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The schwa is a special vowel: it is often the result of reduction or epenthesis. It is indispensable, 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 of the language, or it is a result of reduction; etc. Linguistic theory has to explain this special behaviour of schwa: why is it exactly this segment which behaves exactly in this way in so many languages? A lot of subtheories of phonology have dealt with schwa. For articulatory theory, stress theory, stress-syllable theory, 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 extensively 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 proposals 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 (monosyllabic) theories one may deny the role of vowel epenthesis or deletion altogether and claim that the occurrence or non-occurrence of schwa 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, a (or non-)derivational 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 appelle [apel]-spale to call] ‘appelle [apel]‘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: schwa is a vowel for which no inherent articulatory target has been specified, or as a vowel which targets a neutral vocalic position, ‘the mean tongue-trac variable position for all the full vowels’. (Cf. Browman and Goldstein 1992).

From a standard autosegmental/métrical 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 (e- or i-con) 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. ([1] and [2] are two such options).

In Van Oostendorp (1995) it is proposed that the following general constraint scheme:

\[
\begin{align*}
\text{Fill}: & \text{Empty segments are disallowed.} \\
\text{FILL}: & \text{Empty segments are disallowed.} \\
\text{PROJECT (F, P)}: & \text{If a vowel is [+F], it should appear in position } P \text{.} \\
\text{PROJECT (P, F)}: & \text{If a vowel appears in position } F \text{ it should be [+F].}
\end{align*}
\]

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 ‘schwa’ 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 epenthized beyond the feature [+sonantal]. In the case of deletion, it is simpler to delete only the specification [+sonantal] than to delete this feature alongside others (which is needed in case of the non-empty full vowels). It is therefore 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 constraint forms for this (i.e. 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 in those contexts which 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.

Type A: Prevocalic schwa deletion. The evidence for this type of process is that schwa cannot be given a fully phonetic interpretation. It is actually hard to find evidence for the rule in French, because we need a context where /aV/ surfaces as [XaV], and schwa is deleted at the end of a word or phrase in many cases anyway. French has the masculine singular determiner le is sometimes cited as evidence for the rule. This cletic surfaces as /la/ before a consonant-initial stem (le camerade ‘the comrade’ [lakamrd]) and as /la/ before a vowel-initial stem (l’amie ‘the friend’ [lamsi]). However, the fact that similar alternations apply 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 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./tomt/ + /e/). This schwa never surfaces at the end of a phrase, and in those environments in which it does surface (autre ‘other woman’ [rotam]), this may well be due to schwa epenthesis (alternation type C discussed below). Although the indirect phonological evidence, which is based on the process of liaison but which I cannot review here (cf. Encr̄ 1988 and references cited there), is favourable to the hypothesis that the feminine singular suffix is a schwa, it is not 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 (elie ‘elites’) is followed by a vowel-initial suffix [-ir [ri]], the schwa gets deleted (elitair ‘snobish’) (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 postulate a constraint to account for 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:

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 in a word 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 onset (*[lkamrad]‘comrade’). 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 example is the deletion of a 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 an h in earlier stages of the language but that is presently phonetically empty, although phonologically it still behaves as a full-fledged consonant:

*(6)* meta le desus retraite le meta le dehors retraite las
muldoy retum roodos
put it on! put it outside! raise it again!

It is standardly assumed that this behaviour can be understood by assuming some more or less abstract constraint 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 in certain context, this being a rule epenthising 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 morpheme combinations, 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 another form B based on alternations. A schwa in form A corresponds to another form B based on alternations. A schwa in form A corresponds to another form B based on alternations. A schwa in form A corresponds to another form B based on alternations. A schwa in form A corresponds to another form B based on alternations. A schwa in form A corresponds to another form B based on alternations.

For this type of process is that schwa cannot be

(9) porter clefs ‘keyring’

garde fou ‘railing’

porte manuot ‘coat rack’

garde magner ‘coat 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 vowel in the foot should be a schwa. These assumptions are not uncontroversial, but as far as I know alternatives to Charette’s proposals are yet to be worked out.

Type E: Schwa deletion in phrase-initial syllables. The existence of 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 alterations such as the following:

(10) revenu (domain) [rvn] ‘revenue’ [rvn ‘revenue’]

tes [fais] ‘pay off’ [fs ‘pay off’]

don’t worry

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

According to Charette (1991) this type of deletion is restricted to bisyllabic words: cheval ‘horse’ may be pronounced as [ʃawl], but chevalier ‘knight’ is (savale). It is possible that unstressed syllables allow more complex onsets. On the other hand, the explanation given here seems to run counter to the explanation of porter clefs vs. porte manuot. Finally, French speakers do not try to build a ‘perfect’ iamb ([savale]).

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 French, Durand 1986).

(11) je te croir ‘I see the other’

voil mon oncle ‘there is my uncle’

tes est plate ‘the earth is flat’

la route est longue ‘the road is long’

It is not immediately clear why schwa should be

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deleted in final position. One possibility, suggested in Van Oostendorp (1995), is to extend the scope of the constraint FnalC to the phrase. The constraint would thus say that all phrases have to end in a constituency. 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 role 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 ([paquet][ɛ])
- 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 favours 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.

The projection 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) (Alderete 1996): 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 FAITHFUL-structure.

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

(13) Unstressed vowels turn to schwa.

Alderete (1996) claims that FAITHFUL-structure constraints can also be put to use in analyses of phenomena other than reduction. For instance, he shows that the phonetic rules usually avoid stressed positions (even if they are not schwa) can be understood in terms of this constraint. It is worse to epenthize into a stressed position and violate both FAITHFUL-structure and FAITHFUL, than to epenthize 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 FAITHFULNESS >> *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) unstress vowels turn into schwa

More in general, a FAITHFULNESS account implies that the constraints on unmarked stressed positions than in unmarked stressed 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 unmarked

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) Project-foot (a) FAITHFUL-foot (b) FAITHFUL-structure

A language that only allows schwa in stressed positions is not part of the typology in this system: if we rank 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 FAITHFUL-structure.

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

Similar facts can be found in other languages as well (Burzio 1994:113). In English, /stol-á/ ‘office’ /petrol/ ‘petrol’ /kanstír/ ‘car’ /kántor/ ‘office’

This is the same restriction as the one that Burzio (1994) claimed to exist for English. Yet in the literature on 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 reduction is licensed to 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 minimal vowel 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.

5. 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, Muisens 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.

5.1. Syllable type

Vowels in open syllables are easier to reduce than those in closed syllables; in some ideophones (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.

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).

(17) /brazína/ /brazína/ /petrol/ /petrol/ /kanstír/ /kántor/ /office/

In Dutch — as in English and apparently any other language in which reduction is dependent on the position in the metrical structure — only un- stressed vowels get reduced. As a matter of fact, we cannot 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.

5.2. Stress position

In Dutch — as in English and apparently any other language in which language is reduction is dependent on the position in the metrical structure — only unstressed vowels get reduced. As a matter of fact, we cannot 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.

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 stressing rules into this apory 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 un- stressed syllables ending in a sonorant; in this way, he can get rid of the so-called ‘sonorant destressing’ rule.

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 de-emphasised. There is no independent evidence for the latter assumption.
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).

2. Vowel reduction in Dutch


Here it seems that the position next to (on the left hand side of) a non-tonal vowel is less likely to undergo full reduction than vowels in other positions.

v. Position in the word:

Vowels in absolute initial- or absolute word-final position do not reduce. This observation is absolute surface-free in Dutch. No matter what the (surface) mode 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) /lêzit/ [läzit] ‘fun’
/lês/ [les] ‘even’

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 one syllable 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) /hês/ [hes] ‘[h]alf’

We will see in the next section that also underlying schwa cannot 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. Bezuijen 1970) have claimed that Dutch schwa is always the result of reduction: words such as mode ‘fashion’ are underlyingly specified as [mod]; in that case reduction is obligatory in these environments and the word tofog ‘coal’ syllables closed by a non-tonal vowel is phoneticized as [təfog] ‘coal’. Words such as hennep ‘tobacco’, 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 syllable type, and only in an unstressed position. The fact that vowels can preferentially occur in syllables headed by a full vowel and only in an unstressed position.

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 languages such as Dutch, in which stress always falls on the penultimate syllable if the final syllable contains a schwa (ballade [balade], *[balade]); if the final syllable contains a full vowel, stress is falling on the second syllable of the word (a-b).

Alternatively, schwa can behave as if it is completely invisible for stress; under the standard assumption that Dutch feet are trochaic, we should say that schwa can only occur in the weak position of a troche.

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 which are inconsistent with their theories. I suspect that we will not have a satisfying theory of schwa until we have a satisfying Theory of Everything.
**A schwa bibliography**


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 thin, 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 1980/95), 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 process of constructing 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 of inferential activity is 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 scene 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 to not 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 inferential 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 inconceivable relevance: it would overturn all our conceptions of the occult if it were 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 notion 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 ‘interprettive 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 disassociating myself from such a proposition, perhaps with contempt.

Dan Sperber (1984) 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 lying.

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 — 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 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 pretense 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.

References


THE SYNTAX OF PAST PARTICLES. 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 participle 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 participal V:

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

(2) ... co’ denari avresti la persona perduta.
with your money you would have your life[lost]
(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 is established and the construction illustrated in (1) and (2) in Old Italian are given in (10):

(10) *Conosciuta Elena Gianni, ... known Helen John, ... (8)

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. Participal 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 afore, given that a similar shift takes place from (3)/(4) to (6).

The advantage of this analysis is that it allows us to generalize a well-established approach to Nominaive 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 SpecAdj 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 absolutive. This 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.

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.

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):

(3) serrata la cella con la chiave, dirittamente se n’andò...
locked[–Agr] the cell[–Agr] with the key, immediately went away...
(Decameron; I:4)

(4) ... il re di Francia, molte truppe fatte con gli alamanni, morì
the king of France, many troops[with] made[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: a aver trovato la casa mia... and having found[–Agr] my house[–Agr]
(Decameron; I:2)

(6) absolute participle: serrata la finestra se ne andarono...
locked[–Agr] the window[–Agr] they went away...
(Cene: Introdat.)

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According to the classical analysis, the lexical subject in (5) is represented by an expletive element, pro, that occupies the specifier of Infl; cf. Rizzi (1982), (1986) and Chomsky (1986). Consider the structures in (9):

(9) Spec AgrO VP

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(10) Spec AgrO VP

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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 of 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’

Verner Egerland


Yves D’Hulst
Word order in the Old Italian PAC with two arguments could be VOS, SOV, or VS0; I have not found examples of *VOS or *OVV. The generaliza-
tion 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-
choice. If the reflexive particular pronominal case 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: DPO moves overtly to SpecAgrO; DPO(S) stays in situ, being copied by an expletive pro in
SpecAgrS; the verb raises overtly to AgrS.
2. VSO: DPO moves overtly to SpecAgrS; V raises to AgrS; DPO moves overtly to SpecAgrO
or stays in situ, being copied by an expletive pro in SpecAgrO.
3. VSO: The verb raises overtly to SpecAgrS; DPO(S) and DPO(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.

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 parti-
ciples are attested:

(13) Modern Italian (from Belletti 1990, 95):
*Non arrivata Maria. Gianni tiro un sospiro di sollievo.
nott arrived Mary. John relieved

(14) 14th century: 
... il papa non dimenticato il saggio intelletto a
la mente dentro rimaneva loro umana, ...

According to Zanuttini (1991) negation depends
on the presence of Tense. Belletti (1990) has
argued that the Modern Italian PAC lacks a Tense
node, contrary to 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 Bellet-
ti, V-raising to Comp is necessary for Case rea-
sons, 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, where-
as a lexical argument cannot coccur 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 divertiamo ugualmente.
although left M, we amused ourselves anyway


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 (Scandi-
navian) 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, 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 and how these specu-
gistive 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
structures 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 inter-
preted 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, independent of
word order. Subsequently, agreement became
more marginal whenever the object followed the
participant. In both periods of time, there appears
to be some variation among authors and excep-
tions 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. Fre-
quently, 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 histo-
rys had little reluctance manipulating the origi-
nal 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

References
Aspects of Verb Syntax. Rosenberg & Sellier,
Toronto.
Praeger, New York.
Linguistic Theory.’ In K. Hale & J. Keyser,
ed., The View from Building 20: Essays in
Linguistics in Honour of Sylvain Bromberger.
Egerland, A. (1999) ‘Past Participle Agree-
ment in Dutch and English.’ In E. Reuland &
W. Abraham, eds., Knowledge and Language.
II: Lexical and Conceptual Structure. Kluwer
Kayne, R. S. (1992) The Minimalist Sup-
parte Agreement.’ In P. Benincà, ed., 85–103;
Dialect Variation and the Theory of
Grammar. Foris, Dordrecht.
Kayne, R. S. (1990) ‘Toward a Modular Theory of
Auxiliary Selection.’ Studia Linguistica 47,
Rizzi, L. (1982) Issues in Italian Syntax. Foris,
Dordrecht.
Rizzi, L. (1980) ‘Null Objects in Italian and the
the three stages of Italian involves two parameters: to have occurred at a later stage, the fact remains agreement in VO construction score extremely low. exactly the three texts where both OV order and

\[ \text{(1)} \]

b. Operators yes yes no no

\[ \text{b. Locus of checking AgrOP AgrOP AspP AspP} \]

a. Strength yes no no no

\[ \text{a. Strength yes no no no} \]

\[ \text{b. Locus of checking AgrOP AgrOP AspP AspP} \]

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, Gianna tiro un sospiro di sollievo arrivando Maria. Gianna was relieved
b. Conosciuta Maria, Gianna ha subito cambiato il suo stile di vita

known Gianna. Gianna 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. Recent work on modern Italian, and they do not allow the (transitive) subject to be expressed overtly, witness (6b).

(6)

a. Maria arrivata, Gianna ti ho dato un sospiro di sollievo
b. Conosciuta Maria, Maria ha subito cambiato il suo stile di vita

known Gianna. Gianna has immediately changed his lifestyle

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 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 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 yes no yes
b. Overt S and O yes no yes no

e. Maria arrivata, Gianna ti ho dato un sospiro di sollievo
f. Conosciuta Maria, Gianna ha subito cambiato il suo stile di vita

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Egerland’s account of the different settings of medieval, renaissance and modern Italian with respect to the word order property in (7a) is based on the fact 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 case V. Hence, medieval and renaissance Italian license two overt DPs (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 Italian pac’s (see (14) in the author’s summary), but not so in modern Italian.

Egerland’s proposal that modern Italian absolute past participle constructions exhibit a strong tendency towards V-to-C raising and lack a tense projection is close in spirit to what Belletti (1990) has 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 or accusative for the object of transitive verbs. In Egerland’s account, the link between agreement and case assignment / checking is preserved only for intransitive 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 SpecCaspP in order to check the nominal feature of AspP, and move further up to SpecAgrOP to insure agreement with the passive predicate.

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).

Egerland argues that examples like (10) need not constitute counter-evidence against a passive analysis and that the anaphor could be bound directly by the matrix subject. However, the argumentation seems 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 cassa Maria, praised him or her, but not that some arbitrary person praised someone else. Nevertheless, even that 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 context-free delimitation 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 its 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.

References


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 adulthood by age 5, 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 3 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 present 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

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. Hypothesis 1986). 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. it 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 adulthood by age 5, at age 2 object scrambling and object clitics are optional even where they are obligatory for adults.
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] [SpecP (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 (Xe) 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. [ArgSp Gianni] [TP [SpP pro] la [AgrOP [VP mangia t, ...]]] b. [dat [ArgSp Jan] [DiscP [TP] [SpP het boek, [Sp Opsit niet dat] [John [TP], the book [not [ArgOP [VP, wanneer 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, but that it + 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 DP s without a [+specific] feature. I argue that these object DPs are interpreted referentially, just as in adult language, but that children will 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 sentence contains a tree indexing the Noun, the Determiner and an operator in SpecDP, which makes referentiality visible.

(4) D-chain

[Op, 1e, the, het, het, the, tree]]

Furthermore, I propose that there is an Referentiality)-Chain, which coindexes the 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

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 (more or less) real world directly, that is, without an R-chain, as illustrated in (6):

(6) No R-chain

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? 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

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 SpecP 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 SpecP 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 5 children perform roughly adulthood 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 proposes a unification theory of parameters. Further research is needed to create his or her first multi-word utterance — accounts for the referential interpretation of ‘normal’ definite DPs, such as the tree, as is predicted by the analysis proposed here.

References


Sportiche, Dominique (1992). Clitic Constructions. Manu-

cript UCLA.

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 Fo 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 method 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 child’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 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.

Dissertations


14
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 unmarked in at least Dutch language. This explains utterances as the following:

1. Bert gaat niet de peer opeten (C. 2;9)
   Bert does not the pear up-eat
   Schaeffer p. 57, ex. 1.4)

2. Ermie goes not two trees out-cut
   ‘Ermie via guide out two trees.’
   Schaeffer p. 59 (ex. 2.4)

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 semantics. 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 prefers scrambling over 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 be more firmly 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 bears the +specific feature, 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. She 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.

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 Specificity 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 realized 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, many children phonologically realize definite (and specific indefinite) determiners that are argued to be unmarked for specificity. Since direct object clitics and definite determiners (+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 cannot 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 (e.g. VI 74).

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

Schaeffer p. 62, Figure III

<table>
<thead>
<tr>
<th>Italian children</th>
<th>overt clitic</th>
<th>omitted clitic</th>
<th>full direct object</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>22% (22)</td>
<td>64% (63)</td>
<td>14% (14)</td>
</tr>
<tr>
<td>3</td>
<td>62% (179)</td>
<td>15% (43)</td>
<td>23% (68)</td>
</tr>
<tr>
<td>5</td>
<td>91% (227)</td>
<td>0% (0)</td>
<td>9% (23)</td>
</tr>
<tr>
<td>adults</td>
<td>100% (439)</td>
<td>0% (0)</td>
<td>0% (0)</td>
</tr>
</tbody>
</table>

Schaeffer p. 64, Figure IV

<table>
<thead>
<tr>
<th>Dutch children</th>
<th>full object</th>
<th>dem. pron.</th>
<th>strong pers. pron.</th>
<th>clitic</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>52%</td>
<td>26%</td>
<td>6%</td>
<td>16%</td>
</tr>
<tr>
<td>3</td>
<td>15%</td>
<td>26%</td>
<td>1%</td>
<td>58%</td>
</tr>
<tr>
<td>5</td>
<td>7%</td>
<td>7%</td>
<td>0%</td>
<td>86%</td>
</tr>
</tbody>
</table>

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 marking. This is making 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. If the results of all cross-sectional research presented 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 the individual differences would shed more light on the amount of functional structure. Since 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, it will inspire further research along the same lines.

References


Somer ville, Mass., Cascadilla Press.
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.

A tree 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.

The main chapter (chapter 4, “Parametric Rules”) introduces a set of parameters which determine a more restricted class of permissible rules. These three parameters correspond to specific subtheories organized as independent 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-geometric 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.

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 (not nece., 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;
- b. both α and β are associated to the same anchor tier and no anchor intervenes on that tier between the anchors to which α and β 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 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 p1, p2 containing α, μ and its anchor μα, and μα and its anchor μα, respectively, then if μ precedes μα, μ precedes μα, 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 bemissible (undersetupled), 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 segment” in a 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 following: +LO → –BK. Since any of them can be unspecified underlyingly there are 2 = 8 combinations, namely those shown in (7a). The conditions in (7b–d) eliminate three items, those represented by 7b–d.

(7) a. * +HI +HI +HI +HI +HI
b. +LO +LO +LO +LO +LO +LO
Path Conditions: (Universal) if +HI then +LO
if +HI then not +HI
(Particular) if +HI 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 +ATR and ±ATR.

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

(8) a. * –HI –HI –HI –HI
b. +LO +LO +LO +LO +LO +LO
+BK +BK +BK +BK +BK +BK

We now have four combinations containing +LO which are not excluded by any universal condition like (7c). An obvious question is whether such a case could be a possible case, but it is surely very marked. In general, given two representations, like a1 = [+LO, +BK] and a2 = [+LO], there are three possible outcomes. First: they are generally 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 a1 and a2). The third possibility is that the more complex vowel is eliminated. This exclusion is achieved through the combined action of two principles, Representationality 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 a₁ and a₂, but if there is clear phonological evidence that both a₁-vowels have a role in the language; otherwise simplicity will rule out a₂, the more complex one.

Thus, combinatory specification differs from radial underspecification in that it only implies 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. Underspecification is only possible 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 do not create an F-value which does not have to precede the rule that mentions this value.

Archangeli and Pulleyblank consider the size of vocative inventories predicted by combinatory 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!·2. Two examples of such cases (all elements linked underlyingly, all elements free and analyzed) are the vowel systems of Barrow Inupiaq and Tiv. Barrow Inupiaq has three free F-elements, [+HI], [+RD], [+LO] which define the set {a, u, i, 1, 2}, i being the totally unspecified V. Tiv defines with the three free F-elements, [-HI], [-RD], [-LO] the system {e, a, o, 2, 3} a representing a low rounded V. In Tiv, however, the total number of vowels is 2^2=4, since two combinations, those which are [+HI], [+RD], are not permitted.

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 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 lan-

guage 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-combinations, the path conditions, are formulated in the shorthand notation F1/F2, interpreted as ‘if F1 then not F2’. Thus the HI/ATR Condition states that most languages do not use the feature [ATR]. The HI/ATR specification indicates the probability (between 0 and 1) that a language might have a [ATR] vowel. 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 of F-elements. Chapter 5 is devoted to the third type of markedness statements, marking of path condi-
tions. Path conditions, now grounded conditions, are formulated in the shorthand notation {i, e, a, u}! (grounded conditions/context) for delinking and the grounded conditions/context notation. The phonetic articulatory basis of conditions is overgeneration of phonological inventories, as shown in (12), which is phonetically motivated.

Archangeli and Pulleyblank indicate, quite correctly, that there “have been few attempts at a constrained theory of graphically represented rules” (285). By “graphical rules” or “rule pictures” they mean standard notations using autosig-

ments, association relations depicted as lines, the common stricken line (=|=) for delinking and the dashed associative line (—) for spread-
ing, together with more or less mixed SPE nota-
tion. It should be stressed though, that the problem with graphical rules does lie in the graphic character (a two dimensional representa-
tion), but in the items themselves, 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 rules have the following format (12):

The action of a rule is thus limited to insertion or deletion (b) of an association line (spreading and deleting, respectively). If one requires that 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), it is written with an arrow as in (12d) 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 Menominii (13). The argument, [+ATR], is subject to a contextual requirement, namely the presence to its right of a [+ATR] and some because of ATR/LO. Thence 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 be long (homorganic: µ).

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 (bimoraic: \( mm \)). Hence it must be nonlow, and that be long (‘moraic: \( \mu \)).

There is an important part of the book that I have not reviewed, namely the detailed analysis of many phonological processes (naturally ATR insertion and spreading) on which the theory rests. These analysis include 16 different lan-

guages families, among them Niger-Congo (7 languages) and Bantu (4 languages). Further research on ATR systems, and vowel systems of languages (among them Niger-Congo, 5, "Conclusion", there are many open ques-

This in 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 confer-

ence in Hyderabad, India, earlier this year, is the start of a new branch of activities; the next Asian GLOW is planned for next year in Japan. (GLOW’s website is: <http://cis.kub.nl/~fdl/research/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 (ordinary), 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 gradu-

ate 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 con-

ferences than in the past. Looking at GLOW this year it is rather gratifying to see how many of the public is. There are young generations, and they do tend to come. But they 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 quibbles about whether mini-

malism 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 establish-

ment. 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 cent-

ers 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 genera-

tive 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 EUROTYPO, funded by the European Sci-

ence 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 gram-

mar. Their actual plan was: we do complementa-

tion, 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
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, these are interesting — 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 feel an obligation 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 also typological, are at a low level. 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 content on the screen of your computer any time. You can download it for free. 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.

When I’m thinking that 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, putting, 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, in a way, 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 association of linguistic organizations. There is the Polynesian Linguistics. 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 administrative circles. Not too surprisingly, it didn’t work out. But maybe some day or another we will join up. It has been number this time 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 introductory remarks, the history of the 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 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 organization to make it possible. With an unflagging sense of dedication they did an enormous amount of work. This GLOW was a truly “Extraordinary conference in a Extraordinary” way.

The conference started on the 19th of January with a lengthy inaugural session where the 80-old participants had the chance to hear the history of the organization of the conference by K. A. Jayaseelan, the idea of GLOW 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. The conference is organized by a certain number of 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 the old generation feels that these superiors tend to be ratheraloof. They could certainly do with some rejuvenation at the content level and we could do with some of their clout in administrative circles. In Prague you still have these relics of the Prague school. So what you see is that the young are eager to do other things, like generative grammar, but their superiors tend to be ratheraloof. They would love to be part of Prague school and they are helped if their superiors tend to be ratheraloof. They want to perpetuate the Prague school. But the Prague school hasn’t led to any significant insights for 30 years. Sometimes you and I 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 do because their superiors tend to be ratheraloof and the controls. So they have to do Prague school. So what you see is that the younger generation give talks in which they sort of display how apathetic 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 tramps 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 association of linguistic organizations. There is the Polynesian Linguistics. 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 administrative circles. Not too surprisingly, it didn’t work out. But maybe some day or another we will join up. It has been
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 clausal embedding. The 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 Bondoisation has taken place, 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 assumption that the 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 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 how the 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 insufficient to account for 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 an alternative to the Reichenbachian approach: the first, the specification, as part of the representation of certain temporal and aspectual relations, of the mapping of the predicate’s event structure into the parameters that are to take 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. Finding that this resolution was a successful one was 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 the right boundary of the 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 movement model. In either case the tenses were not open but they will certainly be exciting future episodes of the theory Mamoru Saito outlined.

Four talks addressed issues in optimality theory where the focus was on the restrictions morphologically manifested in Sanskrit are covertly present in the syntax of English. In his presentation entitled “Aspect and Tense and Aspectivity in Kannada” by Rajendra Singh and Probal Dasgupta, “Only in Hindi” by Rajendra Singh and Probal Dasgupta, “Light Verbs, Empty Prepositions and Zero Complementation in English” by Zas, “A Critical Discussion of Asymmetricity, Rightward Movement, and the Syntax/Pragmatics Interface” by Steven Schaufele. The theme of the GLOW workshop on 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 “Plurational Verbs in Hua”). 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 Hirohoko 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, “‘It’ and ‘be it’ in the Abkh Mabkhaz: m-reduplication in Abkhaz, weightless infixation in Miskito” by Paolo Acquaviva (University of Venice, “Uniform Lexical Spell-Out” who proposed a high賓struency A-structure Revisited’) presented a highly enlightening analysis of object experiencer verbs like *frig or friguen*, *keen*, *concern, delight, worry*, etc. She showed that these verbs may have three readings (agentic, eventive, and stative), and that only the stative reading is associated with a “psych proper-

ties’. Moreover, any construction can be interpret- ed as a psycho-verb construction, once certain conditions are met (having to do with the animacy of the experiencer and the incapability of physical action). In the above example, even if it was a declarative clause, what the speaker has said is that they are doing or planning to do something that involves the experiencer and the incapability of physical action. The system was demonstrated in a range of types of tenses and aspects from various languages.

Anna-Maria Di Sciuollo (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 re- lations (the complement, adjunct, and specifier relation). Morphological objects are binary branching adjunct-head configurations, and the only syntactic objects contained in them 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, ‘On the Projection of Transitive Verbs’) proposed that the grammar for Language Research, ‘On the Projection of Transitive Verbs’) proposed that prosodic phono- logical 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 details of the argument are stated: in Basque, the features to be formal features, and adopts Alexia- nia, ‘Sorting out you, me, and the rest of the syntax’ was concerned with motivating in syntactic terms a typology of polar- ity items well established in the semantic litera- ture, 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. Roovers in Prod. 1994).

Grecon Müller (University of Stuttgart, ‘Feature Strengthening’) discussed the problem that remnant topicalization in German may create unbound traces, but not unbound in- definite existentials. Therefore, in the context of the more general question of how to describe successive cyclic wh-movement in terms of feature checking (it involves feature strengthening of the wh-features of the embedded CP, for instance), it is not always the same feature that is occupied by the wh-features of the embedded CP, forcing Jackendoff (1991). Lea Nash and Alain Rouverture (University of Paris VIII, ‘Feature Fission and the Syntax of Argument DPs and Clitics’) applied their theory of 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 particu- lar, argument clitics. The analysis takes argument features to be formal features, and adopts Alexa- nia and Anagnostopouloou’s (above) conjecture that the internal and external argument of the verb cannot both be realized inside VP at Spell-Out.

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Jairo Nunes and Eduardo Raposo (Uni- camp and UCSC, ‘Portuguese Inflected Infinitiv- als and Configurations for Feature Checking’) discussed the phenomenon of Portuguese, where the verb in intransitive clauses is inflected 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 Agree- ment as a case study) was concerned with the iterativity of Toronto, ‘Asymmetries in Featural Marked- ness: Place of Articulation’) argued that there are two problems concerning place of articulation 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 also the case that place features to specify the interpretation of the main 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 (Univer- sity of Toronto, ‘Asymmetries in Featural Marked- ness: Place of Articulation’) argued that there are two problems concerning place of articulation 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 also the case that place features to specify the interpretation of the main 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).

Javier Ormazabal and Juan Romero (University of the Basque Country/Basque Center for Language Research and Instituto Universitar- io Ortega y Gasset, ‘Attract-F: A Case Against Case’) proposed to replace Case features (which are present for theoretical reasons only) by animacy features (which are interpretable) and use these as a way of checking if constructions which lack Case, they proposed an aspectually-based ac- count of agreement. 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. Further, if a language does not have voice, velar consonants count as unmarked in these segments. On the other hand, certain languages do not allow these segments, because they disal- low completely empty place nodes. Rice and Causley also presented a generalization from 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 repre- sentational oddities that may not be acceptable to all scholars: e.g. the features [Lahial] and [Dorsal] are subsumed under a class node [Peripheral].

Elizabeth Ritter and Heidi Besley (Uni- versity of Calgary and University of Pennsylva- nia, ‘Sorting out you, me, and the rest of the world: A feature-geometric analysis of person and number’) presented a feature geometry of pronom- inals for English and Spanish based on 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 preserved under a wide range of operations. The representation involved an analysis of pronominal paradigms from a variety of languages.

GLOW ’98 Syntax Workshop ‘Representative 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 ac- count of agreement. Thematic roles are aspectual features that are generated in functional catego-
Oystein Vangsnes (‘The role of gender agreement in Scandinavian: a study of possessive constructions’). In his talk, he pointed out that only in those dialects that do not mark agreement within the possessed NP can a non-agreeing subject pose serious problems for Optimality Theory (OT). Glot International, Volume 3, Issue 5, May 1998

David Odden (‘Cyclicity and Counterfeeding in Kimatumbi’). In his talk, he pointed out that the paper on ‘Non-agreement and definiteness’ did not examine definiteness, so agreement was not examined. His data were drawn from several (Bantu) languages and he showed that an OT analysis of these facts would involve a heavy machinery, including Sympathy Theory. The first level of the output-output relation. Neither of these three ⊂ ‘et al’ positions 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 pointed out that the conclusion out of such an analysis would therefore be preferable. From an empirical point of view, both theories seem to be equally equipped to handle these phenomena.

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 in the first position.

Ronald Sprouse unfortunately was not able to be physically present at the workshop. His paper was read by Orhan Orgun. Mr. Sprouse’s paper ‘Enriched Input Sets as a source of opacity in 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 optional and the two-level model: 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 which are only relevant in the second 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 constrained facts of Yowlumne (= Yawelmani).

Orhan Orgun’s own proposal ‘Phonological Opacity and Synchronically Arbitrary Alternations: What Two-Level Phonology can contribute to OT’ focused on the role of the two-level function in 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 phonological opacity. He based his thesis, 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.

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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 $u$ is not a functional head. She discussed 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 different from the cross-linguistic behavior of elements that agree 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,TP. She pointed out that verb movement versus focus projection. Under this analysis, no improper movement arises, and the case and agreement facts are explained.

Micha Luciana Stomin (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 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 (MASH), according to which segment duration reflects syllable structure.

Marina Vigário (University of Minho, 'On the Prosodic Status of Stressless 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.

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 $\text{ABLE}$. 

Verbal $\text{Small Clauses}$ was the topic of Joan Rafel's paper (University of Girona). He argued that $\text{Small Clauses}$ are realised by verbal or verbal-like $\text{subject}$s and $\text{object}$s. He then applied such a structure to the analysis of three constructions: pseudo-relatives in French, naked infinitives in English and the Prepositional Initial Construction in European Portuguese.

Susanne Glück and Roland Pfü (Goethe University, 'On Classifying Classification as a Class of Infection 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 phenomena and prosodic and in lexicographical constructions.

Maria João Freitas and Matilde Miguel (University of Lisbon, 'Prosodic and Syntactic Interaction: the Acquisition of NP Functional Parts in an Atlantic 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 interaction being masked by typical lexical codas. The analysis proposed for fricatives in coda was also productive for the explanation of emergence of syllabic material occupying determiner positions.

Michael Vinka (McGill University, 'Nomodative 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 AsP, and, from this position, block NP-movement.

Hironobu Hosoi (McGill University, '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 general idea accounts for 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 remove 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 Theodora Marinis’ talk (University of Potsdam). She 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.

Dolina Kalluli (University of Durham, ‘The Common Basis of Clitic Doubling and Scrambling’) suggested that clitic doubling and scrambling have a common basic: 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.

Estrella de Roó (HIL/Leiden University, ‘Nominal Projection in a case of Agrammatic Aphasia in Dutch’) investigated the functional structure in a homogeneous 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-citication, Raising to Subject, and Case’) made a new proposal for en-citication, explaining the asymmetry between quantitative and genitive when extraposed from derived subjects. Her analysis was based on Case. She claimed that the absence of an overt nominal (quantitative en) makes the required NP unable to check Case, rendering raising to Spanish illicit.

Danny Fox (MIT, ‘Economy, Asymmetricity, and the Ellipsis Scope Generalization’) revisited 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-Langeved and Anikó Lipták.
WinSAL, which stands for Windows Speech Audio Lab with Video option, is a tool for digital speech processing and acoustic analysis. The WinSAL program uses the various 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, which are a little hard to appreciate without a video option. 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 controls are 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 requiring a trip to the manual. 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 means that only short segments 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 laser printer 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 came across the low-cost 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 used for both speech and music analysis. 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 to be very helpful and useful tutorial. The tutorial is full of tips and techniques for signal analysis and also contains an excellent example of how to add your own set of functions, so the standard trade off of frequency analysis tools make this package inappropriate for large research projects. In addition, the variety of minor annoyances like the strange form of highlighting an on-line help window which 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.
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 he had not killed anybody 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”. They were all in the office next to Bill’s. They were in the office right next to Bill’s. He had told them 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 had made an 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 situation. She was always so very nice to them. Who was it? Do you know her name?

Paul smiled at her. Wasn’t she wonderful?

“Anyway,” James continued. “As to those seven people, I talked to all of them, the other three will come in this afternoon. The four I talked to this morning. They were in the office right next to Bill’s. He was right, of course. Bill had had their beers. Thanks, she said. He had had their beers. She was saying: “As I can see now.”

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 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 of anything like that. I think you don’t have enough paper!’ He says this in a sneering tone. And I heard him say something like: ‘I can see anything unusual. I was wondering over this question, he sat down at his desk and said angrily. “So, what happened and do you think you can tell me everybody’s exact words?” 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 walked 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 much more nice 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 away as he 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 like friends, no animosity. But now, Nartin was fuming. ‘Did you hear what he said?’ he asked me. I said, ‘Yes. I think 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 was 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!”

But they were talking about the reasons why Nartin did not get the job when you were up there earlier, right?”

“Yeah.”

“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

Interview

The first big dose of Optimality that I had that really impressed me was a paper that Doug Puleyblank gave a couple of years ago about vowel assimilation varying 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 got 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 term "LF" has an etymological relationship to formal, which ties in to logic: it is something that ties in to logic. It can't be cross-referenced because if you think of logical form literally, it's something that doesn't in logic: it would involve scopal associations being represented explicitly. The term "LF" has an etymological relationship to formal, 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 scopal things 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 uh-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 charitable misrepresenting 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 assumptions 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.

With regard to Optimality in syntax, I’ve heard some talk about how 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 ago to a journal with a request for an anagram 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 whether 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
Interview


27

Licensing and interpretation of inverted subjects in Italian

Manuela Pinto

Licensing and interpretation of inverted subjects in Italian aims at identifying the structural and interpretive primitives involved in the phenomenon of subject inversion in Italian and at transliterating them into formal terms as suggested in Chomsky (1995) and Reinhart (1995, 1996). The modular account Pinto proposes views the phenomenon of subject inversion as the result of the interaction of the principles of the computational system with the features of the subject and with conditions of derivational economy and interface economy.

In analyzing Italian subject inversion and inverted subjects, special attention is paid to focus contexts and often unrealized loco-argumental arguments.

The major aim of this study is not only to offer an exhaustive and explanatory analysis of inversion in Italian, but also to achieve this without burdening syntax with unnecessary or stipulative devices. Although the empirical domain is restricted to Italian, the analysis and proposals here will hopefully form the basis for a crosslinguistic approach to inversion in Romance.


First steps in wh-movement

Jacqueline van Kampen

In first steps in wh-movement the acquisition of language is considered from a generative point of view. The empirical basis is a longitudinal corpus of two Dutch children (aged 1;9–10;0 and 1;7–7;0). The main topic is the acquisition of V2 and wh-movement.

van Kampen presents analyses for (a) two-word utterances, (b) de-insertion, (c) wh-drop/topic drop, (d) left branch violations, (e) spell-out of intermediate C position.

In allational questions how neutral representations are learnable by means of an input-driven procedure.


The Modular Acquisition of Syntax

Genitive Case and genitive constructions

Petra de Wit

Genitive Case and genitive constructions explores the syntax of arguments and a variety of possessive constructions in nominal clause structures in languages such as Dutch, English, German and Russian. Couched within a minimalist framework, De Wit presents a coherent theory of how arguments are licensed in the functional layers of DP and she explains the role of derivational affixes, case markers and prepositions in this process. The theoretical framework to examples of predication in DP as in binominal noun phrases. In addition, it takes into consideration the historical development of the morphological genitive and its relation to current genitival constructions.


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 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 theory, and why is 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, which was before the bar. Since the mid 80’s you’ve had all sorts of other stuff 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, negated conjunctions, etc., and they share the syntax of those parts of speech up to whatever properties they have in virtue of being negative. Calling something a NegP obscures the syntactic properties that those words have in virtue of being adverbs, adjectives, or whatever. In addition, in the case of 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 MT/MA stuff is that it’s really become hard for M/MA people to say “modifier” anymore. I mean, all sorts of things that to me are obvious modifiers now get re-presented as heads of things that they aren’t heads of. This particular conception of categories has become undeservedly popular and is yielding seriously screwied 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 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 word “Second World Congress of Phoneticians.” So that was a memorable, reasonably bad, conference.

And since 1965 there hasn’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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In allational questions how multilevel representations are learnable by means of an input-driven procedure.


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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 Chomsky’s 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 look up the 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 1989, 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 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, which 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.

References


