (4b) and (4c) is reminiscent of what has been argued for in the literature on the interaction between scrambling and definiteness in Germanic. The alternation between (4a) and (4d) recalls recent proposals on subject inversion and should be evaluated with respect to information packaging of the clause.

4. Participial absolute clauses

Modern Italian participial absolute clauses (henceforth PAC) allow the inaccusative subject and the transitive object to be expressed overtly (see Belletti 1990, from which the examples below are drawn):

(5)
a. Arrivata Maria, Gianni tirò un sospiro di sollievo
arrived Maria, Gianni was relieved
b. Conosciuta Maria, Gianni ha subito cambiato il suo stile di vita
known Maria, Gianni has immediately changed his lifestyle

Both construction types, display two curious properties. Constructions like (5a) impose a strict VS order, witness (6a). Furthermore, they challenge the fairly standard view that only finite sentences have the possibility of licensing nominative case. Constructions like (5b), on the other hand, impose agreement with the object *in situ*, a property which is lacking in ordinary auxiliary + past participle constructions in present-day Italian, and they do not allow the (transitive) subject to be expressed overtly, witness (6b).

(6)
a. * Maria arrivata, Gianni tirò un sospiro di sollievo
b. * Conosciuta Gianni Maria, Federica si è sentita tradita
known Gianni Maria, Federica felt herself betrayed

According to Belletti (1990), absolute past participles only license case if they move to C, either overtly or covertly. This hypothesis straightforwardly accounts for the word order facts observed in (5–6a) and for the restriction on the overt realization of the subject in (6b).

With this background in mind, Egerland considers PAC's in medieval, renaissance and modern Italian, showing that both the word order restriction in inaccusative PAC's and the subject restriction in transitive PAC's have dramatically changed during the ages, as shown in the summary in (7).

(7)

	Restriction	Medieval Italian	Renaissance Italian	Modern Italian
a.	Strict V1	no	no / yes	yes
b.	Overt S and O	yes	yes	no

Egerland's account of the different settings of medieval, renaissance and modern Italian with respect to the word order property in (7a) is based, as expected, on the assumption that there are differences in verb-raising between the three grammars. Egerland adopts Belletti's hypothesis that there is a very strong tendency towards verb-raising to C in modern Italian. As for medieval Italian, he assumes that the verb only moves to Agr. This hypothesis is supported by the fact that (structural) subjects can precede the verb while (structural) objects cannot. Interestingly, renaissance Italian appears to require the verb to move to a position between Agr and C: while (structural) subjects generally follow the verb, as in modern Italian, pronominal subjects precede the verb,

a pattern which is reminiscent of the variation Poletto (1993) has observed in the clausal syntax of northern Italian dialects. An interesting consequence of Egerland's analysis is that there appears to be a gradual shift in the raising options of the verb form medieval to modern Italian, a fact which, both from an empirical and a theoretical point of view, deserves more attention.

Egerland assumes that the properties in (7b) follow from a difference in the functional structure of PAC's in medieval and renaissance Italian with respect to modern Italian. More specifically, he assumes that, in the older periods, PAC's had a fully fledged functional structure, including a Tense projection, and that modern Italian lacks (at least) this Tense projection. As a consequence, medieval and renaissance Italian have two case assigners, T and V, while modern Italian only has one, V; hence, medieval and renaissance Italian license two overt DP's (see (12) in the author's summary) and modern Italian only one. The hypothesis that TP has disappeared from modern Italian PAC's is well supported: several phenomena that, in one way or other, are related or have said to be related to the presence of TP are attested in both medieval and renaissance Italian, but are typically lacking in modern Italian. Perhaps the clearest case is that of negation: possible in medieval and renaissance PAC's (see (14) in the author's summary), but not so in modern Italian.

Egerland's proposal that modern Italian absolute past participles exhibit a strong tendency towards V-to-C raising and lack a tense projection is close in spirit to what Belletti (1990) argued for. However, Egerland's and Belletti's proposal differ in two fundamental respects: the account of obligatory past participle agreement and the mechanism of case assignment (checking).

In Belletti's account, the two issues are tightly related: agreement is required in order to make case available under government: either nominative for the subject of inaccusative verbs (5a) or accusative for the object of transitive verbs (5b). In Egerland's account, the link between agreement and case assignment / checking is preserved only for inaccusative verbs, i.e. for the checking of nominative of the subject. As for the object of transitive verbs, Egerland maintains the reasonable claim that accusative need not be checked overtly. As a consequence, the object should be able to remain in situ and past participle agreement should not be available. This conclusion appears to be correct for renaissance Italian but not for modern Italian.

Egerland's solution to the agreement paradox in modern Italian is both innovative and appealing. His analysis is based on the following assumptions: (i) past participle constructions like (5b) are always passive; (ii) passive predicates require agreement with their internal argument and (iii) absolute past participle constructions always project a delimitedness feature (AspP), which overtly checks a nominal feature. As a consequence, the inner object will move to SpecAspP in order to check the nominal feature of Asp_{+del} and move further up to SpecAgrOP to insure agreement with the passive predicate.

(8)
$[_{AgrOP}$ SpecAgrO $[_{AspP}$ SpecAsp$_{+del}$ $[_{VP}$ V NP]]]

The assumption that PAC's involve a delimitedness projection in modern Italian but not in earlier stages has a very welcome consequence: it explains the fact that PAC's in medieval and renaissance Italian are not subject to specific lexical restrictions while they are in modern Italian: only verbs which are compatible with a delimited interpretation seem to be admitted in PAC's, witness the following contrast:

(9)
a. Finita la casa, l'architetto andò in pensione
finished the house, the architect retired
b. * Temuta la sconfitta, il generale rinunciò al suo piano
feared the defeat, the general give up his plan

The drawback of this assumption is that in Egerland's framework the projection of the delimitedness feature implies lexical suppression of the external theta-role, hence a passive analysis for constructions like (5b). A passive analysis seems not quite adequate, considering that PAC's of transitive verbs can contain an anaphor, as in (10) (from Egerland 1996: 262).

(10)
Elogiata solo se stessa, Maria restò del tutto isolata
praised only herself, Maria remained completely isolated

Egerland argues that examples like (10) need not constitute counterevidence against a passive analysis and that the anaphor could be bound directly by the matrix subject. However, the argumentation does not seem quite satisfactory. One expects a short distance anaphor like *se stesso* to be bound within the PAC, as the interpretation of sentences like (10) suggests: the only available interpretation for (10) is the one where some person x, *in casu* Maria, praised him- or herself, not that some arbitrary person praised someone else. Note, furthermore, that even in Reinhart & Reuland's (1993) approach to binding, one cannot account for the properties of (10): if a reflexive predicate can lexically satisfy binding requirements, one would not expect (quasi-)obligatory coindexation of the anaphor with the matrix subject.

5. Conclusion

There is little doubt that the data Egerland has worked with, offer interesting contrasts in the participial syntax of three stages of Italian. This holds both for the puzzling data of participles in compound tenses and for those in absolute clauses. Because of their complexity, the data constitute a strong test case for the descriptive power of a linguistic theory and Egerland undoubtedly has gone a long way towards showing the minimalist program's qualities of managing such complex data. At times, it is true, the analyses give the impression that the theory is there to play with; and perhaps the main reason for this is the fact that Egerland did not undertake any general attempt to search for the causes of variation in the three stages of Italian. This most certainly would have offered a better basis for evaluating some of his proposals and could have strengthened the theoretical underpinnings of his analyses. Nevertheless, the overall picture remains that of a very solid descriptive work where the various word order phenomena and agreement patterns nicely fall into place.

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DIRECT OBJECT SCRAMBLING IN DUTCH AND ITALIAN CHILD LANGUAGE

by *Jeannette Schaeffer*
reviewed by *Astrid Ferdinand*

Summary
by *the author*

Over the past decade many theories of first language acquisition have utilised the concept of "Parameter Setting and Resetting" to explain linguistic development (cf. Hyams 1983; 1986 among others). Despite the fact that the idea of Parameter Setting and Resetting is conceptually attractive, it turns out that, to the extent that we can formulate parameters such as the "Head-final/Head-initial Parameter or the Pro-drop Parameter, they are set extremely early, usually before the onset of speech. Therefore, language development cannot solely be explained by Parameter Setting and Resetting. The question then remains as to how development should be accounted for. A key to solving the so-called 'developmental problem' can be found in the 'Modularity Hypothesis', as first proposed by Fodor (1982). According to the Modularity Hypothesis the language faculty is divided up into a lexicon, a computational system (syntax, semantics, phonology), a pragmatic system and possibly more modules. Uneven development or growth within the separate modules and the interaction between them can explain why children's grammars develop the way they do.

In this thesis I argue that development within the pragmatic component of language causes development within syntax. The main hypothesis is formulated in (1):

(1) *Hypothesis*
(i) specificity is not always grammatically marked in the grammar of 2-year old children;
(ii) (i) is possible because 2-year olds lack pragmatic Discourse Rule.

In order to test this hypothesis I chose two syntactic processes which are purported to involve specificity in the literature about adult language, namely object scrambling and object clitic placement in Dutch and Italian child language. I argue that while object scrambling and object clitic placement are close to adultlike by age 3, at age 2 object scrambling and object clitics are optional even where they are obligatory for adults.

I carried out an elicited production task with 49 Dutch and 35 Italian children between the ages of 2 and 6. The Dutch children were tested on their knowledge of object placement with respect to negation and adverbs. The Italian children were tested on their knowledge of object clitic placement in present tense and *passato prossimo* (present perfect) constructions. The main results indicate the following. Dutch 2-year olds hardly use any adverbs, but fail to move specific objects over negation 70% of the time (though this movement is obligatory for adults). Furthermore, Italian 2-year olds omit object clitics 64% of the time in obligatory contexts. Dutch three year olds fail to scramble specific

objects over negation at a rate of only 28%. Italian three year olds omit overt object clitics in obligatory environments at a rate of 15%.

I assume the following sentence structure for Dutch and Italian (DiscP = Discourse Phrase; SpP = Specificity Phrase'):

(2)
[CP [AgrSP [DiscP (adverbs) [TP [SpP [NegP *(Dutch)* [AgrOP [VP]]]]]]]]

The Dutch 2-year olds' lack of object scrambling over negation (70%) and the Italian 2-year olds' dropping of overt object clitics (64%) provide evidence for the hypothesis that children's nominal expressions are optionally marked with respect to specificity, which is elaborated on below.

In the adult grammar scrambling and clitic placement are driven by specificity. Sportiche (1992) proposes a unification theory of object clitic placement (in Romance) and object scrambling (in Dutch), in which specific objects (XPs) and clitics (Xs) need to be licensed under spec-head agreement in a functional phrase (SpP in (1)) just below TP. I extend this analysis and claim that there are two types of specificity: 'discourse-related specificity' and 'non-discourse-related specificity'. Discourse-related specific object DPs must have an antecedent in the discourse and are licensed in SpecDiscP; non-discourse-related specific object DPs are licensed in SpecSpP. Object clitics (inherently specific) are base-generated in the head of SpP and licensed by a *pro* object (specific because of its pronominal character) that has moved from its VP-internal position to SpecSpP. Specific overt object XPs move from their VP-internal position to SpecSpP and are licensed by an inherently specific null clitic, base-generated in the head of SpP. This is illustrated for Italian (3a) (underlying structure of clitic placement) and for Dutch (3b) (scrambling):

(3)
a. [AgrSP Gianni [TP [SpP pro_i [Sp la [AgrOP [VP mangia t_i]]]]]]
John it- eats
'John is eating it'
b. dat [AgrSP Jan [DiscP [TP [SpP het $boek_i$ [Sp 0clit [niet
that John the book not
[AgrOP [VP t_i heeft gelezen]]]]]]]
has read
'that John didn't read the book'

I propose that in (2a) the finite verb *mangia* incorporates the clitic when it head-moves to AgrS in order to check its Tense and Agreement features, so that *la+mangia* end up in AgrS. Furthermore, the Dutch object *het boek* in (2b) moves on to SpecDiscP if it is discourse-related.

Returning to the experimental results. I propose that a nominal expression is marked for specificity only if the distinction between discourse- and non-discourse-relatedness is made. If this distinction is not made, (the syntactic feature) specificity is lacking. If the object (whether overt or *pro*) is not marked for specificity, it cannot move up to SpecSpP where specificity is licensed under spec-head agreement. In the case of an overt object, this will result in a 'non-scrambled' object, i.e. one which remains below NegP. In the case of a *pro* object, this will result in object clitic-drop: the specificity of the clitic cannot be licensed, and therefore the clitic is not spelled out.

Although the analysis just proposed accounts for the syntactic differences between early child grammar and adult grammar, it is not immediately clear how children interpret object DPs without a [+specific] feature. I argue that these object DPs are interpreted referentially, just as in adult language, but that children arrive at this referential interpretation in a different way than adults. Let us first consider the adult interpretive mechanisms.

As is shown in (4), I follow Hyams (1994) and Hoekstra and Hyams (1995) in the claim that every DP contains a D-chain, coindexing the Noun, the Determiner and an operator in SpecDP, which makes referentiality visible.

(4) *D-chain*

$[_{DP}$ Op_i $[_{Di}$ the $[_{NumP}$ $[_{NP}$ $[_{Ni}$ tree]]]]

Furthermore, I propose that there is an R(eferentiality)-Chain, which coindexes the object DP with an operator in SpecCP, which hooks up the object DP to the relevant antecedent in the discourse. This is illustrated in (5):

(5) *R-chain*

the tree [CP Op_i ... $[_{DiscP}$ $[_{DP}$ Op_i ... [D_i **the** [N_i **tree**]]]...$[_{SpP}$...]]]

This mechanism — which I refer to as 'grammatical interpretation' — accounts for the referential interpretation of 'normal' definite DPs, such as *the tree, the girl, the book*.

On the other hand, the much smaller, and more exceptional class of definite DPs such as *the sun, the president, John* refer to their referents in the (model of the) world directly, that is, without an R-chain, as illustrated in (6):

(6) *No R-chain*

[CP ...$[_{DiscP}$ ...$[_{SpP}$ $[_{DP}$ Op_i ... [D_i **the** [N_i **sun**]]]...]]]

This mechanism does not make use of an R-chain and a linguistic antecedent, and therefore it is referred to as 'direct interpretation'.

Returning to the child data, I propose that 2-year old children have both options to interpret definite DPs such as *the tree*. This is illustrated in (7) and (8), respectively.

(7) *Grammatical interpretation*

the tree $[_{CP}$ Op_i ... $[_{DP}$ Op_i [D_i **the** [N_i **tree**]]]

(8) *Non-grammatical direct interpretation*

$[_{DP}$ Op_i [D_i **the** [N_i **tree**]]]

If they choose to specify specificity on the DP, interpretation takes place in an adultlike fashion, as in (7). However, if the [+specific] feature is absent, the DP is interpreted directly, as in (8), similar to the way adults interpret DPs such as *the sun*.

Now why do 2-year old children have access to both interpretive options for DPs such as *the tree*, as opposed to adults only one? I claim that this is due to a missing rule in the child's pragmatic system, namely the 'Discourse Rule', which is stated in (9):

(9) *Discourse Rule*
Preceding linguistic discourse and knowledge of interlocutor MUST be taken into account.

If children do not have the Discourse Rule, they do not always take the discourse into account. This implies that in these cases they do not distinguish between discourse-related and non-discourse-related, resulting in the absence of the [+specific] feature. Consequently, movement to either SpP or DiscP is not prompted and does not take place.

In contrast, adults, who do have the Discourse Rule, always take discourse into account and thus always distinguish between discourse-related and non-discourse-related, resulting in the marking of specificity, and thus movement of the [+specific] DP to SpP or DiscP.

Concluding, I have shown that Dutch and Italian 2-year old children optionally mark specificity, resulting in optional object scrambling and optional object clitic placement, but that by the age of 3 children perform roughly adultlike in this respect. I furthermore argued that this development within syntax is due to the acquisition of a rule in the pragmatic system, namely the Discourse Rule. This provides support for an acquisition theory that exploits the Modularity Hypothesis to explain language development.

Topics that remain for future research include the following: a) can the proposed analysis of the behaviour of [+specific] *objects* also account for the behaviour of [+specific] *subjects* in child and adult language? and b) is it true that 2-year old children do not distinguish between discourse-related specific DPs such as *the tree,* and non-discourse-related specific DPs such as *the sun,* as is predicted by the analysis proposed here.

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Review
by *Astrid Ferdinand*

1. Introduction

Three different hypotheses have been put forward in the literature concerning the amount of syntax the human child disposes of when starting to create his or her first multi-word utterances. The Lexical Thematic Hypothesis claims that there is no syntactic structure. According to the Full Competence Hypothesis the opposite is true: the complete structure is available. The Partial Competence Hypothesis occupies a position between these two extremes.

In Schaeffer's dissertation, the Full Competence Hypothesis is defended, in order to explain young children's linguistic behavior with respect to direct object scrambling in Dutch and direct object clitic placement in Italian. I will discuss Schaeffer's methods of research, her analysis of these phenomena in two-year-old Dutch and Italian children and I will try to determine to what extent this analysis supports the Full Competence Hypothesis.

2. Methods

It is known from earlier literature that, in contexts triggering scrambling in adult Dutch, young Dutch speaking children do not scramble (Hoekstra and Jordens 1994) or optionally scramble (e.g. Barbier 1993, Schaeffer 1995) the direct object. Schaeffer remarks that the empirical basis of these claims suffers from several weaknesses. To obtain more and more reliable data, Schaeffer tested children from 2 to 6 years old, with an elicited production task.

The tests were designed and carried out with care. Since the researchers visited the children's day-care center or school repeatedly before the actual testing began, the children were familiar with them and with the toys used in the tests. According to Schaeffer, they performed the tasks with pleasure. This is an important detail, since it shows that although the scrambling/non-scrambling sentences seem to be rather complex for children below six, the children were apparently not forced to perform above their level. The reliability of the tests was increased because the children's results were compared with the performance of adults on the same tests.

3. Analysis: specificity and discourse relatedness

3.1. Specificity

Schaeffer obtained two major results. First, in Dutch two-year-olds, direct object scrambling does not take place in about 70% of the obligatory contexts. Secondly, Italian two-year-olds leave 64% of the direct object clitics empty. Schaeffer's analysis of these results is based on the following assumptions about adult language:

– Phrase structure contains a Discourse Phrase and a Specificity Phrase: [DiscP [SpecP [NegP [AgroP [VP]]]]].
– Scrambled direct objects (higher than negation) move because of the feature [+specific] to the specifier of Specificity Phrase. The feature [+specific] is the syntactic expression of referentiality.

– Scrambled direct objects move further to the specifier of Discourse Phrase to be related to the discourse.
– Direct object clitics are inherently specific and are generated in the head of Specificity Phrase.

In Schaeffer's analysis, there are two important differences between child language and adult language. In the first place, specificity is optionally marked in child language. This explains utterances as the following:

(1)
Bert gaat niet de peer opeten (C. 2;9)
Bert goes not the pear up-eat
'Bert is not going to eat the pear.'
Schaeffer p. 57, ex. (1.e)

(2)
Ernie gaat niet twee bomen uitknippen (M. 2;4)
Ernie goes not two trees out-cut
'Ernie is not going to cut out two trees.'
Schaeffer p. 59. ex. (2.a)

Without focus on any constituent, sentence (1) is excluded in adult Dutch, since it contains a definite, hence [+specific] unscrambled direct object. Schaeffer argues that in this child utterance, the direct object is unmarked for specificity, despite its definite determiner. Similarly, the indefinite unscrambled direct object in (2) would be unmarked for specificity, even if it has a referential interpretation.

In the second place, Schaeffer argues that the child is able to interpret DPs "directly" instead of by movement to SpecDiscP. This explains why the direct objects in (1)–(2) do not move to SpecDiscP either.

I would like to discuss what kind of development is predicted by this analysis of (1)–(2). Schaeffer explains the development in syntax by a development in pragmatics, which causes discourse relatedness, and hence specificity to become obligatorily marked. One may wonder whether this excludes language-specific development or not. Note that the child's grammar differs from the adult's in the following ways.

In Schaeffer's analysis, a DP in adult language that is referential, semantically, always has the feature [+specific]. For the child, however, this does not hold. The child may express referentiality with a [+specific] D and then scramble the direct object DP. S/he also has the option not to express it at all (as in (1)–(2) and then leave the direct object below negation. This means that in child language the interface between semantics and syntax must be different. This is the first difference. This difference between child and adult language has impact on development. During the development toward the adult state, the child must acquire the obligation of expressing referentiality syntactically. This seems to be possible by acquiring language-specific features, such as those that are related to scrambling. The child has to acquire the ways in which referentiality is expressed in syntax (e.g. scrambling) and in morphology (e.g. determiners). Summing up, the acquisition facts cannot be explained by development in pragmatics only. There must be language-specific development with respect to specificity.

The second difference is related to the status of Specificity Phrase in the grammar of the two-year old. If the specificity feature is not marked, as would be the case in (1–2), the feature on the head of Specificity Phrase cannot be checked. As a result, Specificity Phrase cannot be projected, since if it were projected, the derivation would crash (Chomsky 1993). The fact that the child does not project Specifity Phrase in context requiring this projection in adult language pleads against Full Competence. Yet, there are still the 30% utterances in which scrambling **does** take place. In the representation of these utterances, Specificity Phrase must be present, if they are analyzed as containing the feature [+specific]. Schaeffer uses the optional presence of scrambling as an argument for Full Competence. I will review this argument in section 4.

Another question the optionality of specificity raises is how it affects the form of DPs in the child's output. Specificity is expressed on the D-head of a referential nominal expression. The overt definite (or the specific indefinite) determiner is taken to be the morpho-syntactic expression of specificity (p. 21–22). If specificity is optional at an early stage, there are, in principle, two ways in which this optionality can affect the child's output. The child may either leave the D-head phonologically empty, or have a lexicalized D that is unmarked for specificity. Schaeffer's analysis allows both options. On the one hand, the optional spell-out of specificity is argued to lie at the basis of the optional phonological realization of direct object clitics by Italian two-year-olds. On the other hand, Dutch children phonologically realize definite (and specific indefinite) determiners that are argued to be unmarked for specificity. Since direct object clitics and definite determiners are both inherently [+specific] in adult language, it is not immediately clear why the child would treat them differently. Why do direct object clitics have to be phonologically empty when they are unmarked for specificity, whereas this does not hold for definite determiners? An explanation of these differences would strengthen the analysis considerably.

3.2. Discourse relatedness

Schaeffer's data contain several interesting facts that support the claim that children's discourse differs from adult's discourse. The fact that there are differences confirms earlier findings. Children differ from adults in that they use pronouns without first establishing a referent for that pronoun by using a DP (e.g. Karmiloff-Smith 1979). Further, they use definite DPs instead of indefinite DPs, which is confirmed by Schaeffer's results (Fig. VI, p.73).

Schaeffer finds another difference. Her Italian and Dutch subjects below six produce much more full direct objects than adults:

Schaeffer p. 62, Figure III
Italian children

	overt clitic	omitted clitic	full direct object
2	22% (22)	64% (63)	14% (14)
3	62% (179)	15% (43)	23% (68)
4	89% (237)	0% (0)	11% (28)
5	91% (227)	0% (0)	9% (23)
adults	100% (439)	0% (0)	0% (0)

Schaeffer p. 64, Figure IV
Dutch children

	full object	dem. pron.	strong pers. pron.	clitic
2	52%	26%	6%	16%
3	15%	26%	1%	58%
4	7%	7%	0%	86%

Schaeffer explains the Dutch data by the assumption that the direct object clitic remains empty because of the absence of the feature [+specific], so that the direct object must be realized as a full DP. Yet, the similarities between Dutch and Italian may also point to a more general tendency.

These results show exactly the opposite of the 'out of the blue' use of pronouns known from the literature. Here, children use full DPs instead of pronouns, although the referent has been made clear in the previous discourse. Apparently, the child can both use a pronoun for a DP (the 'out of the blue use') and a DP for a pronoun. It may be the case that this is related to the fact that the 'out of the blue' use of pronouns has been found in subjects, whereas Figure III describes the use of pronouns and DPs in direct objects. The informational status of subjects is different from that of direct objects, which may have an impact on acquisition.

The differences between child and adult discourse cannot be explained completely by making the assumption that the child ignores the discourse and does not distinguish between discourse-related and non-discourse-related. MacNamara (1982) shows that children distinguish definite from indefinite DPs in reception before the age of two. Therefore, children are not completely ignorant of the discourse.

As Schaeffer notes herself, this issue deserves more research. Further inquiries concerning discourse in children may also shed more light on the development of focus. Focus effects fall outside the scope of this book, but they interact with scrambling in adult Dutch, and may have an influence on acquisition, too.

4. The Full Competence Hypothesis

In this section, I would like to comment on the following question. Does the optional presence of functional elements in the child's production provide evidence for the Full Competence Hypothesis? The reliability of the answer to this question depends on the reliability of the methods with which the optional presence of functional elements is assessed. At least three variables should be taken into consideration: age, variation between individuals, and variation between different types of construction.

With respect to age, it is important to acknowledge that direct objects are used by many children below age 2. Schaeffer's youngest Dutch subject is aged 2;4. Further research with younger children may give different results, comparable to those of Hoekstra and Jordens (1994), who find that object scrambling is initially absent in child Dutch.

Variation between individuals may also influence the results. Because of this variation, it is rather problematic to assess optionality in cross-sectional research. One of the possibilities that I can think of is that among the two-year-olds there is a group of children showing no direct object scrambling, a group that has optional scrambling, and a group with obligatory scrambling. If one takes all results together, these three types of individuals are all considered as one type, showing optional scrambling. To exclude this possibility, a characterization of individuals should be made.

Further, not all functional elements appear at the same time. Some constructions are early, others are late. At early stages, some constructions are present in the child's production, others are absent. Therefore, the presence of one construction does not provide a sufficient argument for Full Competence, as opposed to Partial Competence.

Summing up, further research with younger children, taking into account their individual differences would shed more light on the amount of functional structure available. Ideally, the results on direct object scrambling and object clitic placement are compared with results on other constructions.

5. Conclusion

In the literature on first language acquisition, there has been relatively little attention for the development of the direct object, whereas the development of the subject has been documented in great detail. Therefore, Schaeffer's dissertation fills a gap in our knowledge about the acquisition of syntax. Since the topic of research is relatively unexplored in general, the analysis still leaves several questions unanswered, as I have shown above. In my view, the strength of the book lies in the intermodular approach. In this respect, I hope it will inspire further research along the same lines.

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BASE RULES PHONETICALLY

by *Joan Mascaró*
Review of **GROUNDED PHONOLOGY**
by *Diana Archangeli and Douglas Pulleyblank*

1. Introduction

Grounded Phonology is basically a theory that defines the class of permissible feature combinations and a theory of rule structure based empirically on a deep analysis of ATR systems. Although published in 1994, the manuscript has been circulating since 1992 (a more archaic version since 1986) and it has much influenced phonological research since then.

The theory has three basic components: a theory of underspecification, which is (correctly, I think) not based on the segment; a set of conditions on permissible structures which is crucially related to phonetic interpretation (conditions must be "phonetically grounded"); and a theory of rule structure which restricts the class of possible phonological rules to a set of parametric combinations. The theory of feature combination defines the general mechanisms that determine both lexical and derived feature combinations; this subtheory is presented in the first of the three basic chapters of the book, chapter 2, "Combinatorial Specification". It also defines the way feature combination relates to their physical interpretation: representations are impossible or permissible, and in the second case, less or more marked depending on the strength of their phonetic plausibility (chapter 3, "Grounding Conditions"). The third main chapter (chapter 4, "Parametric Rules") introduces a set of parameters which determine a more restricted class of permissible rules. These three parts correspond to specific subtheories organized as independents modules of phonological theory.

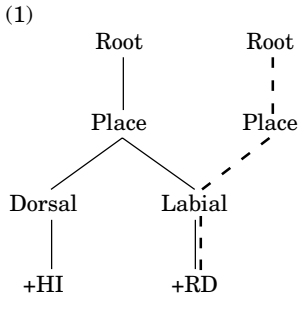
There is a general "Introduction" (1–41), and a final chapter (chapter 5, "Conclusions") in which some of the shortcomings of the theory are discussed and the integration of the theory in an archaic version of optimality theory is explored.

2. Segments and paths

The "Introduction" (chapter 1) stresses the modular character of the theory presented, and sets up some preliminary theoretical tenets on which subsequent developments are based.

Archangeli and Pulleyblank assume a feature-geometric approach to feature organization, in particular one in which well-formed nodes must dominate feature content. There are two basic notions, F-element and *path*. An F-element is a feature (binary or monovalent) or any other node. The notion *path* is presented in this introduction and further developed in chapter 2. A *path* is a subtree of a prosodic and feature-geometrical structure. It is formed by a set of nodes (features, prosodic categories, or other nodes) linked by association lines such that only one token of each node type is present. In (1) there are two main paths, marked with a continuous line and a broken line.

(1)



Some lower elements (e.g. +HI) are language particularly assigned to an *anchor*, which is always a node dominating them in the same path, e.g. an ATR node can have a mora or a syllable head as an anchor. When structural changes in the structure motivated by rules or conventions assign some F-element [= feature, node] α to some anchor β a path from α to β is created, i.e. association lines with the necessary intermediate nodes are generated (*Node Generation convention*).

A set of conditions, *path conditions* (discussed in chapter 2), define path well-formedness. Path conditions are conditions on feature/node combinations: if a condition disallows two F-elements, they are incompatible in the same path, but they can be present in a representation if they are not within the same path, e.g. if one is floating. Notice also that conditions hold at the underlying levels and at any other level (this follows from the Well-formedness Principle (2)):

(2)
Well-formedness Principle: Representations [nota bene, at all levels — JM] and relations between representations [i.e., rules — JM] are well formed.

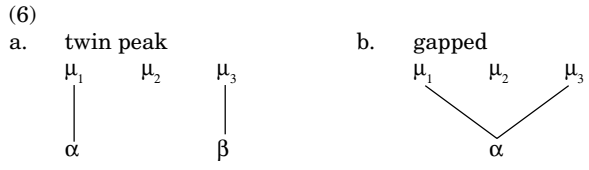
(3)
Locality Condition: Phonological relations respect Adjacency and Precedence.
Adjacency: α is structurally adjacent to β iff:
a. at least one of the two is unassociated, both are on the same tier, and no element intervenes between the two on that tier; or,
b. both α and β are associated to the same anchor tier and no anchor intervenes on that tier between the anchors to which a and b are associated.

(4)
Precedence Principle: Precedence relations cannot be contradictory.

(5)
Anchor Hypothesis: Anchor paths establish core-tier ordering.

Adjacency (the definition is almost identical to Myer's (1987)) has important consequences for rule application. In connection with the OCP, it defines, for instance, floating elements as adjacent, hence subject to it. It also defines adjacency in representations with three elements on a tier: in (6) α and β are not adjacent in the "twin peak" configuration (6a). On the other hand, the gapped configuration (6b) is ill-formed, being disallowed by the Locality Condition and the Precedence Principle.

(6)



In (6a) there is an anchor (μ_2) intervening between μ_1 and μ_3 to which α and β are associated, therefore by (3b) α and β are not adjacent, hence no OCP effect. (6b) is ruled out because α is in a path with μ_1, hence since μ_1 precedes μ_2, α also precedes μ_2. The same applies to α and μ_3, except that in this case μ_2 precedes μ_3, hence μ_2 precedes α. The result is that α both precedes and is preceded by μ_2, in contradiction with the Precedence Principle.

Principle (4) might in fact not be needed. Since structures must be well-formed (2) and precedence must be respected (3), any contradiction in terms of precedence is a violation of well-formedness. No formal definition of the Anchor is given. From the discussion it seems to follow that it states the following: a) If there is a path *p* containing α and its anchor μ, then α does not precede μ, and μ does not precede α ; b) If there are paths p_1, p_2 containing α_1 and its anchor μ_1, α_2 and its anchor μ_2, respectively, then if μ_1 precedes μ_2, α_1 precedes α_2, and vice versa. (Notice that precedence is irreflexive.)

3. What are permissible feature combinations?

Combinatorial Specification, developed in Chapter 2, differs from both contrastive and radical underspecification. The primitives of the theory are F-elements, i.e. features like [+ATR], or nodes like Place and association status (linked/free); in the cases analyzed in the book F-elements are vowel features. F-elements can be missing (underspecification), linked, or free. Representations are sets of F-elements related by association (by association lines in the usual notation) supplemented with prosodic structure. Segment is a derived notion, and a segment is a path (the converse is not true).

Specific combinations of elements in a path can be banned universally or language particularly. This corresponds approximately to the notion "possible segments in language X." Thus the number of possible segments depends on the number of F-elements and on the combinatorial restrictions, which are expressed by (path) conditions. Consider a language with a vowel system defined by the features +HI, +LO, –BK. Since any of them can be unspecified underlyingly there are $2^3 = 8$ combinations, namely those shown in (7a). The conditions in (7b–d) eliminate three items, those represented by '*'.

(7) a.

*	*	u	i	*	a	o	e
+HI	+HI	+HI	+HI				
+LO	+LO			+LO	+LO		
–BK			–BK	–BK		–BK	

Path Conditions: (Universal) b. If +HI then not +LO
c. If +LO then not +HI
(Particular) d. If +LO then not –BK

If we eliminate condition (7d), then there will be two low vowels, e.g. [a] and [æ]. If all three conditions apply, then we get (7a), a regular five vowel system. The more elements a language allows, the more complex the segment system will be. For vowels, in addition to ±HI, ±LO, ±BK, there can also be ±RD and ±ATR.

There is another way to restrict the set of available "segments." Consider a a language with the same features but with different feature values, namely –HI, +LO, +BK. We get (8).

(8)

a_1	a_2	e	u	a_3	a_4	u	i
–HI	–HI	–HI	–HI				
+LO	+LO			+LO	+LO		
+BK			+BK	+BK		+BK	

We now have four combinations containing +LO which are not excluded by any universal condition like (7b,c). An eight system like (8) constitutes a possible case, but it is surely very marked. In general, given two representations, like a_3 = [+LO, +BK] and a_4 = [+LO], there are three possible outcomes. First they may surface as phonetically different vowels. Or they can have an identical phonetic representation, in which case there is absolute neutralization and the system is marked: there must be strong evidence for positing these two different underlying representations — evidence is of course based on their different phonological behavior (this is the case of Ainu which has two vowels like a_3 and a_4). The third possibility is that the more complex vowel is excluded. This exclusion is achieved through the combined action of two principles, *Representational simplicity* and *Recoverability*. The former states that the complexity of a representation is determined by the number of terminals plus the number of association relations; the second one states that "Phonological representations and phonetic content are related." They are interpret-

ed in the sense that complexity should be minimal while allowing phonetic content to be assigned unambiguously. The consequence is that a language can have underlying paths like a_3 and a_4, but only if there is clear phonological evidence that both a-vowels have a role in the language; otherwise simplicity will rule out a_4, the more complex one.

Thus, combinatorial specification differs from radical underspecification because it merely prefers underlying redundant information to be absent, but does not disallow it (if present, redundant information corresponds to a marked situation, which shows up in the phonology). Underspecified structure may be specified in the course of the derivation, by F-insertion or spreading, or by redundancy rules. In p. 110–111 Archangeli and Pulleyblank argue that redundancy rules introducing an F value do not have to precede necessarily the first rule that mentions this value.

Archangeli and Pulleyblank consider the size of vocalic inventories predicted by combinatorial specification. The size depends on the number of elements, n, and on their linked or free status. If all combinations are allowed for *n linked* F-elements (specified features), and since any element can be underspecified in a representation, there are 2^n possible combinations; if the F-elements are *n free* elements, then the number of possible paths is of course n+1. Two examples of such cases (all elements linked underlyingly, all elements free) are studied, the vowel systems of Barrow Inupiaq and Tiv. Barrow Inupiaq has three linked F-elements, [+HI], [+RD], [+LO] which define the set {a, u, i_1, i_2}, i_2 being the totally unspecified V. Tiv defines with the three free F-elements [–BK], [+RD], [+LO] the system {i, e, a, u, o, ɔ}, ɔ representing a low rounded V. In Tiv, however, the total number of vowels is 2^n–2=6, since two combinations, those which are [+HI, +LO] are universally disallowed.

Thus, the class of possible representations can vary only along the following parameters: the (sub)set of F-elements, their underlying or (only) derived structures, their combinatorial restrictions, and their linked/free character. In other words, the class of representations of a given language is defined by the class of F-elements active in its phonology, the conditions that limit the class of combinations of these elements, by the possibility that they appear underlyingly or only in derived representations, and their free vs. linked character. This yields quite (but not exceedingly) large figures. For vowels, I get 167 systems, without considering the effect of language particular conditions which surely will produce a much bigger figure (notice, though, that these figures represent often systems that are "segmentally" identical but structurally different: they impose different phonologies, which means that the system of phonological processes is considerably restricted). Archangeli and Pulleyblank make an approximate estimate of the number of phonological systems generated by their theory taking 20 linked F-elements, which gives 2^{20}=1,048,576 possible combinations. The obvious need to constrain the combinatorial system leads to the theory of grounding.

The statements that establish the conditions of well-formedness of F-element combinations, the *path conditions*, are formulated in the shorthand notation F1/F2, interpreted as 'if F1 then F2', or 'if F1 then not F2'. Thus the HI/ATR Condition might be interpreted as 'if [+HI] then [+ATR]'. Here an improvement of the notation to make the shorthand F1/F2 more transparent would have been helpful. In many cases the + or – value for the feature is not obvious, nor is the negative or positive value of the consequent. A more transparent shorthand notation might have been a better alternative, e.g. expressions like HI/ATR,–ATR/–HI, DORS/*LAB, where a '+' value is assumed unless '–' is mentioned, and '*' stands for the negator.

3. Sympathetic and antagonistic feature combinations: grounding

Chapter 3 develops the theory of grounding, that is phonetic grounding of path conditions. Combinatorial specification defines, for any given language, a set of underlying paths, ("segments"), through the choice of features and conditions. This causes two problems. On the one hand there is overgeneration of phonological inventories, as mentioned above; on the other hand, systems are "flat" in the sense that no difference in naturalness or marking is established. The obvious solution is to add mechanisms that introduce marking; highly marked objects are impossible (hence the class of objects is restricted), possible objects are differentiated in terms of marking. There are three basic types of marking specifications: marking of F-elements (features, nodes), marking of rules (discussed in the following chapter), and marking of path conditions. Marking of F-elements is not investigated in the book; only *ATR* is discussed in this respect (some speculations about *high* appear in p. 466 fn. 66, and some references to *low* and *round* on p. 376). The format of markedness statements is as in (9);notice that if a feature is monovalent, only (9a) applies. (10) gives a specific example.

(9) *Format for Markedness statements*
a. F tends to be used [*X-amount*] actively.
b. If F is used actively, its active value tends [*Y-amount*] to be [αF]; the passive value of F tends [1–*Y-amount*] to be [αF].

(10) *[ATR] Markedness statement*
a. [ATR] tends to be used <*0.15*> actively.
b. If used actively, its active value of [ATR] tends <*0.6*> to be [+ATR]; the passive value of [ATR] tends <*0.4*> to be [–ATR].

(10a) states that most languages do not use the feature [ATR]. The [*X-amount*] specification indicates the probability (between (0 and 1) that a language might use [ATR]; since Archangeli and Pulleyblank do not give specific values, I have introduced a value only for illustration. (10b) gives the probability that a language that uses [ATR] actively, will use [+ATR] or [–ATR] as the active feature.

Chapter 3 is devoted to the third type of markedness statements, marking of path conditions. Path conditions, now *grounded conditions* are assigned, like F-elements, what we might term a "grounding value." This value establishes an ordering of conditions in terms of their relative degree of phonetic grounding or phonetic plausibility. More specifically, Archangeli and Pulleyblank propose the following principles:

(11)
I Path conditions invoked by languages must be phonetically motivated.
II The stronger the motivation for a path condition Φ,
  a. the greater the likelihood of invoking Φ,
  b. the greater the likelihood of assigning a wide scope to Φ within a grammar, and vice versa.

To illustrate, a condition like *if [nasal] then [+voice]* is phonetically grounded (and a condition like *if [nasal] then [–voice]* is not) because velic opening reduces pressure in the glottal zone thus facilitating vocal cord vibration; in addition voicelessness in nasals reduces perceptibility. It follows that the first condition is grounded. Consider for instance the conditions in (12).

(12)

a.	If [+high] then [–low]	maximally sympathetic
b.	I f [+high] then [+ATR]	sympathetic, nonantagonistic
c.	If [+high] then[–ATR]	antagonistic, nonsympathetic
d.	If [+high] then [+low]	maximally antagonistic

Sympathetic relations are an instance of "enhancement" in the sense of Stevens, Keyser, and Kawasaki (1986): in (12) tongue body lowering and tongue root advancement enhance tongue body raising. (12a), which is maximally sympathetic, yields totally unmarked, hence phonetically *necessary* representations, while (12d) yields maximally marked, phonetically *impossible* representations. (12b), which is phonetically grounded, must not but can be a (grounded) condition of a natural language. It will yield more or less marked structures according to the extent to which is sympathetic, i.e. phonetically grounded; (12c), on the other hand, is not phonetically grounded, it expresses a phonetically implausible representation. The result is that, given (I), both (12c, d) are prohibited, they cannot be part of the set of grounded conditions.

The phonetic articulatory basis of conditions that relate to tongue body height and tongue root position is examined (of course the basis can also be perceptual). The degree of strength in phonetic grounding is not discussed in detail in the book (provisionally, four degrees of grounding are used, very strong, strong, medium, week). The assumption is that values can vary from minimally grounded to maximally grounded.

Grounded conditions like those in (11) take as their domain a) the well-formedness of underlying and derived representations, b) specific rules which are positively specified for one or more grounded Grounded conditions.

While systems like {i, e, a, o, u}, {i, ə, a, ɔ, u} are possible and common, {ɪ, e, ɐ, o, ʊ}, {i, ə, ɐ, ɔ, u} (ɐ = [–lo, –ATR]), are extremely rare or not found. This follows from the existence of the strong grounded conditions that establish that high vowels are [+ATR], that [–ATR] vowels are nonhigh, that low vowels are [–ATR], and that [+ATR] vowels are nonlow.

As for rules, since grounding determines well-formedness and well-formedness holds also of derived structures, a rule that creates an ungrounded structure is proscribed. An example would be a rule spreading [–ATR] on high vowels but not on mid and low vowels. Specific conditions can hold of particular rules: a rule inserting [–ATR] only on low vowels is specifically subject to *If [–ATR] then [+LO].*

4. Rules are parametric combinations

The form of rules is addressed in chapter 4. Archangeli and Pulleyblank indicate, quite correctly, that there "have been few attempts at a constrained theory of graphically represented rules" (285). By 'graphic rules' or 'rule pictures' they mean standard notations using autosegments, association relations depicted as lines, the common stricken line (≠) for delinking and the discontinuous association line (-----) for spreading, together with more or less mixed SPE notation. It should be stressed though, that the problem with graphic rules doesn't lie in the graphic character (a two dimensional representation), but in the fact that they are notationally vague and unconstrained.

"Parametric" rules can insert F-elements, delete them, and spread or delink them. The class of rules includes rules associating underlyingly unlinked ("free") elements, since as Archangeli and Pulleyblank argue, there are no universal association conventions, and their effect is taken up by individual particular rules. Parametric rues have the following format (12):

(12)

a.	Argument *(some F-element)*		Default	Default
b.	Parameters	Function	INSERT	DELETE
c.		Type	PATH	F-ELEMENT
d.		Direction	LEFT TO RIGHT	RIGHT TO LEFT
e.		Iteration	ITERATIVE	NONITERATIVE
f.	Structure	A-Structure	NONE	FREE
g.	requirements	T-Structure	FREE	NONE
h.	Other	A-Conditions	*(grounded conditions / context)*	
k.	requirements	T-Conditions	*(grounded conditions / context)*	

The action of a rule is thus limited to insertion or deletion (b) of an association line (spreading and delinking, respectively), or of an F-element (c); this action can proceed iteratively in one or the other direction (d), and might require that the argument, the F-element, or the target be free (f, g) — otherwise it can be free or linked — and that the F-element or the target meet a set of varied conditions. This set of "other requirements" enlarges potentially the class of possible rules, since Archangeli and Pulleyblank do not restrict the

structure of contexts. In fact it can be considered a kind of "segmental *residue* " (in the sense of Poser (1982)), that will be hopefully incorporated into a sufficiently constrained theory by future research.

As an illustration, consider the process rule that inserts [+ATR] leftwards in Menomini (13). The argument, [+ATR], is subject to a contextual requirement, namely the presence to its right of a [+ATR] and nonlow (it is nonlow because of ATR/LO = If [+ATR] then [–LO], which forces the [–LO] value). It is also subject to a requirement on the target, namely that it obey also ATR/LO, hence it must be nonlow, and that it be long (bimoraic: μμ).

(13) Menomini [+ATR] Spread

Argument	+ATR		
	Default	Nondefault	
Parameters	Function	INSERT	
	Type		F-ELEMENT
	Direction		RIGHT TO LEFT
	Iteration		
Structure	A-Structure	NONE	
requirements	T-Structure	FREE	
Other	A-Conditions	___<+ATR & ATR/LO>	
requirements	T-Conditions	ATR/LO, μμ	

There is an important part of the book that I have not reviewed, namely the detailed analysis of many phonological processes (normally ATR insertion and spreading) on which the theory rests. These analysis include 16 different languages families, among them Niger-Congo (7 languages) and Bantu (4 languages). Further research on ATR systems, and vowel systems of these languages, will be heavily influenced by *Grounded phonology*.

As Archangeli and Pulleyblank themselves point out, in particular at the beginning of Chapter 5, "Conclusion", there are many open questions left: the analysis is restricted to F-elements (especially to vowels), to the feature ATR, and to insertion mechanisms; it does not determine how rules interact, and the sample of languages is limited. I think that this last *mea culpa* is totally unjustified. Not many works can offer an analysis which is supported by a similarly broad and varied empirical base. The shortcomings, or the potential problems of the *Grounded phonology* theory lie rather in the restriction of the evidence to ATR insertion and spreading phenomena. It would come as no surprise if some of the theoretical proposals put forward in the book would run into difficulties when other features, other processes, consonantal systems, syllable structure, stress, prosodic structure, etc. were considered.

In any case I think that, even if theoretical changes of various sorts have changed many aspects of phonological theory since *Grounded phonology*'s elaboration, most of the proposals in the book are sufficiently motivated and insightful enough to remain valid when adequately incorporated in current and future theoretical models.

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CONFERENCES

GLOW Special:

Reports

GLOW Hyderabad, India, January 20–23
by Georges Tsoulas

GLOW Tilburg, April 15–18, including the workshops
by Ileana Paul, Marc van Oostendorp, and Jan-Wouter Zwart

Interview with Henk van Riemsdijk
by Lisa Cheng and Rint Sybesma

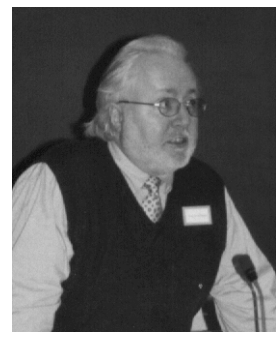
ConSOLE 6, December 15–17, University of Lisbon
(by João Costa*)*

GLOW 1978–1998

In this issue we devote some extra attention to GLOW. The GLOW ("Generative Linguistics in the Old World") Organization was founded 20 years ago by Jan Koster, Henk van Riemsdijk and Jean-Roger Vergnaud, "to further the study of Generative Grammar in Europe."

Making sure that the GLOW colloquium is held somewhere in Europe every year is just one of the activities of the organization. Among other things, GLOW has also been actively involved in co-organizing summer schools (like the ones in Salzburg and Girona). The GLOW satellite conference in Hyderabad, India, earlier this year, is the start of yet another, Asian, branch of activities; the next Asian GLOW is planned for next year in Japan. (GLOW's website is: <http://cwis.kub.nl/~fdl/research/gm/glow.)

In this issue of *Glot International*, we have a report of the GLOW in Hyderabad as well as a report of the 1998 GLOW colloquium (ordinair), which was hosted by Tilburg University, the home institution of Henk van Riemsdijk, one of GLOW's founders. What follows is a short interview with him.



"We're a great field, but it is small and in considerable danger"

A short interview with Henk van Riemsdijk by Lisa Cheng and Rint Sybesma

It looks like a pretty good GLOW this year, but the number of people is not overwhelming. There don't seem to be many graduate students either. In the past, especially as a graduate student in Europe, you needed a damn good reason for not going to GLOW. Now if you don't go to GLOW, you don't go to GLOW. Clearly the field is in disarray, it is in bad shape.

I don't think you are right. There are simply more conferences than in the past. Looking at GLOW this year it is rather gratifying to see how young the public is. There **are** young generations, and they do tend to come. But they come selectively. GLOW still is an important conference, it simply is not exclusive anymore.

So the field is in good shape?

No, we are **not** in good shape and it sometimes frightens me. Sometimes I am worried about the continuity of the field.

Intellectually it is in pretty decent shape. I have personal little qualms about whether minimalism is on the right track, that sort of little things. Listening to the talks here, I think that the level is quite high and there is lots of interesting stuff going on. So I am excited about that.

What I mean is: Is it in good shape from the perspective of logistics, the infrastructure, the power structure? How easy would it be for the opposition or the "bad" world out there to get rid of us? From this perspective, things don't look that good.

The "bad" world out there — you mean the "other" linguists?

Yes. Linguistics at large. The linguistic establishment. Looking back to the initial stages of GLOW, 20 years ago, I had something like an idea that that was the start and we would slowly conquer some power basis in Europe, not to become the only game in Europe, but at least one of the major ones. I think many generativists, especially young ones, tend to be quite mistaken about the extent to which that has been successful. The reason is that we have made some progress, we have centers where we attract students. These students get a good training, they go to conferences, the whole world in which they move is a generative world. But they often don't realize that it is an extremely small world. Take a country like Germany. It is not a poor country, they've got a lot of universities. But if you want to pin point the centers of generative grammar, depending on your generosity, you end up with maybe half of a dozen.

Chomsky would kill me, if he heard this (and you're gonna write it up so he **will** kill me), but I actually think that generative linguists have been doing too little to entrench themselves in what you could call "the linguistic establishment". I am looking at this from the European perspective, where distinctions and diversities are perhaps bigger than what you would find in the United States. But, take Germany again, it is people in discourse analysis and pragmatics and so on that are in power.

Occasionally you see what that means. Some years ago we had this big typological project called EUROTYP, funded by the European Science Foundation. The persons who were asked to organize it were Simon Dik and Ekkehard König. In the original plans that they had, they had nine sub- compartments in that project. They agreed that one of those should be on generative grammar. Their actual plan was: we do complementation, word order, themes like that, they went up to 8, then they said: now we have to do something with generative grammar. Let's call the ninth project the Principles and Parameters Approach. Then they called and asked me whether I would do it. I said no. It is ridiculous to have a Principles and Parameters Approach next to a theme like word order. I said: you can give me a topic and we work on it from our perspective just like the

typologists, the pragmaticists etc. work on their topics from their perspective. That is how it worked out, we did a group on Clitics.

This is just an illustration of how we are perceived by a big international organization like the European Science Foundation in combination with the linguistic field at large.

Is there anything we can do about it?

One thing that we can do, which we certainly don't do enough, is step outside the trodden paths of the generativism. I feel embarrassed pursuing this matter now, because we were talking about GLOW, which is the exact opposite. But even here, for the workshop we decided to invite Greville Corbett, who, for all intents and purposes, is not a generativist. He is a very good morphologist. He is here and he seems to be enjoying himself.

Personally, instead of going to conferences like NELS these days, I go to non-generative conferences in places like Prague. It has nothing to do with preaching the gospel, but it is to show these people that you can interact with them in a significant manner. I think that it has to be done. Sooner or later, we will have to be there, on a par with the rest.

So you think that there is not enough interaction between the different types of linguists? Is it because we have the wrong attitude? Too aloof?

It comes from both sides. It definitely comes from both sides. For example, in March, I went to Prague for a conference called Bridges and interfaces. In Prague you still have these relics of the Prague school. I am not the youngest anymore myself, but these people are a generation above me. They are 60ish, 70ish, and they want to perpetuate the Prague school. But the Prague school hasn't led to any significant insights for 30 years now. So that it makes no sense and you can see that the younger generation would actually like to do other things, like generative grammar, or formal semantics. They would love to be part of an international enterprise, which they don't get to be because their superiors tend to be rather fierce and control the jobs. So they have to do Prague school. So what you see is that the younger generation give talks in which they sort of disguise what they really want to do under a lot of Prague school terminology. And they are helped if there is some diversity in their conferences so you get to talk to them, and they get to talk to you. And all of a sudden you have some interaction.

Another area where that is true is typology. There are many people out there doing typology. In our tradition, lots of people also have big traps open about lots of typological generalizations. But nobody knows what the others are doing, because they don't go to each other's conferences.

There are lots of sub-disciplines that we don't have a quarrel with. We don't interact. We don't meet those people at conferences, but they are there. The thing is, they constitute a certain establishment.

A number of years ago, I was spending a summer in Vienna. Vienna is sort of the center of the Societas Linguistica Europaea, our European sister organization. They publish the *Folia Linguistica*. If you ask me, it is piss poor in content, but it is run by some of the real Establishment of Linguistics in Europe. I actually explored with the people there the possibility of joining up in one way or another. They could certainly do with some rejuvenation at the content level and we could do with some of their clout in adminstrative circles. Not too surprisingly, it didn't work out. But maybe some grains from that discussion are still around, maybe in a few years, we should try again.

There is a lot of linguistic activity. GLOW, as a conference, may have lost its focal point function, but the field seems pretty lively. There are many conferences and workshops, some on very specific topics. Or is there a downside to that too? That even generativists don't talk to eachother anymore?

There are a lot of specialized conferences and of course, most of that is good. Where I am a little hesitant is that there are these ad hoc workshops on very specific topics — interesting and adequate topics, there is nothing wrong with that — but setting them up, getting the money, organizing them, getting people to come, and so on, consumes a lot of time and energy. I feel that too much time and energy goes into that type of activity, as if people really lack the imagination to think of other things that they might put their energy into from which the field may benefit equally well or even more. If anybody that wants some suggestions as to what else they could do, there are lots of things to suggest. I was talking about the establishment angle. Certainly people could invest more in that. Get ourselves entrenched in the echelons that matter.

Another thing is related to the fact that linguistics, not only generative linguistics, but linguistics at large is a poor field. And it is a relatively small field. The consequence of that is that the infrastructure of the field is quite bad. We have journals and stuff like that. But that is about it. In my university, economics is dominant so I know that for most economics journals you can get the contents on the screen of your computer any time. These are projects sponsored by Kluwer, Elsevier, the big companies. But for linguistics? Zero. Well, there's *Glot International*, it's a nice exception, but it is you guys who did it. Not Kluwer or CUP or one of the other big guys. Okay, there is some movement. I understand that MIT Press is going to make *Linguistic Inquiry* electronically available. And there is this abstract project which is done by Blackwell, that too is a step in the right direction.

What I'm trying to say is that, on the whole, we are badly organized. The number of publications is overwhelming but, clearly, Kluwer is not going to think of doing anything big for us themselves. So here is something that people could take the initiative for. Instead of organizing another ad hoc workshop, someone might actually take this particular thing on and start asking around, pushing, bitching with the publishers, stuff like that. That is just one example. I think that there is a lot more that could be done.

And GLOW is going to do it?

Sometimes I do this *Gedanken*-experiment: suppose GLOW hadn't been created 20 years ago, would the situation now be such that people would spontaneously create it? I find that extremely hard to answer, partly also because generally speaking I'm in favor of anarchistic interpretations of how science progresses. On the other hand, I also think that people shape events to a certain extent. If you have a field full of *Machers*, the field is different from a field that is full of chaotic, unorganized people.

We have inherited a great field, we have been really lucky. The field is, to put it very modestly, interesting and exciting enough for us to want to continue at least at the present level (maybe not at a much higher level because we cannot find employment for the people anyway). I have always found it a privilege to be part of this enterprise. But I think that people underestimate the danger in which we are in that it might get lost or collapse somehow. GLOW can play a role in this, but it can't do it on its own.

GLOW's role ought to be: looking after the welfare of the field, particularly in Europe, and to a certain extent elsewhere in the world. I think that's a good purpose. It is a purpose which is as acutely necessary now as it has ever been before.

GLOW Reports

Extraordinary GLOW 1998

Central Institute for English and Foreign Languages,
Hyderabad, India, January 20–23
by *Georges Tsoulas*

Usually, GLOW conferences are accompanied by a number, this one isn't, usually GLOW conferences are held in Europe, this one wasn't. The Hyderabad GLOW, was an "Extraordinary" venue, had it been numbered it would have been GLOW 21, but as Henk van Riemsdijk carefully explained in his inaugural address GLOW counting is a tricky business (GLOW's 20th birthday will be celebrated during the Tilburg meeting). The idea of an Asian GLOW, or a GLOW in "the very old world" as K.A. Jayaseelan, the GLOW convenor, put it, or a GLOW in "the really old world" according to Chomsky's message that was read during the opening session, has a history of some four years. One can only applaud to the initiative and offer the highest praise to those involved in the organisation to make it possible. With an unfailing sense of dedication they did an enormous amount of work. This GLOW was a truly "Extraordinary" conference in all the senses of the word.

The conference started on the 19th of January with a lengthy inaugural session where the 80-odd participants had the chance to hear the history of the organisation of the conference by K. A. Jayaseelan, and the history of the GLOW organisation by one of its founding members, Henk van Riemsdijk.

In a conference report, it is somewhat unusual to focus on the inaugural addresses — and with good reason as they are usually innocuous and devoid of interest. In this case we hope to be excused for staying a bit longer on it. One of the issues that was addressed by K. A Jayaseelan and H. van Riemsdijk was the future of the Asian GLOW. Undoubtedly, one needs to recognise the indisputable, and undisputed, linguistic richness of the Asian continent, the number of active theoretical linguists but also the sometimes difficult economic conditions and the rather wide economic gap separating countries. The latter fact as well as the geographical hugeness of the continent were given quite some attention in the inaugural addresses. they seemed to be presented as obstacles to be overcome if the enterprise of an annual GLOW event in Asia were to succeed (the precise example given by H. van Riemsdijk was of a GLOW venue in Japan or Korea and the difficulties that linguists from say India would have to face in meeting the travel expenses etc...). However, given the success of the present venue it seems that these issues should not be considered insuperable problems, although at some point or other one would have to address them. Perhaps this is not the place for suggestions but on one of the above counts, how does one address the "problem" that Asia is about 4 times the size of Europe? Or, on the financial side, everyone who has talked to local GLOW organisers knows that they had to show extreme ingenuity and inventiveness in order to meet the costs. The same would have to happen in Asia. Despite these questions, it would be a great pity, we think, if the Asian GLOW was a one off venue. Fortunately, there seems to be an offer for the organisation of the next GLOW in Asia from Nanzan University in Japan. One can only wish that the above questions will find appropriate answers and that the second Asian GLOW will soon be announced.

The actual conference started on the 20th of January with a talk by **Andrew Simpson** on "VP-Final Modals and Pied Piping in East and South East Asia". In this talk Simpson discussed the distribution of certain modal verbs in a variety of East and South East Asian languages such as Thai, Cambodian, Vietnamese, Cantonese etc. concentrating mainly on contemporary Thai. His main focus was the distribution of the modal verb **dai** in that language. This verb has a curious distribution: all modal verbs in Thai occur in

normal pre-VP positions with the only exception of **dai** which occurs postverbally. Simpson argued that this raises important problems for Cinque's version of the Universal Base Hypothesis of clause structure. Technical points aside, his analysis involved raising of a whole part of the clause from a fully regular base structure in accord with the Universal Base Hypothesis. Finally he suggested that an extensive process of Borrowing had taken place, as this paradigm, although very marked, regularly occurred in the languages of the region in which the construction was at various stages of development and reanalysis.

George Tsoulas and Kook-Hee Gill's talk, "Multiple Subjects and Multiple Specifiers: A Necessary Connection?", focused on the multiple Nominative construction in Korean and put into question the widely held assumption that Korean and Japanese pattern in the same way with respect to their phrase structure properties accounting for these constructions. Their evidence from scope and binding facts showed that, at least for the cases they examined the C-command relations between the two subjects must be assumed to be different in the two languages. They finally gave a negative answer to the question in the title of the talk, as the Korean data pointed to an analysis which made no use of the possibility of projecting multiple specifiers.

In his presentation entitled "Aspect and Event Structure in Vedic", **Paul Kiparsky** addressed the issue of the proper analysis of the past tenses in Vedic and the problems it raises for the "two dimensional" theory of tense and aspect as inspired by Reichenbach's classic work. He argued that the set of primitives provided for in this theory is not only not sufficient to provide a unitary representation for the aorist and the perfect in Vedic, but the primitives do not even allow one to distinguish between the two tenses. As a remedy he proposed two refinements to the Reichenbachian approach: first, the specification, as part of the representation of certain temporal and aspectual categories, of the mapping of the predicate's event structure into the parameters that define temporal relations; the second refinement is simply the blocking effect from morphological theory. The default readings of the tenses are defeated by morphologically specified tenses. Finally Kiparsky argued that the distinctions morphologically manifested in Sanskrit are covertly present in the syntax of English.

Two talks in the conference addressed questions arising from the analysis of Kannada. **Jeffrey Lidz**, in his talk "Causation and Reflexivity in Kannada: Evidence for Post-Syntactic Morphology" concentrated on a valency puzzle in Kannada, and argued that it provided evidence for a model of grammar in which syntactic structure for a verb is generated independently of its lexical meaning, the grammaticality of the sentence depending on the satisfaction of some correspondence conditions between the syntax and the lexicon. The puzzle comes from the distribution of the causative and reflexive morphemes. In Kannada, lexically causative verbs cannot take an external causative morpheme whereas verbs that are non-causative in the lexicon can. However, both types of verbs have an anticausative variant marked with the reflexive morpheme. Lidz was led to propose that the reflexive has no semantic content in itself: it is simply the spell out of a head in a particular configuration. The second talk on Kannada was by **R. Amritavalli**: "Kannada Clause Structure". Amritavalli argued that Tense and Aspect in Kannada are distinguished functionally rather than categorially, finiteness in Kannada being located in the Agr, Neg, or Modal heads whose aspectual complements may be interpreted as Tense depending on the context.

Tara Mohanan, in her paper "Morpheme Internal LF Representations", (read by K.P Mohanan), argued that the GB model of the grammar should be modified in order to permit access to morpheme internal semantic representations. The alteration of the model she suggested amounts to the recognition of a lexical LF mediating between LCSs and Argument structure on the one hand and phrasal LF on the other. She motivated her lexical decomposition account by a variety of constructions in English (among others, scope of *almost*, word internal negation etc., and their counterparts in Malayalam and Mandarin).

Still on the Malayalam front **K. A. Jayaseelan** presented a fascinating set of data and an equally fascinating account of the relation between question formation and quantifier formation, in his talk entitled "Questions Quantifiers and Zero Derivation in Malayalam".

The invited speaker of the conference was **Mamoru Saito** who gave a lecture in which he presented joint work with **Hiroto Hoshi** on the Japanese light verb construction. Their analysis was complex and intriguing and it is not possible to do justice to it in the limited space alotted to me here. The main thrust of their argument was that the light verb construction in Japanese is best analysed as an instance of LF incorporation. They used Grimshaw & Mester's (1988) analysis as guide for their account of the Japanese construction. The LF incorporation takes place for theta role assignment where theta roles are considered formal features on the assigners and more generally they are associated with categorial features. The more radical conclusion that Saito and Hoshi drew from their analysis is that they construed it as evidence against the existence of A-chains and A-traces which they indeed argued should be dispensed with. Saito and Hoshi argued that the LF-incorporation analysis presented evidence for the minimalist model. However, one wonders how this evidence should be interpreted when taken in conjunction with their conjecture concerning the elimination of A-chains. More precisely one wonders what should happen to the heads of A-chains once the latter have been eliminated. The options, of course, are initial merging at their spell-out position or traceless movement. It seems rather pointless to have the latter option but the consequence of the first one is clearly that it lends support to a representational rather than a purely derivational model, or perhaps a movementless mono-stratal model. These issues were left open but they will certainly be exciting future episodes of the theory Mamoru Saito outlined.

Four talks addressed issues in optimality theory: the three phonology talks "Paradigmatic infixation in Miskito" by **Vivian Lin**, "Abkhaz Mabkhaz: m-reduplication in Abkhaz, weightless syllables, and base reduplicant correspondence" by **Benjamin Bruening** and "Conflict Resolution in Optimality Theory: The Case of The Anti-Faithfullness Requirement in Reduplicative Morphology" by **K.G. Vijayakrishnan** and finally **K.P. Mohanan**'s talk "Crosslinguistic Variability of Laws and Their Interaction: Parameter Setting or Constraint Parametrisation?" who focused on issues of reflexive binding in Malayalam.

A talk focusing on non-Asian languages was **Henk van Riemsdijk**'s "On Light Nouns" who presented an analysis of the partitive construction in continental Germanic, based on the line of thinking that assumes functional categories to be not independently specified bundles of features but rather defined in terms of the categorial features on their lexical hosts.

Other presentations in the main conference included "Ability Modals and their Actuality Entailments" by **Rajesh Bhatt**, "Only in Hindi" by **Shravan Vasishth**, "Ergativity and Ergative Splits in Asian Languages" by **Gloria Gocchi**, "NO Raising" by **Masanori Nakamura**, "Compounding in Hindi and Bangla" by **Rajendra Singh and Probal Dasgupta**, "Light Verbs, Empty Prepositions and Zero Derivation in Malayalam" and "Rightward Ho! A critical Discussion of Antisymmetry, Rightward Movement, and the Syntax/Pragmatics Interface" by **Steven Schaufele**.

The theme of the GLOW workshop, which was jointly sponsored by CIEFL and the Department of Linguistics of the University of Trondheim, was "Verb Typology of African and Asian Languages". There were seven regular papers and one invited speaker, Chris Collins (who spoke on "Pluractional Verbs in Hoa"). The other presentations included "A typology of serial verb constructions in Dagaare and Cantonese" by K.K. Luke and Adams Bodomo, "Japanese Adjunct 'Control' verbs" by Hironobu Hosoi, "In Support of Agr as a Functional Category" by K.V. Subbarao, "Lexical Logical Form and Verb Typology: A Case Study from Malayalam" by Tara and K. P Mohanan, "The structure of Maithili verb Morphology" by Yogendra P. Yadava, "A Typology of Counterfactual Marking in Modern Indo-Aryan Languages" by Rajesh Bhatt and "The Four Copulas in Odia" by Bibhuti Bhusan Mahaparta.

Trying to present a full report on such a conference in such a limited space is obviously a near-impossible task. I hope that I have managed to convey the deep sense of satisfaction at all levels, intellectually and in personal interaction alike, felt by all the participants of this conference. There is truly a lot which the organisers should be commended for. It was a pity that most of the European and American linguists in the initial program failed to turn up, which not only caused a lot of last minute alterations in the programme but some resentment as well (though by no means with our gentle hosts), as one came to realise that the support given to this enterprise by many European and American researchers failed to materialise in the end. (This does not apply to everyone and the importance of the support by the GLOW organization must not be underestimated.)

Finally let me just reiterate everyone's wish that the Asian GLOW continues after this year. It provided a prestigious and geographically accessible forum for discussion and exchange of ideas for Asian linguists — among themselves and with those who had come from other places.

GLOW 21, Tilburg, 15–17 April 1998

GLOW Workshops, Tilburg, 18 April 1998
by *Ileana Paul, Marc van Oostendorp, and Jan-Wouter Zwart*

GLOW Colloquium

The following papers were presented at the 21th GLOW Colloquium, organized at the University of Tilburg, April 15–17, 1998, featuring 'features' as the colloquium topic.

Paolo Acquaviva (University of Venice, 'Uniform Lexicalization: Deriving Spell-Out without [±strong] Features') addressed the issue of how to describe word order variation without using the notion of [±strong] features. Adopting the morphology-after-syntax approach, he proposed that a Principle of Lexicalization ('if a feature bundle F under the head X° is matched by a unit of *autonomous morphology*, then F is lexicalized under X°', where a unit of *autonomous morphology* is any category by reference to which morphological generalizations can be made) accounts for which position in a phrase structure tree gets spelled out.

Artemis Alexiadou and Elena Anagnostopoulou (ZAS Berlin and ZAS Berlin/University of Tilburg, 'Restrictions on Argument Placement: Adjacency on Case-Assignment and an Ambiguity with Attract-F') aimed at establishing the following generalization: Subject-inversion where the subject is VP-internal is not possible when there is an object inside the VP, unless this object is a PP. Evidence for this generalization was taken from the syntactic conditions on the availability of VP-internal subjects across languages. The proposed generalization was shown to reduce to a constraint against *covert* Case-driven movement out of VP of both the subject and the object.

Maya Arad (UCL/University of Geneva, 'Stativity, Eventiveness, Agentivity: Object Experiencers Revisited') presented a highly enlightening analysis of object experiencer verbs like *frighten, amuse, concern, delight, worry*, etc. She showed that these verbs may have three readings (agentive, eventive, and stative), and that only the stative reading is associated with 'psych proper-

ties'. Moreover, any construction can be interpreted as a psych-verb construction, once certain conditions are met (having to do with the animacy of the experiencer and the incapability of physical action of the subject, as in *an idea hit me*). Arad showed that what is characteristic of the psych-interpretation of verbs is that the perception of the stimulus and the resulting mental state are not ordered in sequence but in parallel. Given certain assumptions about the mapping of the time path onto phrase structure, it follows that in psych-constructions both the stimulus and the experiencer are generated in specifier positions of (shell structured) VPs (rather than vP). The analysis argues against earlier inversion accounts of Object Experiencer verbs.

Jonathan David Bobaljik (Harvard/McGill, 'The Autonomy of Syntax and the Typology of Movement') discussed consequences of the shift in explaining word order variation from different *timing* of movements (the overt vs. covert distinction) to different *spell-outs* of copies. He was especially interested in cases where the higher copy was irrelevant to both spell-out (PF) and interpretation (LF), mentioning as candidates expletive association and quantifier float.

Richard Campbell (Oakland University, 'Case, *pro*, and definiteness effects') claimed that Case features are not features of noun phrases but of pronominals. Common noun phrases can contain *pro*, whose Case features must be identified (hence the Case Filter). Nominals not assigned Case contain a non-pronominal empty category different from *pro*, and must be indefinite existentials.

Rose-Marie Déchaine and Mireille Tremblay (University of British Columbia/MIT and Queen's, 'On Category Features') presented a universally acclaimed paper on the conceptual necessities of lexical and functional categories, showing how certain functional categories require certain lexical categories in their complement, but not the other way around. Using data from a wide variety of languages, they reached nontrivial conclusions—for instance, that Aspect requires V, but T does not (it does not have V-features). The evidence leading to this conclusion is that stative predicates can be any category, but eventive predicates (eventivity being the result of aspect shift) are always spelled out as V. The set of conceptually necessary categories proposed includes D (which has N-features), N, T, Aspect (which has V-features), V, and P. Lexical category features such as [±N,±V] are epiphenomenal. These features are not needed to define categories, which are basically labels for syntactic objects existing only in the linguist's eye. There is no inherent distinction between lexical and functional categories. Both types of categories are configurationally defined. Functional categories in addition have semantic features which must be checked. In the lexical domain, Merge is not driven by valency, but by predication. The authors conclude that their approach favors a representational architecture of grammar.

Hamida Demirdache and Myriam Uribe-Etxebarria (University of British Columbia and University of the Basque Country/Basque Center for Language Research, 'On the Projection of Temporal Structure') proposed that the grammar of Tense and Aspect be reduced to the same set of universal substantive and structural primitives. Tense and Aspect are defined as dyadic predicates of spatiotemporal ordering. Utterance Time, Assertion Time, and Event Time are represented as specifiers of the heads Tense, Aspect, and V, respectively, of which the first two can be instantiated as expressing spatiotemporal ordering relations (inclusion, subsequence, precedence). The system was demonstrated in a range of types of tenses and aspects from various languages.

Anna-Maria Di Sciullo (UQAM, 'Features and Asymmetrical Relations in Morphological Objects') focused on the features computed under the word-level, and defended the hypothesis that their associations in 'morphological objects' and their visibility at the interfaces is based on the satisfaction of specific local asymmetrical relations (the complement, adjunct, and specifier relation). Morphological objects are binary branching adjunct-head configurations, and the only features supported by morphological objects are the ones associated with the adjunct relation: the iterative feature and the delimiting feature.

Marcel den Dikken and Rint Sybesma (University of Tilburg and University of Leiden, '*Take* serials light up the middle') took up the analysis of transitive constructions in Chomsky (1995, chapter 4) involving a layered structure of a light vP and a lexical VP and applied it to *take* serial verb constructions and middle constructions. Middle constructions were argued to lack the vP, in contrast with *take* serial verb constructions, where *v* is occupied by *take*. They showed that the lower verb in serial verb constructions looks more like a middle verb than like a genuine transitive verb.

Gorka Elordieta (UCLA/Basque Center of Language Research, 'The phonological import of syntactic features') claimed that prosodic phonology needs to be supplemented with another set of syntactic domains for phonological processes. Two adjacent syntactic heads which stand in a feature-checking relation can form such a domain. The data on which Elordieta based his claims were vowel assimilation in Basque and liaison in an informal register of French. In the talk he showed how standard assumptions of prosodic phonology are insufficient: the phenomena take place between a lexical head and certain functional heads, but not others. According to Elordieta the reason behind the asymmetry is that there is no syntactic feature checking going on in the latter cases, whereas in the former, there is.

Ricardo Etxepare (Basque Center of Language Research, 'Paratactic Dependents and Covert Merge') discussed embedded clauses with root properties, arguing that these involve merger in the covert syntax of an abstract performative element.

Maria Rita Manzini and Leonardo M. Savoia (University College London/University of Florence and University of Florence, 'Object Inflection') presented and analyzed a wide variety of data from Italian dialects involving object cliticization. They argued that clitics are ordered in an aspectual and an inflectional string. Ordering parameters do not reflect the underlying order of the string, but only the circumstance that clitics can in general be realized either in their aspectual or in their inflectional positions.

Ana Maria Martins (University of Lisbon, 'On the need of underspecified features in syntax. Polarity as a case study') was concerned with motivating in syntactic terms a typology of polarity items well established in the semantic literature, by adopting a theory of features integrating the notion of underspecification. The strong/weak distinction familiar from the semantics literature is described in terms of specified, underspecified, and variable-underspecified negative features (cf. Rooryck in *Probus* 1994).

Gereon Müller (University of Stuttgart, 'Feature Strengthening') discussed the problem that remnant topicalization in German may create unbound traces, but not unbound *intermediate* traces. This problem was discussed in the context of the more general question of how to describe successive cyclic wh-movement in terms of feature checking (it involves feature strenghtening of the wh-features of the embedded CP, forcing extraposition).

Léa Nash and Alain Rouveret (University of Paris VIII, 'Feature Fission and the Syntax of Argument DPs and Clitics') applied their theory of feature checking through feature fission (creation of a higher functional projection for an additional feature checking operation) to the licensing of arguments either in or outside VP — in particular, argument clitics. The analysis takes argument features to be formal features, and adopts Alexiadou & Anagnostopoulou's (above) conjecture that the internal and external argument of the verb cannot both be realized inside VP at Spell-Out.

Jairo Nunes and Eduardo Raposo (Unicamp and UCSB, 'Portuguese Inflected Infinitivals and Configurations for Feature Checking') discussed the phenomenon of Portuguese, where the appearance of inflection on infinitives is conditioned by the presence of an adjective with agreement features in the infinitive's complement. They argued that this supports a description of agreement patterns without making use of Agreement Phrases, and discussed consequences for checking theory.

Marc van Oostendorp (University of Leiden, 'Phonological Feature Domains and the Content of Epenthetic Vowels') presented arguments that we need to take into account the morphological affiliation of phonological segments in order to account for root-controlled vowel harmony in Turkish and for the exceptional behavior of epenthetic vowels in both Turkish and Icelandic.

Javier Ormazabal and Juan Romero (University of the Basque Country/Basque Center for Language Research and Instituto Universitario Ortega y Gasset, 'Attract-F: A Case Against Case') proposed to replace Case features (which are present for theory-internal reasons only) by animacy features (which are interpretable) and used a discussion of the properties of double object constructions to show that the feature relevant in object movement is animacy rather than Case. In conjunction with the Minimal Link Condition, this explains the ordering restrictions on clitics in Romance (the Person-Case constraint of Bonet 1991).

Keren Rice and Trisha Causley (University of Toronto, 'Asymmetries in Featural Markedness: Place of Articulation') argued that there are two problems connected to the markedness of place features. On the one hand, there is a great deal of cross-linguistic consistency as to what patterns as unmarked phonologically. But on the other hand, it is not always the same feature that patterns as unmarked cross-linguistically. Rice and Causley solved this by presenting a theory of place both for the consonantal and for the vocalic domain that involved different types of unmarkedness. For instance, central vowels and velar consonants are represented by an empty place node. This representation is quite literally unmarked in the sense that it involves a minimal structure. For this reason central vowels and velar consonants count as unmarked in these segments. On the other hand, certain languages do not allow these segments, because they disallow completely empty place nodes. Rice and Causley presented their analysis in terms of Optimality Theory but it is reasonably clear that their approach could be transferred without too many problems to other theories of phonology. This does not mean that their proposal will be uncontroversial because it involves a few representational oddities that may not be acceptable to all scholars: e.g. the features [Labial] and [Dorsal] are subsumed under a class node [Peripheral].

Elizabeth Ritter and Heidi Harley (University of Calgary and University of Pennsylvania, 'Sorting out you, me, and the rest of the world: A feature-geometric analysis of person and number') presented a feature geometry of pronominal elements and agreement features involving the notions Participant, Addressee, and Speaker (for person features), and Individuation, Group, Minimal, and Augmented (for number features). The geometric structure of these morphosyntactic features is provided by Universal Grammar. The presentation involved an analysis of pronominal paradigms from a variety of languages.

GLOW '98 Syntax Workshop 'Agreement Systems'

The workshop began with **Rita Manzini and Leonardo Savoia**'s paper 'Subject Verb and Expletive Associate Agreement.' Noting parallels between expletive-argument constructions and clitics, they proposed an aspectually-based account of agreement. Thematic roles are aspectual features that are generated in functional catego-

ries and must move to a DP position to check these features.

Øystein Vangsnes ('The role of gender agreement in Scandinavian: a study of possessive constructions in four Swedish dialects') pointed out that only in those dialects that do not mark agreement within the possessed NP can a prenominal possessor and a definite article cooccur. He argued that this restriction is due to the possibility of checking gender agreement only once within DP.

Drawing on a wide range of languages, invited speaker **Greville Corbett** noted the frequent feature mismatches and other irregularities in agreement systems ('Agreement systems: feature mismatches and the pervasiveness of agreement choices').

Maria Teresa Guasti and Luigi Rizzi ('Nonagreeing *do* in child English and eht expression of Agr') focused on data from first language acquisition to show that tense and agreement are distinct syntactic positions. Although agreement is optional at a certain stage in English child grammar, the optionality disappears in interrogative contexts. They argued for a principle and a parameter that connect overt movement and morphological agreement. Finally, they addressed the structural nature of uninflected clauses in early English.

Henry Davis discussed 'Person splits, phi-features and temporal architecture.' He showed that unrelated languages manifest a distinction between 1/2 subjects and 3 subjects and related these differences to structural hierarchy. Moreover, he argued that person features are connected with certain temporal projections in a clause and reduced the EPP to the necessity for T to be related to phi-features.

Presenting data from Zuni, a linguistic isolate, **Lynn Nichols** ('Agreement and locality in lexical structure') showed that what might appear to be an irregular agreement pattern is in fact a result of complex predicate formation via P incorporation. Agreement is sensitive to argument structure and is disrupted by lexical derivation in a well-defined class of verbs.

Invited speaker **Maria Bittner** addressed the well-known link between agreement and definiteness ('On agreement and definiteness'). She argued for a loss of functional categories to account for the gradation in semantic individuation that arises with such processes as antipassive, noun incorporation, "stripping" and lack of person agreement marking. Lexical categories are assumed to be basic predicates, while functional categories introduce existential presupposition by adding a pro.

GLOW '98 Phonology Workshop 'Phonological Opacity'

The emphasis of the GLOW colloquium sessions traditionally is on syntax rather than phonology. This year's conference featured two lectures on phonological structure and one on the interface between phonology and syntax, reported on above. All three gave arguments in favor of an enriched theory of phonological representations rather than concentrating research on the issue of rules versus constraints.

The phonology workshop on opacity on the other hand went straight into the heart of the discussion between proponents of bistratal Optimality Theory and the defendants of polystratal generative phonology based on extrinsically ordered rules. Since the two extremes were both represented, the discussion was quite lively. As Morris Halle pointed out during this day, the phenomenon of opacity was one of the driving forces for setting up the mechanism of extrinsic rule ordering. On the other hand, the existence of these phenomena pose serious problems for Optimality Theory, because their analysis appears to crucially involve some additional levels of representation beyond the input and the output. Several ways of dealing with these phenomena have been proposed within the OT literature, and it was one of the purposes of this day to evaluate those proposals.

The contribution of **David Odden** was called 'Cyclicity and Counterfeeding in Kimatuumbi', but Odden was quick to point out that the paper didn't deal exclusively with either cyclicity, counterfeeding or Kimatuumbi. His data were drawn from several (Bantu) languages and he showed that an OT analysis of these facts would involve a quite heavy machinery, including Sympathy Theory, two-level constraints and output-output relations. Neither of these three 'supplements' to OT on its own is sufficient to handle all the relevant facts. An analysis of Bantu in terms of extrinsically ordered rules is less problematic, but Mr. Odden seemed hesitant to draw the conclusion out of this that such an analysis would therefore be preferable. From an empirical point of view, both theories seem to be equally equipped to handle the relevant facts.

Marc van Oostendorp in his talk 'Non-derivational opacity in allomorph selection' claimed that there are certain types of opacity which can be handled in a sympathy approach but not in a rule-based approach. There are cases in Dutch where a form behaves phonologically as if it has chosen the unmarked allomorph, even though on the surface the form displays the marked allomorph of a given affix.

Ronald Sprouse unfortunately was not able to be physically present at the workshop. His paper was read by Orhan Orgun. Mr. Sprouses paper 'Enriched Input Sets as a source of opacity in Optimality' presented a tristratal version of Optimality Theory, in which we have two passes of generation. The first generator function is only able to add material to the input, not to delete anything (this function is therefore comparable to the original definition of Gen in Prince and Smolensky 1993). The second function can apply all sorts of operations to the output of the first Gen. Furthermore there are sets of faithfulness constraints relating input, enriched input and output. Mr. Sprouse demonstrated that this extension of Optimality Theory (which is more restricted than Sympathy Theory) can handle the data of Turkish epenthesis and deletion and the complicated facts of Yowlumne (= Yawelmani).

Orhan Orguns own paper 'Phonological Opacity and Synchronically Arbitrary Alternations: What Two-Level Phonology can contribute to OT' also proposed to extend the Gen function of Optimality Theory, but in a completely different manner. Mr. Orgun tried to give an articulated, language-specific implementation of the generator, formulated in terms of the two-level theory of phonology that Mr. Orgun developed in his thesis, based on the work of Koskenniemi. Mr. Orgun proposed that the generator function of a given language may only generate those candidates that conform to this two-level system. He claimed that this is only a minimal extension to the theory (we may need language-specific rules anyway) and that the two-level system is rich enough to account for complex cases of opacity such as the ones in Hebrew, Icelandic, Yowlumne and Kashaya. The question then arises what the division of labor is between the two-level generator and the evaluator function; Mr. Orgun left this question open to further research.

The talk by **Ania Lubowicz** was called 'Derived Environment Effects in OT'. Ms. Lubowicz argued that Derived Environments are by definition in most cases also instances of opacity: certain segments do not undergo a given phonological process, because they do not occur in a derived context. Ms. Lubowicz analysed this as the result of constraint conjunction; a faithfulness constraint is conjoined with a well-formedness constraint in the domain of a segment. If the faithfulness constraint is violated in a given (derived) context, we are not allowed to violate the well-formedness constraint at the same time. Therefore, the segments in derived environments are likely to undergo a certain change in order to become more well-formed. Yet segments in underived environments do not have to violate the same faithfulness constraint, and therefore it becomes irrelevant wether or not they violate the well-formedness constraint: they can stay the way they are. Ms. Lubowicz showed that this analysis works both for phonologically and for morphologically derived environment effects in languages like Polish and Slovak.

Bill Idsardi studied some formal properties of 'Opacity, sympathy and derivations'. The emphasis was on the treatment of opacity within sympathy theory. Taking the analysis of Tiberian Hebrew vowel epenthesis and glottal stop deletion, and Icelandic *j*-deletion and vowel epenthesis as a starting point, Mr. Idsardi showed that current Sympathy accounts are less parsimonious (there are more constraints/rules at stake), more abstract (the analysis involves a larger numer of relevant but non-surfacing representations), and more delicate (changing the grammar slightly may have dramatic consequences for candidates which seem unaffected by the basic analysis) than the available rule-based accounts. Furthermore, Sympathy Theory also is not more restricted than rule-based theory according to Mr. Idsardi: also instances of the so-called Duke-of-York Gambit may be analyzed using sympathy. The latter point was also made earler during the day by David Odden.

The last talk of the workshop was **Morris Halle**'s 'English Stress 1968–1998'. In this talk, Mr. Halle gave an overview of the developments of the theory of English stress since the appearance of *The Sound Pattern of English*. The main development is an enlarged insight into the representation of phonological structure. The idea of grouping segments into syllables, projecting syllables onto an independent stress plane and enriching that plane with a limited number of boundary symbols, allows us to formulate the relevant rules in a way that is maximally simple. The theory thus established is supplemented with a small number of exception markings. Mr. Halle furthermore adopted Burzio's proposal that certain word-final 'super-light' syllables such as *-ure* and *-y* do not get projected to the stress plane at all. This further simplified the rule-based account of English word-level stress.

ConSOLE 6, December 15–17, University of Lisbon

by *João Costa*

Last year's issue of ConSOLE (Conference of the Student Organization of Linguistics in Europe) was hosted by the Department of General Linguistics of the University of Lisbon.

As usual, the event was a good opportunity for graduate students to exchange ideas about their ongoing work, and to divulge in their results. There were 24 presentations, which enabled some discussion on a wide set of topics. In this report, I present a very brief summary of the subject of each talk, just to make you anxious for the proceedings!

Olaf Koeneman (OTS/Utrecht) opened the conference presenting the results of joint work with Ad Neeleman ('Transitive Expletive Constructions'). Developing proposals by Williams (1980) on predication and a non-universal hypothesis on clause structure, they claimed that Transitive Expletive Constructions are only possible in languages that project CP. This projection creates a position for expletives to be inserted which does not violate unicity of predication, since only in this case is the expletive outside VP's predicational domain. Covering a wide range of Germanic languages, they were able to derive Vikner's (1995) generalization that Transitive Expletive Constructions are dependent on V2.

Elizabeth McCoy (University of York, 'Heads') proposed an analysis for the following word-order variation fact in Irish non-finite clauses. In Irish, non-finite clauses are SOV. However, in Southern Irish, there may be only one overt argument before V. If the two are overt, the emergent word order is SVO. Her explanation was based on a dual analysis for the particle *a* in Irish: in Northern Irish, it heads AgrO, while in Southern Irish, it is a verbal particle. Assuming that at

least one out of two adjacent strong functional heads must be visible, she derives the impossibility of having more than one argument in Southern Irish: it is impossible, since *a* is not a functional head. This paper raised some interesting issues concerning the meaning of feature strength.

Anikó Lipták (HIL/Leiden University, 'Focus and ECM') argued that Hungarian displays a Long Focus Movement construction, which is rather unexpected, since the moved constituent agrees with and gets Case from the matrix verb. Lipták followed Rooryck's (1997) analysis of ECM, according to which the embedded AgrSP raises to Spec,CP, from where the moved constituent may raise to Spec,AgrOP of the matrix verb and to the focus projection. Under this analysis, no improper movement arises, and the case and agreement facts are explained.

Luciana Storto (MIT, 'Karitiana: A Verb Second Language From Amazonia') described several properties from Karitiana, a language spoken in Amazonia, arguing that this language is V2. She claimed that, based on the Karitiana data, V2 might be interpreted as V-movement to I or Focus, but not necessarily to C.

Juliette Waals (OTS/Utrecht University, 'Syllable Structure and Consonant Duration in Dutch') presented the results of three experiments on the temporal structure of consonant clusters in Dutch. The conclusions were that consonant duration decreases as sonority increases. The results also confirmed the *Metrical Segment Duration Hypothesis*, according to which segment duration reflects syllable structure.

Marina Vigário (University of Minho, 'On the Prosodic Status of Stresless Functional Words in European Portuguese') argued that stressless functional words in European Portuguese are prosodic clitics, attached postlexically to the Prosodic Word. Her arguments were based on asymmetries of behavior between functional words and other words with respect to several prosodic and phonological phenomena. She showed that stressless functional words have a behavior which in general does not respect constraints applying to Prosodic Words.

Laurel LaPorte-Grimes (University of Connecticut, 'Infl as Agr or T, but not both') presented a new approach to the V-movement contrast between English and French, extending and modifying Lasnik's (1995) morphological analysis. Grimes' analysis was based on a modification of the assumptions concerning the featural specification of the different verbal forms. It also succeeded in explaining the fact that in *what about answers* bare verbs appear inflected in English, but in the infinitive in French.

Anabela Gonçalves (University of Lisbon, 'On Restructuring Constructions in European Portuguese') discussed clitic climbing and restructuring in European Portuguese. According to her proposal, the difference between restructuring and non-restructuring verbs has to do with the activity vs. inertia of the embedded functional projections.

Rajesh Bhatt (UPenn and MIT, 'Actuality Entailments of Ability Modals') opened the second day of the conference, describing the behavior of ability modals with respect to actuality entailments. He showed that ability modals may be ambiguous between past episodic and past generic readings, which raises problems for the traditional LF for these modals. He then proposed an analysis in terms of a decomposition of ability modals into a generic operator and a stage-level predicate $ABLE_{slp}$.

'Verbal Small Clauses' was the topic of **Joan Rafel**'s paper (University of Girona). He argued that small clauses may be headed by verbal categories. He then applied such a structure to the analysis of three constructions: pseudo-relatives in French, naked infinitives in English and the Prepositional Infinitival Construction in European Portuguese.

Susanne Glück and **Roland Pfau** (Goethe University, 'On Classifying Classification as a Class of Inflection in German Sign Language') looked at classifiers in German Sign Language. They compared the traditional incorporation analysis with their proposal that classifiers are inflection markers. They argued for this analysis on the basis of agreement patterns and pro-licensing in left-dislocation constructions.

Maria João Freitas and **Matilde Miguel** (University of Lisbon, 'Prosodic and Syntactic Interaction: the Acquisition of NP Functional Projections in European Portuguese') investigated the distribution of word-final fricatives in children's first productions, and argued that codas may emerge earlier than expected, if they correspond to morpho-syntactic information being mastered by the children since very early. The analysis proposed for fricatives in coda was also productive for the explanation of emergence of syllabic material occupying determiner positions.

Mikael Vinka (McGill University, 'Nonpredicative Particles in Swedish') investigated the behavior of non-predicative particles in Swedish and argued that, in order to explain the interaction between these particles and NP-movement, they should be analyzed as maximal projections, which are able to undergo XP-movement to the Specifier of AspP, and, from this position, block NP-movement.

Hironobu Hosoi (McGill Universiy, 'The Lexical Extension Control Verb Analysis') argued that the Japanese V-V Compound "Control" Verb is not a control verb. He proposed instead that control verbs are formed under lexical extension. This proposal enabled him to explain some differences between this type of verb and regular English control verbs.

Viola Miglio (University of Maryland 'A Formal Approach to Dialectology: Vowel Reduction in Romance') showed that Optimality Theory may account for dialectal differences by means of minor re-rankings of constraints. The empirical domain of her analysis was Vowel Reduction in Romance.

Susan Garrett (UPenn, Positional Faithfulness and Truncation in Child Speech: What Gets Kept and Why?') looked at child Spanish truncations, and argued that truncation is not arbitrary. Children systematically keep the stressed syllable/foot, which was explained in terms of prosodic faithfulness to the relevant prosodic domains.

'The acquisition of expletive definite articles in Modern Greek' was the subject of **Theodore Marinis**' talk (University of Potsdam). He showed that determiners are acquired in accordance with their semantic content: the more contentful they are, the earlier they are acquired. The early acquisition of expletive determiners followed from a theory of triggers, according to which contrasting environments are relevant for determining the order of acquisition.

Adolfo Ausin (University of Connecticut, 'English Verbal Morphology and Feature Movement') extended Lasnik's analysis of English verbal morphology, defending the F-movement approach. He claimed that the difference between auxiliaries and the other verbs is that only the former are opaque to F-movement. Hence, only the former must move as categories.

Laura Siegel (UPenn, 'Gerundive Nominals and the Role of Aspect') described some asymmetries between two types of gerundive nominals (POSS-ing and PRO-ing). She claimed that these two types of gerundive nominals may not receive a unified analysis, since they are not able to refer to the same type of events. She explained these asymmetries, connecting them to the role played by progressive aspect.

Dalina Kallulli (University of Durham, 'The Common Basis of Clitic Doubling and Scrambling') claimed that clitic doubling and scrambling have a common basis: defocusing. Her analysis was based on a set of similarities between these two phenomena in Germanic and Greek, Albanian and Rumanian.

José Luis Méndez (Universidad Autónoma de Madrid) proposed 'A morphosyntactic analysis for nominal compounds in Spanish,' based on the small clause analysis for possessive and attributive relationships.

Esterella de Roo (HIL/Leiden University, 'Nominal Projection in a case of Agrammatic Aphasia in Dutch') investigated the functional structure in spontaneous speech of a Dutch agrammatic patient, in order to determine whether functional structure is completely absent or just unrealized. Her results were related to studies on extended projections and to underspecification of functional heads.

Marie Claude Boivin (MIT, '*En*-cliticization, Raising to Subject, and Case') made a new proposal for *en*-cliticization, explaining the asymmetry between quantitative and genitive *en* when extracted from derived subjects. Her analysis was based on Case. She claimed that the absence of an overt nominal (quantitative *en*) makes the raised NP unable to check Case, rendering raising to Spec,TP illicit.

Danny Fox (MIT, 'Economy, Asymmetries, and the Ellipsis Scope Generalization') revised his own work on the scope of ellipsis, arguing on the basis of down-stressing data that the Strict Cycle Condition is not necessary to explain restrictions on scope. He now used accommodation (as result of inferences) as a new tool to explain the relevant data.

The invited speaker of the conference was **Eduardo Raposo** (University of California, Santa Barbara) who proposed an analysis of clitics in Portuguese, arguing for Postal's idea that clitics are determiners.

Next year's ConSOLE will be in Bergen, Norway, organized by the new board: Michael Redford, Tina Cambier-Langeveld and Anikó Lipták.

CONFERENCE CALLS

Society for Pidgin and Creole Languages
Los Angeles, USA (in conjunction with LSA)
8—9 January, 1999

Abstracts are invited for a 20-minute paper on phonology, morphology, syntax, semantics, lexicon, social aspects of language, history of the discipline or any pertinent issue involving pidgin and creole languages.

Deadline: **31 July**, 1998

Info: aschwegler@uci.edu (Armin Schwegler); http://www.ling.su.se/Creole/calendar/

ConSOLE 7
University of Bergen, Norway
9—11 December, 1998

Papers are solicited from students in the field of generative linguistics, more specifically in, but not limited to, phonology, morphology, semantics, sign language, language acquisition and syntax.

Deadline: **1 August**, 1998

Info: console7@babel.ling.uib.no; http://www.ling.uib.no/ConSOLE-7

14th Comparative Germanic Syntax Workshop
University of Lund, Sweden
8—9 January, 1999

Abstracts are invited for 30 minute papers on comparative Germanic syntax.

Deadline: **1 August**, 1998

Info: christer.platzack@nordlund.lu.se (Christer Platzack)

International Conference of the Linguistic Society of Belgium
University of Antwerp, Belgium

11—12 December, 1998

Topic: Modal Verbs in Germanic and Romance Languages.

Deadline: **15 August**, 1998.

Info: pdendale@uia.ua.ac.be (Patrick Dendale); auwera@uia.ua.ac.be (Johan van der Auwera)

SIGNAL ANALYSIS AND DIGITAL SIGNALS

by *Stefan Frisch*
Review of **WinSAL-V**
by *Ingolf Franke*

WinSAL-V, which stands for Windows Speech Audio Lab with Video option, is a tool for digital speech processing and acoustic analysis. The *WinSAL* program provides a wide variety of analysis methods which cover all of the basic speech analysis techniques. The video option also allows the program to load and display video files, which is a feature that is not found in most other speech analysis programs. The CD-ROM version of the program comes with a database of sound files and a manual with an informative speech analysis tutorial. The basic *WinSAL* package costs 399 DM (about $220), the CD-ROM version is a great value for only 50 DM (about $28) more. The video option costs another 50 DM. The *WinSAL-V* package on CD-ROM is a solid value for those interested in doing basic speech analysis, especially if you are interested in audiovisual signals, as it provides many of the same functions as software packages that cost $1,000 or more. (*WinSAL* is an especially good deal for students, who receive a 300 DM discount.)

WinSAL requires a PC compatible computer with a 486 or faster processor, 8 MB of memory, Microsoft Windows 95 or NT, and a 16-bit sound card. The program occupies about 10 MB of disk space. Audio and video is handled through windows, so no special setup or equipment should be required. A Pentium processor, 16 MB of memory, and a video card which can display 16-bit color is recommended. The software was tested on a 486 DX2 50 MHz processor, 24 MB of memory, Windows 95, with an 8X CD-ROM drive and a Sound Blaster AWE 32 sound card. The program worked perfectly well and performed all of its functions with adequate speed on this machine, however, those interested in doing a lot of spectrographic analysis or working with signals of sentence length or longer will want to use a Pentium machine.

WinSAL is a speech signal analysis and digital signal processing program capable of loading, saving, and recording WAV format sound files. With the video option, *WinSAL-V* can also load AVI video files. Audio and video are played back together, allowing some capability for audiovisual speech analysis. *WinSAL-V* cannot record video files, but such software is available from Media Enterprise for a price (1,999 DM, about $1,100). The *WinSAL* interface provides a variety of control options, including pull-down menus, a button bar, and some functions available through a context sensitive right mouse button menu. There are some keyboard shortcuts, but only for the set of functions accessible through the right mouse button menu. A click of the left mouse button plays back a small portion of any signal in the area in which it was clicked. While this feature is nice, it has apparently made the usual click-and-drag highlighting unusable. To select a portion of a signal, you have to press the shift key and click at the same time, then drag. The coordination required is rather precise, and in many cases it took me several attempts to get it to work. Once a portion of the signal is selected, it can be played, saved as a separate file, or expanded to fill the entire analysis window using a special menu which pops up after highlighting. However, after choosing from the menu, the highlight is removed, so it is very difficult to play an arbitrary portion of a signal over and over again without zooming in on it. To zoom out requires an equally unintuitive Ctrl+click combination. In all windows, the program displays the coordinates of the mouse cursor in the relevant units, which I find to be a very handy feature for measurement. Since this is a windows program, the display of the windows can be adjusted, minimized, and maximized. One small annoyance is that once a window is maximized, it cannot be minimized in the usual fashion (there is no restore command in the window menu and the buttons in the upper right corner of the window disappear, only leaving options like cascade, tile, or close all windows).

Signal analysis options are divided into three groups: long time, short time, and spectrogram. The long time analysis options include a standard waveform, energy display, and fundamental frequency display. The energy and fundamental frequency displays include a small waveform window which is time aligned with the display. The energy display shows RMS amplitude as a default. The energy window can be configured using a parameters dialog box available from the context sensitive menu. The window size over which each segment of the measure is computed and other options for measuring energy (average and max/min over the window) can be selected. Similar control over parameters is available for all of the analysis functions, making the program fairly configurable. However, for the benefit of the novice user, it would be nice if there was a way to reset the parameters to the defaults without reinstalling the program from scratch. The lack of a default discourages exploring the options. The fundamental frequency display computes an estimate of the fundamental frequency using the average mean distance function (AMDF, which is very similar to an autocorrelation). The maximum and minimum allowable frequency for the pitch track is adjustable, a very important feature for proper analysis.

The short time functions are linear predictive coding (LPC), fast Fourier transform (FFT), Cepstrum, AMDF, and autocorrelation. Each short time window is accompanied by a time aligned waveform which has red cursors marking the segment of the signal which is displayed in the analysis window. The relative display of the waveform and analysis window is fixed and cannot be configured (though the window itself can, of course, be resized). The size of the analysis window is adjustable for all of the short time functions, so the standard trade off of frequency resolution versus time resolution is available. Unfortunately, the frequency range over which these functions operate cannot be parameterized, so displays of the most relevant frequency ranges can only be created by digitizing at a lower sampling rate. The LPC order can be parameterized, another crucial feature. The Cepstrum, AMDF, and autocorrelation operate just like the LPC and FFT, displaying values just over the analysis window. The program cannot compute these functions for intervals over the entire signal, which makes these displays of limited value for the researcher.

The values of the short time displays (and long time displays) can be saved into an ASCII text file to be used for subsequent processing. Overall, I believe the short time displays will be of most value to the student learning about digital signal processing. Since the analysis tasks cannot be automated by macros or special functions, they are useful analysis tools for small projects or those requiring careful hand measuring only. The displays can also be printed or saved as BMP format graphics files. I tested the printing of an LPC window on a 300dpi HP Laser Jet IIIP and found the print of the waveform to be of good quality and the overall display to be satisfactory. The program can also save a screen shot to a BMP file, so more intricate displays can be created and either printed out, inserted in another document, or modified with a graphics program.

The final analysis option is the spectrogram. *WinSAL* uses color spectrograms, and this option is unfortunately not configurable. Not surprisingly, the spectrogram was by far the slowest analysis tool, but its speed was reasonable on my machine even for short phrases. The frequency range of the spectrogram can be adjusted. The spectrogram can be printed out or saved as a graphics file. The program uses a color spectrogram for the printout as well, which I suspect would look very nice on a color printer. A printed spectrogram on my laser printer came out as white on black, and was very dark and not very legible to me. To make black on white output I would recommend saving the spectrogram as a BMP file, converting it to a grayscale image in a graphics program, and then reversing the polarity of the scale.

The handbook that comes with the CD-ROM version has a short manual on the use of the program, though all of this information is also contained in the on-line help for the program. The handbook also contains a basic introduction to signal processing which I found a very helpful and useful tutorial. The tutorial is full of tips and techniques for signal analysis and also contains an explicit example stepping through some of the common functions and telling you what you should see for a sample sound file included on the CD. The CD contains a catalog of sound and video files for both English and German. All of the segments are covered for each language. Consonants are given in word initial position if possible. This is the same catalog of sounds in the *Speechlab/Sprachlabor* program (reviewed in *Glot International* **3**,4; a demonstration version of *Speechlab* is included on the CD). The CD includes a few videos and sound files not found in *Speechlab*. There are videos of laryngeal function, which are a little hard to appreciate without a way to view the video frame by frame, and some videos of an infant vocalizing. The sound files include some isolated vowels, two formant synthesized vowels, and sentence length recordings of read speech in English and German. There are also longer passages in German read by two female talkers.

Overall, the *WinSAL* package is a good value, especially the CD-ROM version with handbook. The video catalog on the CD-ROM can only be fully appreciated with the video option, though I think the best use of the video is for instructional rather than analysis purposes as there are no video controls. The ability to step through the video frame by frame with buttons or the keyboard would enhance this portion of the program quite a bit. This software has valuable features for students and researchers who need to do a small to moderate amount of speech analysis. I wouldn't recommend this program as the primary speech analysis tool in a speech lab, as it does not have the full functionality and programmability of more expensive programs designed for speech professionals. The lack of macros and automated processing tools make this package inappropriate for large research projects. In addition, the variety of minor annoyances like the strange form of highlighting and color only spectrograms might actually interfere with efficient analysis and generating printed results on a daily basis. This software will be an outstanding value if these complaints are addressed in future versions. *WinSAL* would make an good supplemental speech analysis package for any lab, used for instructional purposes and smaller projects, as copies could be purchased for several machines at a reasonable cost.

THE NUMBER OF DEATH

A linguistic mystery in eight installments

by *Chris Sidney Tappan*

Chapter 4

Towards some hypotheses

Paul had just had a flat tyre, that was all.

It did not take him much time to convince James that her idea that Bill Fyffe had been killed by mistake and that he — Paul — had been their real target was absurd. "I'm a linguist and I'm a nice guy — who would want to kill me?!" he told her.

He was right, of course.

So they went and had lunch together, as previously planned, in the restaurant called "The Drinking Donkey". It used to have a different name, "The Hungry Horse", and in those days it was their favorite hang-out. After lunch they would go to Paul's office, where Fyffe's body had been found, to see whether anything was missing or something else had been disturbed. So they had thought it a nice idea to do lunch together. They had not had lunch together on a weekday for a very long time. Even the restaurants had changed names.

After getting some beer, they placed their order (two simple sandwiches with lettuce) and looked at each other, both thinking that they should do this more often.

"So what did you find out this morning?" Paul asked.

"I talked to several people, but there was not much of interest," James said. It was clear that she was disappointed. "I mean, most just confirmed what we already know, that everybody hated Bill and for good reasons too." She opened her notebook and leafed through it, not looking for anything in particular, just going through it and see what was the thing she was most eager to talk with Paul about.

The waiter brought the food and asked them whether they wanted another beer. They did.

James said: "I got the list of everybody who was in the building the night of the murder, that is to say, all the people who were officially there, who had picked up a key, of course. There are only seven. Well, eight, including you. 'A quiet night,' the janitor-in-chief said when he gave me the list. When I told him that my idea of a quiet night did not include a murder, he said that a dead person is always more quiet than one who is not dead, which he himself thought was very funny. I am thinking of getting the smart ass detained for 24 hours, or maybe 48, just to irk him. What do you think?"

Paul smiled at her. Wasn't she wonderful?

"Anyway," James continued. "As to those seven people, I talked to four of them this morning. The other three will come in this afternoon. The four I talked to came early, left early, did not see or hear anything. Two of them could confirm each other's presence, they had bumped into each other in the restroom, but they had been on a different floor than where Bill's office is, so they could hardly be expected to be able to provide any interesting information. We'll check their alibis. One of the others was a Martian and he at least could tell me something useful. He said he had actually been there to see Bill. Everybody apparently knows that Bill is in the office every night. His name is Nichael. He said he had been there with Stephem. I will see Stephem this afternoon. They were in the office right next to Bill's. He said there was nobody there. Bill never showed up and they left at about nine."

"Ah, Nichael," Paul said. "I know him, he is one of my informants. He is interesting because his phonology is all screwed up. Did you notice?"

"I had a hard time understanding him, yes. Why do these guys have such funny names, anyway?" James asked, putting a piece of sandwich in her mouth. "I mean, like Johm, our neighbor, why are they called *Johm* instead of just *John*?" she added, barely understandable.

"They have some funny phonological rules, that's all," Paul answered, looking around to see whether he could find the waiter with their beers. "Rule number 1 of the Martian phonology of English is: turn N into M at the end of a word **or** M into N at the beginning of a word. Rule number 2 is: Rule 1 applies only once. And Rule number 3 is: It only applies to names of people. The interesting thing about Nichael's speech is that he violates rules 2 and 3, though not all the time, of course (that would be too easy). So he does not limit rule number 1 to names of people but to all unique objects. So he says *sum* instead of *sun*, *Nars* instead of *Mars*. And he says *noom* instead of *moon* because he violates rule number 2 too. How many nooms does Nars have, do you know? I neam, how unique is a noom?"

James looked at him. She had no idea what he was talking about. So she simply ignored him, saying: "As I said, I haven't gotten much further. I mean, apart from the guys who were in the building that evening, I also talked to many other people. The only interesting but at the same time ridiculous thing I heard was that somebody, a Martian actually, threatened to kill our Mr. Bill the day of his death. And you know why? Because Bill had told the Martian that there was not enough paper in the printer! Apparently, the Martian was printing something in the printing room, and Bill had been there too and in going out he had made that comment about there being not enough paper. Someone — I think she is in your department, let's see (leafing through her notes) Esperanza Holyfield — overheard this while entering the printing room, and she found this Martian there, fuming with anger, apparently because of this comment about the paper, mumbling that he would kill him for it. But, I mean, killing somebody because he tells you there is not enough paper in the printer — I mean, how much more farfetched is this whole story going to be?"

Much to James' surprise, Paul did not burst out in laughter, but instead, he reacted very seriously. "A Martian threatened to kill Bill?" he asked in what seemed sincere astonishment. "That is remarkable. He is always so very nice to them. Who was it? Do you know his name?"

James looked at her notes again. "I think his name was Nartin or something." All of a sudden, she was much more alert too. As a consequence, her appetite was gone, and she felt very thirsty. And then the waiter — another one actually — brought their beers. Thanks, she nodded to the waiter, and she took an eager bite.

"Nartin?" Paul was saying. "Hmm, that is interesting. Nartin had applied for a job and he didn't get it. Bill was on the committee of course, but it is well-known that Bill was very much in favor of hiring Nartin. He even said it on TV, the other day, remember?, that more jobs should go to Martians?"

James did not remember but she believed him. Paul was going to say something, but then their own waiter came with two more beers. They said they already had their beers, thank you, and wanted to continue their conversation, but the waiter would not let them. He stood at their table and said angrily: "No, you did **not** get your beers. **These** are your beers, the ones on your table are not yours!" And he got even more angry when he discovered that they had already been sipping from the beers the other waiter had put on their table. "Now they can no longer be served to the people who ordered them!" he exclaimed. "You should have waited for me!"

"**You** should have waited on **us**!" Paul declared with much too much drama. And the joke was not appreciated anyway.

"Exactly!" the waiter said triumphantly. "I'm glad you see my point." Then stood there silently.

"Can't you bring the beers on your tray to whoever the beers we are drinking were for?"

"They won't accept them. They did not order these. They ordered those!"

Paul was stunned. "What kind of place is this?" he asked, looking at James, but obviously not expecting an answer from her. "I mean" — turning to the waiter — "How are we supposed to know that these beers were not for us! We order them, a waiter brings them —"

"Exactly!" the waiter interrupted him victoriously at the top of his voice. "**A** waiter brings them, not **the** waiter. Not the same one you placed the order with!" And he pointed at the text in bold red print on the menu, right under the name of the restaurant. It read: *If you order a beer, make sure you get it!* When Paul and James looked up, the waiter had stalked off, leaving the beers on their table.

"Arrest the smart ass!" Paul said to his wife. But the police detective was flabbergasted and did not move.

After lunch they went to Paul's office. The technical staff of the police had done their work: finger prints, photos, everything. Now, all James wanted to know from Paul was whether anything was missing from the office.

Paul looked around. "No," he declared. "Not as far as I can see now."

They looked around some more, but Paul did not see anything unusual.

"So he was lying here?" Paul asked, pointing at the chalk marks on the floor.

"Yes," James said.

"Can I work in the office now?" He said. "I don't know whether I'd feel like it, of course. It remains a strange idea that somebody was killed right here."

"Yeah." James was silent for a while. Then she said: "Yes you can work here. We are finished with this room. I'll go up and talk to some more people. I have a lot of appointments lined up."

They looked at each other. Let's try to get home early today, they thought.

The conversation between Nartin and Bill in the printing room kept bothering Paul. What had actually been said? While pondering over this question, he sat down behind his computer and turned it on. But before the starting up had been completed, Paul had already left the office. He had decided to pay a visit Esperanza Holyfield.

She was in. She was also some sort of a linguist, although it had never really become clear to Paul what aspect of language she was interested in let alone that he had any idea as to what aspect of language she was studying.

"Hi, Esperanza."

"Hi, Paul, come in."

"Thanks."

"How are you doing? It must be quite a shock to find out that somebody got killed in your office. And somebody you know too. I'm glad he did not get killed here."

Why would he? Paul thought. That is actually an interesting question, he thought. Bill Fyffe had been in his — Paul's — office for a purpose. Presumably, he was there for something he could not have done in somebody else's office, like Esperanza's. Unless he was going from office to office and had already been here. Forget this line of thought, Paul told himself.

"Is your wife making any progress?" Esperanza asked.

"Not really," he said. "But you told her something that interests me. About the printing room incident with Nartin so to speak. Could you tell me exactly what happened and do you think you can tell me everybody's exact words?"

"Strange, isn't it? I was really shocked. I had never heard Nartin say anything like that." Esperanza still **looked** shocked. "But their exact words..."

"Try it. I have the feeling that it is important," Paul encouraged her.

"Well, I was going to print something, I don't even know anymore what it was. So I walk up there and Bill and Nartin are in that room. Nartin is printing something, and they are talking about the reasons why Nartin did not get that job in the department. I had forgotten something so I went back downstairs to get it. Coming back, I bumped into Bill in the doorway. He was just coming out. And I heard him say something like: 'I think you don't have enough paper!' He says this in a joking manner. You know how he is, I mean was, always so much nicer to them than to us."

"Were those his exact words?" Paul asked her, ignoring her last comment and before she could continue her story.

"Something like that. 'I think you don't have enough paper!'"

"And then what happened?"

"So I go in and I was shocked to see that Nartin was angry as hell. I mean, I had not expected this because Bill had made that comment jokingly, and when I had been there earlier, the atmosphere had been fine, they were sitting there like friends, no animosity. But now, Nartin was fuming. 'Did you hear what he said?!' he asked me. I said, You mean that comment about the paper? 'Yes!' he says. 'I could kill him for it!' he said. 'I could kill him for it,' that were his exact words. I could hardly believe what I heard. Especially from Nartin. And I mean, it was only about the paper in the printer!"

"You said that they were talking about the reasons why Nartin did not get the job when you were up there earlier, right?"

"Right."

"What did they say? What were they talking about?"

"I don't know, I wasn't there for a long time, I walked in, and walked out again, right away."

"WHAT'S RIGHT ABOUT X-BAR SYNTAX IS THE X AND THE BAR. WHAT'S WRONG IS EVERYTHING ELSE!"

by *Lisa Cheng & Rint Sybesma*
An interview with **James McCawley**

Continued from page 1

tional Grammar, the multiple notions of a subject that you get by looking at multiple strata like Perlmutter has done, and how that has turned up facts of great interest.

So frameworks are only useful as a heuristic? To find more facts?

Well I **love** facts. Newmeyer has described me and my generative semantics buddies as data fetishists, and I'm quite happy to be so described. You can really get high on data.

But of course, let me stress that we data fetishists are able to get such gigantic highs out of our experiences with data partly because of the interactions between theory and data. So, yes, to my mind, the main value of theories in any subject is how they lead people to the discovery and appreciation of facts, how they help you find things that were there that you didn't know about, or make you appreciate the significance of things that you knew were there but that you did not know the significance of. In the preface of my syntax textbook I mention black holes. It takes some awfully specific theories of light and the distribution of matter, and so forth, before it is possible for astronomers to say, "By God, I've got a black hole here." We've got the same sort of thing in linguistics. The sort of approach to syntax that I take, taking constituent structure really seriously, as well as a very specific version of the Cyclic Principle, has given me all sorts of neat stuff, like explanations of why quantifier float and *there*-insertion and so forth disambiguate quantifier scope. The way that I set it up, that follows from strict Cyclicity. Of course, in the process of coming to these neat facts, I discover holes in my theory and deficiencies, which I then alter. Theories lead to facts, which lead to better theories which lead the data fetishist, if he is lucky, to a yet greater high!

When it comes to theories, I also keep in mind the very important point that Lakatos, the philosopher of science, made, that when people talk about the success of a theory, it really isn't a theory they're talking about. They're talking about some community in which research is done over decades, maybe even centuries, where the associated theory will often undergo enormous changes. The continuity that people perceive, is there in virtue of a research program, which does not change much over the course of the decades or even the centuries. The people working within that research program keep altering their theories in what look to them like small ways. But changes in a theory are never inherently large or small — they're only large or small from a perspective such as a research program.

So you got into linguistics for the facts?

Originally, I was in mathematics, but for no particularly good reason — I really hadn't thought much about what I was getting myself into. I was taking some language courses and enjoyed them and at the same time I got more and more turned off by mathematics. Eventually I thumbed through the University of Chicago time schedule and saw that there was something listed as linguistics. So I sat in on a linguistics course taught by Eric Hamp, which I greatly enjoyed. Then I got a mathematics scholarship to study in Germany, at the University of Münster. But instead of doing very much mathematics, I took all sorts of language courses, including a Dutch course. During that year, I got more and more turned off by mathematics. After I got back to Chicago, I wanted to take a language course just for the fun of it. Japanese was offered at a convenient time, so I took it and I fell in love with the language right away. I also started looking around in the library for linguistic books. I came upon *Syntactic Structures* and it really turned me on. Not long after that I saw the announcement for the new linguistics graduate program that they were starting at MIT. I applied, got accepted, went there, became a linguist and I have been enjoying life much more ever since then.

How come?

Linguistics is fun!

Why?

Languages are weird and wonderful things. As long as you are perceptive enough there is plenty to keep you happy and busy. In that respect, I think linguistics is way to hell more interesting than mathematics. There's a wonderful quotation from Bertolt Brecht at the beginning of Feyerabend's *Against Method*: *Ordnung gibt es meistens wo nichts ist. Sie ist eine Mangelerscheinung.* "There is order mainly where there is nothing. It's a phenomenon of absence." And, well, *Ordnung gibt es in der Mathematik.*

Not in linguistics?

There is this principle, associated with Ebeling, to the effect that phonemic analysis is most effective on the languages that one knows less about. It's probably true for any linguistic analysis. This is an illustration of the Brecht quotation. This is why linguistics is so interesting.

In addition, there are tie-ins between linguistics and virtually everything. That's another thing that really turns me on about linguistics — it's an excuse for me to be a perpetual student. Since linguistics ties in with pretty much everything, I can, with a straight face, sit in on philosophy courses and claim that is to my professional benefit.

Back to your surfing the frameworks...

The sort of moving around that I do is a lot like the shopping tour I just described. What I actually do in a lot of cases is what I call "to charitably misrepresent" what is done in other frameworks. Let me explain what I mean by "charitable misrepresentation". In a paper I did recently, I talk quite a bit about this really nice paper that Carolyn Heycock had in *Linguistic Inquiry* about two years ago that presents some very interesting facts. Heycock explains why certain interrogative sentences with anaphoric relations are ungrammatical, while others are grammatical, in terms of how they differ in what she calls LF. She says "LF" but I can charitably misrepresent her as having talked about "logical form". Logical form, of course, isn't LF because if you think of logical form literally, it's something that ties in to logic: it would involve scopes being represented explicitly. The term "LF" has an etymological relationship to logical form, but not much of a relationship beyond that. In LF — so-called — you don't have enough nodes of the right categories to represent all the scopes that you get in a sentence with multiple quantified expressions. This results from the demand within that framework that syntactic representations be homologous, that whatever you've got in one level have a counterpart in other levels. This limits how many sentence nodes you have to serve as scopes. Anyway, I read Carolyn Heycock's paper and I say: Great, she's got a wonderful idea and the difference that she's got in what she calls LF, is in fact the difference in honest-to-God logical form. In that paper, she talks about a difference between *wh*-expressions that involve predicate elements like *how proud of Barbara* versus things that involve non-predicate elements like *how many accusations against John*. She comes up with the, I think, absolutely correct conclusion that the predicate expressions have to be inside their clauses in logical structure (here I charitably misrepresent her) versus the other ones which, in logical structure, could be outside the clause. Putting that into my framework, where you've got deep structures that are at least approximate semantic structures, and making heavy use of the really powerful version of the Cyclic Principle that I have, I can then say that the derivations that you get with these respective deep structures give me a violation of one of the anaphora constraints in the one case, and no violation in the other case. And I'm very happy: I got an explanation of something interesting, and I got it through the charitable misrepresentation of something done in another framework.

So when something new comes up like Optimality Theory, do you plunge into it and see how you can charitably misrepresent it?

I generally don't "plunge". But I come into contact with things done in various approaches, like by thumbing through journals and by going to conferences and staying in the same room because it isn't worth the trouble to get up and move to another room. (It's like watching television.) I get exposed to all sorts of things, a whole lot of which have nothing particular to recommend them, but quite a few of which have interesting stuff. And if a certain approach seems to be leading somewhere, in that it recognizes or discovers interesting facts or that it appreciates the significance of things that have not been previously appreciated, then I'll look at more stuff done in that framework.

The first big dose of Optimality that I had that really impressed me was a paper that Doug Pulleyblank gave a couple of years ago about vowel assimilation in Yoruba. He gave neat explanations for why the assimilations go in one direction in one set of words and in another direction in another set of words.

With regard to Optimality in syntax, I've heard some talks and I've read miscellaneous papers that have at least caused me once in a while to think in, let's say, Optimality theoretic terms about things in syntax. For example, I submitted a paper a few months ago to a Festschrift for a person whose name I guess I'm supposed to still keep in confidence. In this paper, I got into some odd differences between declarative sentences and questions in English with regard to when you've got an auxiliary verb and a negative, whether the auxiliary verb and the negative stay together or not. So take, for example, *Can't you see what he's doing?* Perfectly normal question. *Can*

you not see what he's doing? Sort of okay, but the preferred form would be the first one where you've dragged the negation along in the inversion. Now take questions corresponding to *You really can't see what he's doing*: *Can you really not see what he's doing?* That's perfect, way to hell better than: *Can't you really see what he's doing?* Here, I think, you've got something that you can describe in terms of ranked constraints, in the sense that there's a tendency — you can call it a constraint if you want — to have surface scope relations match logical scope relations. The obligatory inversion you've got in English questions would then force you **in that particular case** to have some sort of discrepancy between logical constituent structure and surface constituent structure. We're talking about the interrogative counterpart of something where you've got *really* above *not* and *not* above *can* and you've got to do inversion. That's gonna give you a surface structure where at least the *can* is higher than the *really*. But if you strand the negation, then you can at least minimize the amount of discrepancy that you've got between surface c-command relations with three elements and the corresponding logical relations. So maybe I benefitted to some extent from having been exposed to some Optimality stuff.

Any comments on what is going on in the field in other respects?

I am quite annoyed at a lot of stuff done in syntax recently, especially this absolutely screwy conception of syntactic categories that now prevails in the kinds of syntax coming out of MIT and its various branch offices. Let me give you a little bit of background. In the 1980s I would say, "What's right about X-bar syntax is the X and the bar. What's wrong is everything else". When I said "what's right is the X and the bar", I was referring to the X as it was used in the 70's and early 80's, when it was still a part of speech. Since the mid 80's you've had all sorts of stuff other than parts of speech being used as the X of X-bar categories, as in this thing NegP that now turns up all over the place. Well, the negative elements in the various languages of the world belong to particular parts of speech. There are negative adverbs, negative adjectives, negative prepositions, negative conjunctions, etc., and they share the syntax of those parts of speech, in addition to whatever properties they have in virtue of being negative. Calling something a NegP obscures the syntactic properties that those words have got in virtue of being adverbs, adjectives, or whatever. In addition, in the case of negative adverbs, it misrepresents their syntactic status. Negative adverbs, like adverbs in general, are modifiers and are not the heads of larger things. Actually, one of the things that annoys me about syntactic categories as they're treated in real recent MITish stuff is that it's really become hard for MITish people to say "modifier" anymore. I mean, all sorts of things that to me are obvious modifiers now get represented as heads of things that they aren't heads of. This particular conception of categories has become undeservedly popular and is yielding seriously screwed up analyses.

But in principle, according to what you said earlier, even that should still lead to something.

That's true. Even holding all sorts of crazy ideas like this conception of syntactic categories that I've been complaining about hasn't prevented the more astute practitioners from coming up with really neat stuff.

Do you think that people should not specialize in syntax or phonology or so and keep working in as many fields as possible?

It certainly is worth the linguist's while to stay in touch with all the things that have contact with the field he wants to specialize in. It may sometimes give you a slightly different perspective, a perspective that may help you see things. There's nothing wrong with specialization — it's actually highly desirable. But specialization shouldn't mean that you close yourself off completely from the rest of the world. It should be concentrating on a particular field, keeping in mind that it is to your advantage to be aware of things that are relevant to that field, but aren't strictly speaking within it.

But whether people should or should not specialize... Different people get different degrees of pleasure out of different things. My advice is: get your jollies however you can. I can tell you what sorts of things I have gotten fun out of, but I'm sure there'd be all sorts of fields that other people have gotten lots of fun out of. Good for them!

And for you, what is the thing that turns you on the most?

I guess a sort of broadly conceived syntax, where "broadly conceived" means I really don't particularly give a hoot about demarcating it from semantics, pragmatics, discourse structure, and what have you. I do syntax, but I pay attention to all these things, and I do not particularly care whether what I'm doing is what every self-proclaimed syntactician would call syntax.

Are there any properties which you think are crucial for being a good linguist?

I don't know. There may be some characteristics which are advantageous. Others probably don't matter. Or at least they don't hurt. I mentioned my background in mathematics to you. I often tell my students: the advantage to a linguist of a background in mathematics is like a vaccination. It gives you immunity against horrible diseases that you might otherwise catch, but it doesn't have any positive value in itself.

Somebody described you as probably one of the most well-travelled linguists in the world. What was your worst conference?

One memorably bad conference was on my first trip to Japan in 1965. It was a conference called, "Second World Congress of Phoneticians," organized by the Phonetic Society of Japan. It was a weird conference. First of all, it was an international conference, but most of the Japanese participants gave their papers in Japanese. The second thing was, a huge proportion of the papers had to do with a recent kidnapping case. There were tape recordings of the kidnapper's telephone requests for ransom and there were papers by phoneticians who had been doing phonetic analyses of these recordings, trying to identify the person from the voice. All the papers ended with the phoneticians saying that the culprit is yet to be apprehended. Another thing that was memorable was that the pre-prints for the conference were being distributed in plastic envelopes that had on them the words: "Second World Congless of Phoneticians." So that was a memorable, reasonably bad, conference.

And since 1965 there haven't been any bad ones? That must say something about the field!

Give me some time, I'm sure I can think of one or two more!

Any other odd place you did linguistics?

I once did a question and answer session for the International Institute of Dravidian Linguistics, in Trivandrum, Kerala, India. A transcript of the thing was published in their newsletter. I've listed it in my publications as "freely translated into Indian English."

One other interesting experience that I had was a three week course at Guangzhou Institute of Foreign Languages in 1987. While I was talking with some of the faculty there, including a German instructor, I mentioned that I had given

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lectures in German at various places. Then this German instructor started pleading with me to give a lecture in German. I said, what the hell, how can I turn down a request to give a lecture in German in Guangzhou China?! So I worked up this lecture titled: "Kritische Bemerkungen zu Chomskys neuster Syntaxtheorie". It was a special experience.

It must take some extra time to prepare a talk in German.

I've done a whole lot of lecturing in German. It takes me a bit of extra work. I'll have to dig up a dictionary and look up unfamiliar vocabulary. But the last two weeks I've been giving lectures in German at a whole bunch of universities. I studied in Germany a long time ago, so that's no big deal. I've done that lots of times.

I find it really interesting and enlightening to work up lectures in other languages. (I've also given lectures in Spanish a couple of times.) In a lot of cases, it alerts me to terminological gaps in the other language. I really had quite a hard time working up a lecture on the logic of conditional sentences that I gave in German in Hamburg a few days ago. German linguists that I consulted asking for terminological suggestions told me that this is a topic which, when Germans write about it, they generally write in English. Or if they write it in German, they publish it as working papers that you couldn't find in Chicago.

You are also quite famous for your knowledge of all the best restaurants all over the world in whatever cuisine.

You are exaggerating. I try to acquire such knowledge simply because having such knowledge is very useful for someone who likes good food.

There's a story of somebody organizing a conference in New York, who asked you to write the conference restaurant guide, although you were in far away Chicago.

That's a bit of an exaggeration too. Somebody asked me for suggestions and I gave them some.

Still, the New Yorkers organizing that conference apparently trusted your knowledge of the restaurant situation in New York better than they did their own.

I make a point of exploring New York's Chinatowns. There are some really interesting restaurants that are not that well known to round-eyes, or hairy barbarians, whatever the appropriate term is.

I remember that even in a place like Madison you knew all the good places.

Madison is a great place to visit. It's the only place outside of Nepal that I've been to that has two Nepali restaurants.

When you know where to find the good restaurants, you supposedly know where to find the bad restaurants as well.

I once ate in a very bad Chinese restaurant in Oslo, probably the most memorably bad Chinese restaurant I have ever been to. When I visited Oslo for a few days in 1969, I asked friends of mine there if they could recommend a Chinese restaurant. You know, good Chinese restaurants do turn up in odd places, like Alice Springs, Australia, for example, to mention one excellent restaurant. So my friend says: "We think the best Chinese restaurant is so and so in such and such *gatan*. Why don't you go there." I went to that place, I walked in and looked around and, evidently, it had, until recently, been a Norwegian rustic restaurant — they had knotty pine paneling on the walls and cartwheel chandeliers. And then they had Chinese lanterns hanging from the cartwheel chandeliers. A Norwegian waitress appeared and greeted me in Norwegian. She seated me and showed me this menu that was

entirely in Norwegian. I ordered a couple of things and they were absolutely wretched. The really memorable thing, though, was that there were little jars on the table containing condiments, and that the labels on the jars were not in Chinese, but in Japanese, and the contents had no relation to what the labels said.

Did you tell the waitress?

No. I didn't think it would be worth the trouble.

Are there any good Chinese restaurants in Holland that you can recommend?

Let me give you a tip on how to identify good Chinese restaurants in Holland: the good ones are the ones that have their menus written in Chinese and English, but not Dutch. If they have a menu in Dutch, don't go in!

You must have a fantastic memory. Not only because of all those restaurants but in the question period of one of the talks this morning you quoted Shakespeare, Play such and such, Act so and so. Just like that.

I was cheating, actually. I consulted something I pulled out of my bag — one of my collections of examples. Every year I print out this collection of linguistic examples, that I've gathered over the last year. The general title is "A Linguistic Flea Circus". I was quoting from my 1995 Linguistic Flea Circus.

Linguistic Flea Circus?

It's a collection of examples. Data fetishism, I told you! I write things down in a notebook that I carry around with me. Things I hear on television, or, while reading a newspaper, whenever I see an interesting example, I'll mark it and tear out that page. As you can see, there is a huge number of Spanish examples. I watch a lot more Spanish television than English language television. Most of these Spanish examples come out of telenovelas. What it comes down to is: I collect linguistic examples.

But data fetishism has paid off in my linguistics. I have all these interesting examples, organized in a way that I'm familiar with, so whenever I need examples, this will help me find them. There's a paper that I published about six or seven years ago in a Berkeley Linguistic Society volume, that was about a really odd phenomenon involving coordination. I had found maybe one or two examples in a year. It was only after I had collected maybe a dozen examples over a period of seven or eight years that I started to figure out what was going on. One example of this turned up in an opera review in one of the Chicago newspapers. It was something like: "The Temple of Dagon, whose exterior we see in Act I and interior in Act III, looks like a Hollywood movie set." Now, *whose exterior we see in Act I and interior in Act III*? You've got a shared determiner, *whose* applies to both conjuncts. You also have gapping, but the possibility of the gapping there is contingent upon your having a shared determiner — *the Temple of Dagon, whose interior we see in Act I and whose exterior in Act III* is much lower in acceptability. After I had collected a dozen or so of those examples, there were enough of them that I decided this was worth trying to write a paper about. So my Flea Circus does pay off!

This Temple of Dagon example you certainly quoted from the top of your head. So you do have a good memory.

I just happened to hit on the right mental file.

Epilogue

After we sent the interview to Professor McCawley for him to have a look at, one of the comments he sent back to us was the following. It is related to our first question:

I am surprised that in the interview I did not express more repugnance at the thought of "everybody working in the same framework, using the same theory" — that's as disgusting a prospect as everybody espousing the same religion or everybody speaking the same language. Fortunately, such a nightmarish situation could arise only out of a political situation that was even more nightmarish.

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