Martin Atkinson – The Minimalist Muse

edited by Andrew Radford

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Dedication

This volume is dedicated to Martin Atkinson, who is shortly to retire after 33 years of service to the Department of Language and Linguistics and to the University of Essex. His formative training in Mathematics, Philosophy, Experimental Psychology and Linguistics has enabled him to make an invaluable and indelible contribution to the intellectual life of the Department, in earlier years in the areas of Language Acquisition and Psycholinguistics, and in more recent years in Minimalist Syntax. He has the keenest mind of anyone in the department, and is the only person in the Department (and one of the few people in the world) to have an in-depth understanding of both technical and conceptual aspects of Minimalism. As one colleague put it:

> Nobody grapples with every last footnote of Chomsky's like Martin.

The short summary CV included at the end of this *dedication* provides ample testimony to his intellectual achievements.

These achievements are all the more remarkable for the fact that much of his time at the University of Essex has been spent in selfless service of others, either as Head of Department or as Pro-Vice-Chancellor (in charge of 'drains and car parks' as he likes to say with characteristic self-deprecating modesty). In his most recent six-year 'stretch' as Head of Department (to use a term which reminds us of the isolation and incarceration involved in being Head of Department), he has overseen a difficult transitional period in the development of the Department, and managed the transition in such a way that it has seemed seamless.

Four major administrative achievements stand out from this period. Firstly, re-integrating Modern Languages within the Department (after a spell in which they had been cut loose as a Modern Languages Unit and left to drift in the stormy seas of

uncertainty), and giving them a sense of permanency and direction at a time when Modern Languages nationally have been in decline. The debt we owe to him is summed up in the following personal expression of gratitude from one member of staff:

> I am very grateful to you, Martin, for the continuous support you have given to Modern Languages at Essex in all the many years you have been our Head of Department. *Vielen Dank und alles Gute zum wohl-verdienten Ruhestand*.

Secondly, showing consummate management skills in personnel development, enabling many members of staff to achieve permanency and promotion, while at the same time overseeing a shift in the age profile of staff in the Department, achieving the Aladdin-like feat of replacing old lamps (or rather luminaries) by even brighter new ones which are more energy-efficient and consume fewer resources!

Thirdly, building up the financial stability of the Department, showing Gordon Brown-like 'prudence' in ensuring that departmental income streams match teaching and research needs and ambitions.

And fourthly, overseeing the introduction of a raft of new taught postgraduate courses designed to expand already successful areas of activity within the Department, thereby ensuring the stability of the Department in the next decade or so.

In his role as Head of Department, Martin has shown five main qualities which have earned him the respect and affection of everyone who has worked with him. Perhaps the most salient of these is his face-to-face, *walk-and-talk* consultative style of management. One colleague characterised it thus:

> Thankfully, Martin prefers to do things face to face rather than by email. This involves walking down the corridors of the department, knocking on doors and *talking* to people. Invariably the talk is half serious stuff, but is pretty much always enveloped in some juicy story or a funny incident, so afterwards

(usually about 60 minutes later) you discover you've agreed to do something but hadn't realised at the time because of the bonhomie of the occasion.

Another commented:

> Martin's a very visible, approachable Head – he always has time to talk. He recognises that it can be more valuable – and more effective – to have a quick word in someone's ear than to blast off emails. I've seen Martin on his way to any number of 'stroking' missions in the department (and I'm sure we've all had to be stroked at one time or other!). In fact, I'm not sure that his natural habitat isn't the corridor…

A second quality which has endeared him to those who work with him is his dogged determination to stand up to the bureaucratic juggernaut which has threatened to crush the creativity, productivity, morale and spirit of academics, and drown them in a quality quagmire of programme specifications, feedback forms and misguided middle management meddling and muddling. This spirit of Tiananmen Square which he has so often shown is epitomised in his blunt response to one new initiative when he remarked 'I'm afraid I dug my heels in and refused' (email of 9/10/06). In the words of one colleague:

> I was always impressed with Martin's standing up for common sense in the face of proposed costly, time-consuming and low-yield new fashions in red tape which distract from the essential.

A second observed:

> Martin has been a no-nonsense man when it comes to new bureaucratic 'idiocies' proposed by Management.

A third commented:

> If you do get an *email* from Martin, you know that it's about some piece of pointless dictat from the Quality Office, or some meeting organised by the

Make the Lives of Academics as Difficult as Possible Office that the department has to respond to, that Martin knows we'll have no view about, and these emails always end with a witty or withering comment (such as 'I'm sure you'll all be fighting each other to take part in this' (23/01/07). We also have to thank Martin for protecting us as much as possible from the 'quality' agenda and the machinations of the various little sections that have sprung up to make our lives more stressful over the past years.

A fourth noted:

I have always enjoyed Martin's unconventional and sometimes refreshingly disrespectful way of referring to people higher up in the University management or to administrative initiatives of questionable value. The longer I am here, the more highly I come to value the protective wall which Martin creates by his 'bullshitting' (I am quoting) of nonsensical demands from above. I thoroughly enjoy the feeling of creative space and professional trust which enables me to get on with my work, for example to improve my courses constantly without too much bureaucratic hassle.

And a fifth wrote that:

He has never become a grey bureaucratic blob, desperate to please everyone and upset no-one. Thus he has been able to make a hugely positive impact on many people's lives, not least my own. My feelings towards Martin are of great respect and deep gratitude.

A third of Martin's qualities is his personal integrity and his unerring determination to uphold the principles he passionately believes in (even when these make him unpopular with those he works for) – qualities which have engendered universal respect in those who have worked with him. An outstanding example of this was his courage in refusing to succumb to threats from on high during the industrial action last year. As one colleague put it:

The moment you really shined for us was when you stood by your staff during the industrial action last year, in opposition to the management of the University. You made us feel really proud of our boss.

In relation to the same event, another member of staff commented:

During the AUT actions last year – one of the more serious and difficult recent problems heads of department have to deal with, as it involved not just money but principles – and one which divided staff among many opposing and often heated views – in my opinion, Martin behaved with as much integrity as anyone could desire in a Head, even though this undoubtedly created far more difficulties for him than merely sailing with the prevailing winds would have done. I'm sure we all expected just the sort of stand he took, knowing him, but it was still impressive and will not be easily forgotten.

A third member of staff noted:

He has high moral standards and he has the courage to stand up for his principles. I don't know many people who are like this. Before coming to Essex, I certainly never had a head of department in whom personal qualities, academic brilliance, and management skills complemented one another so well. As a consequence, Martin is a person you can like and deeply respect in equal measure – in my experience, a rare combination indeed for a 'boss'.

And another colleague observed:

In any position of authority and responsibility it is almost impossible not to compromise one's moral integrity and philosophy, yet Martin has striven successfully to achieve this.

A fourth of Martin's qualities is his sense of common humanity, and the unflinching loyalty, support and trust he accords to members of staff. One colleague said of him:

He is a decent man and I truly respect him for his humane qualities.

In a similar vein, another commented:

He's been extremely supportive ever since I first came to Essex. I have a permanent contract, thanks to Martin's persistent and much appreciated efforts … If there was a real issue I needed advice on, I was always able to get instant feedback and support.

Yet another colleague remarked:

Martin was instrumental in getting me this post at a time when the department was under financial pressure and in supporting me when I first arrived. He has not only been very supportive but also genuinely caring. Despite always being prepared to back a winner, he remains to this day a fervent believer in Huddersfield Town.

And a member of our administrative team noted:

Martin always had an 'open door' for us. I feel that he cares as much for our personal as for our professional well being. He always includes the administrative staff in social events, which makes us feel very much a part of the 'team'. Thank you Martin.

The last (but by no means least important) of Martin's qualities which I will comment on here is his disarmingly dry sense of humour (and his wealth of witty personal anecdotes), which enable him to defuse any situation and to offer comfort in times of stress. One colleague said:

You have always been literally a door away ... approachable, friendly, a good listener prone to smile and able to make other people smile even in difficult situations.

Another wrote:

> Who can fail to get amused by the *lobster* story and the *Chinese brandy* story? Martin seems able to tell every individual member of staff the same story with as much relish on the last occasion as the first. I'm sure I'm often one of the last to hear each story, but it's told as if it he'd never told it before.

A third commented:

> Martin is one of the most humourous linguists I have ever met (although your humour is sometimes too deep for me to comprehend).

All five of his qualities are aptly summed up in this glowing tribute from one member of staff, who speaks for all of us:

> A void in the spiritual and social fabric of the Department of Language and Linguistics will appear when Martin retires. He has been at the centre of its academic activities and scholarly development for as long as I have been here, and I am sure this was the case from the moment he arrived, 33 years ago. Martin has inspired affection and respect not just among his departmental colleagues, but throughout the University for his commitment to the job, to the students and to the discipline. Everyone knows him to be a wise arbiter. People have sought his advice on every conceivable topic from the status of the 'Empty Category Principle' within the Minimalist Program (from one of my students) to how to get hold of Dutch hard core pornography (happily, not a request from anyone in the University). He always has a rational and informed response (even if it's not the one people want to hear). It is perhaps the combination of high intellectual ability, sense of collegiality, strong commitment to fairness and his good humour that has made him an outstanding head of department, colleague and friend. There is no better model for a humane academic.

This volume is a tribute to Martin's enormous intellectual, administrative and personal qualities from members of the Syntax-Morphology Research Group who

have come to know and admire Martin's sharp intellect better than most over many years.

Martin, all of us who have worked, partied and dined with you owe you an immense debt of gratitude for enriching our lives. We will miss you immensely.

Andrew Radford, July 2007

Richard Martin Atkinson

CURRICULUM VITAE

DATE OF BIRTH 3 May 1945

HIGHER EDUCATION

1963–1966: University of Manchester. BSc (IIi) in Mathematics and Philosophy.

1966–1967: University of Sussex. MSc in Experimental Psychology.

1967–1968 with intermittent registration thereafter: University of Edinburgh. PhD in Linguistics awarded 1978.

ACADEMIC POSTS

1968–1970: Research Associate. Department of Child Life and Health. University of Edinburgh.

1970–1974: Research Associate. Department of Linguistics. University of Edinburgh.

1974–1985: Lecturer in Linguistics. Department of Language and Linguistics. University of Essex.

1981: Visiting Professor of Linguistics. Department of English. Beijing Institute of Foreign Languages, Beijing, China.

1985–1993: Senior Lecturer in Linguistics. Department of Language and Linguistics. University of Essex.

1993–2007: Professor of Linguistics. Department of Language and Linguistics. University of Essex.

PUBLICATIONS

Books

Atkinson, M. *Explanations in the Study of Child Language Development*. Cambridge: Cambridge University Press. 1982.

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Atkinson, M. 'In the Minimalist Cellar.' In preparation.

Atkinson M. 'Neat brains make tidy minds. Discuss.' In preparation.

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ESRC Consultant for Linguistics in Advanced Coursework Recognition Exercise: 1992, 1994.

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Member of ESRC Linguistics College: 1996–2001.

Chair of Essex County Council Standards Committee: 2001–.

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University of York: MPhil, 1976; PhD, 1987; MPhil, 1988.

University of East Anglia: MPhil, 1978.

University of Edinburgh: MPhil, 1984; PhD, 1987; PhD, 1988; MPhil, 1989; PhD, 1992; PhD, 1996

Monash University, Melbourne: PhD, 1985.

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University of Edinburgh: BA in Linguistics and Psychology, 1981–1983.

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University of East Anglia: BA in Linguistics, 1985–1988.

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DOP-based models for richer grammatical frameworks

Doug Arnold

The existence of a probabilistic dimension in linguistic behaviour is a challenge for contemporary theories of grammar, because it is not straightforward to extend the statistical techniques that have been used successfully with relatively simple grammatical formalisms (such as context-free grammars) to richer grammatical formalisms. In this paper, I review ways in which Data Oriented Parsing (DOP) can be used to give a statistical dimension to two grammatical theories (HPSG and LFG), discuss some implications, and indulge in some speculation about the range of grammatical theories which might be susceptible to this sort of approach.

1. Introduction

Statistical approaches to Natural Language Processing are important for both theoretical and practical reasons. Practically speaking they seem essential for breadth of coverage and to avoid the swamping effect of structural ambiguity. Theoretically, they are important as a response to the overwhelming evidence for a probabilistic component in human language processing (see e.g. Jurafsky, 2003, for a review). From a conventional linguistic point of view, the challenge is to come up with a way in which statistical methods can be integrated with the theoretical and descriptive apparatus of normal linguistic theory.

Here, Data Oriented Parsing (DOP) is very attractive:[1] it provides a descriptively powerful, clear, and above all *elegant* framework for adding a statistical dimension to linguistic theory. The leading idea is that rather than using a collection of rules, parsing and other processing tasks employ a database of fragments produced by decomposing a collection of normal linguistic representations (e.g. trees — a treebank). These fragments can be assigned probabilities (e.g. based on their relative frequency in the fragment database). Parsing a string involves, in effect, finding a collection of fragments with can be combined to provide a representation for it. Representations can be assigned probabilities based on the probabilities of the different combinations of fragments involved.

One issue is that it is not *a priori* given that a DOP model can be provided for any particular linguistic theory. For example, it might be that the scores the model

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[1]Standard references include, for example, Bod (1992, 1998), Bod and Scha (1997) and the papers in Bod et al. (2003).

assigns to representations are not in fact probabilities; it might not be possible to give appropriate (or even sensible) definitions to the decomposition operations that produce fragments, or only possible at the cost of giving up some of the theory's theoretical or formal principles.

In fact, as it turns out, the problem of finding appropriate decomposition operations and ensuring statistical well-foundedness are real issues for DOP models that use representations more sophisticated than simple context-free phrase structures. In this paper, I will review approaches to decomposition for two particular linguistic theories whose representations are richer in this way, Lexical Functional Grammar (LFG) and Head-driven Phrase Structure Grammar (HPSG), and discuss how appropriate decomposition operations can be defined. Superficially, the techniques involved seem radically different, but I will try to bring out the underlying commonality. More speculatively, I will suggest that exploiting this commonality greatly widens the range of linguistic theories which are susceptible to DOP methods. I will also, more briefly, address the problem of statistical well-foundedness. This will turn out to be a more difficult problem, because the source of the problem is pervasive in many linguistic theories, though still amenable to a (somewhat brute force) solution.

The presentation will be as far as possible informal and intuitive, with a focus on general issues rather than technical detail. The paper is structured as follows. Section 2 provides background, introducing basic ideas about DOP. Section 3 discusses LFG-DOP, Section 4 looks at HPSG-DOP, and Section 5 discusses some implications and indulges in some speculation.

2. Tree-DOP: basics

The simplest DOP model, Tree-DOP, begins with a treebank of representations which are simple context-free phrase structure trees (i.e. ones where non-terminals are labelled with atomic categories), like Figure 1.[2] These trees are decomposed into fragments by two operations: *Root*, which extracts complete subtrees from representations, and *Frontier*, which deletes subtrees from *Root* fragments to produce fragments with 'open' nodes (frontier nodes labelled with non-terminal symbols). From the tree in Figure 1, these operations will produce the fragments in Figures 2 and 3.

The fragments that are produced in this way can, of course, be combined together to provide representations for sentences which do not appear in the original

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[2]See e.g. Bod and Scha (2003) and the references in footnote 1.
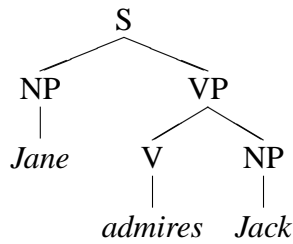
Figure 1 Treebank representation
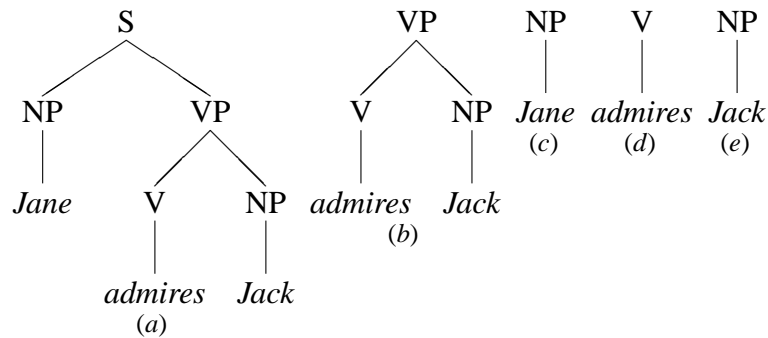
Figure 2 Subtrees produced by the *Root* operation

treebank, as well as those that do. Here the fundamental notion is that of a 'derivation' which involves selecting a fragment from the database, and composing it with other fragments so as to fill any open nodes it (or the other fragments) may have, until a complete representation is produced. The standard composition method involves 'leftmost substitution': let f_a be a fragment with an 'open' node (call this the 'active' fragment), suppose its leftmost open node is labelled with category C. Leftmost substitution involves replacing this node by a fragment f_b whose root label is also C.

For example, the frontier fragment (i) in Figure 3 can be composed with the fragment for *Jack* in Figure 2 (e) to produce a representation corresponding to [$_s$ *Jack VP*]. This can in turn be composed with fragment (k) in Figure 3 to produce a representation of [$_S$ *Jack admires NP*], which can be composed with Figure 2 (c) to produce a representation of *Jack admires Jane* (see Figure 4). Of course, any given representation will typically be derivable in many different ways.

Adding a probabilistic dimension to this is straightforward. We can estimate the probability of a representation by summing over the probabilities of its various derivations, as in (1), where R is a representation, and each d_j is one of its derivations.

$$(1) \qquad\qquad P(R) = \sum_{j=1}^{m} P(d_j)$$
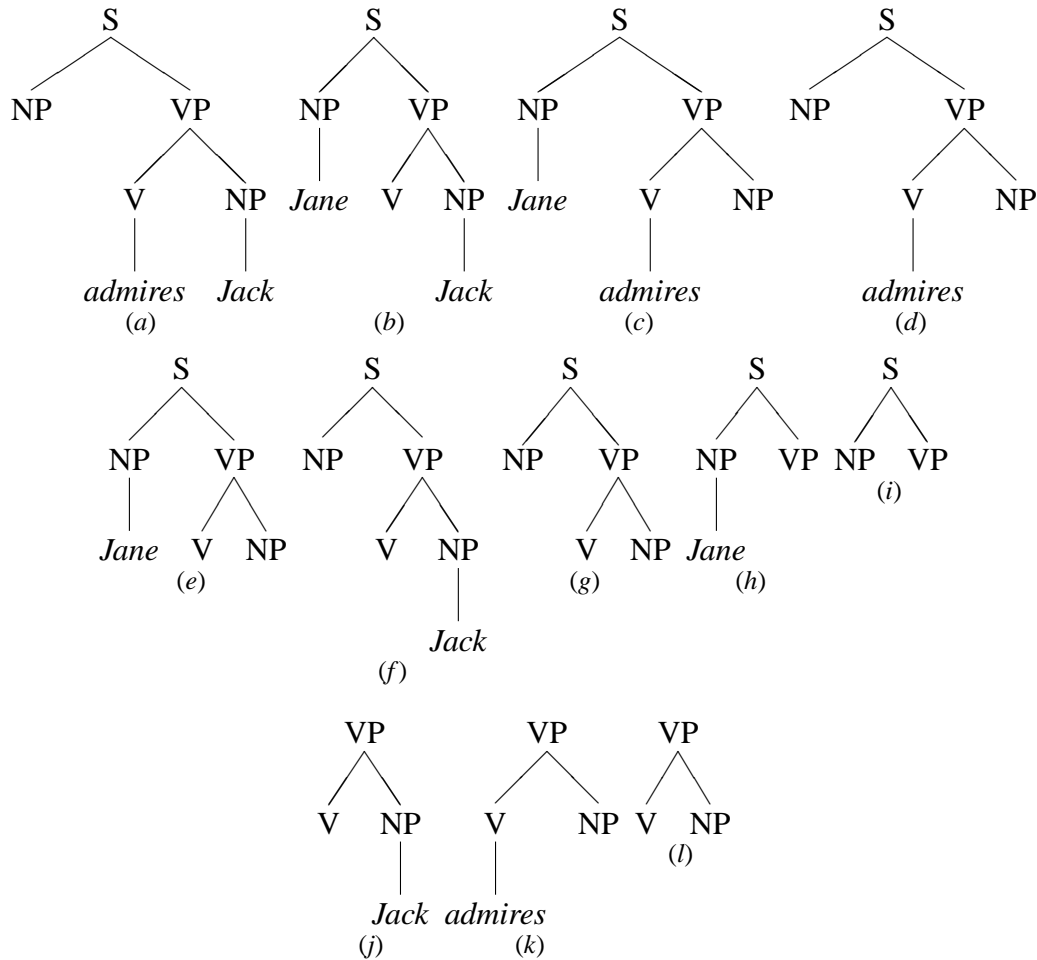
Figure 3 Subtrees produced by the *Frontier* operation

We can estimate the probability of each derivation by taking the joint probabilities of the fragments involved, as in (2), where d is a derivation, and each f_i is a fragment.

$$(2) \qquad\qquad P(d) = \prod_{i=1}^{n} P(f_i)$$

The simplest, and most intuitive, method for estimating the probability to be assigned to individual fragments is based on relative frequency.[3] Here the notion of the 'competition set' is crucial: intuitively, this is the set of fragments we could have chosen at a particular point instead of the one we actually chose. For the initial fragment of a derivation f_0, we can take the competition set to be all fragments in the database whose root is labelled with a particular category (e.g. S), and assign it a probability

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[3]There are some well-known problems with using relative frequency in this way, see Bonnema and Scha (2003), Linardaki (2006) and references there for discussion and possible solutions. However, these problems have no significant impact on the issues under consideration here.
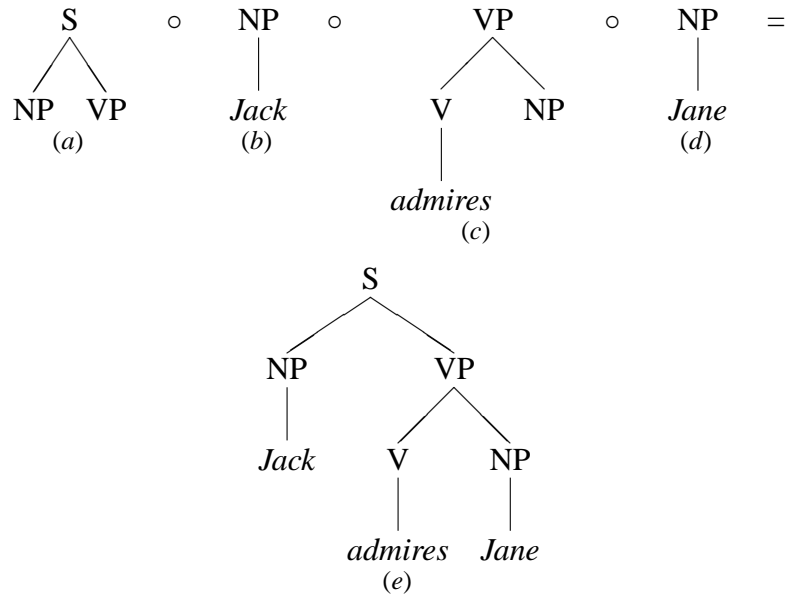
Figure 4 Fragment composition

based on its frequency of occurrence relative to all such fragments (the frequency of a fragment f is written $|f|$).

$$(3) \qquad P(f_0) = \frac{|f_0|}{\displaystyle\sum_{root(f)=root(f_0)} |f|}$$

For non-initial stages, we are dealing with two fragments: an active fragment f_a, which we can take as given (we have already considered alternatives, and calculated a probability for it), and another fragment, f_i. The most obvious and natural way to define the competition set of f_i is just as the set of fragments with the same root label. Thus we assign to f_i a probability based on its frequency of occurrence compared to the frequency of occurrence of all fragments with the same root, as in (4).

$$(4) \qquad P(f_i) = \frac{|f_i|}{\displaystyle\sum_{root(f)=root(f_i)} |f|}$$

It is important, for both theoretical and practical reasons, that competition sets are properly set up, because if they are not, we may end up producing simple numerical scores, rather than genuine probabilities.[4] In particular, it is a defining feature of a

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[4]This is practically important if we intend to create a system where a DOP component is supposed to interact with components that produce genuine probabilities — it will not be straightforward to combine numerical results from different sources if they are not scaled in the same way.
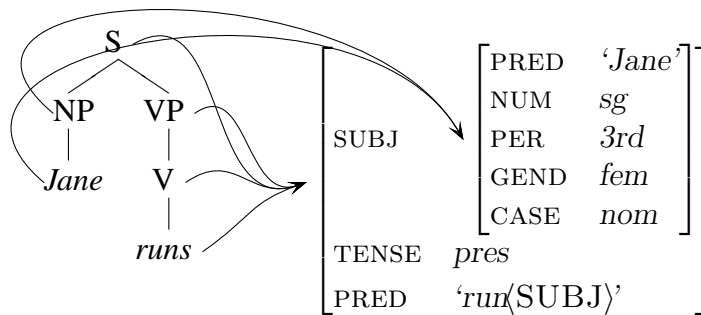
Figure 5 LFG representation of *Jane Runs*

probability distribution that given an (exhaustive) set of alternatives from which a choice is to be made, summing over the probabilities assigned to the members of the set should yield a value of 1 (reflecting the intuition that it is certain that one of the alternatives will be chosen). In this context, what this means is that if we have an active fragment f_a with probability p, and a competition set of fragments $\{f_1, f_2, \ldots, f_n\}$, the sum of the probability of composing f_a with f_1 or f_2, or ... or f_n should again be p — no probability mass should be added or lost. Tree-DOP satisfies this property, because the way competition sets are established is an accurate reflection of the combinatory possibilities — every element of the competition set is a genuine competitor. However, as we will see, it is not always so easy to guarantee this.

Tree-DOP is an attractive model in many ways, but essentially it inherits all the limitations of the context-free grammars on which it is based. It raises the question of whether or how the approach can be adapted to use more sophisticated linguistic concepts and formalisms. The following sections will look at this in relation to two such formalisms: LFG and HPSG.

3. LFG-DOP

The first attempt to adapt DOP for a more sophisticated linguistic theory is Bod and Kaplan (1998) (see also Bod and Kaplan, 2003) which uses LFG structures as its representational basis.[5] Representations are triples $\langle c, \phi, f \rangle$, where c is an LFG c-structure (a representation of surface constituent structure), f is an LFG f-structure (an attribute–value structure representing grammatical relations and various morpho-syntactic features), and ϕ is a correspondence function that relates c-structure nodes to the corresponding units of the f-structure. See Figure 5.

Root and *Frontier* operations are similar to those of Tree-DOP, except that they

---

[5]Discussion of the key ideas of LFG can be found in e.g. Bresnan (1982, 2001), Dalrymple et al. (1995); Dalrymple (2001).

$$
\text{NP} \quad \begin{bmatrix} \text{PRED} & \textit{`Jane'} \\ \text{NUM} & \textit{sg} \\ \text{PER} & \textit{3rd} \\ \text{GEND} & \textit{fem} \\ \text{CASE} & \textit{nom} \end{bmatrix}
$$

Jane
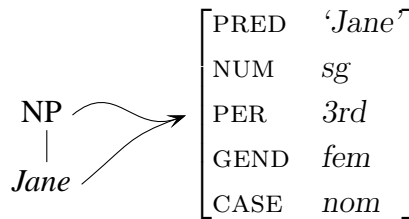
Figure 6 LFG-DOP fragment produced by *Root* from the representation in Figure 5

$$
\begin{array}{c}
\text{S} \\
\text{NP} \quad \text{VP} \\
\text{V} \\
\textit{runs}
\end{array}
\quad
\begin{bmatrix}
\text{SUBJ} & \begin{bmatrix} \text{NUM} & \textit{sg} \\ \text{PER} & \textit{3rd} \\ \text{GEND} & \textit{fem} \\ \text{CASE} & \textit{nom} \end{bmatrix} \\
\text{TENSE} & \textit{pres} \\
\text{PRED} & \textit{`run}\langle \text{SUBJ} \rangle\textit{'}
\end{bmatrix}
$$
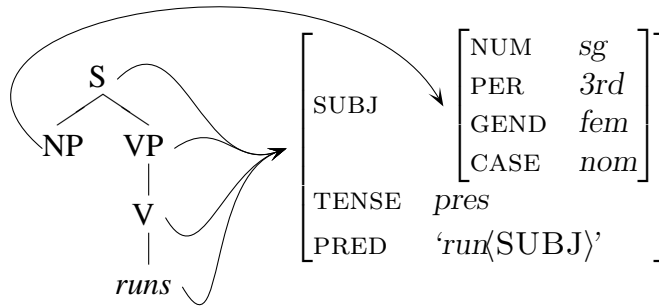
Figure 7 LFG-DOP fragment produced by *Frontier*

must also take account of the associated pieces of f-structure. As well as extracting subtrees from the c-structure tree, *Root* also extracts the corresponding pieces of f-structure, by eliminating all pieces of f-structure that cannot be reached via ϕ-links from the c-structure that is extracted. Applied to the NP *Jane* in Figure 5, this produces Figure 6. Notice that all the information about the verb and the VP have been removed, but that *Jane* is still marked *nom*, because the CASE value can be reached via a ϕ-link from a remaining c-structure node (in fact, it can be reached from both of them).

In a similar way, *Frontier* deletes subtrees of the c-structure, preserving the root category. As regards the f-structure, it deletes ϕ-links from the deleted structure, and removes 'semantic forms' (PRED values) that corresponding to the erased nodes (PRED values contain the minimal semantic information that is syntactically relevant, the intuition here is that when the phonological content of a word is removed, so is its semantic content). Applied to *Jane* in Figure 5, this produces Figure 7. Notice that *Jane* has been removed from the c-structure, and its PRED value has been removed from the f-structure. But notice that no other information about *Jane* has been removed. In particular, notice that the subject of *runs* is still marked as being *fem*.[6]

Fragment composition in LFG-DOP involves category matching in c-structure

---

[6]It is likely that most LFG analyses would not include a GEND:*fem* feature on words like *Jane*, which would instead be simply unspecified for this feature — as opposed to words like *she*, *her*, and *herself* that have a distinctive feminine form. All the points I make here could be made in relation to other features and structures, but it greatly simplifies the presentation if we assume *Jane* is so marked.

and unification of corresponding pieces of f-structure (so, for example, composing Figure 7 and Figure 6 will produce the original structure in Figure 5).

The problem is that these fragments are *over-specific*. The fragment for *Jane* in Figure 6 is marked *nom*, and so will not be usable in any derivation of an example like (5), where *Jane* is an object, and must be marked *acc*. Similarly, the fragment in Figure 7 will not be usable in a derivation of (6), because it requires its subject to be *fem*, and *Jack* is presumably *masc*.

(5) Jack saw Jane.
(6) Jack runs.

To deal with this, Bod and Kaplan (1998) introduce a '*Discard*' operation which generalizes over the structures produced by *Root* and *Frontier*. Informally, *Discard* is allowed to erase any attribute–value pair, apart from the PRED and those attributes whose values are ϕ-linked to the nodes that have not been removed from the c-structure. For the cases in hand, this means, in essence, that everything can be removed apart from the PRED values (the intuition here is that keeping the phonology necessitates keeping the associated semantics).[7]

Of course, this allows us to handle examples like (5) and (6). But at a cost: it produces fragments that are highly *under*-specific. For example, it will allow us to derive examples like the following:

(7) *They runs.   (e.g. by *Discard*-ing *3rd sg* on the SUBJ of *runs*)
(8) *Jane run.   (e.g. by *Discard*-ing *3rd sg* on *Jane*)

To deal with this, Bod and Kaplan revise their definition of grammaticality, so that, instead of a string being grammatical iff it can be assigned a representation, a string is grammatical if it has a non-*Discard* derivation — if it can be analysed using non-*Discard* fragments. A string is ungrammatical only if all its derivations involve *Discard* fragments.

For example, the only way that (7) will have a non-*Discard* derivation is if *runs* appears in the treebank with a third person plural subject (or *they* appears as subject of a third person verb). Likewise, (8) will only have a non-*Discard* derivation if *Jane* appears in the treebank as subject of a non-third person singular verb (or if

---

[7]More precisely, everything can be removed from Figure 7 apart from the PRED value and the value of the SUBJ feature, because this is ϕ-linked to a piece of c-structure. However, all the attribute–value pairs in the SUBJ can be removed, so this comes to the same thing.

runs appears with a third person singular subject). Since this will only happen if the treebank contains ungrammatical examples, it is very plausible that these examples will have only *Discard* derivations, and be correctly classified as ungrammatical by this criterion. On the other hand, (5) will have a non-*Discard* derivation so long as *Jane* appears somewhere in the treebank marked *acc* (e.g. as an object), and (6) will have a non-*Discard* derivation providing *runs* sometimes appears with a *masc* subject. For words as common as *Jane* and *runs* this is quite likely in a treebank of reasonable size, so in such cases Bod and Kaplan (1998)'s revised definition will approximate standard linguistic understanding.

However, in general, their revised definition will be less than satisfactory. In particular, it is not uncommon for words to occur only once even in very large corpora (in fact, it is very common: it is quite normal for around 40% of the words in a corpus to occur only once). For example, in the 100 million words of the British National Corpus (BNC) the noun *debauches* ('moral excesses') appears just once, in example (9) below, where it is the object of a preposition, and would be marked as *acc*. The only way to produce a fragment where *debauches* is not so marked is by using *Discard*. So, even based on large corpus such as the BNC, Bod and Kaplan's revised definition would assign different grammaticality to (9) and (10), classifying the latter as ungrammatical, which it is not. Similarly, the verb *to debauch* 'to corrupt morally', though it is more frequent, and appears several times in examples like (11) and (12), never appears with a first person singular or feminine subject. Thus, fragments which are compatible with such subjects can only be produced by using *Discard*. Hence, Bod and Kaplan's revised definition would classify grammatically perfect examples like (13) and (14) as grammatically deviant, which they are not.

  (9)   [H]e . . . shook Paris by his wild debauches on convalescent leave.
(10)   His wild debauches shook Paris.
(11)   You could not, of course, debauch a Beresford.
(12)   Hitler and Stalin debauched the moral climates of Germany and Russia respectively.
(13)   She has never debauched anyone.
(14)   We did not debauch anyone.

One approach to this would be to try to limit *Discard* by identifying a collection of features as grammatically essential in some way (e.g. Way, 1999); in fact, as noted above, Bod and Kaplan (1998) treat PRED values in this way — their decomposition
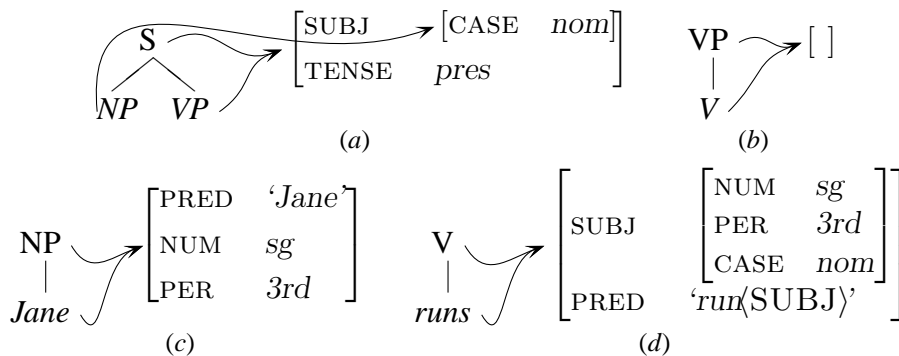
Figure 8 Basic abstract fragments

operations, including *Discard*, are defined in such a way that a fragment will contain a PRED value if and only if its c-structure contains the associated word). But such features have proved elusive. Arnold and Linardaki (2007a) propose an alternative approach.

The proposal is that the operation of *Discard* should be constrained by the existence of what they call 'abstract fragments', which encode information about what collections of features are grammatically required in various contexts. The linguistic knowledge behind this can be expressed in normal LFG grammar notation. For example, the rules and lexical entries in (15)–(18) can be used to generate the basic abstract fragments in Figure 8.[8]

(15)   S →   NP   VP
                   (↑SUBJ)=↓   ↑=↓
              (↑SUBJ CASE)=nom
                   (↑TENSE)=pres

(16)   VP →   V
               ↑=↓

(17)   *Jane*   NP   (↑PRED)='Jane'
                       (↑PER)=3rd
                       (↑NUM)=sg

(18)   *runs*   V   (↑PRED)='run$\langle (SUBJ) \rangle$'
                       (↑SUBJ NUM)=sg
                       (↑SUBJ PER)=3rd

---

[8]These rules are purely for exemplification; I am not suggesting, for example, that this is the right way to capture the association of nominative case with subject position in finite clauses.

$$\begin{bmatrix} \text{SUBJ} & \begin{bmatrix} \text{NUM} & sg \\ \text{PER} & 3rd \\ \text{CASE} & nom \end{bmatrix} \\ \text{PRED} & \text{'}run\langle\text{SUBJ}\rangle\text{'} \\ \text{TENSE} & pres \end{bmatrix}$$

Figure 9 A derived abstract fragment

Formally speaking, these are fragments in the normal sense, and can be composed together in the normal way to produce derived abstract fragments. Abstract fragments can then be used to set an upper bound on the generality of fragments produced by the operation of *Discard*. Specifically, every fragment produced by *Discard* is required to be licensed by an abstract fragment which has the same c-structure, and whose f-structure subsumes its f-structure. If the only lexical entry for *Jane* is (17), then the only abstract fragment for *Jane* will be Figure 8 (*c*), and it will not be possible for *Discard* to create a fragment which is more general than this — e.g. one which is not specified as *3rd sg*. On the other hand, *Discard* will be able to produce fragments which are unspecified for CASE, since this is not specified in Figure 8 (*c*). Similarly, the other abstract fragments in Figure 8 can be composed together to produce the (derived) abstract fragment in Figure 9. This will license the *Discard*-ing of gender on the subject NP, but not permit *Discard*-ing of person, number, or case information.

With this approach, fragments can be produced which have the right level of generality — which provide an analysis of (19) and (20), but not (21) and (22).

(19) Jack saw Jane. (=(5), okay, *Jane* in not necessarily marked *nom*)

(20) Jack runs. (=(6), okay, subject of *runs* need not be *fem*)

(21) *They runs. (=(7), out, subject of *runs* must be *3rd sg*)

(22) *Jane run. (=(8), out, *Jane* must be *3rd sg*)

This seems to be solution to the problem of fragment generality (I will look at some objections in Section 5, below). The issue of probability leak is more problematic.

To see this, notice first that composition in LFG-DOP involves both matching the label on the open slot of the active fragment with the label on the category of

the fragment being adjoined (as in Tree-DOP), and in addition, unification of the associated pieces of f-structure (i.e. adjoining a fragment at an open node involves unifying the fragment's f-structure with the f-structure that is ϕ-linked to the open node). Now, suppose competition sets are defined in the same way as Tree-DOP: the competition set for subject of *runs* will include all NP fragments, and probability will be shared among them on the basis of their relative frequency. But this will include some fragments whose f-structures cannot be unified with the subject of *runs*, e.g. NPs which are plural (*they*, *the children*), or not third person (*I*, *you*, *we*). The probability associated with these fragments will be wasted.

Of course, this problem can be ameliorated if the competition sets are defined more precisely, e.g. as the set of fragments whose root categories match the label on the open slot, and whose f-structures are unifiable with the f-structure associated with the open slot.[9] However, this is not a complete solution, because LFG makes use of a number of operations which cannot be checked at intermediate stages of a derivation, and so cannot be taken into account when constructing competition sets. For example, LFG f-structures are required to be *complete*, in the sense that for every grammatical relation mentioned in the PRED value there must be a corresponding piece of f-structure (e.g. if the PRED value mentions a SUBJ, there must actually *be* a subject). This is something that can only be checked at the end of a derivation, and cannot be used in the construction of competition sets.[10] The problem with any such filtering operation is that the probability mass associated with the structures that are filtered out is wasted — it leaks away. This problem is not necessarily fatal: it should be possible to re-normalize the scores associated with successful structures to take account of the probability mass assigned to structures that have been filtered out. But this solution is not very elegant, and still means that scores associated with intermediate structures are not genuine probabilities.[11]

---

[9]Bod and Kaplan (2003) discuss a number of ways in which competition sets could be defined for LFG-DOP.

[10]By contrast, the LFG *coherence* condition – the condition that the f-structure should contain no grammatical functions *except* those mentioned in the PRED (there should not be an OBJ unless the predicate allows an object) — can be checked at intermediate stages, and taken into account in constructing competition sets (they can be defined to exclude fragments which, if used, would produce incoherent structures).

[11]An alternative approach is to consider less standard probability models. For example, Abney (1997) suggests the use of Random Field models to avoid probability leak with attribute–value grammars. But it is hard to beat relative frequency for simplicity and intuitive appeal.

4. HPSG-DOP

Neumann (2003, 1998, 1999) suggests an approach to DOP based on HPSG.[12] Neumann's decomposition operations begin with a phrase structure tree whose non-terminal nodes are labelled with HPSG-style attribute–value matrices (AVMs). The first step is to produce an abstraction of this where the each node is labelled with an atomic symbol indicating the HPSG rule-schema which would license the immediate subtree: something along the lines of Figure 10 (a). The next step is to remove non-head subtrees from this, marking the removal points as open (with a '*'); this process is applied recursively on the subtrees that are cut off, resulting in a collection of tree-fragments which all have exactly one lexical anchor (corresponding to the grammatical head): Figure 10 (b) and Figure 10 (c). These fragments are then processed from the lexical anchor upwards to the root creating new fragments for each subtree. This process yields Figure 10 (d). Finally, root nodes and any open nodes undergo *specialization*. This process involves replacing the original rule labels (e.g. *subj-hd-ph*) with category labels (e.g. S, NP), derived from the corresponding nodes of the full original representation. These category labels denote equivalence classes of HPSG feature structures (i.e. they are essentially abbreviations for such feature structures). This produces Figures 10 (e,f,g), which are the fragments used in processing.[13]
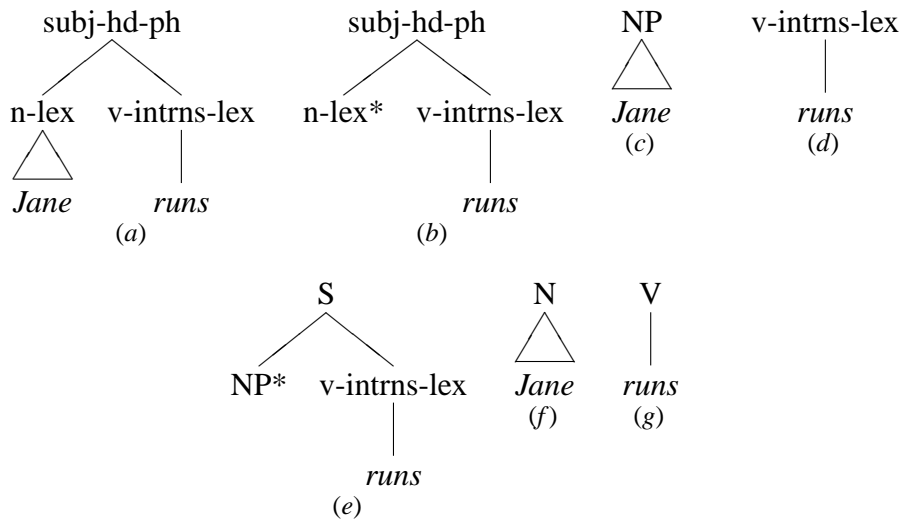
Whether such fragments are over- or under-general will depend on how the category labels are defined, but it is very likely that they will be over-general. For example, it is likely that NP will denote a nominal phrase that is unspecified for person, number and case, leading to over-generation (over-recognition) because fragments can be combined to produce analyses of *Jane run, *They runs, *Jane admire she, etc.[14] This is not a problem in practice, because Neumann assumes that parsing proceeds in two stages: the first stage only takes account of these category labels, and does indeed over-recognize; but this is followed by a second stage where the atomic category labels are replaced by the HPSG feature structures they abbreviate, and associated grammatical constraints are applied. The effect of this is to filter out the

---

[12]Standard references on HPSG include Pollard and Sag (1994); Ginzburg and Sag (2000). Sag et al. (2003) is an introductory presentation.

[13]Again, the content of the representations is purely for the sake of exemplification, and should not be taken too seriously (for example, the assumption that there is nothing equivalent to a VP projection above *runs*). *Jane* goes from being an n-lex in Figure 10 (a) and (c) to an NP in Figure 10 (f) on the basis that an NP is a nominal phrase that does not require a specifier. Nothing hangs on this.

[14]Consider the alternative: suppose NPs *are* so specified, as e.g. nom and 3.sg: this label will wrongly be applied to all NP fragments, including ones that are not third person singular or nominative (*them, us, me*, etc.). This will lead to under-generation.

subj-hd-ph   subj-hd-ph   NP   v-intrns-lex

n-lex   v-intrns-lex   n-lex*   v-intrns-lex

Jane   *runs*   *runs*   *Jane*   *runs*

(*a*)   (*b*)   (*c*)   (*d*)

S   N   V

NP*   v-intrns-lex   *Jane*   *runs*

runs   (*f*)   (*g*)

(*e*)

Figure 10   Neumann-style fragments

over-recognition of the first stage.

As Neumann notes, this gives a DOP model which is actually rather similar to LFG-DOP, in the sense that it is based on fragments which have a context-free spine (c-structure, in the case of LFG-DOP), which is associated with a richer, feature-based, representation. Moreover, the way parsing proceeds corresponds to a fragment composition method where competition sets are established on the basis of the context-free labels (S, NP, etc.), and the associated feature information is used as part of a filtering operation. Neumann's approach thus suffers from probability leak, as the probability mass associated with fragments which are legitimate as regards basic category but fail to satisfy other grammatical constraints is thrown away.

Arnold and Linardaki (2007b) present a different approach to integrating DOP with HPSG, which is intended to avoid probability leak and produce fragments at the right level of generality. Under this approach the initial representations in the treebank are taken to be full HPSG feature structures, which I will draw as attribute–value matrices (AVMs), as in Figure 11, and the decomposition operations take advantage of a notion of *type-expansion*.[15]

According to the representation in Figure 11, the structure associated with *Jane runs* has two daughters (DTRS) (cf. the tags ⑤ and ⑥), the latter identified as the head daughter (HD-DTR). The phonology of the first is */Jane/*, that of the second is */runs/*. The phonology of the whole is the *append* of the phonology of the first

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[15]A further objection to Neumann's approach which this approach is intended to address is that by being based on tree structures, Neumann's approach disregards a fundamental piece of HPSG ideology, namely the idea that all aspects of linguistic objects (including phrase structure) are represented using the same formal apparatus, namely feature *graphs*.
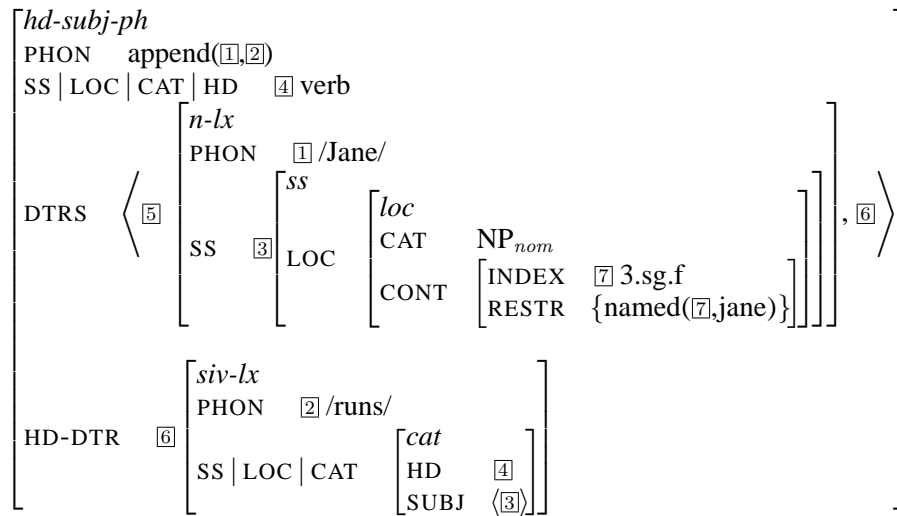
$$
\begin{bmatrix}
\textit{hd-subj-ph} \\
\text{PHON} \quad \text{append}(\boxed{1},\boxed{2}) \\
\text{SS}\,|\,\text{LOC}\,|\,\text{CAT}\,|\,\text{HD} \quad \boxed{4}\ \text{verb} \\[2ex]
\text{DTRS} \quad \left\langle \boxed{5}
\begin{bmatrix}
\textit{n-lx} \\
\text{PHON} \quad \boxed{1}\ \text{/Jane/} \\[1ex]
\text{SS} \quad \boxed{3}
\begin{bmatrix}
\textit{ss} \\
\text{LOC}
\begin{bmatrix}
\textit{loc} \\
\text{CAT} \quad \text{NP}_{nom} \\[1ex]
\text{CONT}
\begin{bmatrix}
\text{INDEX} \quad \boxed{7}\ \text{3.sg.f} \\
\text{RESTR} \quad \{\text{named}(\boxed{7},\text{jane})\}
\end{bmatrix}
\end{bmatrix}
\end{bmatrix}
\end{bmatrix}, \boxed{6} \right\rangle \\[2ex]
\text{HD-DTR} \quad \boxed{6}
\begin{bmatrix}
\textit{siv-lx} \\
\text{PHON} \quad \boxed{2}\ \text{/runs/} \\[1ex]
\text{SS}\,|\,\text{LOC}\,|\,\text{CAT}
\begin{bmatrix}
\textit{cat} \\
\text{HD} \quad \boxed{4} \\
\text{SUBJ} \quad \langle \boxed{3} \rangle
\end{bmatrix}
\end{bmatrix}
\end{bmatrix}
$$

Figure 11   HPSG representation of *Jane runs*

$$
\begin{bmatrix}
\textit{synsem} \\
\text{LOC}\,|\,\text{CAT}\,|\,\text{HD} \quad \boxed{4}
\end{bmatrix}
$$

$$
\boxed{3}
\begin{bmatrix}
\textit{synsem} \\
\text{LOC}
\begin{bmatrix}
\textit{loc} \\
\text{CAT} \quad \textit{NP}_{nom} \\[1ex]
\text{CONT}
\begin{bmatrix}
\text{INDEX} \quad \boxed{7}\ \textit{3.sg.f} \\
\text{RESTR} \quad \{\textit{named}(\boxed{7},\textit{jane})\}
\end{bmatrix}
\end{bmatrix}
\end{bmatrix}
\qquad
\begin{bmatrix}
\textit{synsem} \\
\text{LOC}\,|\,\text{CAT}
\begin{bmatrix}
\textit{cat} \\
\text{HD} \quad \boxed{4}\ \textit{verb} \\
\text{SUBJ} \quad \langle \boxed{3} \rangle
\end{bmatrix}
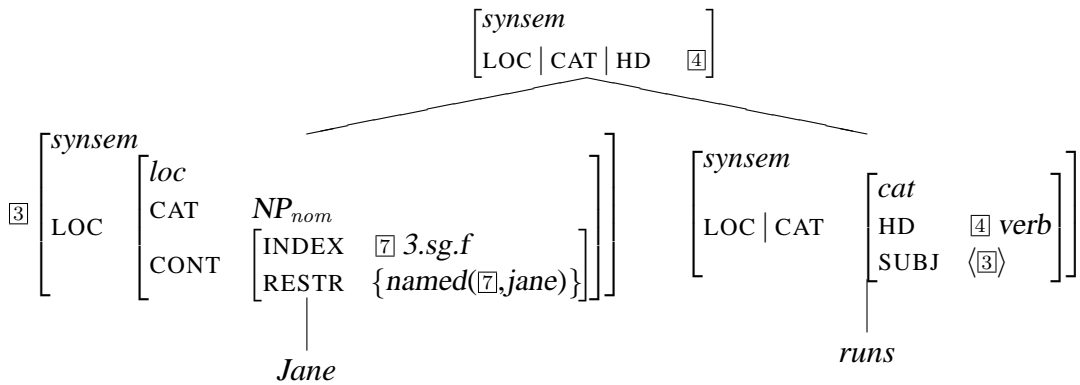\end{bmatrix}
$$

Jane   *runs*

Figure 12   Tree representation corresponding to Figure 11

with the phonology of the second. The first daughter is a nominal lexeme (*n-lx*), the second is a simple intransitive verb lexeme (*siv-lx*). This representation is simplified in various ways (e.g. no semantics is specified for *runs*), and employs a number of abbreviations, notably NP_{nom} (which stands for a nominal structure with an empty specifier, whose CASE value is nom), and in the semantic CONTENT assigned to Jane (it is standardly assumed in HPSG that person, number, and gender features are part of the semantics, specifically part of the index of an NP, which one may think of as the associated discourse variable). Figure 12 presents essentially the same content in what may be a more familiar form (here, DAUGHTERS are represented explicitly on tree branches, the nodes of the tree are labelled by *synsem* values, and the PHONOLGY associated with a structure is written in the leaves the structure dominates).

HPSG feature structures are assumed to be *typed*, so that every substructure has an associated type. Types are arranged in an inheritance hierarchy, and associated

with *type-constraints*, which indicate conditions that the associated structures must satisfy. In the simplest case, a type-constraint may just indicate what attributes a structure of that type should have. So for example, objects of type *lex* and *phrase* — subsorts of *sign* — are associated with attributes PHONOLOGY (whose value might be a list of *phonemes*) and SYNSEM (whose value is of type *synsem*, and which contains syntactic and semantic information). *Phrase*s are also required to have a DAUGHTERS attribute (whose value is a list of *sign*s). Headed-phrases (i.e. of type *hd-phr*) are a subsort of *phrase*, which have in addition a HEAD-DTR feature, whose value is required to be re-entrant with one of the elements of the daughters list. Type-expanding a structure simply means extending it so that all relevant type-constraints are satisfied.

The following are among the sorts of constraint that a reasonable type-system for English might involve:[16]

$$
(23) \quad \textit{hd-subj-ph} \rightarrow
\begin{bmatrix}
\text{SUBJ} & \langle \rangle \\
\text{DTRS} & \langle [\text{SS} \quad \boxed{2}], \boxed{1} \rangle \\
\text{HD-DTR} & \boxed{1}[\text{SS} \,|\, \text{LOC} \,|\, \text{CAT} \quad [\text{SUBJ} \quad \langle \boxed{2} \rangle]]
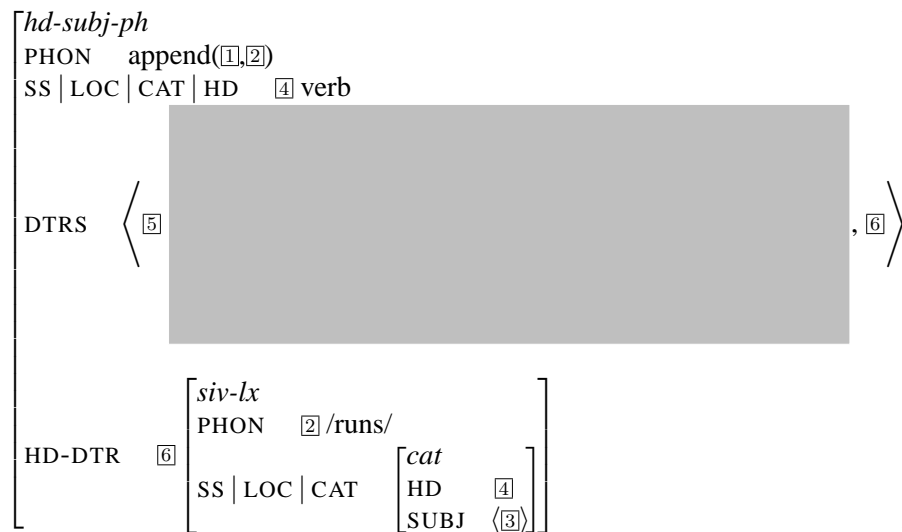\end{bmatrix}
$$

$$
(24) \quad \textit{non-inv-sent} \rightarrow
\begin{bmatrix}
\text{PHON} & \text{append}(\boxed{1},\boxed{2}) \\
\text{DTRS} & \langle [\text{PHON} \quad \boxed{1}], \boxed{2} \rangle \\
\text{HD-DTR} & \begin{bmatrix} \text{PHON } \boxed{2} \\ \text{SS} \,|\, \text{LOC} \,|\, \text{CAT} \,|\, \text{HD} \quad \text{v} \end{bmatrix}
\end{bmatrix}
$$

$$
(25) \quad \textit{3sg-vrb} \rightarrow
\begin{bmatrix}
\text{SS} \,|\, \text{LOC} \,|\, \text{CAT} \,|\, \text{HD} \quad \text{v} \\
\text{SUBJ} \quad \left\langle \begin{bmatrix} \textit{ss} \\ \text{LOC} \quad \begin{bmatrix} \text{CAT} & \text{NP}_{nom} \\ \text{CONT} & [\text{INDEX} \quad 3.\text{sg}] \end{bmatrix} \end{bmatrix} \right\rangle
\end{bmatrix}
$$

$$
(26) \quad \textit{n-lex} \rightarrow
\begin{bmatrix}
\text{PHON} & /\text{Jane}/ \\
\text{SS} \,|\, \text{LOC} \,|\, \text{CAT} & \begin{bmatrix} \text{HD} & \text{NP} \\ \text{CONT} & \begin{bmatrix} \text{INDEX} & \boxed{1} \; 3.\text{sg} \\ \text{RESTR} & \{\text{named}(\boxed{1},\text{jane})\} \end{bmatrix} \end{bmatrix}
\end{bmatrix} \vee \ldots
$$

The import of (23) is that in a *hd-subj-ph* the subject value on the phrase should be empty, and the SYNSEM value of the first daughter should be identical to the subject

---

[16]Notice that, apart from the first, (23), and perhaps the last, (26), these would be consequences of the type theory, rather than axioms. For example, the non-inverted-sentence constraint would be a special case of a more general type of 'head-second' (or 'head-last') phrases. (26) is approximately the lexical entry for *Jane*. The first constraint (23) is based on a constraint from Ginzburg and Sag (2000), but there are differences in the feature geometry assumed, e.g. Ginzburg and Sag (2000) assume that SUBJ is an attribute of *signs*, rather than *cat* values. Nothing substantive hangs on this.

$$
\begin{bmatrix}
\textit{hd-subj-ph} \\
\text{PHON} \quad \text{append}(\boxed{1},\boxed{2}) \\
\text{SS}\,|\,\text{LOC}\,|\,\text{CAT}\,|\,\text{HD} \quad \boxed{4}\ \text{verb} \\[1em]
\text{DTRS} \quad \left\langle \boxed{5} \qquad\qquad\qquad\qquad\qquad\qquad ,\ \boxed{6} \right\rangle \\[1em]
\text{HD-DTR} \quad \boxed{6}
\begin{bmatrix}
\textit{siv-lx} \\
\text{PHON} \quad \boxed{2}\ /\text{runs}/ \\
\text{SS}\,|\,\text{LOC}\,|\,\text{CAT}
\begin{bmatrix}
\textit{cat} \\
\text{HD} \quad \boxed{4} \\
\text{SUBJ} \quad \langle\boxed{3}\rangle
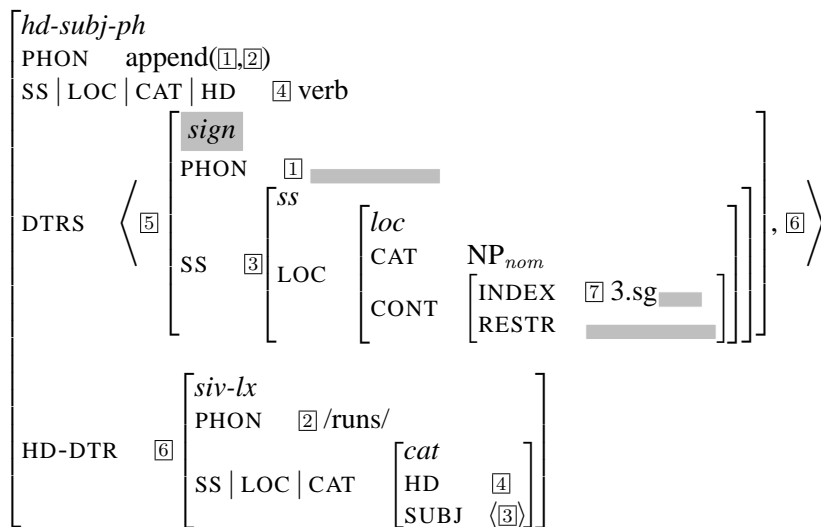\end{bmatrix}
\end{bmatrix}
\end{bmatrix}
$$

Figure 13 Figure 11 with first DAUGHTER deleted

value on the head daughter (so, for example, when *runs* appears as the head in a head subject structure, the subject has to satisfy whatever constraints *runs* requires of its subject). The idea of (24) is to say that in certain kinds of structure (specifically, non-inverted structures) the word order directly reflects the order of the daughters. (25) says that third person singular verbs require their subjects to be nominative 3.sg NPs. This constraint will apply to *runs* if it is indicated that it is of this type. (26) says that one '*n-lex*' has the phonology /Jane/, and the semantics of denoting a singular entity 'named *Jane*' (there will, of course, be other such noun lexemes, hence the '∨ . . . ', indicating alternatives).

Given this, the formulation of *Frontier* is straightforward: a DAUGHTER's value in the structure is deleted, and then type-expanded (and marked as being an open node, which I ignore here). For example, applying *Frontier* to the subject in Figure 11 completely removes the first DAUGHTER, and all its substructure. If matters ended there, this would result in a structure like Figure 13, where the deleted structure has been 'greyed out'. Notice that *all* information about the first daughter has been removed. However, matters do not end here. The idea is that information that is grammatically necessary given the remaining contents of the fragment should be restored. This can be achieved if the fragment is type-expanded.

1. The PHON value of the first daughter must be identified with the first argument of *append* in the PHON of the whole structure (a general constraint on phrases with 'normal' word order — the phonology of the mother is the *append* of the phonologies of its daughters, in order, cf. (24).

$$
\begin{bmatrix}
\textit{hd-subj-ph} \\
\text{PHON} \quad \text{append}(\boxed{1},\boxed{2}) \\
\text{SS} \mid \text{LOC} \mid \text{CAT} \mid \text{HD} \quad \boxed{4}\ \text{verb} \\[2mm]
\text{DTRS} \quad \left\langle \boxed{5}
\begin{bmatrix}
\textit{sign} \\
\text{PHON} \quad \boxed{1}\ \rule{2cm}{2mm} \\
\text{SS} \quad \boxed{3}
\begin{bmatrix}
\textit{ss} \\
\text{LOC}
\begin{bmatrix}
\textit{loc} \\
\text{CAT} \quad \text{NP}_{nom} \\
\text{CONT}
\begin{bmatrix}
\text{INDEX} \quad \boxed{7}\ 3.\text{sg} \\
\text{RESTR}
\end{bmatrix}
\end{bmatrix}
\end{bmatrix}
\end{bmatrix}
,\ \boxed{6} \right\rangle \\[2mm]
\text{HD-DTR} \quad \boxed{6}
\begin{bmatrix}
\textit{siv-lx} \\
\text{PHON} \quad \boxed{2}\ /\text{runs}/ \\
\text{SS} \mid \text{LOC} \mid \text{CAT}
\begin{bmatrix}
\textit{cat} \\
\text{HD} \quad \boxed{4} \\
\text{SUBJ} \quad \langle \boxed{3} \rangle
\end{bmatrix}
\end{bmatrix}
\end{bmatrix}
$$

Figure 14 *Frontier* operation

2. The SUBJ value of *runs* must be identified with the first DAUGHTER (because this is a *head-subject-phrase*, cf. (23).

3. A consequence of this is that first daughter's CAT value becomes NP$_{nom}$ and 3.sg, because this is what *runs* requires of its subject.

This produces a structure like Figure 14. Notice, however, that certain things will remain unspecified here:

1. There is nothing to say what kind of *sign* the first daughter has to be — it might be either as subsort of *lexical*, as it was in *Jane runs*, but it might equally be *phrasal*, as in *The child runs*.

2. There is unlikely to be any constraint that stipulates what the PHONOLOGY of the first daughter should be, so this is unspecified.

3. Similarly for the most of the CONTENT: all that *runs* requires is that the INDEX be third person and sg — there is nothing that requires it to be feminine, or have the meaning associated with *Jane*, so these aspects remain unspecified.

The result is thus a fragment of the right level of generality: the subject of *runs* is required to be 3.sg and nom, but not required to be fem. See Figure 13, where the location of the missing information relative to the original structure is indicated in grey.

The formulation of *Root* is more complicated, but the general idea is straightforward. Again, the idea is to remove information that is just a result of the context in which the fragment originated, but retain what is grammatically necessary. Figure 15 (a) is *Jane* as it appears in the original structure (Figure 11). Some of the information here might have come from the context from which *Jane* has been re-

moved (roughly, [Δ *runs*]), this information can be found by comparing Figure 15 (a) with what appears as a result of type-expansion of [Δ *runs*], which was calculated above (see the relevant part of Figure 14). This 'potentially contextual information' is shown in Figure 15 (b). If all this potentially contextual information is thrown away, what results is Figure 15 (c). The information here must be inherent to *Jane* itself, independent of context. Of course, it is rather underspecified, but the desired result can be obtained if it is type-expanded. In particular, if we try to find a lexical type which has the phonology /Jane/, and the semantic content of denoting an entity named *Jane*, it is very likely that this will be required to be an NP, and to be 3.sg. Again, this is at just the right level of generality.[17]

With this apparatus it is not only possible to produce fragments of the right level of generality, it is also possible to set things up so as to avoid probability leak. In particular, suppose composition is defined (as in Tree-DOP) as leftmost substitution, and (as seems natural) as involving unification.[18] Suppose that F is a fragment whose leftmost open node is $F_{[1]}$. If the competition set for $F_{[1]}$ is defined as the set of fragments F such that each element f_i in F can be unified with $F_{[1]}$, then the probability of each f_i can be identified with its frequency relative to this set, without probability loss. No probability mass will be lost, because this definition ensures that all and only the fragments that can be successfully substituted at a node are considered as competitors. Moreover, since HPSG eschews non-monotonic 'filtering' apparatus such as LFG-style completeness checks, every structure that can be produced in this way represents a legitimate grammatical object.
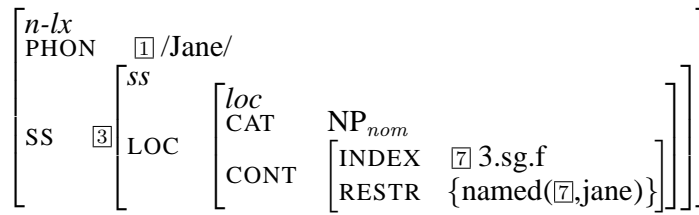
5. Discussion

The two previous sections have sketched approaches to the problem of fragment generality and probabilistic soundness for versions of DOP involving two very different grammatical frameworks. The substantive linguistic assumptions and the formal apparatus of the theories are very different, and this difference is reflected in the very different formulations of decomposition operations. For example, in LFG-DOP there
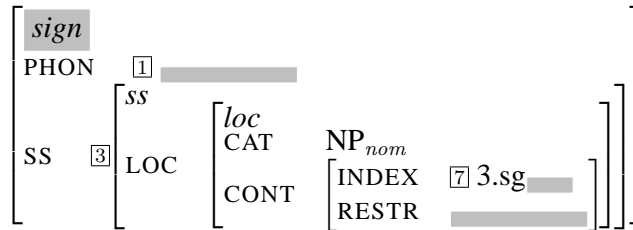
---

[17]Arnold and Linardaki do not discuss this point, but it is clear that *Root* and *Frontier* fragments of the kind described here cannot be the *only* fragments in the database, because this will loose a good deal of useful statistical information about certain variants of expressions being more common. For example, *oats* is much more likely to occur than *oat*, but the procedure given here will produce *Root* fragments which are underspecified for number, losing this information. Rather, the *Root* and *Frontier* fragments should be used to set upper bounds on the generality of fragments. For example, as regards *Root*, let F_0 be a structure like Figure 15 (a), and F_1 be the *Root* fragment produced by the operation described in the text. What should go in the database is every fragment F' such that $F_1 \sqsubseteq F' \sqsubseteq F_0$.

[18]See Linardaki (2006) for discussion of an alternative composition strategy.
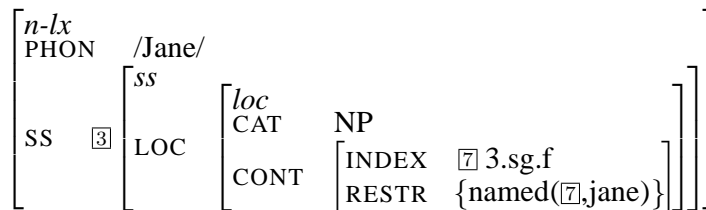
$$\begin{bmatrix} \textit{n-lx} \\ \text{PHON} \quad \boxed{1}\ /\text{Jane}/ \\[2pt] \text{SS} \quad \boxed{3} \begin{bmatrix} \textit{ss} \\ \text{LOC} \begin{bmatrix} \textit{loc} \\ \text{CAT} \quad \text{NP}_{nom} \\[2pt] \text{CONT} \begin{bmatrix} \text{INDEX} \quad \boxed{7}\ 3.\text{sg.f} \\ \text{RESTR} \quad \{\text{named}(\boxed{7},\text{jane})\} \end{bmatrix} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

(*a*) Original: *Jane* in Figure 11

$$\begin{bmatrix} \textit{sign} \\ \text{PHON} \quad \boxed{1}\ \rule{3em}{0.6em} \\[2pt] \text{SS} \quad \boxed{3} \begin{bmatrix} \textit{ss} \\ \text{LOC} \begin{bmatrix} \textit{loc} \\ \text{CAT} \quad \text{NP}_{nom} \\[2pt] \text{CONT} \begin{bmatrix} \text{INDEX} \quad \boxed{7}\ 3.\text{sg}\rule{1.5em}{0.5em} \\ \text{RESTR} \quad \rule{4em}{0.6em} \end{bmatrix} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

(*b*) Potentially contextual features

$$\begin{bmatrix} \textit{n-lx} \\ \text{PHON} \quad /\text{Jane}/ \\[2pt] \text{SS} \quad \boxed{3} \begin{bmatrix} \textit{ss} \\ \text{LOC} \begin{bmatrix} \textit{loc} \\ \text{CONT} \begin{bmatrix} \text{INDEX} \quad \boxed{7} \quad \text{f} \\ \text{RESTR} \quad \{\text{named}(\boxed{7},\text{jane})\} \end{bmatrix} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

(*c*) Inherent features

$$\begin{bmatrix} \textit{n-lx} \\ \text{PHON} \quad /\text{Jane}/ \\[2pt] \text{SS} \quad \boxed{3} \begin{bmatrix} \textit{ss} \\ \text{LOC} \begin{bmatrix} \textit{loc} \\ \text{CAT} \quad \text{NP} \\[2pt] \text{CONT} \begin{bmatrix} \text{INDEX} \quad \boxed{7}\ 3.\text{sg.f} \\ \text{RESTR} \quad \{\text{named}(\boxed{7},\text{jane})\} \end{bmatrix} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

(*d*) Type-expansion of (c)

Figure 15 *Root* operation

is no mention of type-expansion (there could not be — LFG f-structures are untyped), and no use is made of *Discard* in the presentation of HPSG-DOP. But it should be clear that the underlying idea in both cases is very similar: the descriptive apparatus of the grammatical theory is deployed to set an upper bound on the generality of fragments that are produced.[19]

---

[19]Notice that since the approaches use the standard descriptive apparatus of the relevant theories, the approaches can be applied wherever that apparatus applies. Thus, the rather trivial example that has been used for illustration (subject-verb agreement) is not a reflection of the kind of problem that can be dealt with. The approaches are applicable wherever the apparatus is applicable. In the case of LFG-DOP this means the approach is applicable to everything that is described using lexical entries and phrase structure rules with f-structure annotations. In the case of HPSG-DOP this means it is applicable to everything that is described using the type theory. Thus, in principle, the approaches extend straightforwardly to fragments involving word order variation, local and long-distance dependencies,

This approach is open to a number of objections.

One natural objection is that by ensuring that fragments have the 'right' level of generality, the approach will loose robustness — the ability to deal with ill-formed or 'noisy' language, including examples that contain unknown words. This is not just a practical objection: a variety of psycholinguistic experiments rely on language users' ability to process deviant input, and explore differences between this and 'normal' behaviour. This requires a theory of robust processing and its interaction with normal processing, and normal grammatical knowledge. But the lack of robustness is not a fundamental problem with this approach: it would be straightforward to introduce a new *Discard*-like operation to produce less restrictive versions of the fragments produced by these methods, and to use these fragments for robust processing. And of course such an operation would provide the basis for a characterization of grammaticality that is more theoretically sound than one based on the original version of *Discard*.

Notice that although the grammatical apparatus used looks like that of a conventional grammar in the relevant formalism, the descriptions are interepreted in a quite non-standard way: rather than generating or constraining representations of expressions, the descriptions are interpreted as constraints on *fragments*.

One implication of this is that they may lack properties normally associated with grammars as usually understood. For example, they do have to be 'complete' in any sense. For example, interpreted in the normal way, a grammar consisting of just the S rule in (15) above is entirely useless (it generates the empty language). But interpreted as a constraint on fragments, it can perform a useful job in setting limits on fragment generality (specifically, it will require subjects of finite verbs to be nominative). The grammar/type-system that must be constructed under this sort of approach may be radically underspecified.[20]

Second, this descriptive apparatus plays no role in sentence processing (parsing or production), so this proposal has no implications in this area: in particular, it has no implications for the computational complexity of Data Oriented parsing.

Where it does have implications is in the view of what linguistic knowledge a speaker/hearer is assumed to have, and in language acquisition. The implication is that the adult speaker has at her disposal, and the learner must acquire, not just a collection of fragments, but also what are essentially generalizations about frag-

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and to other levels of representation.

[20]This means that in a practical setting, adding such a grammar to an existing DOP system should be much less burdensome than one might fear, since it can be added stepwise in small increments, improving grammatical coverage at each step.

ments. I find this appealingly like the distinction between a performance grammar (the fragment database) and a competence grammar (the LFG grammar or HPSG type-system that guides decomposition). Notice what is implied between the competence and performance grammars on this view: they are formally similar (the same sorts of structure, the same vocabulary of features, etc.), but they are not related by compilation, or anything similar.

However, perhaps the most interesting implication of this approach is that it radically widens the class of grammar formalisms for which plausible DOP models can be provided. The preceeding discussion has sketched out how the approach can be used in relation to HPSG and LFG. These are very different theories, formally and substantively. The fact that DOP models can be provided for them strongly suggests that such models can be provided for any 'surface oriented' grammatical formalism (e.g. Definite Clause Grammars, Tree Adjoining Grammars). But in fact, it seems plausible that the approach described above can be applied much more generally. Consider, for example, the class of transformational grammars. Here the grammatical representations can be taken to be tuples of representations at different levels, e.g. Surface Structure (PF), Logical Form (LF), and (to be old fashioned), Deep Structure (DS). A fundamental problem with providing a DOP model for such a grammar is to provide a sensible way of decomposing such structures. At first glance, this seems to pose an insuperable formal problem. But it is perhaps not so.

The crucial step is to define a 'correspondence' relation between pieces of structure (and features) at different levels, so that one can think about operations which produce sensible $\langle DS, LF, PF \rangle$ triples. But from the present perspective, this is not so much a *formal* problem as a *substantive* one. The formal solution can be 'anything goes'. Of course, it is replaced by the substantive problem of whether general principles and (transformation) rules can be written which will properly constrain the tuples so that only sensible fragments are produced. This is still a somewhat daunting task, but it is a task that can perhaps be approached in a more piecemeal fashion than the formal problem, using the technology of rules and principles that is familar to linguists. Of course, realistically speaking, given the current state of transformational theory, notably its informality and the lack of computational tools, this is possibility that is likely to remain hypothetical in the near future. But it is an interesting speculation: the implication would be that, as regards the formulation of decomposition operations, the only approaches which would resist DOP-based approaches would be grammatical formalisms where representations play no essential role (e.g. Montague Grammar, where the important datastructure is the derivation tree, or various

'dynamic' grammars, e.g. Cann et al., 2005). What this would leave, of course, is the problem of defining sensible probability models — specifically, models which do not suffer from probability leak. Given the propensity for non-monotonic apparatus in linguistic theories, this is likely to pose a more lasting challenge.

Acknowledgments

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Hang on again! Are we 'on the right track'?

Robert D. Borsley

In his 1996 paper 'Now, hang on a minute: Some reflections on emerging orthodoxies', Martin Atkinson identified a series of weaknesses in the Chomskyan acquisition literature and suggested that its conclusions were a lot less secure than generally assumed. Like other critiques of mainstream work Atkinson's paper was effectively ignored. The mainstream is confident that it is 'on the right track'. However, there are reasons for thinking that the confidence may not last much longer. Problems associated with Minimalism, evidence against the parametric approach to language variety, and evidence that the rejection of constructions is untenable are likely to cast increasing doubt on mainstream ideas.

1. Introduction

In Atkinson (1996), Martin Atkinson urged researchers pursuing a Chomskyan approach to language acquisition to 'hang on a minute'. He identified a series of weaknesses in the Chomskyan acquisition literature and suggested that its conclusions were a lot less secure than generally assumed. Like many others who have questioned important aspects of what Culicover and Jackendoff (2005) call Mainstream Generative Grammar (MGG), Atkinson was effectively ignored. MGG does not want to be told to 'hang on a minute'. It wants to be told: 'That's an interesting idea. Why don't we extend it in the following way?' Culicover and Jackendoff (2005: 53, fn. 7) put their finger on this when they comment on Larson's (1988) analysis of double object structures that 'it appears that researchers outside MGG found Larson's analysis too outrageous to merit discussion. Meanwhile, within MGG many researchers adopted it enthusiastically and set out to find ways to apply it further'. There is an apparently unshakeable confidence that mainstream work is 'on the right track', in one of Chomsky's favourite phrases.[1] But there are reasons for thinking that the confidence may not last very much longer. The time may well be coming when it starts to ebb and those who say 'hang on a minute' are no longer ignored.

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[1] The phrase occurs three times in Chomsky (2005a), and three times in Chomsky (2005b).

The paper is organised as follows. In section 2 I consider why there is so much confidence that MGG is on the right track. In section 3 I look at some problematic features of Minimalism. In section 4 I consider the idea that language variety is the product of a set of innate parameters, and in section 5 I look at the related idea that there are no constructions of the traditional kind. Finally, in section 6, I summarise the paper.

2.   The confidence

In his discussion of the acquisition literature Atkinson (1996: 459) remarks that '[i]t is noteworthy that the language employed in much discussion reeks of confidence; we do not here confront tentative suggestion'. This confidence is just as much a feature of the pure syntax literature. Where does it comes from? Perhaps the first point to make is that it is not clear how many of those who work within MGG are really that confident that it is on the right track. It may be that many just feel that it is in their interest to adopt the framework. After all, your chances of publishing in certain journals are very low if you don't, and at least in the US your chances of getting a job are reduced.

No doubt, however, there are many true believers. Why is this? Clearly Chomsky is a large part of the answer. Being with Chomsky is a source of great comfort.[2] Chomsky is clearly the most influential figure in theoretical linguistics, and many who have criticisms of his recent work would agree that *Syntactic Structures* and *Aspects* were important, and many would say the same of 'Remarks on nominalisation', 'On *wh*-movement' and *Lectures on Government and Binding*. He has been a dominant figure for so long that many are convinced he must be on the right track (even if they are not entirely sure where he is going, a matter to which I return below). There is also a widespread impression that his ideas are the product of a deeper kind of thinking than others can manage. Only Chomsky would have references to Alfred Russel Wallace, Darwin, Jared Diamond, D'Arcy Thompson

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[2] Being with Chomsky is not entirely unproblematic, however. Sometimes researchers seize on some idea of his only to find that he abandons it as soon as they have made use if it. This happened to Manzini and Roussou (2000), who proposed an approach to control involving feature-movement, which Chomsky rejected in Chomsky (2000).

and Alan Turing in the first few pages of a paper in *Linguistic Inquiry* (Chomsky 2005a).[3] Many are impressed.[4]

One thing that is fairly clear about the confidence is that it has not just emerged spontaneously. Chomsky and others have worked hard to promote it.[5] One aspect of this is the implicit suggestion that there are no alternative approaches that deserve any attention. This is seen in Chomsky's early use of the term 'Standard Theory', and in his recent assertions that 'the radically simplified form of transformational grammar that has become familiar is a kind of conceptual necessity' (Chomsky 2004), and that 'some version of transformational grammar seems to be the null hypothesis' (Chomsky 2005a: 12). One consequence of such talk is that it is common for terms like syntactic theory and generative grammar to be used to mean MGG as if there were no other approaches. Interestingly, Chomsky makes it clear – in Chomsky (1995: 162), for example – that by generative grammar he just means explicit grammar, but he seems to have little influence here. Another consequence is that whereas those working in other frameworks normally have some knowledge of MGG, those working within MGG typically know nothing of other approaches.[6] A situation has been created in which it is respectable to write a book on relative clauses but not mention what is probably the most detailed generative analysis of English relative clauses in Sag (1997) or to write about *wh*-interrogatives without mentioning the lengthy discussion in Ginzburg and Sag's (2000) book. Atkinson (1996: 454) remarks in connection with Fukui's (1986, 1993) proposal that Japanese lacks C and AGR: 'lets not close our minds to alternative construals of UG simply because they don't originate in the right corridors'. In fact, many minds are firmly closed to ideas from the wrong corridors.

Although alternative approaches are generally not mentioned they are sometimes alluded to. Thus, for example, Chomsky (2000) remarks that the

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[3] However, something similar can be found in Boeckx (2006).

[4] As a callow youth (or at least a callow 21-year-old) I was deeply impressed by the reference in Chomsky (1965) to 'what the frog's eye tells the frog's brain'. Things I had read before, e.g. Bloomfield, were not like that.

[5] Chomsky has had the services of some dedicated spin doctors, notably in recent years Boeckx and Uriagereka. For some discussion of the former see Newmeyer (forthcoming). For discussion of the latter's efforts see Levine (2002).

[6] An interesting exception is Jan Koster, who argues in Koster (2000) for the replacement of the minimalist operation Move by the slash mechanism of GPSG and HPSG.

Minimalist Programme 'encourages us to distinguish genuine explanations from "engineering solutions" – a term I do not mean in any disparaging sense'. The suggestion is that Minimalism provides explanations while other (unnamed) approaches merely engage in engineering. This is nonsense (and I mean that in a disparaging sense). Consider a concrete example, namely English non-finite relative clauses. Unlike finite relative clauses, they only allow a PP and not an NP/DP as the filler constituent. Thus, we have the following contrast:

(1)   someone $\left\{ \begin{array}{l} \text{who you can rely on} \\ \text{on whom you can rely} \end{array} \right\}$

(2)   someone $\left\{ \begin{array}{l} \text{*who to rely on} \\ \text{on whom to rely} \end{array} \right\}$

These data raise the following question:

(3)   Why do non-finite relatives only allow a PP as the filler?

In his detailed HPSG (Head-driven Phrase Structure Grammar) discussion of relative clauses, Sag (1997) proposes that non-finite relatives are instances of a phrase-type whose non-head daughter is required to be a PP. Thus, HPSG gives the following answer to (3):

(4)   Because the relevant phrase type only allows a PP as a non-head daughter.

For Minimalism, the properties of phrases are a consequence of the feature makeup of their heads. In the case of relative clauses, the head is a complementiser which is phonologically empty when there is an overt filler. Thus, Minimalism must give the following answer:

(5)   Because the relevant phonologically empty complementiser only allows a PP as its specifier.

These are different answers, but there is absolutely no reason to think that one is just engineering whereas the other offers an explanation.

Another suggestion that one sometimes hears is that alternative approaches are not 'interesting', unlike MGG. The obvious response to this is that interest is in the eye of the beholder. Some may find MGG more interesting than the alternatives, but others find just the opposite. It may be hard to believe, but there are even people who don't find any theory of syntax interesting. Of course, what really matters is not what is interesting but what is true. As Culicover (2004) notes, it would be interesting to discover that the moon is made of green cheese, but the fact remains that it isn't.

Another aspect of the promotion of confidence is a certain picture of the history of the field. Most of Chomsky's papers include a history lesson implicitly suggesting that current work is a logical outcome of the work of the 1950s and 1960s. In effect, the message is that if you liked *Syntactic Structures* or *Aspects*, it would be illogical to have qualms about more recent work. As far as more recent developments are concerned, the suggestion is that the Government-Binding (GB) framework was a major success and Minimalism has built on this, taking things to a higher level.[7] I will challenge this view in the following sections.

3. Problems with Minimalism

Minimalism appears to dominate syntactic theorising in much the same way as GB did. However, there are reasons for thinking its position is not as strong. GB won converts, for example Jim McCloskey, who previously worked in a version of Montague Grammar (McCloskey 1979), Sandy Chung, who previously worked in Relational Grammar (Chung 1976), and Andrew Radford, another former adherent of Relational Grammar (Radford 1977). In contrast, Minimalism has suffered defections. Thus, Fritz Newmeyer, who wrote enthusiastically about GB in Newmeyer (1986), came out as a trenchant critic of Minimalism in Newmeyer (2003). Similarly, Peter Culicover, who published an introduction to MGG in Culicover (1997), has more recently developed a detailed critique of Minimalism in Culicover and Jackendoff (2005). Then there is Gert Webelhuth, who edited an important GB anthology in Webelhuth (1995), but who has worked within constraint-based frameworks more recently (Ackerman and Webelhuth 1998). From the point of view of a committed minimalist, a number of researchers who were once on the right track have gone astray. This must be rather disturbing.

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[7] For examples of this view, see Boeckx (2006), Boeckx and Hornstein (2003) and Boeckx and Piatelli-Palmarini (2005).

Almost certainly one reason for Minimalism's limited success is the lack of clarity as to what exactly it is. This is apparent in the various replies to Lappin, Levine and Johnson's (2000a) critique of Minimalism. There is agreement among the authors of the replies that Minimalism is a good thing but no agreement as to what exactly it is. In their response to the replies, Lappin, Levine and Johnson (2000b: 888) comment that 'there seems to be considerable disagreement among the authors of the five replies about the content and the status of the MP [Minimalist Programme]' and suggest that '[t]his sort of discord on central issues raises the suspicion that the adherents of the MP may have bought into a research program which is undefined at crucial points'.

In early days some saw an emphasis on avoiding unnecessary theoretical apparatus as the defining feature of Minimalism. For example, Radford (1997: 515) defines Minimalism as 'a theory of grammar … whose core assumption is that grammars should be described in terms of the minimal set of theoretical and descriptive apparatus necessary'. However, as Atkinson (2000) points out, this can't be right. Avoiding unnecessary theoretical apparatus is just good scientific practice. Occam's razor was not discovered in Cambridge, Mass. in 1990. No framework employs theoretical devices that are thought to be unnecessary.[8] Thus, this cannot be the defining feature of Minimalism.[9]

Early work in Minimalism stressed the notion of 'virtual conceptual necessity', which suggests some special kind of simplicity. However, it is far from obvious what the term might mean and, as Postal (2003) demonstrates, various mechanisms which have been claimed to be (virtually) conceptually necessary are in fact not necessary at all. As Postal (2003: 19) observes, talk of virtual conceptual necessity can be seen 'an attempt to provide certain views with a sort of privileged status, with the goal of placing them at least rhetorically beyond the demands of serious argument or evidence'.

In one early attempt to explain Minimalism, Marantz (1995) remarks that it envisages 'the end of syntax per se'. This conjures up images of syntacticians with no

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[8] Boeckx (2006: 7) calls Radford's definition a 'caricature'. This raises the question: how did Radford come to propose a caricature? In my view, Chomsky's lack of clarity is largely to blame. Boeckx tries to explain how Minimalism is more than Occam's razor, but it seems to me that he is not very successful. See Newmeyer (forthcoming) for a similar view.

[9] As Culicover and Jackendoff (2005) emphasise, the extreme complexity of minimalist representations suggests that Occam's razor is wielded very selectively.

more work to do forced to retrain as phonologists or sociolinguistics. It also invites the question 'so you have solved the syntactic problems of English and the world's other languages, have you?', to which, of course, the answer is rather obviously 'no'. Marantz goes on to explain that by the end of syntax he doesn't really mean the end of syntax. This being so, it is not a very helpful phrase.

More recent discussion has focused on the notion of perfection. For example, Chomsky (2002: 58) remarks that 'it has become possible to pose in a productive way the question of the "perfection of language": specifically, to ask how closely human language approaches an optimal solution to design conditions that the system must meet to be usable at all'.

Interestingly, the idea that language is perfect is not very prominent in some textbook introductions to Minimalism. Radford (2004: 9) introduces the idea that 'language is a perfect system with an optimal design in the sense that natural language grammars create structures which are designed to interface perfectly with other components of the mind – more specifically with speech and thought systems', but it is not mentioned again. Adger (2003) says nothing at all about this idea. It looks as if some of those who are trying to promote Minimalism doubt whether this is an idea they can sell.

An obvious problem with the idea that language is perfect is that it seems to conflict with Chomsky's long standing insistence that language is a mental organ which should be studied in the same way as physical organs. As Lappin, Levine and Johnson (2000b) note, biologists do not ask of physical organs how closely they approach an optimal solution to design conditions that the system must meet to be usable at all. (They consider the example of the urinary tract.) It is, of course, logically possible that this is because biologists are not doing their job properly, and it may be that this is Chomsky's view. Johnson and Lappin (1999: 127, fn. 88) report that Chomsky suggested in a lecture entitled 'Explorations in Minimalism' at the University of London in June 1995 that 'the reason that grammar might appear to be unique among biological systems in displaying optimal design is that biological theories may have misconstrued biological phenomena and failed to discover the underlying principles which govern them'. Chomsky (1995: 1–2) also appears to suggest that biology is unsatisfactory when he remarks that work in linguistics poses 'a problem for biology and the brain sciences, which, as currently understood, do not

provide any basis for what appear to be fairly well established conclusions about language'. This leads Pinker and Jackendoff (2005: 229) to comment that

> Given the relative rigor and cumulativeness of biology and linguistics, this strikes us as a wee bit presumptuous (especially since the Minimalist Program is 'still just an "approach"', 'a conjecture about how language works'). There is a simpler resolution of the apparent incompatibility between biology and Minimalism, namely that Chomsky's recent claims about language are mistaken. Rather than being useless but perfect, language is useful but imperfect, just like other biological systems.

Chomsky's more devoted followers may be happy to tell biologists that they have got it wrong, but most who aspire to be minimalists are likely to balk at this.

Perhaps the moral of this is that those who talk a lot about biology do not necessarily take much notice of what is generally assumed in biology. Essentially this point was made in Atkinson (1996: 458) when he noted in connection with the reluctance of language acquisition researchers to accept a role for maturation that 'my biology is not up to much, but I have a sneaking feeling that most biologists would probably view maturation as the norm in the development of biological systems'.

There is, then considerable uncertainty about what exactly minimalism is.[10] No doubt this is one reason for its limited success.[11]

Another weakness of Minimalism is that it seems to have very little in the way of empirical results. As Newmeyer (2003: 589, fn. 7) observes, when Chomsky is asked in an interview what the 'results' of our field are, 'he responds by citing descriptive generalisations uncovered in pre-minimalist work, such as the distinction between strong and weak islands, rather than pointing to concrete empirical problems

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[10] There is also considerable uncertainty about how various theoretical proposals within Minimalism should be understood. This is highlighted in Atkinson's unpublished writings on Minimalism, which are full of words like 'puzzlement' and 'obscurity'. It is rather surprising that a theoretical framework that has been around for fifteen years and has had the services of some of the best minds in the field should be so beset with uncertainty.

[11] Of course, obscurity is not always a problem. As the success of the French intellectuals who are the subject of Sokal and Bricmont (1998) shows, there are those who see obscurity as a sure sign of profundity. It may well be that the obscurity of Minimalism is an attraction for some.

solved under the MP' (see Chomsky 2002: 151, 153). Occasionally it is claimed that there are some important results, but then qualifications are made which suggest that the claims should not be taken very seriously. Thus, Chomsky (1995: 249) claims that 'phrase structure theory can be eliminated entirely, it seems, on the basis of the most elementary assumptions', but then he remarks later that 'we still have no good phrase structure theory for such simple matters as attributive adjectives, relative clauses, and adjuncts of different types' (1995: 382, fn. 22). In an apparent attempt to justify the absence of results, proponents of Minimalism repeat the mantra that it is a programme and not a theory. This can only work for so long. As Culicover and Jackendoff (2005: 541) observe, '[t]he response that it is still just a "program", a way of asking questions, may have been justifiable in 1995, but a decade later it rings hollow'.[12]

One aspect of the lack of results is the absence of the kind of detailed and precise analyses that one would expect within generative grammar.[13] There is a sharp contrast here with some other approaches. In HPSG in particular, it is not uncommon to find substantial appendices setting out formal analyses. See, for example, Sag's (1997) paper on English relative clauses and especially Ginzburg and Sag (2000), which has a fifty-page appendix. Such appendices are unheard of in minimalist work.[14] One result of the lack of detailed analyses is that MGG has had virtually no influence in computational linguistics. Curiously, though, the word 'computational' is used extensively in Minimalism.

It is worth noting that the absence of detailed analyses has certain advantages. Minimalism values simple analyses which avoid stipulations. A sketch of an analysis is inevitably simpler and less encumbered by stipulations than an analysis which is worked out in detail. Hence it looks better if one forgets that it is only a sketch. The absence of detailed analyses also makes it difficult for potential critics. They cannot argue that minimalist analyses make incorrect predictions because being only

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[12] Chomsky (2002) suggests that the more mature sciences also lack firm results. He remarks that '[e]ven in the advanced sciences everything is questionable'. See Levine and Postal (2004) for some critical commentary on this idea.

[13] The absence of detailed analyses in earlier MGG work led Gazdar et al. (1985: 6) to suggest that generative grammar 'includes little of the research done under the rubic of the "Government and Binding" framework, since there are few signs of any commitment to the explicit specification of grammars or theoretical principles in this genre of linguistics'.

[14] One proponent of Minimalism responded to this observation by drawing my attention to Julien (2002). However, this work contains an appendix presenting the data with which it is concerned and not one setting out a formal analysis. See Spencer (2004) for an interesting review of the book.

sketches they don't really make any predictions. Before critics can discuss what predictions analyses make they first have to consider how they might be worked out in detail and it is always open to authors to say that they would have fleshed the analysis out in some other way. Hence it becomes very difficult to criticise favoured analyses.

Minimalism has an ideology which seeks to justify the absence of detailed analyses. It is embodied in Chomsky and Lasnik's (1995: 28) suggestion that providing a rule system from which some set of phenomena can be derived is not 'a real result' since 'it is often possible to devise one that will more or less work'. Instead, they say, 'the task is now to show how the phenomena ... can be deduced from the invariant principles of UG with parameters set in one of the permissible ways'. In other words, providing detailed analyses is a job for unambitious drudges; real linguists pursue a more ambitious agenda. Postal (2004: 5) comments that what we see here is 'the fantastic and unsupported notion that descriptive success is not really that hard and so not of much importance'. He points out that if this were true, one would expect successful descriptions to be abundant within transformational frameworks. However, he suggests that 'the actual descriptions in these frameworks so far are not only not successful but so bad as to hardly merit being taken seriously'. Postal does much to justify this assessment with detailed discussions of MGG work on strong crossover phenomena and passives in chapters 7 and 8 of his book.

While there is a shortage of results in Minimalism, there is no shortage of rhetoric. In a sense there is nothing new here. Back in the 1980s, Postal (1988) drew attention to various rhetorical devices that have been employed in the mainstream literature, the 'Phantom Principle Move', the 'Phantom Theorem Move', the 'Social Conformity Move', the 'Epistemology of Desired Error Move' and others. However, the amount of rhetoric seems to have increased. Newmeyer (2003: 586) comments that 'one is left with the feeling that Chomsky's ever-increasingly triumphalistic rhetoric is inversely proportional to the actual empirical results that he can point to'.

A further problem for Minimalism is the way certain features have made it hard to maintain the standard fable about steady progress. A major theme of Minimalism is that some central ideas of GB, notably government and D-structure, were mistaken. It follows that Chomsky was on the wrong track in the 1980s when others, such as those working within HPSG and Lexical-Functional Grammar (LFG), and Koster (1987), who did not subscribe to these ideas, were on the right track. Of

course, if Chomsky was wrong then, he could also be wrong now. Recent developments in Minimalism create further problems. Hauser, Chomsky and Fitch (2002: 1573) propose that 'FLN [the "Narrow Language Faculty"] comprises only the core computational mechanisms of recursion as they appear in narrow syntax and the mappings to the interfaces'. Pinker and Jackendoff (2005) see this as 'the position that very little is special to language, and that the special bits are minor modifications of other cognitive processes' and note that many have viewed the paper as 'a major recantation'. I will return to this below.

There are, then, a variety of reasons why Minimalism has not been as successful as GB. It seems likely that these factors will lead to increasing doubts about MGG.

4.   Parameters

I have argued that there are various aspects of Minimalism which mean that its grip on syntactic theorising may not last very much longer, but it is not just the minimalist version of MGG which is problematic. There are problems with two aspects of MGG which go back to the early 1980s and which have been major selling points. The first of these is the idea that language variety is the product of a set of innate parameters.

One point to emphasise at the outset is that this is more than just the position that there is a language faculty which limits the range of possibilities in languages. The latter position is found in Chomsky (1965) or Culicover and Jackendoff (2005: 5), who suggest that '[t]he language faculty ... provides human communities with a toolkit of possibilities for cobbling together languages over historical time. Each language, in turn, "chooses" a different selection and customisation of these tools to construct a mapping between sound and meaning'.[15] The idea that language variety is the product of a set of innate parameters goes beyond this, entailing that there is a finite number of grammatical systems.[16] It also has implications for the study of grammar acquisition and grammar change, suggesting that the former is a matter of parameter-setting and the latter a matter of parameter change.

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[15] Ackerman and Webelhuth (1998: 130), arguing in favour of 'grammatical archetypes', present a similar view, suggesting that 'there is a well defined core of notions and structures that we find in most languages, but ... each language significantly extends that core in unpredictable ways'.

[16] For an interesting early critique of this position see Pullum (1983). According to Pullum (personal communication), this paper, like Atkinson (1996), was ignored.

There is no doubt that it is an attractive conception. As Newmeyer (2006: 6) puts it: 'The original vision of parametric theory was a marvellous one – from the interaction of a small number of simply-formulated parameters, the observed complexity of human language grammar would fall out as a matter of course.' Strong claims are made for this approach. For example, Boeckx (2006: 61) suggests that 'the P&P [Principles-and-Parameters] approach "solves" Plato's problem'.[17] One might suppose that he means that it would solve Plato's problem if it were viable. However, he also remarks (2006: 59) that 'grammarians came to the conclusion [in the 1980s] that something like a P&P account of the language faculty was essentially correct'.[18] In fact, there has been very little progress, and there has been little attempt to give real substance to the theory.

An obvious question about the approach is: how many parameters are there? In his recent book, Newmeyer (2005: 44) remarks that 'I have never seen any estimate of the number of binary-valued parameters needed to capture all of the possibilities of core grammar that exceeded a few dozen' and notes that Lightfoot (1999: 259) suggests that there are about 30 to 40. However, Roberts and Holmberg (2006) comment that '[n]early all estimates of the number of parameters in the literature judge the correct figure to be in the region of 50–100. This is a plausible, if conservative, conjecture'. Developing the view that parameters are 'treelets', Fodor 2001: 388) suggests that '[e]xpanding the capability of this system from twenty or thirty binary parameters to the full-scale variability of a natural language will entail, I assume, expanding out each parametric treelet into a family of related treelets, with less and more marked members'.[19] It looks as if no one really has much idea of how many parameters are needed to accommodate the full range of syntactic phenomena. It also looks as if the estimates are growing. This is not a good sign. As Newmeyer (2006: 6) observes, 'it is an ABC of scientific investigation that if a theory is on the

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[17] Similarly, Epstein and Hornstein (1999: xi) refer to 'a consensus that principles-and-parameters accounts may well answer Plato's problem in the domain of language'. In typically forthright fashion, Postal (2004: 4) suggests that this is 'so far from reality as to be little more than a dream'.

[18] Even allowing for the role of 'something like' and 'essentially', this is not true. It may be true that MGG grammarians came to this conclusion, but this is a different matter. We see here the common pretence that there is nothing outside MGG.

[19] Fodor's (2001) proposal that parameters should be viewed as 'a family of related treelets' was part of a response to Culicover's (1999) argument that the variety of 'peripheral' constructions are problematic for the parametric approach.

right track, then its overall complexity decreases with time as more and more problematic data fall within its scope. Just the opposite has happened with parametric theory. Year after year more new parameters are proposed, with no compensatory decrease in the number of previously proposed ones'.

Another obvious question is: what would be a good example of a parameter? In the 1980s the answer would probably have been the null-subject parameter. It figured prominently in Chomsky (1981) and was the subject of a book, namely Jaeggli and Safir (1989). In the early days a central idea was that the parameter was responsible for a number of phenomena, not just the possibility of null-subjects in finite clauses but also the possibility of a postposed subject, as in (6).

(6) Ha mangiato Giovanni.
     has eaten   Giovanni
     'Giovanni ate.'

Newmeyer (2005: chapter 3) highlights various problems with attempts to make the null-subject parameter precise. He also observes that Jaeggli and Safir (1989) and subsequent work 'all but omit discussion of the rich constellation of syntactic properties, whose unification under the aegis of a single parameter only a few years earlier had been heralded as the great success of the parametric approach to typology' Newmeyer (2005: 92).

One might suppose that there would have been extensive efforts to give some real substance to the parametric approach. However, it seems that there has been just one monograph devoted to it, Baker (2001), and Newmeyer (2005) shows that his version of the approach faces a variety of empirical problems. He also notes (p. 51, fn. 17) that Baker (2001) is a 'book for a general audience', presupposing very little knowledge of grammatical theory and hence is 'a popularization of research results that were never argued for in the scholarly literature in their full technically elaborated form'. Obviously this is a rather strange situation.

A number of other facts suggest that the parametric approach is not in fact taken very seriously. Newmeyer (2006: fn. 6) notes that very little space is devoted to the idea in minimalist textbooks and anthologies. Also relevant here is den Dikken's (2005) response to Culicover and Jackendoff's (1999) implicit suggestion that the comparative-correlative construction, exemplified by (7), is problematic for the parametric approach.

(7) The more I read, the more I understand.

Den Dikken (2005: 497) purports to defend 'a principles-and-parameters approach to language and its acquisition', and asserts that the construction is 'analyzable in keeping with the principles and parameters of UG'. However, he makes no attempt to show that its properties can be attributed to settings of plausible parameters. Moreover, he simply ignores a number of properties. For example, he says nothing about the fact, noted by Culicover and Jackendoff (1999: 559), that correlative *the* cannot be preceded by a pied-piped preposition. This is seen in the following contrast.

(8) a. The more people Kim talks to, ...
    b. *To the more people Kim talks, ...

He also says nothing about the fact highlighted by Culicover and Jackendoff (1999: 554), that a copula can be omitted under certain circumstances.

(9) The more intelligent the students (are), the better the marks (are).

What he does do is show that a number of languages have constructions that are broadly similar to the English construction. Of course, broadly similar constructions can differ in important ways. Thus, for example, the French comparative correlative construction allows a pied piped preposition, as in (10), and allows the insertion of *et* 'and' between the two clauses, as in (11).[20]

(10) Plus longtemps tu pars,   de meilleure humeur tu es à ton retour.
     more long   you go-away of better   mood   you are on your return
     'The longer you are away, the better mood you are in on your return.'

(11) Plus je lis   et   plus je comprends.
     more I read and more I understand
     'The more I read, the more I understand.'

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[20] See Abeillé and Borsley (2006) for discussion.

It looks, then, as if what den Dikken understands by 'a principles-and-parameters approach to language and its acquisition' is just an approach which rejects Joos's (1957: 96) position that languages 'differ from each other without limit', but, as noted at the beginning of this section, the parametric approach is something more than this.[21]

A further problem with parameters is that it is not at all clear what their position is in Chomsky's current thinking. Minimalism is standardly seen as a principles-and-parameters approach. However, as noted earlier, Hauser et al. (2002: 1573) propose that 'FLN [the 'Narrow Language Faculty'] comprises only the core computational mechanisms of recursion as they appear in narrow syntax and the mappings to the interfaces'. It seems to be generally assumed that parameters are a lexical matter. For example, Chomsky (1995: 131) remarks that 'parameters of UG relate, not to the computational system, but only to the lexicon … each parameter refers to properties of specific elements of the lexicon'. It looks, then, as if Chomsky may think that parameters are not part of FLN. This conclusion is also suggested by Chomsky's (2005a) remarks that '[t]here is no longer a conceptual barrier to the hope that the UG might be reduced to a much simpler form' (p. 8) and that 'we need no longer assume that the means of generation of structured expressions are highly articulated and specific to language' (p. 9). It is hard to see how such remarks are compatible with the assumption that UG includes 50–100 parameters. But if parameters are not part of UG, it is not at all clear what their status might be.

After 25 years, there is very little reason to think that the parametric approach is on the right track. A few years ago, Ackerman and Webelhuth (1998: 126) concluded that 'a systematic comparison of the hypothetical insights of the principles and parameters framework with its actual achievements shows a wide gulf between the two'. More recently, Newmeyer (2006: 9) concludes that '[a]fter a quarter-century of its well-documented failures and retreats, one is forced to conclude that the parametric program … is little more than an exercise in wishful thinking'. Such conclusions are likely to be accepted more and more widely and continued claims for the essential correctness of the approach unsupported by serious evidence are likely to bring MGG into increasing disrepute.

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[21] Rizzi (2004: 334) also appears to see the parametric approach as no more than a rejection of Joos' position, when he comments that Baker (2001) 'forcefully argues against an "anything goes" approach to language variation'.

5. Constructions

A second long-standing feature of MGG which is looking increasingly dubious is the idea that there are 'no grammatical constructions of the traditional sort within or across languages' (Chomsky 1995: 6).[22] This idea is closely related to the idea that grammars are sets of parameter-settings. If a principles-and-parameters approach is anything more than just the position that languages do not vary without limit, the number of parameters must be fairly small. If there is a fairly small number of parameters, it seems to follow that the properties of a specific construction should follow from parameter-settings with effects elsewhere. Although constructions are repeatedly declared to be unnecessary, there has been very little attempt to show that this is so The large literature which postulates constructions is largely ignored and, as Culicover and Jackendoff (2005: 535) note, 'much of the fine detail of traditional constructions has ceased to garner attention'. There is very little reason to think that the rejection of constructions is a tenable position.

As Rizzi (2004: 328) puts it, the idea that there are no constructions is the idea that there are 'more elementary computational elements' and 'constructions are mere conglomerates of such finer ingredients'. Underlying this is the observation that constructions share properties with other constructions. For example, it has been clear since Ross (1967) and Chomsky (1977) that *wh*-interrogatives, relative clauses, comparatives, topicalisation sentences, clefts, *easy-to-please* sentences, and non-finite clauses associated with *enough* and *too* have properties in common. A satisfactory analysis needs to treat them as a class and associate the shared properties with the class. This is no problem for an approach which recognises traditional constructions. Such approaches normally assume that constructions are organised into an inheritance hierarchy (see, for example, Sag 1997 and Ginzburg and Sag 2000). Given an appropriate hierarchy, the properties that constructions share are no problem at all. Thus, the similarities between different constructions are no problem for construction-based approaches.

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[22] Note the emphasis here on constructions 'of the traditional sort'. If by construction we just understand phrase type, then one could argue Minimalism has constructions since it has a number of types of phrase, at least phrases which are the product of External Merge and phrases which are the product of Internal Merge.

One might still argue, however, that traditional constructions are not necessary. This would be the case if they had no properties of their own that are not predictable either from the more general constructions that they instantiate or from the lexical items that they contain. For example, one might suggest that there is no need for a prepositional phrase construction because prepositional phrases are just head–complement structures whose distinctive properties stem from the prepositions that head them. But what about the constructions mentioned in the last paragraph? They undoubtedly have properties of their own. The following illustrate some differences between *wh*-interrogatives and *wh*-relatives:

(12) I wondered $\left\{\begin{array}{l}\text{what}\\ \text{which}\end{array}\right\}$ he read.

(13) the book $\left\{\begin{array}{l}\text{which}\\ *\,\text{what}\end{array}\right\}$ he read

(14) I wondered $\left\{\begin{array}{l}\text{who to talk to}\\ \text{to whom to talk}\end{array}\right\}$.

(15) someone $\left\{\begin{array}{l}*\,\text{who to talk to}\\ \text{to whom to talk}\end{array}\right\}$

(12) shows that *what* and *which* are possible in *wh*-interrogatives, while (13) shows that only *which* is possible relative clauses.[23] (14) shows that non-finite *wh*-interrogatives allow both an NP/DP and a PP as the filler constituent, while (15) shows that non-finite relatives only allow a PP as the filler, as noted earlier. The MGG view is that the distinctive properties of constructions stem from the lexical items that they contain. In the present case, however, there are no visible lexical items to which these properties can plausibly be attributed. As noted earlier, the standard view is that the properties stem from an invisible element, a phonologically

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[23] The MGG literature often uses the term '*wh*-word' as if there were some single set of words used in a variety constructions. In fact there are different sets of *wh*-words used in *wh*-interrogatives, relatives and *wh*-exclamatives.

empty complementiser. What evidence is there for such an empty element? In the case of *wh*-interrogatives, it seems that the evidence comes from main clause examples such as (16).

(16) $\begin{Bmatrix} \text{What} \\ \text{Which} \end{Bmatrix}$ did he read?

Here the auxiliary *did* appears between the fronted *wh*-element and the subject. Thus, there is sometimes an element between these two elements. It doesn't follow, however, that there is always such an element. In much the same way, it doesn't follow from the fact that some birds fly that they all do, even ostriches, which appear to stay firmly on the ground. Evidence that pre-subject auxiliaries occupy a complementiser position supposedly comes from certain clauses which have either an overt complementiser or an auxiliary but not both before the subject. The best examples are conditional clauses.

(17) $\begin{Bmatrix} \text{If I had been there} \\ \text{Had I been there} \\ *\text{If had I been there} \end{Bmatrix}$, I would have seen him.

But these do not necessarily show that pre-subject auxiliaries occupy the same position as complementisers. A singular count noun can be preceded by the definite article or a possessive expression but not both.

(18) $\begin{Bmatrix} \text{Kim's} \\ \text{the} \\ *\text{Kim's the} \end{Bmatrix}$ book

However, it is not normally assumed in MGG that they occupy the same position.[24] Thus, the evidence that *wh*-interrogatives are headed by an invisible complementiser is quite weak. In the case of *wh*-relatives, the assumption that they always contain a

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[24] This is generally assumed in the HPSG literature. See, for example, Pollard and Sag (1994: chapter 1.8).

complementiser comes from examples containing the complementisers *that* and *for* such as the following:

(19)   the book that he read

(20)   someone for us to talk to

However, it doesn't follow from the fact that relative clauses sometimes contain a complementiser that they always do. Thus, the argument that *wh*-interrogatives and *wh*-relatives are headed by an invisible complementiser is not a strong one.

There are other cases where there appears to be no theory-independent argument for the invisible head that seems necessary if a construction is to be avoided. Consider again the comparative correlative. On the face of it, it is necessary to assume here that the first clause is a specifier and the second a complement of some invisible head. In other words, it is necessary to assume that (7), repeated here for convenience as (21), has the structure in (22):[25]

(21)   The more I read, the more I understand.

(22)   [[the more I read] [[e] [the more I understand]]]

There seems to be no theory-independent evidence for this invisible head.

It may be that a proponent of MGG would not be concerned about the absence of any theory-independent evidence for the head in (22). It often seems to be assumed in MGG that invisible heads are freely available. If they are, whenever some phrase type has properties which do not seem to stem from its overt constituents one can postulate an empty functional head and attribute the properties to that. Thus, instead of (23) one has (24).

(23)   XP
       /   \
      YP   ZP

(24)   XP
       /   \
      YP   X'
           /   \
          X   ZP
          |
          e

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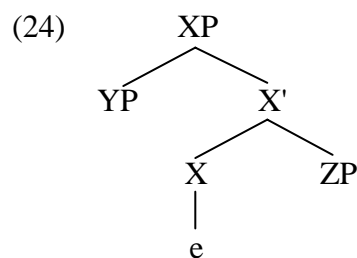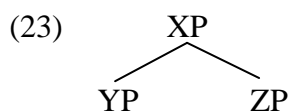
[25] Den Dikken (2005) in fact suggests that the first clause is adjoined to the second clause. This, however, leaves its obligatory character a complete mystery.

However, if invisible heads are freely available, the claim that there are no constructions has no real content.

It is not clear why anyone would think that postulating an invisible head is preferable to postulating a construction. In fact, as Culicover and Jackendoff (2005) point out, idioms seem to provide a good reason for preferring constructions to empty heads. They note that there is a continuum with canonical idioms such as *kick the bucket* at one end and canonical constructions such as the resultative construction, illustrated in (25), at the other.

(25)   The chef cooked the pot black.

In between are what Culicover and Jackendoff call constructional idioms, idioms with open positions which can be filled by any expression of a certain kind. The following illustrate:

(26)   Elmer hobbled/laughed/joked his way to the bank.
(27)   Hermione slept/drank/sewed/programmed three whole evenings away.

Canonical idioms and constructional idioms show that linguistic knowledge includes phrases with full and partial lexical content. Hence, it is hard to see what objection there could be to allowing phrases with no lexical content as a further component of linguistic knowledge. But then it is hard to see why anyone would prefer invisible functional heads to constructions.

It is worth noting here that some early HPSG analyses of unbounded dependency constructions made use of invisible heads. In particular, they were used for relative clauses in Pollard and Sag (1994: chapter 5) and for *wh*-interrogatives in Johnson and Lappin (1997). In more recent work, it has been argued that a hierarchically organised system of constructions permits a more satisfactory analysis. Sag (1997) argues that this is the case of relative clauses, and Johnson and Lappin (1999) argue for it in the case of *wh*-interrogatives, showing among other things how this approach can accommodate optional *wh*-movement and partial *wh*-movement. There is work here that anyone committed to an invisible head approach should read carefully.

The MGG preference for invisible heads over constructions is essentially a preference for lexical accounts of phenomena over syntactic accounts. No real arguments are offered for this preference, and the obvious implication – that the lexicon should be a major focus of research – is ignored. In HPSG, where the lexicon is important but less so than in MGG, detailed proposals about its nature have been developed, e.g. in Pollard and Sag (1987: chapter 8) and Koenig (1999). In particular, it has been proposed that the lexicon involves complex hierarchies of lexical types, which allow properties that are shared between different words to be spelled out just once. In contrast, Chomsky (1995a: 235) suggests that the lexicon provides an 'optimal coding' of lexical idiosyncrasies but does not develop any real proposals as to what this might mean. Newmeyer (2005: 95, fn. 9) comments that

> in no framework ever proposed by Chomsky has the lexicon been as important as it is in the MP [Minimalist Program]. Yet in no framework proposed by Chomsky have the properties of the lexicon been as poorly investigated.[26]

What this means is that the invisible head approach has not really been developed sufficiently for its viability to be assessed. What is needed is invisible-head-based analyses that are as detailed as the construction-based analyses in Sag (1997) and Ginzburg and Sag (2000).[27] In the absence of such analyses, there is no reason to think that invisible heads provide a viable alternative to constructions.[28]

6. Concluding remarks

If the preceding discussion is sound, the Minimalist Programme is either unclear or implausible and both the parametric approach and the rejection of constructions are untenable. These have been major selling points of MGG over the last 25 years.

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[26] Andy Spencer has pointed out to me that one could say something similar about features. They are central to Minimalism, but they have received very little attention. See Asudeh and Toivonen (2006) for some discussion.

[27] With my *Journal of Linguistics* hat on, I have twice suggested to proponents of Minimalism that it would be interesting to have a submission which tried to show that Minimalism could provide a treatment of English relative clauses as detailed as that of Sag (1997). Neither was prepared to take up the challenge.

[28] None of this implies that a syntactic approach is always preferable to a lexical approach. See Müller (2006) for some very interesting discussion from an HPSG perspective.

Without them, it will be increasingly hard to sell, and the conviction that it is on the right track is likely to be weakened considerably.

There is another factor which is likely to undermine this conviction. The dominant role of Chomsky within MGG has been a strength, but it is also a potential weakness. Chomsky won't be with us for ever. When he is no longer around, it will not be possible or linguists to see what Chomsky is doing and do the same or something very similar. Inevitably there will be changes. It is quite likely that MGG will split into competing groups, all claiming to be the 'real' Chomskyans. There could be an unseemly squabble. And there could also be a wholesale abandonment of currently accepted positions.

Thus, there are a number of reasons for thinking that the confidence that is a characteristic of MGG will begin to ebb in the near future. It is likely that there will be a growing audience in the coming years for those who say 'hang on a minute' to MGG, and many may come to the conclusion that rather than being on the right track, syntactic theorising has gone astray in a major way.

Acknowledgements

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Psycholinguistic perspectives on grammatical representations
Harald Clahsen

This paper will make some proposals on how to bridge the gap between psycholinguistic research and theories of grammatical knowledge. Firstly, a set of criteria will be established that psycholinguistic evidence should meet to be relevant for theories of grammar. I will then present two case studies, one from language processing and one from language disorders to illustrate what the theoretical linguist can learn from psycholinguistic studies about the nature of grammars.

1.   Introduction

What is the use of psycholinguistic evidence from the study of language acquisition, language disorders, and real-time processing of language for the theoretical linguist? Looking at the vast majority of studies on grammar, one gets the impression that psycholinguistic evidence is of little use. It is true that theoretical linguists, including those working from the perspective of generative grammar, often pay lip service to the potential relevance of psycholinguistic evidence. Chomsky (1981: 9) noted, for example, that psycholinguistic evidence from language acquisition and experimentation on language processing, and evidence from language deficits is relevant to determining the properties of both Universal Grammar and particular grammars, but at the same time, he observed that evidence from these sources is for some unspecified reason, 'insufficient to provide much insight concerning these problems', and that, therefore, the theoretical linguist is compelled to rely on grammar-internal considerations. Indeed, casual inspection of the major journals in theoretical linguistics reveals hardly any reference to results and findings from psycholinguistic studies.

Against this background, I will make some proposals of how to bridge the gap between psycholinguistic research and theories of grammatical knowledge. Firstly, a set of criteria will be established that psycholinguistic evidence should meet to be relevant for theories of grammar. I will then present two case studies, one from language processing and one from language disorders to illustrate what the theoretical linguist can learn from psycholinguistic studies about the nature of grammars.

2. A framework for employing psycholinguistic evidence

One reason that may have led Chomsky (1981) to conclude that psycholinguistic research is not particularly informative is that many psycholinguistic studies deal with issues linguists do not really care about, e.g. the details of developmental sequences in language acquisition, the intricacies of language impairments, and the precise time-course of language processing. It is also true that psycholinguistic studies often fail to explicate the potential implications of their results for theories of language.

Some common ground is required for psycholinguistic findings to bear on linguistic theories. I suggest that this can be achieved by taking seriously the generative view of language. Generative grammar regards human language as a cognitive system that is represented in a speaker's mind/brain with the mental grammar as its core element. The ultimate aim of generative research is to discover the most appropriate *mental representations* for language, and this encompasses both linguistic and psycholinguistic studies. From this perspective, a linguist examining the grammar of a particular language deals with a mental structure consisting of grammatical representations which are somehow manifested in a person's brain, and which describe what it means to know a language. Research into language processing investigates how grammatical representations are constructed *in real time*, during the comprehension and production of language. We may conceive of these processes as a sequence of operations, each of which transforms a mental representation of a linguistic stimulus into a mental representation of a different form. Language acquisition research is concerned with *changes* of grammatical representations over time. From this perspective, studies of the acquisition of grammar posit a sequence of transitional grammar, i.e. changes to the mental representations of language over time. Studies of language disorders may provide insight into what has been called subtractivity (Saffran 1982), transparency (Caramazza 1984) or residual normality, i.e. selective representational deficits of an otherwise intact system of grammatical representations. Clearly, each domain – language acquisition, language processing, and language impairments – requires its own theories, but if results from psycholinguistic studies are interpreted with respect to the nature of mental representations of grammar, then the theory of grammar can potentially draw on evidence from all these sources.

But how should linguists go about employing psycholinguistic findings for evaluating grammatical analyses and theories of grammar? Simply scanning the psycholinguistic literature for confirming evidence, i.e. for findings that appear to support the preferred analysis or theory is insufficient as one may overlook potential counter-evidence. Instead, a more systematic approach is required. Here, I will make some suggestions of how this could be achieved by setting out some criteria against which results from psycholinguistic studies should be evaluated before they are taken as evidence for particular grammatical analyses or theories.

One important consideration is whether there are any *confounding factors* or alternative explanations for a given psycholinguistic finding. All linguistic data are performance data and are affected by a range of non-linguistic factors (Schütze 1996, 2004). It is possible, for example, that a particular experimental result, e.g. longer response times for condition X than for Y, is due to the fact that X is more demanding in terms of working-memory or more general cognitive resources than Y. The role of such factors needs to be assessed before they are taken as evidence for positing any linguistic difference between X and Y. Below I will discuss a case in point, experimental results on word order and their potential implications for the analysis of clause structure in German.

Another relevant consideration is whether a given psycholinguistic finding is supported by *converging evidence* from other sources. Any data set or experiment is in danger of producing artifacts, e.g. due to an experiment's specific task demands, weaknesses of individual techniques, or gaps in particular data sets. This holds not only for the standard techniques employed by linguists but also for all kinds of psycholinguistic studies. One way around this problem is to look for converging evidence from different sources. Thus, in the same way in which linguists do not, for example, rely on just one test for determining constituent-hood, psycholinguistic findings should only be used as evidence if they are replicable, ideally across different experimental techniques and data sources.

A final consideration is whether a given psycholinguistic finding confirms or disconfirms a *specific* linguistic theory or analysis, or whether it is compatible with different theoretical treatments. Experimental findings may be consistent with a given linguistic analysis, but this by itself is a relatively weak case as the same findings may also be consistent with alternative linguistic analyses. Demonstrating that some

experimental findings favour one analysis over its alternatives represents a much stronger case because in this way psycholinguistic evidence may help to adjudicate between competing linguistic analyses or theories. However, as will be shown below, it is rarely the case that experimental evidence uniquely favours one particular linguistic analysis while at the same time disconfirming all available alternatives.

3.   Sentence processing and the analysis of German clause structure

Sentence processing research addresses the question of how the mental grammar is employed in the production and comprehension of sentence. The most direct way to approach this relationship is to adopt the correspondence hypothesis (originally proposed by Miller & Chomsky 1963), which takes the mental grammar to be directly involved in how we understand and produce sentences in real time. Thus, when producing or comprehending a sentence, the speaker/hearer is said to make use of essentially the same processing units and operations that are used in linguistic analysis. The appeal of the correspondence hypothesis is that it provides a parsimonious and straightforward account of how grammatical knowledge and processing are related in that the parser is said to make basically the same distinctions as the grammar (see Jackendoff 1997, Phillips 1996, among others for discussion). The correspondence hypothesis prevents experimental psycholinguists from positing any specialized parsing and production strategies for sentence processing that have nothing much to do with the units and operations of the mental grammar. Instead, it provides accounts of sentence processing using the normal structures and operations of the grammar. Thus, the correspondence hypothesis is a sensible starting point for employing sentence processing studies as evidence for theories of syntactic representation. At the same time, however, it should be acknowledged that the theory of grammar does not provide a *complete* account of sentence processing. Comprehension difficulties arising in garden-path sentences (*The soldiers marched across the parade ground are a disgrace*) and centre-embeddings (*The pen the author the editor liked used was new*), for example, indicate that sentence comprehension may be affected by additional factors (e.g. by working-memory limitations), and not just by the grammar (but see Weinberg 1999). These factors need to be considered before any experimental finding can be taken as evidence for or against a particular syntactic theory.

The specific case I will discuss here concerns word-order preferences during sentence comprehension and what such preferences might reveal about conflicting syntactic analyses of word-order phenomena; see Farina (2005), Sag & Fodor (1995), Nakano et al. (2002), Featherston et al. (2000) for other cases. For word order, evidence from psycholinguistic experimentation points to a general subject-first preference in on-line sentence comprehension (see e.g. Kaan 1997 for review). That is, the parser seems to find it easier to comprehend sentences in which the subject precedes the object than sentences with the reverse, O–S order. This even holds for languages such as Dutch and German, in which the O–S order is perfectly grammatical. In contrast to the subject-first preference, psycholinguistic studies have not produced any indication that (S)OV sentences are more difficult to comprehend than (S)VO sentences or vice versa. Instead, a series of cross-modal priming experiments have revealed that Japanese and German head-final VPs (Nakano et al. 2002, Clahsen & Featherston 1999) are as optimal in sentence comprehension as English head-initial VPs (Love & Swinney 1996) indicating that order preferences for VO or OV are language-specific. Another question, however, is whether there are any order preferences for finite verbs in sentence processing. Consider, for example, the positioning of finite verbs relative to subjects and objects in German. Finite verbs may occur in the initial, the second, or the final position of a clause, depending on the type of clause: Yes-no questions and imperatives have the finite verb in first position, declarative main clauses in second position, and embedded clauses in final position. This intralanguage difference raises the question of whether the parser shows a preference for any of these different placement patterns in on-line sentence processing.

The analysis of finite verb placement in German is controversial among syntacticians. The best known account is the double-movement analysis illustrated in (1); see e.g. Thiersch (1978), Fanselow (1988), von Stechow & Sternefeld (1988), Grewendorf (1988), Schwartz & Vikner (1996), among others. According to this analysis, Here, verbs first move to a clause-final INFL head (see [e_j] in (1)) and in main clauses subsequently raise to the COMP position of a head-initial CP. In addition, some constituent will raise to Spec-CP in declarative main clauses, i.e. in front of the finite verb. This double-movement analysis ensures that the finite verb will always be in second position in this type of clause. Since the COMP-position is

filled with lexical complementizers such as *dass* 'that' in embedded clauses, the analysis also ensures that the finite verb only raises in main clauses. In this way the double-movement analysis accounts for all possible verb positions.

(1) $[_{CP}$ [Die Aufgabe]$_i$ $[_{C'}$ $[_{COMP}$ hat $_j]$ $[_{IP}$ Pauline mittlerweile $[_{VP}$ [e$_i$] gelöst] $[_{INFL}$ e$_j]]]]$

  'In the meantime, Pauline has solved the task.'

Several syntacticians have pointed out descriptive problems of the double-movement analysis and proposed alternatives. Haider (1993) believes that there is no convincing evidence for a head-final IP projection in German. Travis (1984, 1991) suggests that in German SV$_f$X clauses, finite verbs (V$_f$) are located in a head-initial IP, so that in sentences with preverbal subjects neither the finite verb nor the subject have to undergo any movement to COMP or Spec-CP. Sentences with postverbal subjects, on the other hand, involve leftward movements of the finite verb and other constituents, as illustrated in (1), and in embedded sentences the finite verb is said to be located within VP in clause-final position; see Reis (1985), Kathol (1990) and Zwart (1993) for similar proposals. These accounts are meant to capture the syntactic and interpretative differences between SV$_f$X sentences with the subject directly preceding the finite verb, sentences with derived main-clause word orders such as (1) below, and clause-final finite verb placement in embedded sentences. In contrast to the double-movement analysis, these latter accounts assign a privileged status to SV$_f$X sentences in German, and if this is correct, we may expect to find a corresponding order preference in sentence processing.

To examine order preferences for finite verbs, Weyerts et al. (2002) investigated the on-line comprehension of correct and incorrect word order in main and embedded clauses in German using different experimental paradigms. The critical items for experiments 1 and 2 are illustrated in (2).

(2) a. Main clause with correct SV$_f$O word order
    Es ist Ostern, und die trauernde Witwe *opfert Kerzen*.
    'It is Easter time, and the mourning widow sacrifices candles.'

b. Main clause with incorrect SOV$_f$ word order

   *Es ist Ostern, und die trauernde Witwe *Kerzen opfert*.

   'It is Easter time, and the mourning widow candles sacrifices.'

c. Embedded clause with correct SOV$_f$ word order

   Der Priester sieht, dass der fromme Novize *Kerzen opfert*.

   'The priest sees that the pious novice candles sacrifices.'

d. Embedded clause with incorrect SV$_f$O word order

   *Der Priester sieht, dass der fromme Novize *opfert Kerzen*.

   'The priest sees that the pious novice sacrifices candles.'

Experiment 1 was a self-paced reading task with 26 native speakers of German. Self-paced reading times have been shown to reflect the amount of parsing effort required in sentence processing (e.g. Gibson 1998, among others). Given that ungrammatical sentences require more parsing effort than corresponding grammatical ones, a comparison of reading times for sentences with correct and incorrect verb placement should reveal an effect of ungrammaticality. Thus, reading times for sentences with incorrect verb placement (2b, 2d) should be significantly longer than reading times for sentences with correct verb placement (2a, 2c). In addition, if there is an order preference for either SV$_f$O or SOV$_f$, then the preferred order should produce shorter reading times than the dispreferred one on the assumption that the preferred one requires less parsing effort than the dispreferred one.

Experiments 2 and 3 measured event-related brain potentials (ERPs) during reading. ERPs are minute voltage fluctuations of the electrical activity produced by the neurons in the brain that are recorded from various points on the scalp while the participant is performing some task. ERPs possess time resolution in the millisecond range and thus provide an excellent on-line measure of language processing in real time (see Kutas & Schmitt 2003 for review). Several ERP studies have found two different waveforms, an anterior negativity (also labelled LAN, Left Anterior Negativity) and a syntactic positive shift (also labelled P600) that reflect processes involved in sentence comprehension. The anterior negativity is an early negative-going wave with a frontal distribution (sometimes larger over the left than over the right hemisphere) that occurs as a response to phrase structure violations, agreement violations, and less familiar but grammatically well-formed syntactic structures (see

e.g. Friederici 2002, Felser et al. 2003). The syntactic positive shift is a late positive-going wave with a centro-parietal distribution that occurs in response to violations of phrase structure, subjacency, subject-verb agreement, and temporarily ambiguous (garden-path) sentences (see Osterhout 2004 for review). Given these findings, ungrammaticality caused by word-order violations should elicit a measurable ERP effect, with the ungrammatical sentences in (2b) and (2d) eliciting a larger anterior negativity and/or P600 than the corresponding grammatical sentences. In addition, if there is a word-order preference for either SV_fO or SOV_f, then the dispreferred order should produce a larger anterior negativity and/or P600 than the preferred one.

Note, furthermore, that the materials used for experiments 1 and 2 included lexical verbs such as *opfern* 'sacrifice' as critical verbs. Thus, it could be the case that any order preference obtained in these experiments is due to the verb's lexical-semantic properties, rather than to its finiteness features. For example, if SOV_f sentences are found to be more difficult to parse, this could be because the thematic verb is encountered later in the clause than in SV_fO causing a higher degree of temporal ambiguity. To address this possibility, Weyerts et al. performed an additional ERP experiment with auxiliaries as the critical items:

(3) a. Embedded clauses in correct S–O–V–AUX order

    Der grüne Politiker verspricht, dass der Naturschutz   *den* Wald

    the green politician promises that the nature-conservation the forest

    retten wird.

    save will

    'The green politician promises that nature conservation will save the forest.'

  b. Embedded clauses in incorrect S–AUX–O–V order

    *Der grüne Politiker verspricht, dass der Naturschutz *wird* den Wald retten.

If there is any order preference that is determined by a verbs' finiteness features (rather than by its lexical-semantic properties), then we would expect to find parallel ERP effects in experiments 2 and 3. The results from experiments 1–3 of Weyerts et al. (2002) are summarized in Table 1.

Table 1 Word-order preferences in German sentence processing

Experiment 1	
- SOV$_f$ (incorrect) vs. control	45 ms*
- SV$_f$O (incorrect) vs. control	–4 ms
Experiment 2	
300–500 ms: SOV$_f$ vs. SV$_f$O	Anterior Negativity
700–1000 ms:	
- SOV$_f$ (incorrect) vs. SOV$_f$ (correct)	P600
- SV$_f$O (incorrect) vs. SV$_f$O (correct)	No effect

The asterisk (*) indicates that this difference is statistically significant.

Experiment 1 revealed slower reading times for the critical region (shown in italics in (2) above) in main clauses with the incorrect SOV$_f$ order than the correct SV$_f$O order with a significant difference of 45 ms. In contrast, the comparison of incorrect SV$_f$O in embedded clauses to correct SOV$_f$ yielded a non-significant difference of 4 ms. These results, particularly the finding that in embedded clauses, the SV$_f$O order did not produce longer reading times than the SOV$_f$ one, even though SV$_f$O is ungrammatical in such cases, provides the first indication of an SV$_f$O preference in German sentence processing. Experiment 2 showed that the ERPs to the critical penultimate word of each stimulus sentence (e.g. *opfert* in (2a)) were associated with a significant anterior negativity for SOV$_f$ (compared to SV$_f$O) in the 300–500 ms time-window. Moreover, in the later 700–1000 ms time-window, a large P600 with a centro-parietal maximum was found for the incorrect SOV$_f$ order, i.e. for SOV$_f$ in main clauses, whereas there was no significant effect for incorrect SV$_f$O.

These results are in line with those of experiment 1. The anterior negativity for SOV$_f$ irrespective of grammaticality and the P600 for ungrammatical SOV$_f$ (but not for ungrammatical SV$_f$O) are indicative of a preference for parsing finite verbs in second position, immediately after the subject and before the object. The ERPs to the critical words (shown in italics in (3) above) in experiment 3 revealed an anterior negativity for grammatically well-formed Subject…AUX sentences in the 150–300 ms time-window compared to the incorrect Subject AUX… order. This finding replicates the anterior negativity obtained in experiment 2 and confirms that the SV$_f$O preference is caused by the morpho-syntactic features (finiteness) of verbs rather than

by their lexical-semantic properties. Taken together, these results indicate that sentences with the finite verb in second position and immediately following the subject are easier to parse than sentences with the finite verb in final position, and this preference holds even for embedded clauses for which the grammar of German requires clause-final placement of finite verbs.

We now turn to the question of what these results from sentence-processing studies might mean for the conflicting syntactic analyses of German clause structure mentioned above.

Consider first whether there is any converging evidence from other psycholinguistic studies for an SV_fO order preference. Such evidence is indeed available from studies of German child language and different kinds of language disordered populations. Several acquisition studies have shown that in early stages of the acquisition of German, finite verbs are almost always placed in second or first position, i.e. before objects (see e.g. Clahsen & Penke 1992, Poeppel & Wexler 1993) indicating that the verb-second construction of German is acquired early. It has also been shown that the verb-second construction is typically not affected by developmental language impairments. Most children with Specific Language Impairment (SLI), for example, produce fewer finite verb forms than unimpaired control subjects and make inflectional errors, but the finite verb forms they use are correctly placed in second or first position (Clahsen et al. 1997). Thus, SLI children are capable of discovering the placement properties of finite verbs in German main clauses, despite their impairment in forming correctly inflected finite verbs. Acquired language disorders such as Broca's aphasia show the same picture. German-speaking Broca's aphasics often produce root infinitives, i.e. main clauses in which a finite verb form is replaced by a nonfinite form such as an infinitive or participle, and these nonfinite verb forms are generally placed clause-finally. However, when the aphasics produce finite verb forms, these are (correctly) placed in second or first position (Penke 1998, 2001; Wenzlaff & Clahsen 2005). These findings show that the $(S)V_fO$ order is not only preferred in comprehension but also early in child language acquisition and typically not affected in developmental language disorders or in aphasia.

The next consideration is whether there are any potentially confounding factors that may account for the findings obtained. Schlesewsky et al. (2002) provided

a critique of the Weyerts et al. (2002) study focusing on the results of experiment 2. They argued that the ERP effects seen in this experiment are due to differences between nouns and verbs rather than due to order preferences. Recall that the critical comparison in experiment 2 involved ERP effects on a bare plural noun in the SOV$_f$ conditions (2b, 2c) in comparison to a finite verb form in the SV$_f$O conditions (2a, 2d). Thus, Schlesewsky et al. argued that word order is confounded with word category in this experiment. Note, however, that to determine whether the ERP effects seen in experiment 2 can be attributed to word-category differences, Weyerts et al. performed an additional control experiment in which all the critical nouns and verbs from experiment 2 were tested together with pseudo-words in a simple word list. This experiment yielded a significantly larger N400 effect for verbs than for nouns, i.e. a more negative-going waveform for *verbs*. Experiment 2, however, elicited the opposite pattern, a more negative-going waveform for *nouns* (appearing in the object position of SOV$_f$ sentences). Hence, the anterior negativity in experiment 2 cannot be due to lexical differences between nouns and verbs. Schlesewsky et al. dismissed the results of this control experiment because it tested the critical items in a word list rather than in sentences. Instead, they pointed to an ERP sentence study (Federmeier et al. 2000) in which similar ERP effects to Weyerts et al.'s experiment 2 were seen for nouns versus verbs despite the fact that the sentences they tested were perfectly well-formed in terms of word order. According to Schlesewsky et al., this indicates that experiment 2 taps a word-category difference, and not a word-order difference. Note, however, that Federmeier et al. (2000) did not test a pure word-category difference between nouns and verbs; instead their materials manipulated word category in relation to contextual appropriateness, and as they noted themselves (p. 2564), their results do *not* simply reflect a semantic or lexical difference. Thus, due to different kinds of experimental manipulation, a direct comparison of the results of Federmeier et al. with those of Weyerts et al. does not seem to be appropriate. Another reason for rejecting the idea that the brain response is caused by word-category differences comes form the results of experiment 3 of Weyerts et al., in which the SOV$_f$ order yielded an anterior negativity similar to the one seen in experiment 2 even though the critical words in experiment 3 were determiners and auxiliaries rather than nouns and verbs. Taken together, word-category differences cannot explain the set of experimental findings reported in Weyerts et al. (2002).

Another potentially important factor is frequency, as it might be the case that a particular word order is preferred simply because it is the most common one. To address this possibility, Weyerts et al. (2002) reported frequency counts that were based on spontaneous speech corpora from 45 native speakers of German (Schmid 2002; corpus size: 186,858 words). It was found that of the 16,292 unambiguous sentences 32% had a simple finite verb in second or first position, 26% had a finite verb in final position, and 41% had a finite verb or auxiliary in second or first position and a nonfinite verbal element in final position. These frequency counts indicate that discontinuous verb placement is the most frequent pattern and that overall, verbs are common in both second or first position and/or final position, with similar frequencies. With respect to the placement of *finite* verb forms, however, there is a clearly dominant pattern: 74% of the finite verb forms appear in second or first position, and only 26% in final position. Thus, the possibility that the SV_fO order preference in sentence comprehension is due to frequency cannot be ruled out.

Consider, finally, the possibility that the observed differences between SV_fO and SOV_f are due to different working-memory demands of these two word orders. In on-line comprehension, sentences are comprehended incrementally, and upon encountering a given constituent the parser makes predictions as to what the next constituent might be. The longer a predicted constituent must be kept in memory before the prediction is satisfied, the higher the memory cost (Gibson 1998). Note furthermore that in the grammar of German, the subject is closely related to verb finiteness, as reflected by case marking and subject-verb agreement, and that in both the SOV_f and SV_fO sentences tested by Weyerts et al., the first NP which the parser encounters is the subject NP. It is, therefore, conceivable that once the subject NP is processed, the parser predicts a finite verbal element, i.e. I(nfl) or T(ense), by virtue of the head-dependent relationship between the subject and the finite verb within IP. This prediction is made for all the sentences tested in experiments 1–3, since they all have initial subjects. The important difference between SV_fO and SOV_f sentences is, however, that only in the former are the finite verb and the subject immediately adjacent, which may lead to higher memory costs for SOV_f sentences. In such sentences, a finite verb is predicted once the subject is encountered, and this needs to be retained in working memory while the object is processed. In an SV_fO structure, by contrast, the finite verb is also predicted upon encountering the subject, but it does

not have to be retained in memory until the end of the clause. Thus, SV_fO sentences are likely to consume less memory effort than SOV_f structures, and hence the ERP effects and the longer reading times Weyerts et al. found in their experiments.

Summarizing this section, the case discussed has revealed an order preference in German sentence comprehension. Converging evidence comes from studies of child language acquisition and people with language impairments. On the other hand, the possibility that the SV_fO preference is due to external factors (frequency, working-memory demands) could not be excluded. Consequently, the experimental results do not decide between the conflicting syntactic analyses of German clause structure mentioned above. It should be emphasized, however, that this conclusion applies to the particular case I discussed, and not to results from sentence processing studies in general. There are indeed results from sentence processing studies that have been argued to provide evidence for or against a particular syntactic analysis. To mention a few examples, Nakano et al. (2002) claimed that their experimental results support configurational analyses of Japanese clause structure. Featherston et al. (2000) argued that their ERP results support a movement-based analysis of raising constructions, and Gibson & Warren's (2004) results provide experimental evidence for the notion of intermediate gaps or traces. These cases need to be examined with care using the three criteria mentioned above, and it may very well turn out that some of these findings do indeed provide decisive evidence for a specific syntactic analysis.

4.   Studies of language impairment and the analysis of passives and binding

Research on language impairments investigates pairings of intact and impaired linguistic skills in different kinds of language-disordered populations and specifically asks whether it is possible to explain language impairments in terms of selective deficits within the linguistic system itself. Selective linguistic deficits are also potentially relevant for the theoretical linguist, at least for those who are willing to adopt what Grodzinsky (1990: 111) called the *breakdown-compatibility criterion*, according to which a linguistic theory or analysis is to be preferred if it can account for patterns of impairment and sparing of linguistic ability in a natural, non-ad-hoc way. The strongest evidence we may get from studies of language impairments are so-called *double dissociations*, i.e. cases in which for two phenomena A and B, A is impaired in one population (where B is spared), and B is impaired in another

population (where A is spared). Double dissociations indicate that the two phenomena in question are supported by different mental representations or mechanisms, and this may provide crucial evidence for evaluating competing linguistic accounts (see also Penke & Rosenbach 2004: 501f.).

The specific case I will consider here to illustrate the use of evidence from language impairments concerns two phenomena, passives and anaphoric binding, for which conflicting analyses have been proposed in the theoretical literature. With respect to passives, there are two broad approaches, one based on syntactic movement and the other on lexical derivation. In transformational accounts (Chomsky 1981, 1995), passive participles are claimed to be unable to assign objective case to their internal arguments resulting in *movement* of the direct object to subject position where it can be assigned nominative case. As illustrated in (4), object-to-subject movement leaves behind a phonologically silent copy of the object (= trace) that is coindexed with the moved object and is assigned a thematic role by the verb. The moved direct object and its trace form an A(rgument)-chain in that both elements are in argument positions of the same verb (*locality*), the moved element *c-commands* its trace, and they share the same syntactic features (*chain uniformity*); see Chomsky (1995: 270ff.).

(4)   [[The fish]$_i$ is [[eaten t$_i$]] [by the man]]

In other syntactic frameworks, e.g. Lexical Functional Grammar (LFG, see Bresnan 1982) and Head-driven Phrase Structure Grammar (HPSG, see Pollard & Sag 1994), the derivation of the passive does not involve any syntactic movement. Instead, the passive verb is considered to be *lexically derived* from the active verb by which the thematic role assigned to the direct object is assigned to the subject of the passive; see Blevins (2003) for recent discussion.

The notion of 'binding' refers to sentence-internal referential dependencies between anaphoric elements (including pronouns and reflexives) and their antecedents. The original version of the Binding Theory (Chomsky 1981) consists of three principles: Principle A states that a reflexive pronoun must be bound by a local antecedent within the same clause, Principle B says that a non-reflexive pronoun may not be syntactically bound by a local antecedent, and Principle C says that names and

other referential expressions must not be bound. These principles are specifically stipulated for binding phenomena, which are claimed to be determined at the level of syntactic representations. Alternatively, it has been argued that binding of (non-reflexive) pronouns is based on *semantic* interpretation whereas binding of reflexives should be defined in syntactic terms (Pollard & Sag 1992, Sells 1991, Reinhart & Reuland 1993). Moreover, attempts have been made to derive the locality conditions on binding from independent syntactic principles rather than stipulating them as separate binding principles (Hornstein 2001, Reuland 2001). Adopting Chomsky's (1995) feature checking account, Reuland (2001) shows that the dependency between a reflexive pronoun and its antecedent (e.g. *John believes that Mary$_i$ likes herself$_i$*) forms an A-chain, as both are in argument positions, have the same syntactic features, and the antecedent c-commands the reflexive. This is not the case for non-reflexive pronouns, as syntactic (phi-)features of the local antecedent and the pronoun may differ (e.g. *John$_i$ believes that Mary likes him$_i$*).

To show how findings from language impairments might bear on the controversial nature of passives and binding phenomena, I will report the results from a study (Ring & Clahsen 2005a) investigating these phenomena in people with Down's Syndrome (DS) in comparison to people with Williams Syndrome (WS).

DS is the most common identifiable cause of intellectual disability, accounting for approximately 20% of the mentally handicapped population. DS is caused by an extra copy of a segment of Chromosome 21 that is associated with specific physical features and cognitive delay. Previous studies (see Tager-Flusberg 1999, Ring & Clahsen 2005b for review) have indicated that language abilities are relatively more impaired than other areas of cognition in this population, and that within the language system, morphosyntax is more impaired than other linguistic domains. Several studies have reported asynchronous patterns of linguistic development in DS, e.g. enhanced levels of lexical skill relative to reduced levels of morphosyntax. Finally, there are studies of DS that discovered patterns of morpho-syntactic skill that are qualitatively different from those observed in normally developing children (Fabretti et al. 1997, Perovic 2004). These results suggest the possibility of a selective within-language impairment in people with DS. WS is a rare genetic disorder associated with learning difficulties and relative strength in language. It is caused by a microdeletion on the long arm of chromosome 7 at 7q11.23, which affects

one allele of the elastin gene and other contiguous genes. Within cognitive skills, there is a spatial disorder, for example in drawing. The development of language is also uneven but there is dispute about the actual performance on language tasks and the best theoretical interpretation of this performance (Bartke & Siegmüller 2004).

Ring & Clahsen (2005a) investigated 8 adolescents diagnosed with DS, 10 adolescents with WS, and control groups of children whose chronological ages were closely matched to the mental ages of the impaired participants but who had no known learning impairments. The impaired participants' mental ages were derived from full IQ scores on the Wechsler Intelligence Scale for Children (Wechsler 1992). The purpose of matching the participant groups on mental age was to control for their level of intellectual development.

Anaphoric binding was tested using the sentence-picture judgment task STOP (Syntactic Test of Pronominal Reference, van der Lely & Stollwerck 1997), in which a picture seen by the participants either matched the contents of a yes-no question spoken by the experimenter (requiring a *yes* response) or did not match (requiring a *no* response). For example, the child was presented with a picture of Mowgli and Baloo Bear in which Mowgli was tickling Baloo Bear. An introductory sentence was spoken by the experimenter (*This is Mowgli, this is Baloo Bear*) which was followed by the experimental sentence *Is Mowgli tickling him/himself?*, to which the children had to reply 'yes'/'no' respectively. The comprehension of active and passive sentences was examined using the sentence-picture matching task TAPS (Test of Active and Passive Sentences, van der Lely 1996), in which participants listened to sentences and were required to indicate for each sentence which one of four pictures most closely matched its contents. The sentences contained action verbs and animate arguments. There were four conditions: (i) active transitive (*The man eats the fish*), (ii) full verbal passive (*The fish is eaten by the man*), (iii) short progressive passive (*The fish is being eaten*), (iv) ambiguous (stative or eventive) passive (*The fish is eaten*). The pictures presented for each sentence depicted four different responses: (i) transitive (a man eating a fish), (ii) reversal (a fish eating a man), (iii) adjectival (an eaten fish on a plate), (iv) semantic distracter (the remains of a man).

The results from Ring & Clahsen (2005a) are summarized in Table 2, which shows percentages of correct responses for the different conditions of the two experiments in the three participant groups.

Table 2 Percentages correct in actives/passives experiment and in binding experiment for participants with Down's Syndrome (DS), Williams Syndrome (WS), and unimpaired controls (CTR)

	DS	WS	CTR
Active/passive experiment			
Actives	76.1	90.0	94.4
Passives	54.5	81.7	88.9
Binding experiment			
Reflexives	54.1	92.5	90.6
Non-reflexives	84.6	96.3	92.1

The WS participants performed almost perfectly in both experiments, and there were no statistically significant differences between the accuracy scores of the WS participants and the unimpaired controls in any condition. Moreover, the types of (occasional) error were also similar to those given by the controls. These results indicate that the grammatical mechanisms for correctly interpreting passives and sentences with reflexive and non-reflexive pronouns are not affected in WS, at least not beyond a general developmental delay (see also Clahsen & Almazán 1998). The results for the DS group were clearly different. The DS participants had significantly higher accuracy scores for non-reflexive pronouns than for reflexive ones, whereas for the control children there was no such difference. Between-group comparisons showed that the DS participants performed significantly worse than the unimpaired controls on the reflexive conditions, whereas there were no statistically reliable differences between the DS and the control participants for non-reflexive pronouns. These results indicate that the interpretation of sentences with reflexive pronouns causes particular difficulties for the DS participants. With respect to active and passive sentences, the DS participants' accuracy scores for actives were significantly higher than for passives, and the DS participants gave significantly more reversal responses than the controls, i.e., they incorrectly took the first NP they heard to be the agent argument. Thus, taken together, the DS participants experienced difficulty interpreting passives and sentences with reflexive pronouns, while they performed better in active sentences and in sentences with non-reflexive pronouns. By contrast,

the WS participants appeared to be unimpaired in these domains.

What are the implications of these findings for the conflicting analyses of binding and passivization outlined above? To address this question, consider first potential confounding factors.

One important factor is the low level of general intelligence in DS which could mean that the difficulties with passives and reflexives are the result of a broader non-linguistic impairment. Note, however, that the WS participants achieved high accuracy scores in both experiments, even though they also had low IQs, in the same range as the DS participants, suggesting that the difficulties with passives and reflexive binding in DS cannot straightforwardly be attributed to their (low levels of) general intelligence. Another possibly confounding factor is that language development is delayed in DS and that the patterns seen for the DS participants may represent an early stage of normal acquisition. It is true that grammatical development in the DS participants was indeed severely delayed as revealed by the standardized 'Test of Reception of Grammar' (TROG, Bishop 1982) in which the DS achieved a score equivalent to that of 4;6-year-old unimpaired children. Note, however, that this developmental delay does not account for the specific patterns of impairment in DS for binding and passives. With respect to binding, many studies have shown that typically developing children display adult-like comprehension of sentences with reflexives from about 3 years of age (McKee 1992), while even 4-year-old children incorrectly take a non-reflexive pronoun to be bound by its local antecedent (Thornton & Wexler 1999). This is the opposite pattern of what was seen in DS. Indeed, the contrast found in DS between correct non-reflexive and impaired reflexive pronoun interpretation has not been witnessed before in any study of anaphoric binding with unimpaired children of any age indicating that, at least in this domain, linguistic development in DS is not simply delayed. Likewise, studies of passivization in young children have shown that typically developing children comprehend the kinds of passive sentences Ring & Clahsen tested by at least 3;6–4 years of age (Guasti 2002: 269), which is in contrast to the low level of performance seen for the DS participants. Thus, we can rule out the DS participants' low IQs and their general delay of language development as confounding factors for their specific difficulties with reflexive binding and passives.

Our next concern is whether there is any converging evidence for the findings obtained by Ring & Clahsen (2005a) from other studies of DS. For binding, there is one study (Perovic 2004) testing four young adults with DS (age range: 17;2–20;7 years) in a picture truth-value judgement task similar to the one Ring & Clahsen used. Perovic's participants achieved near-perfect accuracy scores (> 90%) in sentences containing non-reflexive pronouns and much lower scores (< 60%) in sentences with reflexives, a pattern parallel to the one Ring & Clahsen obtained. Bridges & Smith (1984) tested the comprehension of passives by 24 young adults with DS and 24 non-retarded children matched to the DS children and found accuracy scores of over 80% on actives (similarly to controls) and of around 50% on passives, a score that was significantly lower than the ones for 4;6–5-year-old controls. These results provide converging evidence that reflexive binding and passivization are specifically impaired in DS.

Consider, finally, the syntactic accounts of binding and passives described above in the light of the patterns of impairment seen in DS. The results for binding in DS (compared to younger unimpaired children) revealed a double dissociation. In DS, binding of reflexives (but not of non-reflexives) is impaired, in 3–4-year-old normal children the reverse was found, accurate performance on reflexives and difficulties interpreting sentences with non-reflexive pronouns. Double dissociations are an indication that the two phenomena in question are independent and supported by different mental representations or mechanisms. Thus, the results on binding are not in line with standard Binding Theory according to which conditions on reflexives (= Principle A) and conditions on non-reflexive pronouns (= Principle B) are both being determined at the level of syntactic representations. Instead, the results provide evidence for the view that reflexive and non-reflexive binding involve different kinds of representation (see e.g. Pollard & Sag 1992, Sells 1991, Reinhart & Reuland 1993). The second finding was that in DS, impaired reflexive binding coincides with impairments in passives, i.e. low accuracy scores on passive sentences and many incorrect reversal responses. This finding is more compatible with theoretical accounts such as Reuland (2001) that posit the same syntactic mechanism (= A-chains) for both passives and reflexive binding than with theories according to which passivization and reflexive binding do not have much in common, because from the perspective of Reuland's theory, the pattern of impairment in DS can be ascribed to a

common source (= impaired A-chain formation) which affects both passives and reflexive binding.

In summary, the most important finding reported here is the double dissociation between reflexive and non-reflexive binding. It was also found that impairments in reflexive binding were correlated with impairments in passives. Converging evidence from different studies of DS was reported, and a number of potentially confounding factors for this pattern of impairment (low IQs, developmental delay) could be excluded. While these results are suggestive of a specific impairment of A-chain formation in DS, further empirical studies are required to determine whether the impairment generalizes to other phenomena that involve A-chains, e.g. raising constructions (*John seems to be a nice guy*), *to* infinitives (*John is believed to be a nice guy*) and unaccusatives (*The book arrived yesterday*). Moreover, it is possible that the observed pattern of impairment for reflexives and passives is part of a broader deficit extending to A'-dependencies, which needs to be studied through tasks investigating *wh*-constructions and relative clauses.

5. Conclusion

This paper addressed the question of the potential use of psycholinguistic evidence for theoretical linguists focusing on the nature of grammatical representations. I argued that some common ground is required for psycholinguistic findings to bear on linguistic theories and analyses, and I suggested that the search for the most appropriate mental representations for language provides such common ground. I also pointed out that results from psycholinguistic studies need to be examined with care before being used as evidence for grammatical representations. Three criteria were set out to evaluate the potential theoretical implications of psycholinguistic findings. We should ask whether there are any confounding factors or alternative explanations for a given psycholinguistic result, whether there is converging evidence from other sources, and whether a given finding confirms or disconfirms a specific linguistic account. Two case studies were presented in which these criteria were applied. My overall conclusion is that psycholinguistic findings do indeed provide evidence that the theoretical linguist may find useful (along with other sources of evidence) in developing descriptive and theoretical analyses for a given set of phenomena and that

psycholinguistic results may even help to adjudicate between competing theoretical accounts.

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Deconstructing *what with* absolutes

Claudia Felser and David Britain

In this paper we examine the distribution, syntax and pragmatics of a relatively rare and previously neglected type of augmented absolute introduced by *what with*. Although *what with* absolutes initially come across as highly idiosyncratic form–meaning pairings, closer inspection reveals that they do in fact exhibit a high degree of compositionality. We propose that *what with* absolutes are projections of an abstract *Evaluative* head that forms part of the extended C-system, an analysis that is shown to account for both their syntactic and core interpretive properties. Like other 'peripheral' *wh*-constructions such as pseudo-interrogatives – to which they are argued to be related – *what with* absolutes help elucidate the extent to which pragmatic meaning may be represented syntactically.

1. Introduction

Absolutes have not featured very prominently in recent generative-transformational or minimalist research. The likely reason for this is that they seem fairly obvious candidates for being included in the (continuously growing) set of 'peripheral' phenomena rather than falling within the (continuously narrowing) domain of core or 'narrow' syntax. The two standard types of absolute found in Present-Day English (PDE) include unaugmented absolutes such as (1a) and absolutes augmented by *with* (or *without*) such as (1b) (examples from Kortmann 1991).

(1)   a.   **The coach being crowded**, Fred had to stand.

   b.   **With John driving**, we won't have a lot of fun.

Along with idioms and other types of stylistically marked or peripheral structures, absolutes have been claimed to be 'constructions' representing arbitrary form–meaning pairings (Riehemann & Bender 1999). The particular subtype of absolute that the present paper deals with, comparatively rare and pragmatically restricted absolutes augmented by *what with*, would appear to lend themselves even more readily to this kind of treatment.

   Absolutes introduced by *what with* are tenseless free adjuncts functioning as adverbial sentence modifiers. They can contain V-*ing* participles with accusative or

genitive subjects, V-*en* participial clauses, non-verbal small clauses, or subjectless V-*ing* constituents, as illustrated by the examples in (2).[1]

(2) a. "Thoughtful too," said Wexford, "**what with everyone in Kingsmarkham being bilingual**." A73(1465)

  b. "And **what with his being half asleep**, too, really I don't know what sort of a signature he'll be able to make." (Hardy, *Life's Little Ironies*)

  c. **What with Mrs Clements and the girls also gone for the week**, I suppose I was very conscious of the fact that once I departed, Darlington Hall would stand empty for probably the first time this century… AR3(208)

  d. We might be able to make a bob or two between us there mate, **what with the old man on the pilot boat as well**. B3J(2964)

  e. It certainly was a good day today **what with climbing the mountain and having my tea cooked for me**. GXM(183)

Traditionally, a distinction is often made between absolutes, which contain an overt subject, and subjectless free adjuncts (compare e.g. Kortmann 1991, Stump 1985). As this terminological distinction will be largely irrelevant to the following discussion, we shall use the term '*what with* absolute' (WWA) for all cases shown in (2), including those that lack a lexical subject.

 What with is also used to introduce reason adjuncts containing (often conjoined) noun phrases, as in (3a,b) or derived nominals, as in (3c).

(3) a. **What with her neat black suit, white blouse, rimless spectacles and greying hair,** Ella Shields looked more like a school teacher than a vaudeville and music hall celebrity, toast of two continents. B11(1619)

  b. It's becoming increasingly clear that Class War's gone soft, **what with the film and the book**. CAF(590)

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[1] Unless otherwise indicated, all examples quoted are taken from *The British National Corpus*, version 2 (BNC World). 2001. Distributed by Oxford University Computing Services on behalf of the BNC Consortium. URL: http://www.natcorp.ox.ac.uk/. All rights in the texts cited are reserved.

> c.   I was impressed at the geselligheid, **what with their singing of the (then) latest hits**.   (from the Internet)

Apart from lacking a predicate, this type of adjunct shares the distribution and semantic properties of *what with* absolutes such as those in (2a–e). For these and other reasons to be outlined in section 3 below, we will subsume both types under the label WWA.

Regarding their semantic relation to the superordinate clause, WWAs, according to the *Oxford English Dictionary* (*OED*), typically function as 'reason' adjuncts implying something along the lines of 'in consequence of', 'on account of', 'as a result of', 'in view of', or 'considering'. Kortmann (1991: 202) further notes that the use of WWAs is more restricted than the use of ordinary *with*-augmented absolutes in that the former are mainly found in colloquial speech and "are only appropriate if the matrix proposition denotes some non-event or negative state, or, more generally, some proposition which has certain negative implications (at least from the point of view of the speaker)". The view is echoed by the *Cambridge Advanced Learner's Dictionary* (2003: 1450), which claims that *what with* is used "to talk about the reasons for a particular situation, especially a bad or difficult situation". This pragmatic restriction does not necessarily apply, however, as can be seen from examples such as (2d) and (2e) above. We will attempt later to provide a unified analysis which can account for tokens with both negative and positive 'implications'.

Because of their comparative rarity, their pragmatic restrictions and their tendency to occur with coordination, it is tempting to regard WWAs as some kind of constructional idiom, as suggested by the following quote from *The Cambridge Grammar of the English Language* (Huddleston & Pullum et al. 2002: 626 n.10):

> One idiom that does not belong with any of the structural types considered above is *what with,* used to introduce a reason adjunct, as in [*What with all the overtime at the office and having to look after his mother at home,*] *he'd had no time for himself for weeks.* This idiom has developed out of an otherwise almost obsolete use of *what* to introduce lists or coordinations, especially of PPs – and indeed *what with* is characteristically followed by a coordination, as in the example given.

As Huddleston & Pullum et al. note, WWAs have developed from the use of *what* as a conjunction or adverb introducing two or more conjuncts. As least as far back as

since the Middle English Period, *what* has been used, according to the *OED*, in the now obsolete sense of 'some...others', 'both...and', 'as well...as', or 'partly...partly', introducing conjoined prepositional phrases as in (4), or to introduce prepositional 'reason' adjuncts as in (5) (all quotations taken from the *OED*).

(4)   a.   **1393** G<small>OWER</small> *Conf.* III. 377 (MS. Harl. 3490) And may my selven nought bewelde, **What for sikenesse and what for elde**.

          b.   **1531** T<small>INDALE</small> *Prol. Jonas* Wks. (1573) 28/2 All the noble bloud was slayne vp, and halfe the commons thereto, **what in Fraunce, and what with their owne sword**, in fightyng among them selues for the crowne.

          c.   **1819** S<small>COTT</small> *Ivanhoe* xxvi, I conceive they may be – **what of yeomen – what of commons**, at least five hundred men.

(5)   a.   *c***1386** C<small>HAUCER</small> *Sqr.'s T*. 46 The foweles..**What for the seson and the yonge grene** Ful loude songen hire affeccions.

          b.   **1476** S<small>IR</small> J. P<small>ASTON</small> in *Paston Lett.* No. 775 III. 161, I ame somewhatt crased, **what with the see and what wythe thys dyet heer**.

          c.   **1603** S<small>HAKES.</small> *Meas. for M*. I. ii. 83 **What with the war; what with the sweat, what with the gallowes, and what with pouerty**, I am Custom-shrunke.

In PDE, reason adjuncts of this type almost exclusively involve the preposition *with*, as in the examples shown in (3) above. Presumably the usage of *what* as introducing prepositional reason adjuncts was only later extended to absolutes, which were comparatively rare until the Early Modern English period (compare Rio-Rey 2002).

In the following, we will take a closer look at the distribution and linguistic properties of present-day WWAs, which have received little or no attention in previous research on absolutes or related phenomena. Our primary goals are to show that despite their distinctly idiomatic flavour, reason adjuncts or absolutes introduced by *what with* do by no means defy conventional linguistic analysis, and that they can help inform the current debate concerning the extent to which pragmatic meaning may be grammaticalised.

2. The properties and distribution of WWAs in Present-Day English

2.1 The corpus

In order to examine the scope of WWAs in English, both written and spoken, formal and informal, and to examine whether different types of WWA were distributed evenly or not across these different channels and formalities, we developed a small corpus of tokens. Firstly, we extracted all examples of WWAs from the British National Corpus (BNC), a 100-million word bank of written and spoken British English. Around 89.5% of the corpus is from written sources, and of the remainder just under half is of informal conversation, the rest being recordings of meetings, lectures, TV broadcasts, medical consultations, etc. (Burnard 2007). This enabled us to compare written texts, spoken conversation and other, less informal spoken data. In all, 313 tokens were found in the BNC. To supplement this, we collected 300 further examples from the Internet, using British, Australasian and American search engines. The first 100 relevant examples of WWAs were extracted from each search engine – most tokens came from blogs and discussion groups but a wide range of web material was represented. This enabled us to contrast the BNC material with web language, which bears qualities of both written and spoken language. Table 1 below, first of all, shows the numbers of WWAs in the written, conversation and other spoken parts of the corpus.

Table 1 Distribution of WWAs in the British National Corpus

	Number of WWAs	Number of words in BNC in each category	WWAs per million words
Written	283	87, 953, 932	3.22
Conversation	22	4, 233, 955	5.20
Other spoken	8	6, 175, 896	1.30

WWAs are more likely to occur in conversational data than in written, and this predominance for informal contexts is further highlighted by the fact that several of the WWAs in written data are found in representations of speech in novels. Because of their very low number, and their very different behaviour from informal

conversation, tokens from the 'other spoken' category will be excluded from the remaining analysis.

If we first examine the distribution of the different types of WWA in the corpora we analysed – see Table 2 below – it is apparent that the most frequently occurring types of WWA across all of the datasets are those without predicates, as in (3) above. Overall, these accounted for over 70% of the examples in our data. A further 20% of tokens were of the ACC-*ing* type, as in (2a) above. These two types account for more than 9 out of every 10 WWAs. In our main corpus, we did not find any examples of POSS-*ing* WWAs – although we did find a few examples from other literary or Internet sources.[2] There is, furthermore, a remarkable similarity in the distribution of these different types across the three data sources, conversation and writing, both from the BNC, and examples from the Internet.

2.2 WWAs and coordination

Note that many present-day WWAs still involve coordination, including the insertion of 'dummy' conjuncts such as *and all*, *and everything* (6a,b) – down to the use of the stereotypical expression *what with one thing and another* as in (6c) below, of which the BNC contains a total of 16 instances.[3]

(6)   a.   Don't think I'm hurrying you but we're rather short-staffed **what with Christmas and everything**. CKB(2516)

        b.   Personally I would advise you to give them your blessing, **what with the baby and all**. CR6(1041)

        c.   Gradually she wheedled her way into the kitchen and began to learn the art of French cooking from Alain's mother, and **what with one thing and another** she hardly noticed the days pass by. HGD(3154)

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[2]   Both bare infinitives and infinitival clauses headed by *to* seem to be excluded from WWAs. While predicateless WWAs may contain infinitival postnominal modifiers as in (i) below, we have not come across a single instance of WWAs containing unambiguously clausal infinitival complements of *with*, either in the BNC or on the Internet.

    (i)   But what with Rose to think of and George's drinking we just stayed the way we were. HD7(1914)

[3]   For a discussion of the variation in form and function of these dummy conjuncts, see Cheshire (2007).

Table 2 Distribution of different types of WWA across conversational, Internet and written data

	Conversation (BNC)		Internet		Written (BNC)	
	Nº of examples	Proportion of total WWAs in conversation BNC	Nº of examples	Proportion of total WWAs in Internet corpus	Nº of examples	Proportion of total WWAs in written BNC
ACC-*ing*	5	23%	65	22%	51	18%
V-*en*	0	0%	5	2%	3	1%
Subjectless -*ing*	2	9%	28	9%	18	6%
Predicateless	15	68%	202	67%	211	75%

An analysis of the distribution of coordination in the WWAs in the corpus revealed some interesting differences between WWAs with predicates on the one hand and those without on the other. Table 3 shows the distribution of coordination in WWAs with predicates and Table 4 in WWAs without predicates. Given that language type (conversation, Internet, writing) does not seem to significantly affect the distribution of coordination patterns, the three types are combined, and predicates and non-predicates are contrasted in Figure 1.

Table 3 shows that a majority of the WWAs that do have predicates do *not* demonstrate a coordination of clauses. Table 4, on the other hand, highlights the preference for coordination in predicateless WWAs, particularly in the more formal, written styles. Conversational data nevertheless, in all types of WWA, prefers to avoid coordination, perhaps re-emphasising the relative tendency for conversation to avoid 'heavy' clauses in general. However, despite trawling through one of the largest existing corpora of spoken English conversation, we have too few examples to generalise with a greater degree of confidence.

2.3 Interpretive restrictions

Stump (1985) draws a basic semantic distinction between 'strong' and 'weak' absolutes, with only the latter able to function like conditional clauses restricting a modal or other binary operator in the matrix clause. The 'weak' and 'strong' readings of absolutes are illustrated by the examples in (7a) and (7b), respectively.

(7)   a.   **With her children asleep**, Mary might watch TV.
            ("If her children are asleep...")
       b.   **With her children asleep**, Mary watched TV.
            ("While/because her children were asleep...")

Stump notes that for an absolute to receive a 'weak' or conditional reading it must be derived from a stage-level predicate (in Carlson's (1980) sense) and must be augmented by *with*.[4] Absolutes that contain an individual-level predicate such as *being a doctor* in (8) below are always 'strong'.

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[4]   However, Kortmann (1991: 199ff.) claims that *with*-augmentation is not in fact essential.

Table 3 Coordination in WWAs with predicates

	Conversation (BNC)		Internet		Written (BNC)	
	Nº of examples	Proportion of total WWAs in conversation BNC	Nº of examples	Proportion of total WWAs in Internet corpus	Nº of examples	Proportion of total WWAs in written BNC
No coordination	4	—	58	59%	36	50%
Two coordinated phrases	3	—	34	35%	30	42%
Three coordinated phrases	0	—	4	4%	6	8%
Four or more coordinated phrases	0	—	2	2%	0	0%

Table 4 Coordination in WWAs without predicates

	Conversation (BNC)		Internet		Written (BNC)	
	Nº of examples	Proportion of total WWAs in conversation BNC	Nº of examples	Proportion of total WWAs in Internet corpus	Nº of examples	Proportion of total WWAs in written BNC
No coordination	8	53%	65	32%	46	22%
Two coordinated phrases	4	18%	77	38%	119	56%
Three coordinated phrases	2	13%	44	22%	35	17%
Four or more coordinated phrases	1	6%	16	8%	11	5%

FIGURE 1 Coordination in WWAs: Number of elements in WWA in tokens with and without a predicate
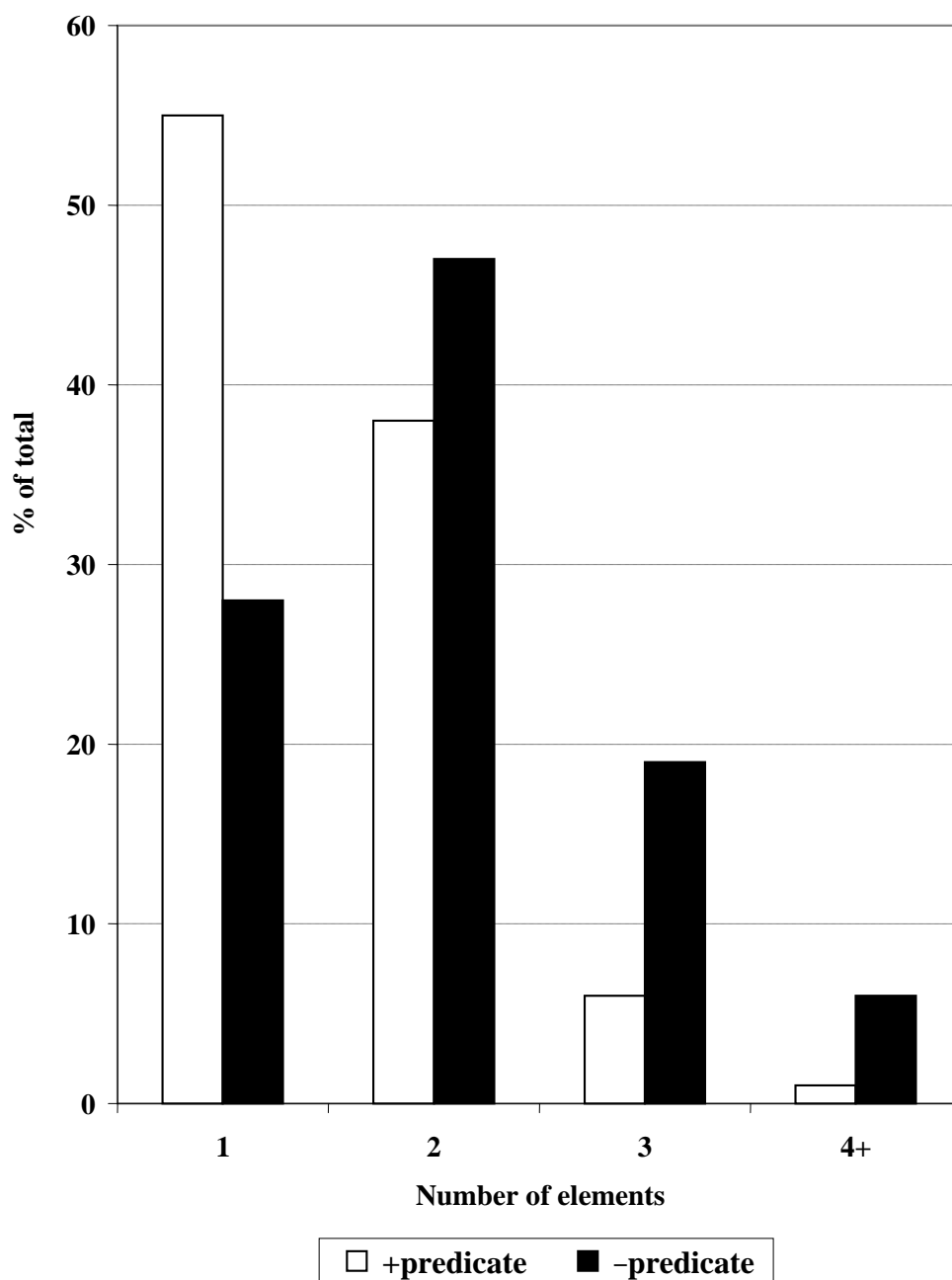
(8) (**With**) **his mother being a doctor**, John would know his way to the Med
      Center. (= "Because his mother is a doctor…")

                                                          (adapted from Stump 1985: 272f.)

Unlike *with*-absolutes such as those in (7) above, however, even WWAs containing
stage-level predicates can only ever have a 'strong' reading, with the truth of the
adjunct clause being entailed by the truth of the matrix clause. That is, WWAs
cannot be interpreted as a conditional clause restricting a modal or other operator in
the superordinate clause, as the examples in (9) below illustrate.

(9) a.   We might be able to make a bob or two between us there mate, **what
           with the old man on the pilot boat as well**.   [= (2d)]
           (≠ "…if the old man is on the pilot boat as well.")

     b.   **What with mother being sick and Ellen on holiday**, I don't know
           how to keep the children under control.   (Kortmann 1991: 203)
           (≠ "…if mother is sick and Ellen on holiday…")

In other words, the presence of *what* restricts an absolute's interpretation in that it
renders it factive. Note that our earlier example (2d), for instance, becomes
ambiguous between a 'strong' (10a) and a 'weak' (10b) reading if *what* is omitted.

(10) We might be able to make a bob or two between us there mate, **with the old
      man on the pilot boat as well**.
      a.   "...because the old man is on the pilot boat as well."
      b.   "...if the old man is on the pilot boat as well."

According to Katz (1993: 130f.), 'strong' adjuncts themselves fall into two subtypes,
'strong-conjunctive' and 'strong-presuppositional', illustrated by the paraphrases in
(11a) and (11b), respectively.

(11)   **With her children asleep**, Mary watched TV.

      a.   "Mary's children were asleep and she watched TV."

      b.   "Because her children were asleep, Mary watched TV."

Unlike *with*-absolutes, WWAs appear to be restricted to the 'strong-presuppositional' reading, that is, they function essentially like *because* clauses.

As noted earlier, WWAs are also often reported as being associated with some negative state or implication, as in the examples in (12) below (from the Internet):

(12) a.   When Stephen came to see us, Alan was near breaking-point, **what with my drinking and the debts and everything**.

      b.   I just couldn't take the rapping squirrel seriously, **what with that British accent and all**.

      c.   But **what with the myriad of carriages thumping and clanging about as they passed, pickpockets and goodness knows what else on the loose**, she couldn't just leave him there.

Table 5 below shows the numbers of tokens in our corpus which were interpreted as being pragmatically 'negative' and those read as pragmatically 'positive'. Just over 80% of tokens were interpreted as being pragmatically negative, supporting Kortmann's (and others') claims.

However, there remains a not insignificant number of tokens with positive readings, such as the examples in (13) below (also from the Internet):

(13) a.   **What with weblogs, online discussions, websites and other more traditional forms of publishing such as online journals**, Australian culture is well represented online.

      b.   As the second closest Alpine resort to Melbourne, Mt Buller is a great place to go for a day trip, **what with brilliant facilities and only 3.5 hours from the city**.

      c.   A quick listen to the CD, **what with Ives' deep and meaningful lyrics, the understated accompaniment and his wonderful voice**, left me with shades of Counting Crows, REM and the Toothfairies.

d. **What with water wheels, worms and waste paper shredding**,
Golspie is bursting with green ideas.

A dominant function of WWAs, therefore, appears to be as an account of a claim made in the matrix clause, whether 'good' or 'bad', with the higher frequency of negative assessments following from the fact that, particularly in spoken interaction, they routinely demand justification more than positive ones. The idea that WWAs function to account for a claim is nicely exemplified by a number of tokens which justify counter-assessments, such as those in (14) and (15) below. Note that in (14) the WWA supports a positive assessment, while in (15) it is negative.

(14) a. While this might not be as cool as it would have been a few years ago, **what with the Wii Virtual Console and emulation all the rage these days**, I still think I would have done pretty much anything to have one of these things in the early 90s.

b. I am not, it must be said, noted for my desire for self-publicity, nor indeed any particular *stroppy princess* streak of behaviour. But I thought, **what with the current Tranniesphere expansion**, I thought I'd get this one in early...

c. I know this blog is popular **what with all the visits from various people and employees of big major companies visiting on their company Internets**. But the thing that is starting to nark me off, is the fact that I am not getting any recognition for doing this blog.

d. **What with the enormous amounts of high quality clones on the market**, more and more players are turning to these to increase their arsenal, but what happens if you want trades?

e. Okay, I would be paying quite a lot per month but in the same note I will be saving lots on my BT landline phone bill **what with the free weekend calls included that includes calls to mobiles**.

Table 5 Distribution of WWAs with a 'negative' and 'positive' interpretation
(Totals do not add up to 100% because, for a very small number of tokens, it was not possible to satisfactorily assign positive or negative readings)

	Conversation (BNC)		Internet		Written (BNC)	
	Nº of examples	Proportion of total WWAs in conversation BNC	Nº of examples	Proportion of total WWAs in Internet corpus	Nº of examples	Proportion of total WWAs in written BNC
Negative interpretation	20	91%	223	74%	232	82%
Positive interpretation	1	5%	66	22%	45	16%

(15) a. Of course, although it provides the emotional center of the fight, you know, **what with the massacre being the point of the movie**, it can't end well.

      b. Being me, I thought about going to the doctor, but **what with one thing and another (twins' birthday, house guests, etc)**, I didn't.

      c. I suppose I could go to the clinic, but **what with the traffic the way it is**, and it always takes so long, etc.

So rather than providing justification for negative statements, WWAs provide one frame (among many, of course) for the justification of accountable assessments in general. The *Longman Dictionary of Contemporary English* (1995: 1629) manages to capture this observation reasonably well when it states that *what with* is "used to introduce a list of reasons that have made something happen or have made someone feel a particular way".

2.4 WWAs as 'constructions'?

Absolutes are among the growing number of structures that have been claimed to be 'constructions' in the sense of arbitrary form–meaning pairings (Riehemann & Bender 1999). We saw above that unlike ordinary absolutes, WWAs not only have a penchant for coordination but are also pragmatically more constrained in that they are necessarily factive, and restricted to a 'reason' or 'account' interpretation. Together with the observation that they also imply a lack of neutrality on the part of the speaker, or speaker evaluation (compare e.g. Kortmann 1991: 202), WWAs come across as a highly idiosyncratic subtype of absolute indeed.

As Kay & Fillmore (1999: 4) point out, postulating an independent construction needs to be justified by showing that (i) "there are specific interpretations associated by convention with just such sentences [...] that are neither given by ordinary compositional processes nor derived from a literal meaning by processes of conversational reasoning", and (ii) sentences carrying such interpretations share certain formal properties. As regards (ii), the formal properties shared by WWAs include the presence of *what with* and the absence of any overt tense marker in its coda. With constructions, or constructional idioms, being conceived of as word, phrase or sentence-level templates with one or more slots unfilled, WWAs would thus seem to conform to the general (simplified) template

[*what with* + XP], where XP can be of either of the types shown in (2) and (3) above.[5]

The remainder of this paper will be devoted to showing that WWAs are structurally more uniform than they might seem and to calling into question the applicability of criterion (i), the lack of compositionality.

3. The internal structure of WWAs

3.1 Overtly clausal WWAs

WWAs containing a noun phrase plus a verbal or non-verbal predicate appear to be further augmented variants of absolutes augmented by *with*, which have previously been analysed as clausal constituents by McCawley (1983), Reuland (1983) and Hantson (1992), among others. Let us first consider WWAs containing ACC-*ing* gerunds. Evidence that *what with* does indeed introduce clausal V-*ing* constituents includes the facts that like *with* absolutes, WWAs allow passivisation (16a) and quantifier float (16b), and admit expletive and pleonastic subjects (16c,d), as well as the fact that the entire subject–predicate unit can be in the scope of clausal negation or negative adverbs (16e,f).

(16) a. **What with health budgets being pruned and cut back** I'm asking the health board if staff shortages perhaps were a contributory factor here. K5D(1272)

b. [...] I said look I apologize for I'd completely forgot that you were coming to collect he said I know I can see that, he hadn't really **what with the kids all running around**… KC8(1008)

c. Admittedly, ER is slightly handicapped in the plot area, **what with there being only a few medical afflictions that can play on prime time**.   (from the Internet)

d. All of a sudden it seemed to be the wrong time and the wrong way to go about it, **what with it being the day of the old man's funeral** and everything… HWP(1667)

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[5]   Using HPSG-type attribute-value matrices should allow for a proper formalisation of the descriptive properties of WWAs, along the lines suggested by Riehemann & Bender (1999: 484) for *with* absolutes.

e.   Anyway, **what with you not being around** and that, I felt constricted to give them a crack of the whip. BMR(1296)

f.   So you see, **what with the Church never keeping adequate records** and relying on the personal network all the time […], we're all rather in the dark. HA2(2308)

ACC-*ing* gerunds have recently been analysed, *inter alia*, as nominalised inflection phrases (Abney 1987), verbal 'small clauses' (Harley & Noyer 1998) or tense phrases (Pires 1999). Leaving aside, for the moment, the question of the syntactic status and function of *what*, and given the arguments presented by Hantson (1992) in favour of analysing *with* in standard *with*-augmented absolutes as a prepositional complementiser, let us assume that the V-*ing* clauses in (16) are non-finite tense phrase (TP) complements of the prepositional complementiser *with*. The WWA in (16b), for instance, then has the internal structure shown in (17), with the floating quantifier *all* stranded in the subject's VP-internal base position.[6]

(17)   what [$_C$ with] [$_{TP}$ the kids [$_{T'}$ $\varnothing$ [$_{VP}$ *all* [$_{V'}$ running around]]]]

In addition to ACC-*ing* gerunds, *what with* also licences V-*ing* predicates with genitive subjects (also known as POSS-*ing* gerunds) as well as subjectless V-*ing* predicates. Although more 'nominal' in character than ACC-*ing* gerunds (compare e.g. Abney 1987, Wasow & Roeper 1972), POSS-*ing* WWAs share with the former a number of clausal properties including the possibility of quantifier float, as shown by the examples in (18) (from the Internet).[7]

(18)   a.   The comparison with Major isn't bad when it comes to Hutton, **what with their both** coming across as bloodless (and dreary) technocrats.

b.   The black-white design of the site is simple and pleasant with nice headings, but the links are somewhat confusing, **what with their being all squeezed together and obscurely titled**.

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[6]   The distinction between V and 'small' *v* (Chomsky 1995, and later) is irrelevant to the present discussion.

[7]   See Williams (1975) for further arguments in favour of analysing POSS-*ing* gerunds as clausal rather than nominal constituents.

WWAs containing a noun phrase plus a V-*en* participle or non-verbal predicate also function like clausal (i.e., subject–predicate) units semantically, and pattern with DP-*ing* WWAs in that they permit quantifier float, as in (19a), and can support clausal negation, as in (19b) (both from the Internet).

(19) a. It was a lucky time for a call, **what with the girls all out** and just an old dour lady like me left.

b. "Yeah, Thomas said it was pretty ironic, **what with her not even able to be in the same room with a tea cup poodle**."

Let us assume, then, that the internal structure of both POSS-*ing* and non-verbal 'small clause' complements of *what with* is essentially the same as those of ACC-*ing* complements, except that small clauses lack an overt verb or auxiliary. That is, example (19a) above has the structure shown in (20).[8]

(20) what [$_C$ with] [$_{TP}$ the girls [$_{T'}$ ∅ [$_{VP}$ *all* [$_{V'}$ ∅ [$_P$ out]]]]]

In the remainder of this section, we will consider two types of WWA whose clausal status is rather less obvious.

3.2 Subjectless WWAs

While the presence of *what* may be optional in WWAs containing a lexical subject, its presence is required in subjectless absolutes, as illustrated in (21).

(21) a. It certainly was a good day today *(**what**) **with climbing the mountain and having my tea cooked for me**.   [cf. (2e)]

b. *(**What**) **with being so uncoordinated and all**, I haven't decided exactly how I'm going to increase my physical activity…

(from the Internet)

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8   Alternatively, non-verbal small clauses may involve an abstract 'predicate' head in the sense of Bowers (1993).

The fact that subjectless WWAs can also contain passives, as shown in (22), and support clausal negation, as illustrated by the examples in (23), suggests that they too form clausal constituents.

(22)   a.   KIWI referee David Bishop has had an exciting year, **what with being 'congratulated' in the Parc des Princes tunnel by Daniel Dubroca**… CHW(206)

          b.   The soldiers' nerves are probably stretched a bit taut, **what with being shot at and exploded at and stuff**...   (from the Internet)

(23)   a.   [...] my brain was a bit dozy **what with not having been in school for close on 3 months.**

          b.   I'm pretty sure I managed to alienate people nicely over the past weekend, **what with not returning phone calls or going out**…

                                            (both from the Internet)

Following generative-transformational tradition, we will assume that WWAs lacking an overt subject do in fact contain a null pronominal subject, which, as indicated in (25) below, can serve as the required local antecedent for reflexive pronouns as in (24a,b) (from the Internet).

(24)   a.   **What with holding myself out as an expert on Magic and so forth**, I find for some reason that people are often writing to me for advice.

          b.   But I barely have time to help with anything Internet wise, **what with drowning myself in gameplay and moderating a chat**.

(25)   a.   what with [$_{TP}$ **PRO$_i$** holding **myself$_i$** out ...]

          b.   what with [$_{TP}$ **PRO$_i$** drowning **myself$_i$** ...]

A question that immediately arises here, though, is why null subjects should be possible in WWAs while being excluded from standard *with*-augmented absolutes. According to Hantson (1992: 86f.), given that PRO is normally barred from occurring in case-marked positions, the assumption that the prepositional

complementiser *with* assigns objective case to the absolute's subject accounts for the ill-formedness of examples like (26).

(26)　　***With driving slowly**, we won't have a lot of fun.

On closer inspection, however, it turns out that it is *with*-augmented absolutes rather than WWAs that behave oddly with respect to the availability of null subjects. Note that null subjects are perfectly possible, for example, in 'negative' absolutes augmented by *without*, as witnessed by (27a,b) below (from the BNC).

(27)　a.　　Ohly, **without being absolutely sure**, thought that US foreign policy was wrong and was at least in need of urgent re-examination. EFA(806)

　　　b.　　**Without calling for one's whole attention**, it so persistently demands a small part of it that concentration on anything else is ruled out. EBR(1343)

The same is true for *with* absolutes modified by focus particles such as *even* or by an adverb, as shown by the examples in (28) (from the Internet).

(28)　a.　　I cropped it down because **even with reducing the pixels count** I couldn't get it small enough for upload.

　　　b.　　I didn't want to be typecast, **especially with being** a native Yorkshire girl.

Thus, it appears that null subjects are possible in augmented absolutes that are introduced by a conjunction or complementiser other than *with*, or whenever some kind of additional augmentation is present. The real question, then, is why null subjects should be disallowed in standard *with* absolutes given that they are licensed in other types of absolute – as well as in V-*ing* complements of prepositional *with*, as in (29) below (from the Internet).

(29)　　It's ok to be secretly happy **with eating banana boats and fried twinkies**.

One possibility is that covert subjects are excluded from *with*-augmented absolutes on prosodic grounds, given that their subjects tend to attract focal stress, but for lack of space and because the current study focuses on WWAs, we will not pursue this matter any further here.

The alternation of lexical and null subjects is not, of course, unique to WWAs but is also seen, for example, in clausal gerunds serving as complements of verbs like *remember*:

(30)   I remember {**him** / **his** / **PRO**} cutting down the tree.

Previous proposals to account for the above alternation include those assuming that PRO-*ing* and DP-*ing* gerunds differ with respect to their transparency for case assignment by an external governor (e.g. Johnson 1988) and those which assume that the subject of gerunds is case-marked internally (e.g. Reuland 1983). Again, space limitations prevent us from discussing the merits and drawbacks of these proposals in any detail here. We merely note that adopting a more recent claim by Pires (1999) to the effect that the subject of PRO-*ing* gerunds is actually 'small' *pro* – which occurs in case-marked positions – would render PRO-*ing*/DP-*ing* alternations far less problematic than they have traditionally been thought to be, no matter whether case is taken to be assigned to the subject directly by *with* (or verbs such as *remember*), or by some functional head within the gerund phrase.

3.3 'Predicateless' WWAs

Let us finally consider WWAs containing a nominal (i.e., DP) coda. Noting that *with* + DP absolutes such as (31a,b) below (from McCawley 1983) have an understood possessional *have* or existential *there be* interpretation, as illustrated by the paraphrases in (32), McCawley argues that this type of absolute also contains clausal constituents.

(31)   a.   **With job offers from three major universities**, Ann is feeling great.
        b.   **With this bad weather**, we had better stay home.

(32)   a.   With *her having* job offers from three universities...
        b.   With *there being* this bad weather...

Syntactic evidence in favour of a clausal analysis of *with* + DP absolutes includes the possibility of conjoining a DP complement of *with* with an unequivocally clausal constituent, the possibility of adverbial modification, and the fact that the absolute rather than the matrix clause serves as the scope for negation. All of these diagnostics test positive for WWAs as well, as shown by the examples in (33)–(35) below.[9]

(33) a. It must be such a difficult decision to make, **what with the little one, and Ben just about to start school**… ASD(2557)

 b. **"What with all this, and Ken still lying in that bed –"** ; She broke off and Tina intervened. BPD(1395)

 c. We shouted but **what with the noise of the water and not knowing their language** of course it wasn't any use and then we were in the water... G1X(1496)

(34) a. **What with my wedding as well in October**, it is really turning into some exciting year, isn't it? CH7(535)

 b. And **what with her heart and now her leg**, she felt fully excused from all effort in that area. HA2(2439)

 c. **What with the heat, the fiddly bit and then him**, I was ready to blow my top. CGU(164)

(35) a. I'm pretty bored though, **what with nothing to do but sleep all day**.

 b. I thought this game would turn out pretty crap, **what with no combat**.

(both from the Internet)

Observe also that unlike negative PPs as in (36a), but like negative *with* + DP absolutes such as (36b) (examples from Liberman 1975, cited by McCawley 1983: 278), negative WWAs as in (36c) fail to trigger auxiliary inversion.

(36) a. **With no job** would Sam be happy.

 b. **With no job**, Sam is happy.

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9   The coordination argument on its own, of course, is not very strong as coordination of unlike constituents is not particularly uncommon (compare e.g. Sag et al. 1985).

c.   […] **what with no tenants and no furniture** you may have done this to save some money…   (from the Internet)

In the light of the above arguments in favour of a clausal analysis of (*what*) *with* + DP absolutes, and given that they share the semantic and pragmatic properties of overtly clausal absolutes, let us assume that the DP in this kind of absolute is the complement of a null verbal head, as indicated in (37) (but see footnote 8 above).

(37)   what [$_C$ with] [$_{TP}$ *pro* [$_{T'}$ ∅ [$_{VP}$ [$_{v'}$ ∅ [$_{DP}$ the little one]]]]]

In summary, we have shown that WWAs admit a range of non-finite clausal constituents including ACC-*ing*, PRO-*ing* and POSS-*ing* gerunds, V-*en* clauses, as well as adjectival, prepositional and nominal small clauses. We argued that, diverse as they may seem, the various types of WWA all share the same basic structure, with the prepositional complementiser *with* selecting a non-finite TP complement. Next, we will examine the syntactic status and function of *what*, whose presence seems to give rise to the interpretive restrictions noted earlier.

4.   What about *what*?

4.1 Preliminary observations

Given our assumption that *with* in standard augmented absolutes is a prepositional complementiser located in C, it would appear that *what* must be one of the following: (i) part of a single (grammaticalised) prepositional complementiser *what with* heading the absolute, (ii) a derived *wh*-operator originating within the complement domain of *with*, or (iii) a base-generated *wh*-operator.

Evidence that option (i) cannot be correct is fairly easy to come by. The following examples (all from the Internet) show that *what* and *with* can be separated by an adverb, indicating that they do not form a single lexical unit:

(38)   a.   No wonder that, what with the charms of the quiet rural landscape and the "purer air," **what also with** the charm of the "still air of delightful studies," of the atmosphere of culture, lettered ease and refinement, […] Princeton should have been becoming increasingly popular…

b. As you can probably tell by now, *Final Fantasy VIII* is very different from its predecessors, **what especially with** the drastic innovations in its battle system.

c. While Motoi Sakuraba's soundtrack may not exactly be on par with his other works, **what primarily with** its painful instrumentation throughout the game…

d. Besides, **what now with** funds looking as if they are net short again, the path of least resistance seems likely to be higher.

e. I thought we need a change, **what just with** Bush.

f. **What, therefore, with** Carter's commands, the seaman's calls, and the violent flinging down of ropes upon the deck, there was a very considerable uproar going on upon deck…

Notice further that *what* can be expanded to *what all*, as shown by the examples in (39) below (from the Internet).

(39) a. So… **what all with** this desk job, it seems I have such various job tasks as data entry, switchboard operation, and signing someone else's name on letters.

b. The Kid was in heaven, **what all with** the sunny day, just a little wind, the San Francsico [*sic*] skyline at his back, and the ball going through the hoop time and time again.

Since, according to McCloskey (2000), *what all* in sentences such as *What all did you get for Christmas?* is a phrasal constituent (a DP headed by the quantificational determiner *all*), *what all* in the examples above must be located in a non-head position.

As to option (ii), the fact that that *what* is not associated with any argument position within the WWA coda strongly argues against a movement analysis. Unlike argumental *what* in standard *wh*-interrogative structures, *what* in WWAs has no obvious base position other than its surface position. The fact that it cannot be modified by *else*, illustrated in (40b), further indicates that it is non-referential.

(40) a. **What else** did you buy?

b. *And **what else with** his being half asleep…   [cf. (2b)]

This would seem to leave us with option (iii), the assumption that *what* in WWAs, like the interrogative adverb *whether*, is some kind of *wh*-operator base-generated in its surface position. The possibility of intervening adverbs illustrated in (38) above suggests that unlike what is normally assumed for the base-generated *wh*-adverb *whether*, *what* in WWAs cannot be located in the specifier position of the CP headed by *with*, however. Pre-modifying adverbs have either been claimed to be adjoined to the projections they modify (e.g. Haider 2000) or to occupy the specifier positions of separate functional heads (Alexiadou 1997, Cinque 1999). Assuming that multiple specifiers (or multiple adjunction to the same projection) are ruled out (Kayne 1994), the data in (38) thus leads us to conclude that *what* is located in the specifier of some functional projection above CP, as indicated in (41).

(41) [$_{XP}$ what [$_{X'}$ $\varnothing$ [$_{CP}$ [$_{C'}$ with [$_{TP}$ ...]]]]]

This conclusion is compatible with the growing amount of cross-linguistic evidence suggesting that the C-system involves multiple functional layers. If CP is permitted to be recursive (Watanabe 1993, Zanuttini & Portner 2003), the constituent labelled 'XP' in (41) above might be a second CP dominating the first; if CP recursion is ruled out, it must be a separate functional category forming part of the extended C-system (compare e.g. Rizzi 1997).

Given their historical origin and the fact that WWAs in PDE also frequently occur with coordination, it is tempting to analyse *what* as a correlative adverb (also called 'initial conjunction' or 'correlative conjunction'). The fact that unlike *with*-augmented absolutes as in (42a), WWAs cannot be further augmented by correlative adverbs such as *either* would seem to support this assumption.

(42) a. ?**Either with** John away or with his doorbell not working, no one
could get into his apartment.   (McCawley 1983: 272)

b. *Either what with …/*What either with** John away or (what) with
his doorbell not working…

Like correlative adverbs, *what* in WWAs is uniquely associated with a specific conjunction – that is, it never introduces conjuncts linked by any other conjunction than *and*.[10] For the sake of concreteness, and following the analysis of correlative adverbs proposed by Johannessen (2005), let us consider the possibility that *what* is located in the specifier of a correlative phrase (CorP), as shown in (43).[11]

(43) [$_{CorP}$ what [$_{Cor'}$ $\varnothing$ [$_{ConjP}$ CP [$_{Conj'}$ and CP]]]]

Analysing *what* in WWAs as a correlative adverb is problematic, however, given that *what* also patterns differently from correlative adverbs in a number of respects. First, recall that coordination, although on the whole preferred, is not in fact obligatory in WWAs. What, then, is the status of *what* in the absence of coordination, assuming that the presence of CorP is contingent upon the presence of ConjP? Secondly, unlike correlative adverbs, *what* (or *what with*) is not limited to introducing only initial conjuncts:

(44) [...] some women held wine to their mouths that they might drink; and **what with** dropping blood, and **what with** dropping wine, and **what with** the stream of sparks struck out of the stone, all their wicked atmosphere seemed gore and fire.   (Dickens, *A Tale of Two Cities*)

Thirdly, unlike other correlative adverbs, which tend not to be selective about the type of constituents they conjoin, 'correlative' *what* in PDE is used exclusively to

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[10] Except for occasional instances of 'through to' or 'not to mention', as in (i) below (from the Internet):

(i) What with the plagiarism debacles of Doris Kearns Goodwin and Stephen Ambrose [...] and the ongoing debate over whether Michael Bellesiles is guilty of fraud or incompetence (not to mention the earlier mad if temporary rush to originalism as liberal historians raced to the defense of Bill Clinton in his impeachment troubles), the history profession has not looked very good lately.

[11] In order to account for the close association between the correlative adverb and its conjunction, Johannessen (2005) assumes that correlative adverbs start out adjoined to ConjP and subsequently raise to (Spec,CorP). The syntax of coordination is still controversial, though – see Borsley (2005) for some arguments against conjunction phrases.

conjoin non-finite clausal constituents introduced by the prepositional complementiser *with*.[12] In view of these observations, an analysis along the lines of (43) does not seem tenable after all. The alternative possibility that *what* could be a (non-correlative) focus particle similarly fails to account for the restricted distribution of 'focus' *what* compared with other focus particles, and for the fact that WWAs can themselves be modified by the concessive focus particle *even*, or by focussing adverbs such as *especially* or *just*, as shown by the examples in (45) (from the Internet).[13]

(45) a. Not as many people can afford to eat out, these days, so it seems, **even what with** working two jobs.
      b. I never really understood how people could like Paris Hilton, **especially what with** her being such a whorish tramp and all.
      c. **Just what with** all the listing, shopping, hiding, wrapping and decorating, and shuttling young'uns hither and yon for lessons, winter rehearsals, recitals and concerts, time's flown.

If *what* is neither a correlative adverb or conjunction, nor a focus particle, then what is it? In the following, we will reconsider the status of *what* in the light of the idea that certain aspects of pragmatic meaning or discourse function may be represented syntactically.

4.2 *What* as an evaluative operator

Recall from section 2 above that besides their 'reason' interpretation, the principal semantic and pragmatic properties of WWAs include factivity and the implication of an evaluation on the part of the speaker. Note that factivity and (implied) speaker evaluation, as well as the presence of an initial *wh*-element, are properties that WWAs share with, for example, exclamatives such as (46a) and interrogatives involving the 'secondary' adjunct *wh*-phrase *how come* such as (46b).

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[12] What is more, *with* is typically absent from non-initial conjuncts, suggesting that we are dealing with TP rather than CP coordination here.

[13] As König (1991: 16) notes, the possibility of combining focus particles is extremely restricted in English.

(46) a. **What** a nice guy he is!   (Zanuttini & Portner 2003: 40)

  b. **How come** when Clark Kent decides to run at super speed, his clothes don't burn up from Friction?   (from the Internet)

Recent proposals arguing that certain interpretive and discourse-related properties of sentences may be encoded syntactically by corresponding functional categories have rekindled formal linguists' interest in these and other 'peripheral' structures. Elaborating or extending Rizzi's (1997) 'split-CP' hypothesis, several authors have proposed that discourse-level properties such as factivity, evidentiality or evaluation are represented in the syntax (see, among others, Ambar 1999, Cinque 1999, Di Sciullo 2006, Munaro & Obenauer 1999, Speas 2004, Speas & Tenny 2003, Zanuttini & Portner 2003).[14] Based on Watanabe's (1993) analysis of factive declaratives, Zanuttini & Portner, for example, argue that exclamatives such as (46a) contain an abstract factive operator (here labelled 'OP$_{FACT}$') that is located in the specifier of an additional C head, as shown in (47).[15]

(47) [$_{CP}$ [$_{DP}$ what a nice guy]$_i$ [$_{C'}$ $\varnothing$ [$_{CP}$ OP$_{FACT}$ [$_{C'}$ $\varnothing$ [$_{TP}$ he is t_i]]]]]

Like WWA-*what*, *how come* in sentences such as (46b) has been argued to be a non-head base-generated in the specifier of a functional category within the C-system (Conroy 2006, Fitzpatrick 2005, Ochi 2004). Similarly to Zanuttini & Portner's analysis of exclamatives, Fitzpatrick (2005) claims that *how come* in interrogatives such as (46b) occurs with a factive complementiser that may itself introduce a covert (factive) operator, as illustrated in (48).[16]

(48) how come [$_{CP}$ OP$_{FACT}$ [$_{C'}$ C$_{FACT}$ [$_{TP}$ ...]]]

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[14] Clearly there are many aspects of pragmatics that cannot feasibly be represented grammatically. For some discussion of how the grammatical representation of discourse-related properties might be constrained, see Speas (2004) and Speas & Tenny (2003).

[15] Two CP layers are required here because a separate specifier position is needed for hosting the fronted *wh*-phrase.

[16] Following an earlier suggestion by Melvold (1991), Fitzpatrick calls this an *iota* operator.

Extending Fitzpatrick's analysis, Conroy (2006) proposes that *how come* merges with the abstract factive operator and thus effectively takes on this function itself. Applying Conroy's suggestion to the analysis of WWAs, and taking into account the evidence noted earlier for the presence of at least two functional layers within the CP-domain of WWAs, leads us to analysing *what* as a factive operator located in the specifier of a corresponding functional head (provisionally labelled 'X$_{FACT}$' in (49) below).

(49) [$_{XP}$ what [$_{X'}$ X$_{FACT}$ [$_{CP}$ [$_{C'}$ with [$_{TP}$ ...]]]]]

Note, however, that the assumption that X in (49) encodes factivity does not provide any account for the observation that *what* (similar to evaluative sentence adverbs such as *unfortunately, luckily,* etc.), also seems to imply some kind of evaluation on the part of the speaker. According to Cinque (1999), evaluative adverbs are located in the specifier of an 'evaluative' mood phrase that forms part of the extended IP-domain. The existence of 'evaluation phrases' as part of the inflectional or C-system has also been argued for by, among others, Ambar (1999), Di Sciullo (2006), Munaro & Obenauer (1999), Speas (2004) and Speas & Tenny (2003). Note that like WWA-*what* or *how come* in (46b), evaluative adverbs are also factive, that is, the truth of the proposition they modify is presupposed (compare e.g. Geuder 2002: 111). If the presence of lexical elements signalling discourse or speaker evaluation necessarily implies factivity, then there would seem to be no need for assuming that evaluation and factivity are encoded by independent functional heads here. In short, we suggest that WWAs are best analysed as *Evaluative Phrases* (EvalPs) along the lines shown in (50), with *what* functioning as an evaluative operator.[17]

(50) [$_{EvalP}$ what [$_{Eval'}$ ∅ [$_{CP}$ [$_{C'}$ with [$_{TP}$ ...]]]]]

On the assumption that the functional head labelled *Eval* in (50) above is neutral with respect to how exactly the proposition in its scope is evaluated, this analysis accounts for our earlier observation that depending on contextual and other non-syntactic

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[17]   We follow Di Sciullo (2006) (rather than e.g. Cinque 1999) in assuming that the evaluative head in question takes CP complements.

factors, WWAs can have either positive or negative 'implications'. A similar analysis has been proposed by Munaro & Obenauer (1999) for 'pseudo-questions' introduced by Bellunese *cossa* 'what' or German *was* 'what', as in (51a,b) below, both of which are used to express a degree of surprise and/or disapproval (as well as implying the truth of the question's propositional content – see ibid.: 247).[18]

(51) a.   **Cossa**   parle-li   de   che?!
                what   speak-CL of   what
                'What on earth are they speaking of?'

                                        (Munaro & Obenauer 1999: 220 n. 21)

      b.   **Was** schaust du   mich so   an?
                what   look   you   me   so   at
                'Why are you looking at me like that?'   (ibid.: 213; our glosses)

Bellunese is a North-Eastern Italian dialect that lacks obligatory *wh*-fronting, and the co-occurrence of *cossa* and the in-situ *wh*-pronoun *che* in (51a) demonstrates that *cossa* must be non-argumental. Like evaluative adverbs or the non-argumental *wh*-pronouns in (51), WWA-*what* does not contribute to the absolute's propositional content but instead expresses speaker evaluation. Since the presence of evaluative *what* also signals factivity, a 'weak' or conditional reading of WWAs (compare section 2.3 above) is necessarily precluded, even for WWAs containing stage-level predicates.[19]

4.3 Underspecified *what*

We saw earlier that the principal function of WWAs is to provide an account of a (positive or negative) claim made in the matrix clause. That the *wh*-pronoun *what*

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[18]   Similar pseudo-interrogatives are attested in French (Munaro & Obenauer 1999) and Hungarian (Ochi 2004).

[19]   On the assumption that *to*-infinitives express an 'unrealised future tense' (Stowell 1982), their factivity may render WWAs incompatible with infinitival clauses (see footnote 2 above). Even though not all speakers find examples like (i) acceptable, infinitival clauses do not seem altogether excluded from absolutes augmented only by *with*.

    (i)   With there (probably) to be a meeting at 1:00, we'd better have a
              quick lunch.   (McCawley 1983: 275)

should give rise to such an interpretation is by no means unique to WWAs, however – and probably no coincidence, either. Note that diachronically, the many uses of *what* include its use as an interrogative reason adverb meaning *why*, as in the examples in (52) below (from the *OED*).

(52) a.   **1579** FULKE *Heskins' Parl.* 148 But **what** stand we trifling about this testimonie?

      b.   **1667** MILTON *P.L.* II. 329 **What** sit we then projecting Peace and Warr?

      c.   ***a*1677** BARROW *Serm.* I. 7 **What** should I mention beauty; that fading toy?

In PDE, *what* is still used in this sense in the expression *what do you care if...* (Ochi 2004) and in the *What's X doing Y?* construction discussed by Kay & Fillmore (1999), both of which are illustrated in (53) below.

(53) a.   **What do you care if** a Democrat doesn't see the White House for a generation?   (from the Internet)

      b.   **What is** this scratch **doing** on my table?   (Kay & Fillmore 1999: 3)

As we saw above, non-argumental *what* is also used colloquially in the sense of *why* in, for example, Bellunese and German.

Given that cross-linguistically, the equivalents of *what* tend to be extremely versatile elements that can be either argumental or non-argumental, occur in different clause types and give rise to different interpretations, Munaro & Obenauer (1999) have proposed that non-argumental *what* is deficient or 'weak' in Cardinaletti & Starke's (1996) sense and semantically underspecified. Unlike clitics, weak pronouns can carry stress and occur in positions normally occupied by maximal projections, but unlike strong pronouns they are excluded from theta-marked positions and are unable to introduce new discourse referents. Munaro & Obenauer argue that deficient or 'why'-type *what* lacks the semantic restriction [+thing] that restricts argumental *what* to being interpreted as quantifying over a set of things, and that its 'reason' interpretation then results from the interplay of various factors including the presence of EvalP, factivity, and a sentence's informational content. We suggest that WWA-

what is underspecified is the same way as *what* in pseudo-interrogative structures and moreover lacks an interrogative feature.[20]

That coordination should tend to be preferred in WWAs might then follow from the assumption that although semantically underspecified, *what* has retained its basic semantic function as an operator quantifying over sets (but see Ginzburg & Sag (2000) for a different view). 'Why'-type *what* in pseudo-interrogatives such as those in (51)–(53) above could then be described as an operator quantifying over a set of possible reasons. We suggest that WWA-*what* shares with pseudo-interrogative *what* the property of quantifying over sets of reasons (rather than over sets of things, like argumental *what*). Although there is no formal requirement that WWAs should contain reason sets whose cardinality is higher than one, coordination of several 'reasons' might often be felt to be more felicitous – especially, as we saw above, in more formal, written styles. From a pragmatic perspective, conjoining two or more 'reasons' – including the use of dummy conjuncts such as *and everything*, *and all*, etc. – moreover serves as a stylistic means for strengthening the justification that WWAs are supposed to provide.

5. Concluding remarks

Our corpus analysis revealed that absolutes augmented by *what with*, whose existence is often barely acknowledged in learner dictionaries or other reference works, are used productively across different varieties and registers of PDE and occur in both written and spoken texts. Although pragmatically more constrained than ordinary absolutes, WWAs are not, as has sometimes been implied, restricted to pragmatically negative contexts but instead provide a (non-neutral, from the point of view of the speaker) justification of accountable assessments in general. We proposed a unified analysis of WWAs as non-finite clausal constituents with *what* being a base-generated, non-interrogative *wh*-operator located in the specifier of a functional projection within the extended C-system. Following Munaro & Obenauer's (1999) analysis of pseudo-interrogatives, which share with WWAs a

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[20]   Underspecified *what* may be far more ubiquitous than we are able to demonstrate here. Underspecified *what* may also occur in exclamatives such as example (46a) above (Munaro & Obenauer 1999), serve as a '*wh*-scope marker' in languages such as German and Hungarian (ibid., but see Felser 2001 for an alternative analysis) or be used as a parenthetical (Dehé & Kavalova 2006).

number of properties including factivity, implied evaluation and a 'reason' or 'account' interpretation, we suggested that WWAs are best analysed as projections of an abstract *Evaluative* head. While other core interpretive properties of WWAs, notably their factivity, directly follow from the proposed analysis, other aspects of their pragmatic meaning or function such as whether a given token has positive or negative 'implications' are determined purely contextually. In short, we hope to have shown that treating WWAs as entirely arbitrary form–meaning pairings is not doing them justice, and that a systematic investigation of WWAs and other 'peripheral' clause types can help advance our understanding of the nature of the syntax–pragmatics interface.

Acknowledgements

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Emergent and innate sources of knowledge
in the early L2 acquisition of English verbal morphology

Roger Hawkins

In speech, low proficiency second language learners of English use finite forms of *be* frequently, but with a range of meanings not found in the input they encounter. They use regular past tense *-ed* and 3rd person singular present tense *-s* forms of main verbs infrequently, but typically appropriately. The present paper first considers possible explanations for this behaviour based on knowledge 'emerging' from the learning of salient and frequent forms in input, without reference to innate linguistic knowledge, but finds conceptual problems with this approach. A 'nativist' alternative is proposed where the facts follow from learners using innately-known interpretable, but not uninterpretable, features to create Vocabulary entries of the kind proposed by Distributed Morphology. These entries are qualitatively different from those of native speakers, being based on context-sensitive co-occurrence information rather than the feature content of single syntactic terminal nodes. The proposed hypothesis is then tested through (a) an oral sentence completion task undertaken with low proficiency L2 speakers of English that systematically disrupts linear co-occurrence patterns; and (b) a preliminary study of how low proficiency L2 speakers of English use 3rd person singular present tense *-s* as a cue to the interpretation of the number of the subject. It is argued that the results from both studies are consistent with the proposal for the early Vocabulary entries of L2 learners.

1. Place of a theory of grammatical knowledge in the investigation of second language acquisition

Universal Grammar (UG) proposes that humans are born with a special ability for language, and that aspects of that ability are not found in any other module of mind. UG-based work on second language acquisition (SLA) is concerned with understanding what grammatical knowledge second language (L2) speakers have, how they acquire that knowledge, and the extent to which it might be determined by innate linguistic knowledge.

Theories of grammatical knowledge and acquisition of grammatical knowledge are just one part of the picture that makes up SLA, however. Theories are also needed of how grammatical knowledge is put to use by L2 speakers to produce

and understand linguistic expressions, the form that language takes when L2 speakers interact with other speakers, how L2 speakers use their grammatical knowledge in speech acts (informing, persuading, phatic use, etc.), how L2 speakers' pragmatic knowledge interacts with their grammatical knowledge, and so on.

The need for theories about a variety of sources of knowledge that contribute to the phenomenon of SLA has always been recognised by researchers who work on grammatical knowledge from the UG perspective. However, to make any progress, it has been practically necessary to set aside many questions in these other areas, perhaps giving the impression (wrongly) to a number of those in the field that a theory of grammatical knowledge can explain SLA. This is not true and has never been true.

Recently, UG-based theories of grammatical knowledge have come under considerable attack from researchers working on SLA from other perspectives, not simply to admonish UG-inspired research for failing to be interested in the range of theories required (a possibly justified criticism), but for pursuing a wrong-headed and misleading avenue of enquiry. Some of these attacks have suggested that there is no need for a theory of grammatical knowledge at all.

In this paper, one such line of argument will be considered that comes from within the 'emergentist' perspective on SLA. The emergentist programme covers a number of different approaches to what constitutes knowledge of an L2 and how that knowledge is acquired. All approaches share the same basic assumption: there are no innate grammatical principles. Knowledge of an L2 'emerges' in human beings as a result of their experience with samples of language. In some emergentist theories innate principles of information processing are assumed (for example, O'Grady 2005, to appear). L2 learners deduce abstract grammatical knowledge through the interaction of these principles with experience. Others appear to assume much less innate endowment, with knowledge of language resulting from the inductive, cumulative, rote learning of items and constructions. No abstract grammatical knowledge results; learners produce and understand new constructions by analogising over the store of learned constructions (for example Ellis 2002, 2006). The claim is that a theory of L2 grammatical knowledge is simply not necessary.

This claim will be questioned here, and it will be asked whether an emergentist approach can in principle provide a solution to one of the puzzles of L2 grammatical knowledge/acquisition. At the same time, it should be acknowledged

that some emergentist work on SLA is salutary for UG-based approaches. Explicit emergentist proposals force a review of assumptions about what is innate, especially if an experience-based account can be given of some phenomena that previously had been assumed to require an explanation in terms of innate linguistic knowledge.

The specific case that will be examined looks tailor-made for an emergentist account, and it has indeed been addressed by emergentists: knowledge of English verb morphology by L2 learners in the earliest stages of acquisition. It will be suggested that things are not as simple as might at first seem from an emergentist perspective, and that the facts pose problems both for emergentist and UG-nativist approaches.

2. Three observations about the early use of English verb morphology by L2 speakers

The first observation is that three characteristics of the distribution of English verb morphology in the speech of early L2 learners have emerged from a variety of studies of early L2 learners of English with different L1 backgrounds and of different ages of first exposure to English:

- Forms of *be* (*I'm hungry*) are supplied more frequently than affixal forms (*She walks*, *walked*). That is, free forms and bound forms are treated differently.
- Bare verb stems alternate with inflected forms in contexts where native speakers require inflected verbs (*Yesterday she walk home*/*Yesterday she walked home*). In other words, there is optionality in production.
- When inflected forms are used, there is little mismatch in S-V agreement (there are few cases of **I walks home*), or use of inflected past tense verbs in non-past contexts (*Now I played*). That is, agreement and tense dependencies are target-like.

A study by Ionin & Wexler (2002) of the speech of 20 L1 Russian child learners of English (age range 3,9–13,10) with varying lengths of immersion in English (from 2 months to 3 years) illustrates these three characteristics:

Table 1 Tense/agreement morphology in obligatory contexts (based on tables 1 and 2 in Ionin & Wexler 2002: 106–107; 3p = 3rd person)

	Cop *be*	Aux *be*	Regular past	3p *-s*
Suppliance	329/431 (76%)	300/479 (63%)	73/174 (42%)	67/321 (21%)
Bare v	69/431 (16%)	158/479 (33%)	101/174 (58%)	250/321 (78%)
Tense/agreement Mismatch	33/431 (8%)	21/479 (4%)	0/174 (0%)	4/321 (1%)

The instances recorded in table 1 are obligatory contexts of use for native speakers. Observe that frequency of suppliance here does not simply divide between free forms and affixal forms, but distinguishes copula *be* from auxiliary *be*, and regular past from 3rd person singular present tense *-s*:

Secondly, Ionin & Wexler (2002) note that their subjects produced a construction they would not have encountered in input: *be + bare V*, e.g.:

(1)   I'm read

          I'm buy beanie baby

Other studies have also observed this use of *be + bare V* by early L2 learners of English (Yang & Huang 2004; García Mayo et al. 2005). This construction is used with a range of meanings other than the progressive/future readings of *be+V-ing* of native English (*I'm reading at the moment*, *She's leaving tomorrow*). In table 2 the frequencies of these uses by subjects in the Ionin and Wexler study are displayed.

Table 2 Range of meanings of *be + bare V* (table 4 in Ionin & Wexler 2002: 112)

	Prog	Generic	Stative	Past	Future	Ambiguous	Non-Prog/Fut
Tokens	32	33	12	21	5	5	71
%	30	31	11	19	5	5	66

e.g.   They *are* help people when people in trouble (generic)
          He *is* run away. I stayed there (past)

Thirdly, Ionin and Wexler (2002: 108) observe that the frequency of suppliance of verb morphology by these Russian speakers is unlikely to be the result of L1 influence. Russian has affixal inflections in all tenses, but lacks a copula in the

present tense, and only has an equivalent of auxiliary *be* in the compound future. Frequency of forms in the L1 appears to be quite different from frequency of forms in the L2.

The questions that these observations pose for a theory of the grammatical knowledge of early L2 learners of English are the following. What kind of mental representations give rise to speech (a) where forms of *be* are supplied more frequently in obligatory contexts than the affixal forms *-ed* and *-s*? (b) where forms of *be* are used with a range of non-target meanings, while *-ed* and *-s* are largely used in a target-like fashion?

3. Possible explanations based on salience and frequency of forms in input (and no innate linguistic knowledge)

Goldschneider & DeKeyser (2001: 36) have offered an account of observations like those above that that they claim makes 'no appeal to any innate blueprints or specific syntactic models … to explain order of acquisition'. On the basis of a so-called 'meta-analysis' of 12 existing studies, they claim that properties of the input alone 'account for a very large portion of the total variance in the accuracy scores for grammatical functors' (2001: 35) without the need to invoke pre-existing linguistic knowledge. The properties in question are the perceptual salience of forms, their semantic complexity, their morpho-phonological regularity, the syntactic category they belong to and the frequency of the forms in input. All of these factors 'constitute aspects of salience in a broad sense of the word' (2001: 35).

Setting aside the problem of how L2 learners come by a notion like 'syntactic category' in the first place, if they have no pre-existing linguistic knowledge, the difficulty with 'salience' is that although it is a plausible feature of input that might lead learners initially to notice (or not notice) particular forms, the phenomena we are dealing with have all been 'noticed' by the learners studied since they are all represented in their speech. Ionin & Wexler's subjects are producing all forms at least 20% of the time. The question is why, having identified and stored forms of *be*, past tense and 3rd person singular present tense *-s*, they are produced differentially. Notions of semantic complexity, morpho-phonological regularity and so on, are not sufficient on their own to account for why one known form is supplied less frequently than another.

One way in which Goldschneider & DeKeyser's proposal might be interpreted is that the infrequent forms of regular past *-ed* and 3rd person singular present tense *-s* in the speech of early L2 learners have in fact *not* been noticed as independent morphemes by early L2 learners. Their presence as part of stored forms is just 'noise' (Larsen-Freeman 2002: 280). There are two problems with this. Firstly, there is a lot of noise: 21% in the case of *-s* and 42% in the case of *-ed*. Secondly, if the suppliance of affixal forms were just noise, random probabilistic use across contexts might be expected, but this is not the case. Tense and agreement affixes are largely used appropriately, although infrequently, as table 1 shows. 'Noise' is not a wholly satisfactory explanation.

Frequency of forms alone in input is often cited as the *prime* determinant of frequency in the speech of early L2 learners. Paradis (2006) outlines one such account, based on the 'network model' of Bybee (2001). Bybee proposes that single- and multi-morphemic words are stored fully inflected in the lexicon and are associatively connected to other lexical items which share phonology and semantic features. The frequency of tokens in the input determines the lexical strength of a stored form. Tokens that are more frequent in input will cause higher levels of activation of the stored form than tokens that are less frequent. Type frequency in input/output – that is, the frequency with which the same property is realised by different tokens belonging to the type (e.g. *-s* in *writes*, *walks*, *hits*, …) – increases the likelihood that a 'schema' (= a rule) for the property in question will be formed.

Paradis collected data from 15 child L2 learners of English in Canada after 9 months, 21 months and 34 months of exposure to English, and compared the frequency and the accuracy with which plural *-s* with count nouns (e.g. *book-s*) and 3rd person singular present tense *-s* (e.g. *write-s*) occurred in their speech. At each sampling, the children were producing more instances of plural noun contexts than 3rd person singular present tense contexts, and within those contexts were producing a higher proportion of plural *-s* than 3p *-s*, both token and type. To consider whether this pattern might be the effect of frequency in the input, Paradis examined the distribution of plural *-s* and 3p *-s* in the British National Corpus (a large collection of written and transcribed oral texts). The pattern of token and type frequency in the corpus, and the pattern of token and type frequency in the speech of the L2 learners is remarkably similar.

On the face of it, this looks like striking evidence that what early L2 learners of English know about properties like plural -*s* and 3p -*s* is a direct function of how often they encounter these forms in input. However, consider the detail of what is involved. The input to learners from native speakers contains, presumably, *categorical* marking of plural in plural contexts (i.e. 100% of regular plural count singular nouns are of the form N-*s*) and *categorical* marking of 3p singular in 3p singular present tense contexts (i.e. 100% of regular verbs are of the form V-*s*). Learners are not exposed to input containing forms like *Two book, *She write* (except, perhaps, in 'slip of the tongue' contexts). Frequency of forms like *books* and *writes* in native speech and writing is relative to other tokens in the sample. For early L2 learners, however, the frequency of suppliance that is usually counted by researchers (and this is true of Paradis and Ionin & Wexler) is the relative frequency of N-*s* to *bare* nouns in obligatory N-plural contexts, of V-*s* to *bare* verbs in obligatory V-3p-singular contexts. To illustrate, suppose, hypothetically, that the token frequency of plural -*s* is 10%, and the token frequency of 3p singular -*s* is 5%; the difference between what this means in a sample of native speech and a sample of early L2 speech is as follows:

(2)   *Native sample*   *Early L2 samples*

$$\frac{\text{N-}s}{\text{All items}} = 10\% \qquad \frac{\text{N-}s}{\text{Obligatory N-plural contexts}} = 10\%$$

$$\frac{\text{V-}s}{\text{All items}} = 5\% \qquad \frac{\text{V-}s}{\text{Obligatory V}_{+3p+sing+present}\ \text{contexts}} = 5\%$$

The claim must then be that L2 learners convert frequency of N-plural and V-3p-singular relative to the whole set of lexical items they encounter into 'probable suppliance' of N-*s* in contexts where N-plural is required and V-*s* in contexts where V-3p-singular is required. How this kind of conversion works is not obvious. One possible construal is that relative frequency of categorically marked forms to total number of lexical items encountered in input is converted into the strength of activation of the memory trace for the item. The activation level of plural -*s* is higher than the activation level of 3rd person singular present tense -*s*, and both levels of activation are weak relative to the activation levels of bare N and bare V. However,

activation of this type would be blind to distributional constraints on N-*s* and V-*s*. As was seen in the results in table 1, both 3p singular -*s* and regular past -*ed* (another form probably with relatively low frequency in input) are not used randomly, but restricted to contexts where the subject is a 3rd person singular noun or the verb is in the context of a T[+past]. An 'input frequency' account, without further qualification, predicts the probability of V-*s* and V-*ed* occurring in any context. The account would need to be supplemented by constraints such as:

(3)   a. /s/ [5% probability] ↔ [V, –past, 3p, +sing] + ___
        b. /d/ [20% probability]↔ [V, +past] + ___

But, of course, specifications like (3) presuppose that learners have existing knowledge of properties like tense, number and person that are used for analysing input prior to any encounter with input. That is, frequency of forms in input interacts with learners' pre-existing knowledge of linguistic properties like syntactic category [V], tense [+/–past], person [1,2,3] and number [+/–singular]. Input frequency is not an alternative to linguistic knowledge in explaining the early development of L2 grammars. The conclusion is that proposals that input frequency predicts output frequency in the speech of early L2 speakers seems to require an intermediate level of representation where pre-existing knowledge of syntactic properties is involved in converting raw linguistic experience into grammatical knowledge.

3.   A possible explanation that assumes innate knowledge

The observations outlined in section 2 are potentially as problematic for a UG-based approach as for an approach that assumes that input solely determines output. If L2 speakers have 'noticed' forms of *be* and affixal -*ed* and -*s,* and this knowledge is derived through the application of the features V, tense, number and person (assumed to be part of innate endowment) in the categorisation of input, then why do speakers not use them categorically, as native speakers do? And why are *be* forms overgeneralised to non-target contexts?

One possible account is a version of the Missing Surface Inflection Hypothesis (Haznedar & Schwartz 1997; Prévost & White 2000; Lardiere 2000). The Missing Surface Inflection Hypothesis (MSIH) assumes a dissociation in grammatical representation between the output of the syntactic computations and the

phonological exponents that realise them. Such a dissociation is found in Distributed Morphology (Halle & Marantz 1993; Harley & Noyer 1999). Phonological exponents are stored in a 'Vocabulary' component with features that specify the kinds of syntactic terminal nodes into which they can be inserted (their 'contexts of insertion'). Syntactic terminal nodes themselves are just bundles of abstract morpho-syntactic features. For example, the syntactic output for the expression *She walks* might be like that in (4), where *u* indicates uninterpretable (inherently unvalued) features that have been assigned a value by corresponding interpretable features in the course of the syntactic derivation. The subject DP has moved to the specifier position of TP, and T (which is affixal) has lowered to adjoin to v:

(4)

```
                          TP
              _____/  _____
            DP                          T'
            |                      _____/  \_____
     [3p, +sing,                  T              vP
     +female, +pron,              |          ___/  \___
     uCase: Nominative]i          tj       DP          v'
                                           |          / \
                                           ti      v+Ti
                                                      |
                              [V, WALK [T –past, uperson: 3p,
                                        unumber: +sing]]
```
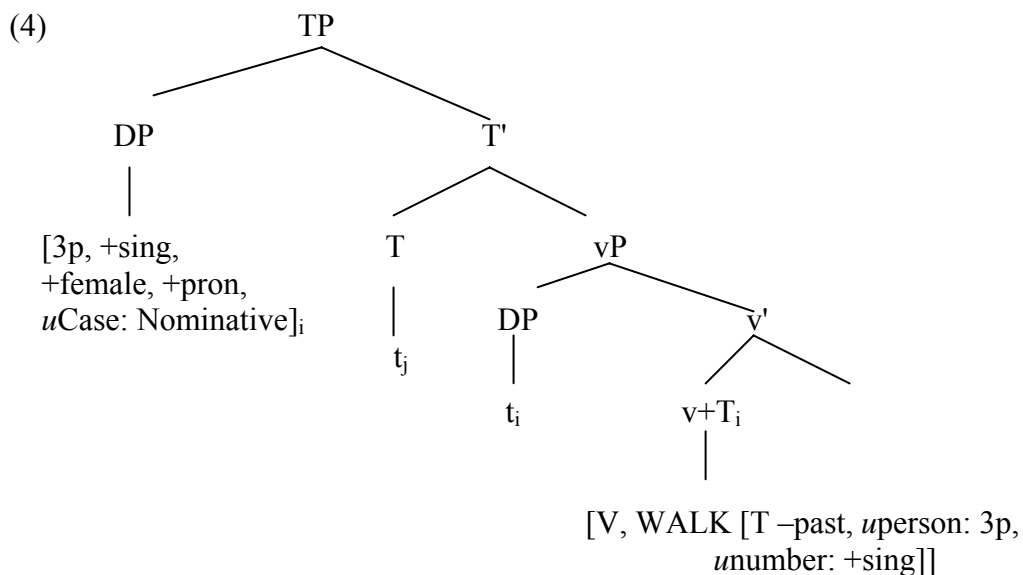
Vocabulary entries for verbal inflection are as illustrated in (5) (simplifying):

(5) a. /s/ ↔ [V, –past, *u*person: 3p, *u*number: +singular] + ___

 b. /d/ ↔ [V, +past] + ___

 c. /Ø/ ↔ [V] + ___

Note that Vocabulary entries may be underspecified with respect to the features of syntactic terminal nodes; whereas /s/ is specifed for categorial V, tense, person and number, /d/ is only specified for categorial V and tense, and the phonologically null inflection /Ø/ is only specified for categorial V. For insertion to take place, a matching operation must determine that the features of a Vocabulary entry are a

proper subset of the features of a syntactic terminal node; i.e. that there are no clashing values and that the Vocabulary entry does not contain features that are not present in the terminal node. The features of both /s/ and /Ø/ in (5) are a proper subset of the features of V in (4), and therefore are potentially insertable. However, *She walk is not a possibility for speakers of varieties of English that require 3rd person singular present tense agreement. A 'subset condition' (Halle 1997) blocks the insertion of /Ø/. The condition is that the Vocabulary entry with the greatest number of matching features is the form that is inserted.

On the basis of a model of grammar of this kind, the MSIH proposes that in L2 grammars the subset condition is faulty. A less specified Vocabulary entry may be inserted where a more specified entry is required. This has two effects. It gives rise to the observation of optionality: L2 speakers produce both *She walks* and *She walk*. But it also predicts correctly that when more specified forms like /s/ and /d/ are used, they are used in appropriate contexts. There are no 'errors of commission' such as *You walks or #She walked used with intended non-past reference.

While this version of the MSIH provides a good account of optionality in the speech of advanced proficiency L2 speakers, it cannot handle two aspects of the speech of early L2 learners outlined in section 2: why /s/ appears to be supplied less frequently than /d/, and why forms of *be* are overgeneralised to a range of non-target meanings. Since the MSIH is based on the assumption that Vocabulary entries are specified in a target-like way, with divergence in L2 speech the single effect of failing to obey the subset condition, it cannot apparently explain why /Ø/ is more likely to be selected when /s/ is involved than when /d/ is involved, nor why forms of *be* have non-target meanings.

One way in which the MSIH could be adapted to these other cases is to assume that early Vocabulary entries also lack certain features. Specifically, the claim that will be made here is that uninterpretatble features – those that determine grammatical dependencies – are absent from the set of features provided by innate knowledge (UG) that are used by L2 learners to categorise input. Only interpretable features are available. This means that when forms of *be*, the regular past /d/ and 3rd person singular present tense /s/ are identified, Vocabulary entries for them are initially established in terms of their co-occurrence possibilities with other Vocabulary items that have interpretable features. Vocabulary entries take the form of context-sensitive rules:

(6)   /s/   ↔   /[V] + ___ /[T, -past] ___ /[N, 3p, +sing] ___

      /d/   ↔   /[V] + ___ /[T, +past] ___

      /(ə)m/ ↔   /[N, 1p, +sing] ___

      /(ɪ)z/   ↔   /[N, 3p, +sing] ___

      /woːk/ ↔   [V]

Here the entry for /s/ is understood as: insert /s/ in the context of a V that is itself in the context of a non-past T and a 3rd person singular N to the left.

It now needs to be assumed that some kinds of context-sensitive Vocabulary entries are 'costly' to access during computation of the expression. While one level of context-sensitivity incurs little cost (and occurs in mature native Vocabulary entries), as in the case of forms of *be*, more than one level is difficult to compute under normal communication pressure. The more nodes required to specify the context of insertion, the less likely the form will always be accessed. This predicts that, of the entries in (6), while bare V forms should always be accessible in speech, /s/ will be the least accessible form. Neither /s/ nor /d/ are as accessible as forms of *be* which are represented with only one level of context sensitivity: a (pro)noun to the left (specified for person and number).

Note that the form of the entries in (6) predicts that when /s/ and /d/ are retrieved they will be used appropriately because reference is made to interpretable properties of tense, person and number. When forms of *be* are retrieved, they will always be appropriate to the person and number of the subject. However, since they are unspecified for tense or complement type, they will be used with a variety of meanings that forms of *be* do not have in target English, and with bare V complements, again a property not provided by input from the target language.

While this account of early L2 Vocabulary entries for English verbal morphology offers a largely correct description of the facts outlined in section 2, its plausibility would be strengthened if it could be shown that it also predicts L2 behaviour in other ways. Since entries are specified in terms of linear contextual information, one prediction is that if contextual information is disrupted, accessibility of the form should be disrupted in speech. For example, although /s/ is already the least likely verbal morpheme to be accessed, accessibility should be further disrupted

where the adjacency between a 3rd person singular subject N, and T and V is disrupted, as in (7b) in contrast to (7a):

(7)   a. My brother own*s* a house

        b. The brother <u>of my best friend</u> own*s* a house

Disruption might take two forms: (a) learners will choose the closest N to determine agreement; (b) retrieval of /s/ will decrease because the context required to select /s/ has been disrupted. For native speakers, /s/ is specified in terms of features of V itself. In this case, disruption of the context should have no effect on retrieval. Casillas (2006) has tested this with a group of intermediate proficiency L2 speakers of English. The details of this study are described in the next section

4. Testing for disruption to the accessibility of /s/ (based on Casillas 2006)

4.1 Participants

Three groups of informants participated in the experiment, L2 speakers of English with Chinese or Spanish as their native language, and a control group of native speakers. The English proficiency of the non-natives was determined as lower intermediate on the basis of the Oxford *Quick Placement Test* (2001). Participant details are presented in table 3

Table 3 Background details of the participants in the experiment

L1	N	Prof test score range	Prof test score mean	Age range	Age mean
Chinese	10	30–39	34.3	21–30	24.2
Spanish	10	31–39	35.4	22–35	27.9
English	10	—	—	18–35	26.2

4.2 Materials

The test was a modified version of the sentence completion task used by Bock & Miller (1991). Participants were shown a lexical verb (e.g. *own*) or an adjective (e.g. *blond*) on a computer screen for 2 seconds. This was replaced by an intended subject of a sentence (a 'preamble') which remained on the screen for 4 seconds. The preamble was either a simple DP (*my brother*) or a complex DP where the N

determining agreement was followed by a PP complement (*The brother of my best friend*) or preceded by a genitive modifier (*My best friend's brother*). When the preamble disappeared from the screen, the participant's task was to utter a complete sentence aloud, beginning with the preamble and using either the stimulus verb or adjective.

The role of the simple DP preamble was to provide a baseline measure of frequency of suppliance of copula *be* (if the stimulus was an adjective) or */s/* (if the stimulus was a lexical verb). The experimental items were those involving a complex DP preamble: (i) where a PP complement disrupted adjacency between the N determining agreement and T; (ii) where a preceding genitive DP did not disrupt the adjacency. The distribution of items in the test is shown in table 4:

Table 4 Details of the sentence completion task

Preambles	Predicates
128 Simple DPs (*The guest*)	128 lexical verbs (*own*)
56 DP of DP (*The guest of my music teacher*)	128 adjectives (*blond*)
56 DP's DP (*My music tutor's guest*)	
36 fillers (*My brother and my friend*)	

The head nouns of simple DPs were presented in either their singular (S) or their plural (P) form. For the complex subject preambles, modifier noun phrases were presented in both their singular and their plural forms, resulting in four number conditions: singular-singular (SS), singular-plural (SP), plural-plural, (PP) and plural-singular (PS). (8) illustrates sample preambles.

(8)   SS   The brother of my best friend

        SP   The brother of my best friends

        PP   The assistants of the math teachers

        PS   The assistants of the math teacher

Head nouns and verbs were on average separated by the same number of syllables in complex noun phrase preambles containing a prepositional phrase: 8 or 9 syllables. All head nouns included in the preambles were animate, but inanimate subject modifiers were introduced in half of the preambles. Collective nouns such as *group*

or *committee* were not included, given that subject-verb agreement with this type of noun varies among native speakers. Stimulus verbs were selected on the basis of their inherent aspectual class – only stative verbs and psych-verbs were included.[1] The inclusion of this variable therefore reduces any effect of inherent lexical aspect on the production of 3rd person singular present tense marking.

4.3 Hypotheses

The hypotheses were the following:

H1: Early L2 learners of English will supply /(ı)z/ more frequently than /s/ because more nodes are involved in specifying the context of insertion of /s/. NS controls will show no difference.

H2: Separation of the N determining number and person agreement from T will lead either to a decrease in suppliance of /(ı)z/ and /s/ or to (mis-)agreement with the closest N by early L2 learners, but not by native speakers.

H3: Complex DP subjects *per se* will not affect the probability of insertion: DP's DP will not affect suppliance of /(ı)z/ and /s/.

4.4 Additional task

To determine whether participants were assigning the appropriate structural analysis to complex DPs (i.e. were correctly identifying the head N, the potential controller of agreement marking on the verb), they were given a supplementary 20-item comprehension test consisting of items of the following form:

(9) a. Tom bought the friend of his sister a book.

Who got a present?

a) his sister's friend   b) Tom   c) his sister d) Tom's friend

b. The tree in the garden was blown away in the storm.

What got damaged?

a) the garden   b) the tree   c) the storm

---

[1] The need for this restriction was apparent after the results of a pilot study showed that for low intermediate speakers of L2 English the inherent lexical aspect of the verbs influenced their production of present tense marking, with stative verbs being more likely to be marked than dynamic verbs.

If participants were assigning the appropriate structure to the complex object DP in (9a) they should choose answer (a), and in (9b), where there is a complex subject DP, they should choose answer (b). Results from this task provide supplementary evidence bearing on whether subjects are likely to make SV agreement between the closest N and the verb or between the head of the subject DP and the verb.

4.5 Results

The percentages of suppliance of /(ɪ)z/ (with adjective stimuli) and /s/ (with lexical verb stimuli) on the sentence completion task are presented in table 5.

Table 5 Mean % of suppliance of /(ɪ)z/ and /s/

	Chinese		Spanish		English	
	(ɪ)z	-s	(ɪ)z	-s	(ɪ)z	-s
(a) The guest	100	80	100	81	100	100
(b) The guests	0	0	0	0	0	0
(c) My music tutor's guest	100	79	99	82	100	100
(d) The guest of my music tutor	99	60	100	61	100	100
(e) The guest of my music tutors	78	47	80	49	100	100
(f) The assistants of the math teacher	22	40	18	35	0	0
(g) The assistants of the math teachers	0	1	0	0	0	0

The results show that for the native speakers there is no effect of the form of the subject on suppliance of /(ɪ)z/ or /s/, consistent with Vocabulary entries that are activated directly by a single syntactic terminal node.[2]

For both groups of L2 speakers, results are strikingly similar, suggesting that the L1 is unlikely to be influential in determining the knowledge that gives rise to these patterns of response:

(i) /(ɪ)z/ is supplied more than /s/ with simple subjects, consistent with H1. There is no overgeneralisation of /(ɪ)z/ or /s/ when the subject is plural.

(ii) There is no decrease in the suppliance of /(ɪ)z/ or /s/ when there is a complex subject with a preceding genitive DP, consistent with H3.

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[2] Previous studies with native speakers by Bock & Miller (1991), Vigliocco & Nicol (1998) found some 'agreement attraction errors', e.g. *The helicopter for the flights are safe*, but in only 5% (B&M) and 6% (V&N) of cases, and mainly in the condition: $DP_{[singular]}$–P–$DP_{[plural]}$. The L2 subjects in the present study show some sensitivity to $DP_{[singular]}$-P-$DP_{[plural]}$ contexts, but their responses are qualitatively different. Observe that the native controls in this study make no agreement attraction errors.

(iii) Suppliance of /s/ with a complex subject is disrupted when there is an intervening PP, consistent with H2. However, suppliance of /(ɪ)z/ is only disrupted where the PP contains a plural N, not when both Ns are singular. This is only partially consistent with H2.

Does disruption take the form of mis-agreement with the closest N, or omission resulting from the non-adjacency of the terminal nodes required for activation of the Vocabulary entries? In the case of /(ɪ)z/ it looks like mis-agreement with the closest N is involved because when *is* is not supplied, *are* is, rather than omission. When /s/ is not supplied, a bare V is. In the case of rows (e) and (f) in table 5, this looks like it might also be the effect of mis-agreement with the closest N, which differs in number from the head N. However, mis-agreement with the closest N cannot explain the decrease in suppliance shown in row (d), where both Ns are singular.

Results from the supplementary comprehension task (table 6) show that informants have knowledge of the headedness of complex subjects and objects in English. A one-way ANOVA with score as the dependent variable showed no difference between groups ($F_{2,\,27} = 1.21$, $p = .32$).

Table 6 Mean % scores on the sentence comprehension task across L1 groups

L1 group	n	Minimum score	Maximum score	Mean %	SD
Chinese	10	17	20	18.8	1.13
Spanish	10	17	20	18.7	1.06
English	10	17	20	19.4	1.07

Together the results suggest that where constituents disrupt adjacency of strings of terminal nodes specified in a Vocabulary entry, this leads either to retrieval of a form whose specification for the context of insertion matches an intervening (and inappropriate) node or failure to retrieve the entry.

5. Inaccessibility of /s/ as an interpretative cue to number (preliminary remarks)

There is a second possible prediction of the claim that early Vocabulary entries for verbal morphology involve degrees of context-sensitivity that become more 'costly' to access as the number of specified nodes increases. Where 'costly' entries are the

only clue to interpretation in a comprehension task, L2 learners may fail to make the appropriate distinctions. An ingenious study by Johnson et al. (2005) used verbs beginning with an 's' like *swim, sleep, stir* to mask the number of the subject N in a test of whether 3–6-year-old L1 learners of English could use verb morphology to determine subject number. In speech, sentences like *The ducks swim on the water*, *The duck swims on the water* can be produced so that the transition from the subject to the verb gives no clue to whether the subject is singular or plural and interpretation depends entirely on the ending of the verb: *The duck-s-wim*(*s*) *on the water*. Using a picture selection task where children heard test sentences and for each had to choose between, say, a picture of one duck swimming on the water and two ducks swimming on the water, Johnson et al. found that the 3–4-year-olds in their sample selected pictures at chance level where the verb was marked with -*s*, while the 5–6-year-olds were significantly better than chance in selecting the correct picture.[3]

Whatever the appropriate interpretation of these findings for a theory of L1 acquisition, a similar experiment with low proficiency L2 learners can potentially inform us about whether the costliness of accessing entries like 3rd person /s/ also affects comprehension. A preliminary study of this by Nagasawa et al. (2007) replicating the study by Johnson et al. with 11 Chinese speakers of English, 11 Japanese speakers of English (both of intermediate proficiency) and 2 native controls, produced the results in table 7.

Table 7 Accuracy and selection bias in selecting pictures showing singular and plural events

	Percent accuracy		Picture bias	
Group	Singular	Plural	Singular	Plural
Chinese (n = 11)	51%	49%	5.1	4.9
Japanese (n = 11)	40%	71%	3.5	6.5
NS control (n = 2)	100%	100%	5.0	5.0

Results show that while the English controls are perfect in selecting the picture depicting a singular event when the sentence heard contains a verb ending in /s/ (*The duck swims on the water*) and the picture depicting a plural event when the sentence heard contains a bare verb (*The ducks swim on the water*), the Chinese speakers are

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[3] However, all their subjects performed at chance level when the verb was unmarked.

performing at chance and the Japanese speakers appear to have a preference for the plural picture, while at the same time randomly selecting the singular picture. While these findings need to be explored further under more controlled conditions (where proficiency is measured, where verbs beginning with 's' are contrasted with verbs beginning with vowels and consonants, and where there is a comparison with use of 3rd person singular /s/ in speech) they are suggestive. They may imply that L2 learners who apparently produce verb morphology appropriately (but infrequently) in speech, may nevertheless have a different representation for those forms from native speakers consistent with context-sensitive Vocabulary entries of the kind described in section 3.

6. Discussion

The observations concerning the use of English verb morphology by early L2 learners are that affixal -*ed* and -*s* are used optionally and less frequently than forms of *be*. However, when used they are typically appropriate. Forms of *be* in early L2 speech are used with a range of meanings not found in the target language. Frequency of use of -*s* decreases in the speech of lower proficiency L2 learners when the subject N is separated from T and V. Furthermore, in a picture selection interpretation task, L2 learners do not make use of the clue provided by the form of the verb (V-*s* or V-Ø) to the number of the subject.

Emergentist accounts that appeal to the 'saliency' of forms or frequency of forms in the input do not appear adequate to deal with this range of observations. While 'saliency' might be relevant to initial noticing of forms, it does not explain why forms that appear to have been noticed by learners, in the sense that they are represented in some form in their grammars, are used infrequently. Simple linking of input frequency of a form with output frequency in the speech of L2 learners appears not to work unless it is assumed that there is an intermediate level of pre-existing linguistic knowledge about features like V, tense, person and number through which frequency can operate.

A nativist account has been argued for where L2 learners initially categorise input using interpretable features. However, early grammars lack uninterpretable features. This forces learners to construct Vocabulary entries involving context-sensitive rules. Where such rules involve more than one level of contextual specification, it is claimed that this is computationally costly. 'Costliness' gives rise

to optionality in the use of forms that mark dependencies, with the most costly being the least frequently supplied. It was suggested that evidence from a production task where context-sensitive dependencies are disrupted and an interpretation task where only verbal inflection provides a clue to the number of the subject both provide evidence consistent with the proposal. Further research will focus on examining the interpretation results by comparing low proficiency and advanced proficiency speakers.

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Split projections, percolation, syncretism

and interrogative auxiliary inversion

Andrew Radford

This paper deals with auxiliary inversion in wh-questions (hereinafter referred to as *interrogative inversion*) in standard varieties of English. It begins by considering the *split projection* analysis of Rizzi (1997), whereby interrogative inversion involves movement of an auxiliary into the head Foc constituent of a Focus Phrase projection. It then turns to the question of why root subject questions like *Who cares?* do not show auxiliary inversion, arguing against the tense-based analysis in Pesetsky and Torrego (2001). It goes on to consider the possibility of developing a *percolation* account, under which (in consequence of economy considerations) agreement features on C percolate down the minimum distance required in order to ensure a convergent outcome, with percolation as far as Foc yielding a convergent outcome for root subject questions, but percolation down to T being required in order to ensure a convergent outcome for other types of question (and triggering auxiliary inversion in order to satisfy PF interface requirements). However, it is argued that such an account faces both theoretical and empirical problems, and the paper concludes by presenting an alternative *syncretism* analysis under which UG permits Foc and T either to be syncretised on a single head (yielding a convergent outcome in root subject structures) or to be projected on separate heads (yielding a convergent outcome in other types of structure, and requiring auxiliary inversion in order to satisfy PF interface requirements).

1. Introduction

Standard varieties of English have a phenomenon of *interrogative inversion* whereby an (italicised) auxiliary is moved into a position above its (underlined) subject in a root interrogative clause like (1a), but not in an embedded interrogative clause like (1b):

(1) a. Where *has* <u>he</u> gone?

   b. *I have no idea where *has* <u>he</u> gone

A well-known *root subject condition* on interrogative inversion is that it is not found in clauses in which the preposed wh-expression is the subject of a root clause. So, for example, inversion (and concomitant *do*-support, in the guise of (*di*)*d*) applies in a

structure like (2a) in which the preposed wh-expression is the complement of the root verb *meet*, but not in a structure like (2b), where the root subject has been fronted:

(2) a. Who'*d* <u>he</u> meet yesterday?
     b. *Who'*d* meet him yesterday

The two key questions which we shall be concerned with in this paper are (i) when auxiliaries move in wh-questions and where they move to, and (ii) how we can provide a principled account of the *root subject condition*. We begin by looking at the first question, and at the answer given to it in Rizzi's (1997) *split CP* analysis of interrogative inversion.

2. Rizzi's (1997) *focus* account of interrogative inversion

In this section, we examine the syntax of interrogative inversion from the perspective of the *split CP* analysis of peripheral clause structure developed in Rizzi (1997, 2001, 2004, 2006). Rizzi argues that the CP projection located on the left periphery of the clause should be split into a number of distinct heads, including a *Force* head (marking a clause as declarative, interrogative or imperative in Force, and serving as the locus of complementisers like *that*[1]), a *Foc/Focus* head (whose specifier position is the locus for a peripheral focused expression), and a *Top/Topic* head (whose specifier position is the locus for a peripheral topic expression)[2]: Top/Foc are only projected in clauses containing a topicalised/focused constituent, and Force is taken to be the highest projection within the clause periphery. Rizzi (1997) proposes an asymmetrical analysis of wh-movement under which an interrogative wh-expression moves to spec-Foc in root clauses[3], but to spec-Force in other clauses. He further develops (1997: 331, fn. 22) an

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[1] More properly, this should be regarded as a *Type* head, since it marks clause type rather than illocutionary force (see Huddleston 1994): hence, it might be argued that the traditional label C/COMP is preferable to *Force*. However, since Rizzi uses the label *Force*, I shall continue to employ it in discussion of Rizzi's work.

[2] The possibility of a separate Finiteness Projection is ignored here, given the arguments in Radford (2006: §8.3) that (in English) Finiteness is only projected as an independent head in *for*-infinitives, but is otherwise syncretised with an adjacent head.

[3] The idea that wh-movement involves focalisation dates back to Rochemont 1978, 1986; Culicover and Rochemont 1983; Horváth 1986; and Brody 1990. However, Rizzi (2006) suggests that an interrogative phrase is attracted by an interrogative head *Q* and a focused expression by a Focus head *Foc*, but notes (p.

analysis of auxiliary inversion under which preposed auxiliaries move into the head Foc/Focus position of a FocP/Focus Phrase, because Foc is a strong head (in the sense that Foc attracts the head of its complement to adjoin to it); by contrast, Force and Topic are weak heads (and so do not attract a subjacent head to adjoin to them).

In the light of these assumptions, consider how we might analyse the syntax of the wh-clauses in the following sentences from the perspective of the *split CP* analysis:

(3) a. Where is it thought that he has gone?

   b. I have no idea where he has gone

In (3a), *where* originates as the complement of the verb *gone* and moves first into spec-Force within the embedded clause before moving into spec-Foc in the root clause – as shown by the solid arrows below:

(4) [ForceP [Force Ø] [FocP Where [Foc is] [TP it [T ~~is~~] thought [ForceP ~~where~~ [Force that] [TP he [T has] gone ~~where~~]]]]
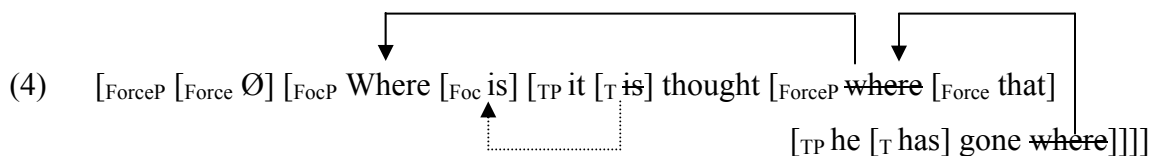
Since *Foc* is a strong head, it attracts the auxiliary *is* to move from T to Foc – as shown by the dotted arrow in (4). In the embedded question in (3b), *where* moves into spec-Force, as shown by the solid arrow in the partial structure below:

(5) I have no idea [ForceP where [Force Ø] [TP he [T has] gone ~~where~~]]
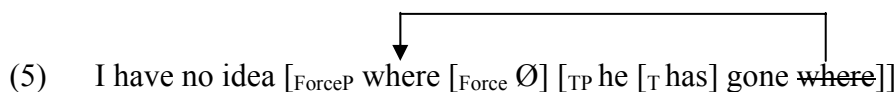
There is no auxiliary inversion in (5) because (as noted above) *Force* is a weak head. Thus, Rizzi's analysis accounts for why interrogative inversion applies in root but not embedded clauses.

Rizzi's claim that interrogative wh-expressions move to spec-Force in embedded clauses but to spec-Foc in root clauses (and that ForceP is the highest projection within

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128, fn. 8) that 'In many languages and constructions, Foc and Q appear to be associated. In Gungbe, for instance, *wh*-phrases are moved to the overtly marked Foc position.'

the clause periphery) also accounts for why an (italicised) interrogative expression precedes an (underlined) topic in embedded clauses such as (6), but conversely topics precede interrogative expressions in root clauses such as (7):

(6)   a. You have to ask [*how much longer*, <u>this kind of behaviour</u>, we can put up with]

       b. *You have to ask [<u>this kind of behaviour</u>, *how much longer* we can put up with]

(7)   a. <u>This kind of behaviour</u>, *how much longer* can we put up with?

       b. **How much longer* can, <u>this kind of behaviour</u> we put up with?

Given the assumption that the topic *this kind of behaviour* is in spec-Top and that interrogative wh-words move to spec-Force in embedded clauses[4], the complement clauses in (6a, b) will have the respective simplified structures shown in (8a, b) below:

(8)   You have to ask

     a. [$_{ForceP}$ how much longer [$_{Force}$ Ø] [$_{TopP}$ this kind of behaviour [$_{Top}$ Ø]

                                     [$_{TP}$ we [$_T$ can] put up with]]]

     b. *[$_{TopP}$ this kind of behaviour [$_{Top}$ Ø] [$_{ForceP}$ how much longer [$_{Force}$ Ø]

                                     [$_{TP}$ we [$_T$ can] put up with]]]

A structure like (7b, 8b) is then ruled out by the requirement for ForceP to be the highest projection in the clause periphery. By contrast, if interrogative expressions move to spec-Foc in root clauses, sentences (7a, b) will have the respective structures shown in simplified form below:

(9)   a. [$_{ForceP}$ [$_{Force}$ Ø] [$_{TopP}$ this kind of behaviour [$_{Top}$ Ø] [$_{FocP}$ how much longer

                             [$_{Foc}$ can] [$_{TP}$ we [$_T$ ~~can~~] put up with]]]]

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[4] The assumption that interrogative expressions move to spec-Force in embedded questions gains empirical support from WH+*that* structures such as the following (reported by Radford 1988: 500 as having been recorded from unscripted sports commentaries in TV programmes):

(i)   I'm not sure <u>what kind of ban</u> *that* FIFA has in mind

(ii)   It'll probably be evident from the field <u>which of the players</u> *that* are feeling the heat most

b. *[ForceP [Force Ø] [FocP how much longer [Foc can] [TopP this kind of behaviour

[Top ~~can~~] [TP we [T ~~can~~] put up with]]]]

In both structures, the interrogative expression *how much longer* moves to spec-Foc, and the auxiliary *are* moves to Foc. What rules out (9b), however, is the fact that *are* cannot move directly from T to Foc because any such movement would violate the Head Movement Constraint of Travis (1984) which requires a head to adjoin to the next highest head above it (the next highest head above T being Top). Moreover, successive-cyclic movement of *are* through Top into Foc is prevented by Top being a weak head.

While Rizzi's analysis accounts for interrogative inversion occurring in main but not embedded clauses, it raises the question of why preposed interrogative wh-expressions should move to spec-Foc in root clauses but to spec-Force in other clauses. A plausible supposition is that this dual behaviour of interrogative wh-expressions reflects their dual semantic properties, in that they are inherently *interrogative* in type but are also *focused* by virtue of asking for new information: accordingly, they can be attracted either by an (interrogative) Force head, or by a Focus head (with the latter also triggering auxiliary inversion).

However, any such claim leaves a number of unanswered questions in its wake. One is why preposed interrogative wh-expressions should not be able to move to spec-Force in root clauses, so generating interrogative root clause structures such as the following, with no auxiliary inversion:

(10)   *[ForceP Where [Force Ø] [TP he [T has] gone ~~where~~]]

The ungrammaticality of such a structure could be accounted for if we followed Branigan (2005) in supposing that only the domain of a root projection can be spelled out. If the root of a sentence is always a Force head, it follows that no overt constituent can be positioned on the edge of a root ForceP, because it would be unable to be spelled out[5].

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[5] However, there is something conceptually rather odd about Branigan's claim. If it is a consequence of the *Transfer* operation which transfers syntactic structures to the PF component and to the semantic component/interface at the end of the syntactic derivation, then it follows that the edge of any root Force

The converse question also arises of what prevents an interrogative wh-phrase from moving to the edge of FocP in a complement clause structure such as the following, with concomitant movement of an auxiliary into Foc:

(11)   *She enquired [ForceP [Force Ø] [FocP where [Foc had [TP he [T had] gone where]]]]

The answer may well lie in the selectional properties of the matrix verb. More specifically, *enquire* is a predicate which selects a complement which is interrogative in force/type; and if we suppose that a clause is only interpretable as a wh-question in English if it contains an interrogative expression on the edge of ForceP, then the ungrammaticality of (11) can be accounted for – as indeed can the ungrammaticality of (8b)[6].

The third question raised by the analysis sketched above is why long-distance wh-movement should involve movement to spec-Force in the embedded clause (as in (4) above), rather than movement to spec-Foc as in (12) below:

(12)   [ForceP [Force Ø] [FocP *Where* [Foc is] [TP it [T is] thought [ForceP [Force that]

[FocP where [Foc Ø] [TP he [T will] go where]]]]]]

One answer is that (if Force is a phase head), movement of *where* from spec-Foc in the embedded clause to spec-Foc in the root clause would violate the following constraint (adapted from Chomsky 2001: 5, ex. 6):

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projection will also be invisible at the semantics interface – raising the question of how we know e.g. that a sentence like *Transfer is mysterious* is declarative in Force. If, on the other hand, a ForceP projection can be transferred to the semantic component but not to the PF component, this creates an obvious interface asymmetry. There will also be potential empirical problems with Branigan's claim if (as Radford 2004: 332 argues) preposed exclamative wh-expressions move into spec-Force in root and embedded clauses alike.
[6] Note that we also need to rule out the possibility of *where* moving from spec-Foc to spec-Force in (11). It may be that (perhaps as a consequence of the *Freezing Constraint* proposed in Rizzi and Schlonsky 2005, and Rizzi 2006) a focused constituent which moves to the edge of FocP is thereafter 'frozen' in place, because spec-Foc is an *interpretive* position (more specifically, a position in which a preposed constituent is interpreted as focused), and movement into an interpretive position terminates a (movement) chain.

(13)   **Phase Impenetrability Condition/PIC**

The c-command domain of a phase head is impenetrable to an external probe (i.e. to a probe outside the maximal projection of the phase head)

The reason why the derivation in (12) would violate PIC is that (immediately prior to the movement operation marked by the lower arrow) *where* would be in the c-command domain of the Force head *that*, and would be attracted by a Foc head in the root clause which lies outside the ForceP projection headed by *that* – in violation of PIC. A second answer is semantic in nature – namely that (because the specifier of a Foc head is interpreted as focused), a clause containing a single focused constituent can only contain a single FocP projection. This precludes the possibility of positing an additional FocP projection in the embedded clause (which in turn also precludes the possibility of *where* moving into the embedded spec-Foc position, and then from there into the embedded spec-Force position, and finally into the root spec-Foc position).

Having looked at Rizzi's (1997) analysis of interrogative inversion and some of its implications, let us now turn to consider how the *root subject condition* on inversion can be handled, beginning with the seminal account in Pesetsky and Torrego (2001).

3. Pesetsky and Torrego's (2001) *tense* account of interrogative inversion

An important constraint on interrogative inversion (illustrated in example (2) above) is that it cannot take place where the preposed interrogative expression is the subject of a root clause. One account of this restriction (variants of which are found in George 1980; Chomsky 1986; Radford 1997; Agbayani 2000, 2006; and Adger 2003) is to suppose that the wh-subject remains frozen in spec-T, with the result that C attracts neither the wh-subject nor T. However, Pesetsky and Torrego (2001) argue that evidence in support of the claim that wh-subjects do indeed move to spec-C comes from the observation by Pesetsky (1987) that phrases like *who on earth* or *who the hell* can occur in subject questions, though not in wh-in-situ questions (cf. *Who on earth arrested him? *They arrested who on earth?*)[7]. But if subject questions do indeed involve wh-movement to

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[7] See Rizzi and Schlonsky (2005: 25–26) and Agbayani (2006: 75–76) for further potential evidence that wh-subjects move to spec-C.

spec-C, this raises the question of why they do not trigger concomitant auxiliary inversion.

Pesetsky and Torrego (2001) offer an interesting answer to this question which can be presented in a slightly simplified form as follows (in terms of the traditional unsplit CP analysis of clause structure which they adopt). In a root wh-question, C has both a WH feature attracting an interrogative wh-expression to move to the edge of CP, and a tense feature (TNS) attracting a tensed constituent to move to the edge of CP: the attracted constituent must in either case be asymmetrically c-commanded by C[8]. A tensed T constituent is assumed to value an unvalued tense feature on the subject, and to carry an EPP feature which triggers movement to spec-T of the subject, with a *tensed* subject being spelled out in the PF component as a *nominative* form (so that what is traditionally called nominative case is a reflex of tense). In a non-subject question like:

(14)   Where (doe)s he go?

a series of merger operations on the CP phase will form the structure shown in simplified form in (15) below (where *Af* denotes an affixal present tense morpheme):

(15)   [$_{CP}$ [$_C$ Ø$_{TNS, WH}$] [$_{TP}$ [$_T$ Af] [$_{vP}$ [$_v$ Ø] [$_{VP}$ he [$_V$ go] where]]]]

The verb *go* in V is attracted by the light verb to move from V to v[9]. The present tense affix in T (= *Af*) serves as a probe and picks out the subject *he* as an active goal, agreeing with it and valuing its tense feature, and attracting it to move to spec-T. The tense feature on C attracts the closest tensed constituent asymmetrically c-commanded by C, and this is the T-affix: since the T-affix is a head, it moves to the head C position of CP. The WH feature on C attracts *where* to move to spec-C, so deriving the structure:

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[8] C cannot attract the TP constituent which it symmetrically c-commands, Pesetsky and Torrego maintain, because of a constraint barring a constituent from being re-merged with a head with which it is already merged. Boeckx (2007: 110) proposes a related *Antilocality Constraint* to the effect that 'Movement internal to a projection counts as too local, and is banned'.

[9] Throughout, I shall ignore the question of why the lexical verb moves from V to v, and whether V-to-v movement is a syntactic operation or a PF operation. For expository purposes, I have taken it to be a syntactic operation.

(16) [$_{CP}$ Where [$_C$ Af] [$_{TP}$ he [$_T$ ~~Af~~] [$_{vP}$ [$_v$ go] [$_{VP}$ ~~he~~ [$_v$ ~~go~~] where]]]]

The affix in C is stranded without an immediately subjacent verbal host to attach to and consequently requires *do*-support, so that the structure in (16) is eventually spelled out as *Where (doe)s he go?*

Now consider the derivation of the corresponding root subject question:

(17) Who goes there?

Suppose that a series of merger operations have formed the following structure on the CP phase:

(18) [$_{CP}$ [$_C$ Ø$_{TNS, WH}$] [$_{TP}$ [$_T$ Af] [$_{vP}$ [$_v$ Ø] [$_{VP}$ who [$_v$ go] there]]]]

As before, the light verb attracts the verb *go* to raise from V to v. The tense affix in T probes, picks out *who* as its goal, and values the tense feature on *who* as well as attracting a copy of *who* to move to spec-T, so deriving the intermediate structure:

(19) [$_{CP}$ [$_C$ Ø$_{TNS, WH}$] [$_{TP}$ who [$_T$ Af] [$_{vP}$ [$_v$ go] [$_{VP}$ ~~who~~ [$_v$ ~~go~~] there]]]]

The tense and WH features on C now attract the closest accessible tensed constituent and the closest accessible interrogative constituent to move to the edge of CP. Because *who* was assigned a tense feature via agreement with the tense affix in T, the most economical way of satisfying the requirements of both the TNS and WH features on C is simply to move the tensed interrogative word *who* to spec-C, thereby deriving:

(20) [$_{CP}$ Who [$_C$ Ø] [$_{TP}$ ~~who~~ [$_T$ Af] [$_{vP}$ [$_v$ go] [$_{VP}$ ~~who~~ [$_v$ ~~go~~] there]]]]

The affix in T can then be lowered onto the immediately subjacent verbal head *go* in v, thereby ultimately deriving the structure associated with *Who goes there?* and correctly predicting that no auxiliary inversion takes place in matrix subject questions.

Ingenious though Pesetsky and Torrego's analysis is, one aspect of it which proves problematic in the light of more recent work in Minimalism is their assumption that the TNS and WH features on C in a finite clause can jointly be satisfied by fronting a wh-subject which acquires a tense value from T and moves to spec-T *before* wh-movement applies. This implicitly assumes a strictly bottom-up cyclic account of syntactic operations within a phase which has been argued against by Chomsky in more recent work (2005, 2006) in favour of an approach under which different probes within a phase can activate in a random order – either sequentially or simultaneously. To illustrate the problem, consider what happens after a series of merger operations on the CP phase have produced the following structure:

(21)   [$_{CP}$ [$_C$ Ø$_{TNS, WH}$] [$_{TP}$ [$_T$ Af$_{TNS}$] [$_{vP}$ [$_v$ Ø] [$_{VP}$ who$_{WH}$ [$_V$ go] there]]]]

The light verb will attract the verb *go* to raise from V to v. In relation to T and C, let us suppose that (as Pesetsky and Torrego assume), T probes first and C next. The tense affix in T will probe and locate *who* as its goal, assigning it a tense feature and attracting a copy of it to move to spec-T, so deriving the structure shown below:

(22)   [$_{CP}$ [$_C$ Ø$_{TNS, WH}$] [$_{TP}$ who$_{WH, TNS}$ [$_T$ Af$_{TNS}$] [$_{vP}$ [$_v$ go] [$_{VP}$ who$_{WH, TNS}$ [$_V$ ~~go~~] there]]]]

The TNS feature on C will now probe and look for the closest goal with a tense feature, and the WH feature on C will likewise look for the closest goal with a WH feature. It might at first sight seem as if the copy of *who* in spec-T is both the closest wh-expression and the closest tensed expression, and hence that C will attract *who* to move to spec-C. However, movement of *who* from spec-T to spec-C would violate three constraints proposed in Chomsky (2005). The first is the following:

(23)   **Invisibility Condition** (Chomsky 2005: 16)

'An A-chain becomes invisible to further computation when its uninterpretable features are valued'[10]

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[10] A related constraint is Rizzi's *Freezing Constraint*, one consequence of which is that a constituent moving into a subject position is thereafter frozen in place; see Rizzi and Schlonsky (2005) and Rizzi (2006).

Once the T-probe values the unvalued case feature on *who* as nominative, both copies of *who* become invisible and hence neither is able to undergo wh-movement. The lower copy is also prevented from moving by the further condition below:

(24)   **Visibility Condition**
        Only the highest copy of a moved constituent is visible

(cf. the remark by Chomsky (2005: 16) that 'lower copies are invisible'). A third constraint which bars movement of *who* from spec-T to spec-C is the following:

(25)   **Chain Uniformity Condition** (Chomsky 2005: 18)
        UG permits 'uniform chains – either A-chains or A'-chains – but no mixed chains'

Accordingly, movement of *who* from spec-V through spec-T into spec-C would result in an illicit mixed chain in which the copy of *who* in spec-T occupies an A-position, and the copy in spec-C occupies an A-bar position. The conclusion we reach is thus that the derivation will crash if T probes before C in (22).

Let us therefore consider the alternative possibility that C probes before T in a structure like (22) above. If C probes first, *who* will not yet have acquired a tense value from T, and so will not be a possible goal for the tense feature of C. Accordingly, the WH feature of C will attract *who* to move to spec-C, and the TNS feature of C will attract the tense affix in T to adjoin to C, so deriving the structure shown in simplified form below:

(26)   [$_{CP}$ who [$_C$ Af] [$_{TP}$ [$_T$ ~~Af~~] [$_{vP}$ [$_v$ go] [$_{vP}$ who [$_v$ ~~go~~] there]]]]

But since there is no immediately subjacent verbal stem for it to attach to, the stranded affix in C will have to be supported by *do* – so wrongly predicting that **Who's go there?* is grammatical.

Now consider what happens if C and T probe simultaneously in (22). Since *who* does not have a tense feature at the point of probing, the WH feature on C will attract a copy of

who to move to spec-C, the EPP feature on T will attract a copy of *who* to move to spec-T, and the tense feature on C will attract the tense affix in T to adjoin to C, so deriving the structure (26) above – once more with a stranded affix in C which will require *do*-support (and so again incorrectly predicting that **Who's go there?* is grammatical).

However, one way in which it might seem as if we can salvage Pesetsky and Torrego's analysis is the following. Assuming Chomsky's (2000) account of agreement and movement, let us suppose that probes are active (undeleted, uninterpretable) *features* rather than *categories*: this means that it is not C and T which probe in a structure like (27) below, but rather the (italicised) undeleted uninterpretable features which they carry:

(27) $[_{CP} [_{C} \varnothing_{TNS,\ WH}] [_{TP} [_{T} Af_{TNS,\ AGR,\ EPP}] [_{vP} [_{v} go] [_{VP} who_{WH} [_{V} \text{go}] there]]]]$

If each probe within a phase can in principle act independently of any other, then the AGR(eement) and EPP features on T in (27) can probe independently of each other. Among the possibilities which this allows for is the following. Suppose that the AGR features on T probe first, and locate *who* as a goal, and that in consequence of this T is valued as third person singular via agreement with *who*, and conversely *who* is valued as tensed by agreement with T – as shown informally by the italicised TNS feature on *who* below:

(28) $[_{CP} [_{C} \varnothing_{TNS,\ WH}] [_{TP} [_{T} Af_{TNS,\ AGR,\ EPP}] [_{vP} [_{v} go] [_{VP} who_{WH,\ TNS} [_{V} \text{go}] there]]]]$

Let us further suppose that once the structure in (28) has been formed, the features on C and T then probe simultaneously and locate the tensed interrogative pronoun *who* as their joint goal, and move a copy of it to the edge of CP; at the same time, the EPP feature on T searches for the closest goal with person[11], and attracts another copy of *who* to move to spec-T. Since the TNS feature on C is satisfied (and deleted) via movement of the tensed pronoun *who* to the edge of TP, it does not attract the affix in T to move to C and there is correctly predicted to be no auxiliary inversion or *do*-support in subject

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[11] If we follow Chomsky (2001) in assuming that the EPP feature on T attracts the closest expression carrying person to move to spec-T.

questions. Thus, it would seem as if some variant of Pesetsky and Torrego's analysis can be retained if we suppose that *agreement features* probe before *movement features* (i.e. features like EPP which induce movement). The requirement for agreement features to probe before movement features might be thought to follow from considerations of computational efficiency: for example, if the EPP feature on T in (27) probed before the AGR features on T, *who* would move to spec-T and (given that, in consequence of the Visibility Condition 24, only the highest link in a movement chain is visible), T would then have no goal to agree with, with the result that the agreement features on T would remain unvalued – causing the derivation to crash.

Nonetheless, there is a problem which arises in (28). If economy considerations favour shorter movements over longer ones, an obvious question to ask is why the tense feature on C doesn't attract the tense affix in T rather than the wh-word *who*, since the affix is closer to C than *who*. If this were to happen, the affix would move to C and be spelled out as a form of *do*, so wrongly predicting that root subject questions show *do*-support. However, it may be that we can get around this problem by positing a principle along the following lines:

(29)   **Attraction Principle**

A movement feature on a head can attract either the closest matching minimal projection which it a-commands, or the closest matching maximal projection which it a-commands (*a-command* = asymmetrical c-command)

This would mean that the tense feature on C in (28) can attract either the tense affix in C (because this is the closest minimal projection carrying a tense feature), or the wh-pronoun *who* (because this is the closest maximal projection carrying a tense feature). Since attracting *who* to move to spec-C satisfies both the WH and TNS features on C, economy considerations mean that this single movement is preferred to a more costly derivation (involving two separate movement operations) on which the WH feature on C

attracts *who* and the TNS feature on C attracts the tense affix in T (and thereby triggers auxiliary inversion and *do*-support)[12].

However, even if we attempt to salvage Pesetsky and Torrego's account in the manner indicated above, there remain a number of questionable aspects of their analysis. One of these concerns the apparent anomaly of the TNS feature on C attracting an affix carrying an *interpretable* tense feature in a complement question structure like (30a) below (cf. (15) above), but attracting a wh-word carrying an *uninterpretable* tense feature in a root subject question structure like (30b) (cf. 28 above):

(30) a. [CP [C Ø*TNS, WH*] [TP [T Af**TNS**, *AGR, EPP*] [vP [v Ø] [VP he [v ~~go~~] where]]]]

  b. [CP [C Ø*TNS, WH*] [TP [T Af*TNS, AGR, EPP*] [vP [v Ø] [VP who*WH, TNS* [v ~~go~~] there]]]]

The apparent anomaly is potentially compounded if we adopt Chomsky's *Invisibility Condition* (23). In earlier analyses, such a condition would have meant that a nominal becomes invisible/inert immediately its case feature is valued; but in Pesetsky and Torrego's analysis, what makes a nominal active for an A-operation is having an unvalued tense feature (which is valued by agreement with a tensed T). Given the Invisibility Condition (23), a subject becomes 'invisible to further computation' immediately it agrees with (and is thereby assigned a tense feature by) a tensed T constituent, and hence will be unable to be attracted by C to move to the edge of CP. This undermines any suggestion that agreement must apply before movement in such structures (since agreement renders the affected goal invisible).

Finally, it should be noted that there are also problems with Pesetsky and Torrego's key claim that the subjects of finite clauses are assigned a tense feature which is spelled out as nominative case (see Radford 2007 for a fuller critique). In this connection, consider the following contrast:

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[12] Economy is by no means straightforward to compute, if we follow Donati (2006: 30) in supposing that movement of a head is in general more economical than movement of a phrase (because Economy requires movement of the minimal amount of material required to ensure convergence).

(31) a. A week earlier, they had demanded [that *he* not **be** executed yesterday]

b. They are now demanding [that *he* not **be** executed tomorrow]

In subjunctive clauses like those bracketed in (31), the bold-printed subjunctive verb shows no overt tense contrast, in that the same (seemingly tenseless) form *be* is required in both past and non-past contexts. This suggests that the inflectional head in this kind of subjunctive clause (let us call it INFL for concreteness) is not specified for the present/past tense contrast found in indicative clauses. Moreover, if only a tensed INFL can attract *be* to raise to INFL, the fact that *be* remains positioned below *not* in such structures (and does not raise to the INFL position above *not*) provides an additional reason for thinking that a subjunctive INFL is untensed. But if INFL in subjunctive clauses like those bracketed in (31) is untensed, it seems clear that the nominative case on *he* cannot be a reflex of tense. Similar considerations hold in relation to imperatives for speakers like me who accept sentences such as (32) below with third person subjects:

(32) Don't *he/they* dare contradict me by saying that this sentence is ungrammatical!

If (as Henry 1995 argues) imperatives are tenseless forms (hence the absence of present-tense *-s* on *don't* in (32), even when it has a third person singular subject like *he*), it is clear that nominative case on imperative subjects cannot be a reflex of tense.

Moreover, there is also developmental evidence against any such correlation. For example, Schütze and Wexler (1996) report that two- and three-year-old children alternate between using nominative and accusative subjects with tensed auxiliaries and verbs (saying e.g. *He/Him can swim* and *I/Me went to school*) – and yet this would be entirely unexpected if the subject of a tensed auxiliary/verb were marked as *tensed* and a tensed subject were spelled out as nominative[13].

In short, Pesetsky and Torrego's analysis falls down not only because its assumption that nominative case is a reflex of tense is questionable, but also because it makes problematic assumptions about activation (e.g. that one head valuing a feature on a goal can make it active for another head), and about the order in which different heads probe

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[13] See Nordlinger and Sadler (2004: 632ff.) for data from Australian aboriginal languages which are problematic in other respects for Pesetsky and Torrego's tense-based analysis of nominative case.

within a given phase. In the next section, an alternative account is developed which makes use of the mechanism of (downward feature) *percolation* sketched in Chomsky (2005, 2006) and Miyagawa (2005, 2006).

4.   A *percolation* account

In recent work, Chomsky (2005, 2006) and Miyagawa (2005, 2006) have argued that the head of a phase is the locus not only of peripheral features (e.g. discourse-related features such as focus/topic) but also of agreement features. Evidence for C being the locus of agreement features comes from the phenomenon of complementiser agreement discussed in Rizzi (1990), Haegeman (1992), Boeckx (2003), Carstens (2003), Kornfilt (2004), Miyagawa (2005) and Henderson (2006). Reviewing relevant evidence, Miyagawa (2005) concludes that 'Agreement occurs on C (universally)'. However, he maintains (p. 4) that (in Indo-European languages) 'Agreement on C may percolate down from C' onto a head below C. Let us suppose that (for economy reasons) agreement features only percolate down the minimum distance required in order to ensure convergence. If we adopt the *split CP* analysis outlined in §2 (but use the more familiar label C to designate the highest Force/Clause-Type head), this means that in root questions, the agreement features will percolate down from C onto Foc if this leads to convergence, but will percolate further down onto T if it does not. If an uninterpretable feature on a root phase head cannot be deleted (because deletion only takes place on the next highest phase, and there is no higher phase in a root clause), it follows that the agreement features on a root C *must* percolate down onto a subordinate head, because any undeleted uninterpretable feature on a root C will cause a crash at the semantics interface.

In the light of these assumptions, consider the derivation of a root subject question like (17) *Who goes there?* Let us suppose that a series of Merger operations on the CP phase (and raising of V to adjoin to the affixal light verb *v*) generate the following structure (with AGR denoting agreement features, FOC a focus feature, and TNS a tense feature):

(33)   [CP [C ∅AGR] [FocP [Foc ∅FOC, EPP] [TP [T ∅TNS] [vP [v go] [VP who [v ~~go~~] there]]]]]

Let us also assume that any head carrying focus or agreement features (after percolation) can also carry a selective EPP feature which enables it to attract the closest matching goal (i.e. the closest goal carrying a matching set of agreement/focus features), so that a selective EPP feature works 'in tandem with' other features on the head (to use the terminology of Miyagawa 2005, 2006)[14]. The phase head C/Force enters the derivation carrying agreement features, and the Focus head carries a focus feature. Percolation of agreement features from C onto Foc will yield the following structure (assuming that a peripheral head carrying focus and/or agreement features in English also carries an EPP feature):

(34) $[_{CP} [_{C} \varnothing] [_{FocP} [_{Foc} \varnothing_{AGR, FOC, EPP}] [_{TP} [_{T} \varnothing_{TNS}] [_{vP} [_{v} go] [_{VP} who [_{V} \text{go}] there]]]]]$

In such a structure, the EPP feature will work in tandem with the agreement and focus features on Foc to attract a single goal which is both (i) the closest agreeing goal, and (ii) the closest focused goal[15]. In this case, *who* is the appropriate goal (since it is focused by virtue of asking for new information, and is active for agreement by virtue of having an unvalued case feature), and so a copy of *who* moves to spec-Foc. If T has no EPP feature when it has no uninterpretable features, no copy of *who* will move to spec-T[16]. Thus, movement of *who* to spec-Foc derives the following structure:

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[14] More precisely, EPP is generally assumed to work in tandem with other *uninterpretable* features on a head (though if Rizzi 2006 is right in claiming that the Q-feature on an interrogative C is interpretable, it may be that EPP can work in conjunction with an interpretable Q-feature). What is here designated as a FOC feature is arguably an uninterpretable A-bar agreement feature, with the relevant head being assigned a focus feature via agreement with the focused constituent *who*: see Radford (2004: §10.10) on A-bar agreement. Chomsky (2005, 2006) argues that wh-movement is driven by an unselective Edge Feature which can in principle attract any constituent to move to the edge of the projection housing the probe. This is supported by the absence of superiority effects in wh-questions in German, he maintains – though (as Bob Borsley points out) Featherston (2005) argues that German does have superiority effects, albeit weaker than in English. Miyagawa (2006) argues for the existence of both selective and unselective edge features, noting that a selective (but not an unselective) edge feature works in tandem with agreement and is subject to locality/superiority constraints. I have followed Miyagawa (2005) in using the more traditional term *EPP feature* here.

[15] This may be because T only allows a single EPP feature in a language like English, so it is not possible for T to have two EPP features, one of which works in tandem with the focus feature on T to raise a (focused) constituent to spec-T, and the other of which works in tandem with the agreement features on T to also raise a separate (agreeing) constituent into a second spec-T position.

[16] By contrast, if (as in Chomsky 2001) T in English is assumed to always carry a person feature and an EPP feature, a second copy of *who* will be moved to spec-T.

(35) $[_{CP} [_C \varnothing] [_{FocP}$ Who $[_{Foc} \varnothing_{AGR, FOC, EPP}] [_{TP} [_T \varnothing_{TNS}] [_{vP} [_v$ go$] [_{VP}$ who $[_v$ go$]$ there$]]]]]$

The resulting structure is then 'handed over' to the PF component. Morphological spellout requirements mean that inflectional features must be spelled out on a single head in a language like English which is *monosuffixal* (in the sense of Aronoff and Fuhrhop 2002). Assuming that tense and agreement are inflectional features, this consideration forces the agreement features on Foc to be united with the tense feature on T. Let us make the assumptions outlined below about how the PF component deals with inflectional features in a monosuffixal language like English:

(36)   **PF realisation of inflectional features in a monosuffixal language**
    In a monosuffixal language like English, all inflectional features (of a given type) must end up on a single head. In the PF component, inflectional features stranded on a superordinate head percolate down onto an immediately subjacent head wherever possible, but if percolation is blocked by an intervening visible constituent, the (inflectional features on the) immediately subjacent head will instead be raised to adjoin to the superordinate head[17].

Given (36), and given that no visible constituent intervenes between Foc and T in (35), the agreement features on Foc will percolate down onto T in the PF component. The further PF requirement for the tense and agreement features on the affix in T to be realised on a lexical verb in turn forces (the tense and agreement features on) T to lower onto the verb *go* in v (with the consequence that *go* is ultimately spelled out as *goes*)[18].

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[17] It may be necessary to stipulate that only certain types of intervening constituent block lowering. It may also be that raising can only happen when the superordinate head is 'strong'. I set these issues aside here, together with the question of what precise properties make a head *strong*.

[18] A question which is raised (but not answered) by the analysis outlined here is why 'lowering' (but not 'raising') should be blocked by an intervening visible constituent. A further question which arises here is whether the traditional 'Affix Hopping' operation can be seen as an instance of *percolation* which applies in the PF component rather than in the syntax. A detail set aside here is how *who* comes to be assigned nominative case. Numerous possibilities can be envisaged (e.g. the agreement features on a peripheral head in a finite clause can assign nominative case) but will not be pursued here.

Now consider what happens in a root complement question such as (14) *Where (doe)s he go?* Suppose that a series of Merger operations on the CP phase (and raising of V to v) generate the following structure:

(37)   [$_{CP}$ [$_C$ Ø$_{AGR}$] [$_{FocP}$ [$_{Foc}$ Ø$_{FOC, EPP}$] [$_{TP}$ [$_T$ Ø$_{TNS}$] [$_{vP}$ [$_v$ go] [$_{VP}$ he [$_V$ ~~go~~] where]]]]]

Percolation of agreement features from C onto Foc will yield the following structure:

(38)   [$_{CP}$ [$_C$ Ø] [$_{FocP}$ [$_{Foc}$ Ø$_{AGR, FOC, EPP}$] [$_{TP}$ [$_T$ Ø$_{TNS}$] [$_{vP}$ [$_v$ go] [$_{VP}$ he [$_V$ ~~go~~] where]]]]]

As before, the EPP feature works in tandem with the agreement and focus features on Foc to look for a constituent which is both the closest agreeing goal and the closest focused goal. However, there is no such single goal in (38) above (*he* being the closest agreeing goal and *who* the closest focused goal), so the derivation ultimately crashes.

As noted earlier, theoretical considerations require uninterpretable features on a root phase head to percolate down onto a subordinate head, but economy considerations dictate that they percolate only the minimal distance required to achieve convergence. Since percolation of the AGR features on C in (37) onto Foc does not yield a convergent outcome, the relevant agreement features percolate further down onto T, so deriving the structure in (39) below, if we assume that T (like Foc) has an EPP feature when it also has one or more uninterpretable features:

(39)   [$_{CP}$ [$_C$ Ø] [$_{FocP}$ [$_{Foc}$ Ø$_{FOC, EPP}$] [$_{TP}$ [$_T$ Ø$_{TNS, AGR, EPP}$] [$_{vP}$ [$_v$ go] [$_{VP}$ he [$_V$ ~~go~~] where]]]]]

The EPP and focus features on Foc will work in tandem to attract the closest focused constituent (= *where*) to move to spec-Foc. The EPP and agreement features on T will work in tandem to attract the closest agreeing goal (= *he*, active by virtue of its unvalued case feature) to move to spec-T, so deriving the following structure (irrespective of whether T and C probe sequentially or simultaneously):

(40) [CP [C Ø] [FocP Where [Foc ØFOC, EPP] [TP he [T ØTNS, AGR, EPP] [vP [v go]

[vP ~~he~~ [v ~~go~~] ~~where~~]]]]]]

What might then be expected to happen (given what has been said so far) is for the tense/agreement features on T to lower onto v in the PF component. However, this would wrongly predict that *Where he goes?* is grammatical. So, something must rule out the derivation in (40). But what?

A key assumption in the answer I will suggest here is that the FOC feature carries by the Focus head is an (abstract) inflectional feature, and (given the monosuffixal nature of English) it has to be united with the (tense/agreement) inflectional features on T. This is not surprising if (as suggested in fn.14) the FOC feature is an agreement feature (in the sense of Radford 2004: §10.10), with the relevant head being assigned a focused or interrogative value via agreement with the focused interrogative constituent *who*. If the focus feature is indeed an agreement feature, it is plausible to treat it as an inflectional feature – and indeed there is some cross-linguistic evidence in support of doing so. Thus, Green (2007: 9) notes that in Hausa, (perfect and imperfect) aspectual auxiliaries exhibit a special focus form in focus structures (including wh-questions). Accordingly, the italicised auxiliary in (41) below is a third person plural perfective *focus* form (the corresponding non-focused form being *sun*):

(41) Wằ yârā sukà ganī?
     who children AUX see
     'Who did the children see?'

Green (2007: 98) argues that the focus marking on the auxiliary/INFL constituent 'represents the spellout or morphological realisation of the features inherited by INFL from F' (i.e. from the Focus head): in other words, she assumes that a focus feature percolates down from Foc onto INFL. Since Foc attracts an interrogative goal in questions, it is plausible to suppose that Foc may carry an interrogative inflection in interrogative clauses[19]: this would account for why in some languages (e.g. West

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[19] This would self-evidently be the case if the attracting head were Q rather than Foc.

Greenlandic: see Sadock 1984) verbs are inflected for interrogative mood in wh-questions. There are potential parallels here with Rizzi's (1996) analysis of interrogative inversion, under which interrogative inversion involves an *interrogative* auxiliary adjoining to a peripheral head which attracts an interrogative wh-expression to become its specifier, thereby satisfying the *criterion*-based requirement for an interrogative head to have an interrogative specifier (and vice-versa).

If the head Foc constituent in (40) carries an (abstract) inflectional focus feature, and morphological requirements in a monosuffixal language like English require the relevant feature to be united with the tense/agreement features on T, we might simply expect the inflectional feature on Foc to lower onto T, and the tense/agreement/focus features on T then to lower onto the verb in v. However (as already noted), the resulting sentence *Where he goes?* is ungrammatical, and so it is clear that something must block any such derivation. But what? The account of inflectional morphology given in (36) presupposes that the inflectional feature on Foc will be prevented from being lowered onto T in the PF component by the presence of an intervening visible constituent. The Visibility Condition (24) tells us that only the highest copy of a moved constituent is visible: hence, *he* is a visible constituent in (40) by virtue of being the highest copy of the A-chain containing it. Accordingly, the inflectional features on T must be raised to Foc as a last resort, as the only licit way of unifying the (tense/agreement) inflectional features on T with the (focus/interrogative) inflectional feature on Foc, so deriving the structure:

(42) $[_{CP} [_{C} \emptyset] [_{FocP}$ Where $[_{Foc} \emptyset_{FOC, TNS, AGR}] [_{TP}$ he $[_{T} \emptyset] [_{vP} [_{v}$ go]

$\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad [_{VP}$ ~~he~~ $[_{V}$ ~~go~~] ~~where~~]]]]]

Since the TNS, AGR and FOC inflectional features on the Focus head are stranded without a verbal stem to attach to, *do*-support is used as a (last resort) way of spelling them out, with the resulting structure ultimately being realised as the grammatical sentence *Where (doe)s he go?*

While the *percolation* analysis in this section accounts for the absence of auxiliary inversion and *do*-support in root subject questions like *Who goes there?* and the presence

of both in other types of question like *Where* (*doe*)*s he go?* it nonetheless has potential theoretical and empirical drawbacks. One theoretical question it raises relates to the uncertain status of *percolation*, which might seem to amount to the re-introduction of feature-movement operations which were barred in Minimalism post-Chomsky (1995): cf. the claims by Chomsky (2000: 119) and Donati (2006: 23) that 'Features cannot move.' Other questions about percolation also arise: for example, if economy considerations mean that agreement features are 'first' lowered onto Foc and 'then' (if this does not lead to convergence) lowered onto T, questions about computational efficiency arise, since lowering agreement features onto Foc will lead to a non-convergent outcome in the case of interrogatives which are not root subject questions. Moreover, if percolation from C to T applies in a counter-cyclic fashion (with the agreement features being lowered first from C onto Foc and then from Foc onto T), there is something conceptually odd about saying that Foc initially acquires a set of agreement features from C, then (through percolation) hands them over to T, and subsequently (via T-to-Foc movement) gets them back again[20].

In addition, there are potential empirical problems posed by any analysis which assumes that a root subject question incorporates a TP contained within a separate FocP. One such problem is posed by the possibility of *have*-cliticisation in sentences such as the following:

(43)   Who've been your sternest critics?

Radford (1997, 2004) argues that *have*-cliticisation is subject to a strict adjacency requirement to the effect that *have* can only cliticise onto an immediately adjacent word[21]. This strict adjacency condition accounts for contrasts such as:

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[20] This objection can be overcome if C 'hands over' its agreement features directly to T via percolation in the relevant structures, and Foc then attracts T.

[21] Note that *have*-cliticisation is much more selective (i.e. subject to much more stringent conditions of application) than some other forms of auxiliary cliticisation: e.g. the clitic form *'s* of *is/has* can unselectively cliticise to any adjacent kind of host across one or more intervening empty categories, as in *Who do you think's telling the truth?*

(44) a. They've always had a shower every morning before going to work

    b. *Every morning, they've a shower before going to work

In (44a), *have* is in T and can cliticise onto the immediately adjacent subject *they* in spec-T. But in (44b), *have* is in v and is prevented from cliticising onto *they* in spec-T by the presence of a null T constituent intervening between the two[22]. Now, if a wh-subject question like (43) had a structure along the lines of (45) below,

(45) $[_{CP} [_{C} \varnothing] [_{FocP}$ who $[_{Foc} \varnothing] [_{TP} [_{T}$ have] been your sternest critics]]]

the *adjacency condition* would mean that the intervening empty Foc constituent would block *have* from cliticising onto *who*. However, the fact that (43) is grammatical casts doubt on the descriptive adequacy of any variant of the FocP+TP analysis of subject questions (irrespective of whether it is assumed that there is a copy of the subject only in spec-Foc, or both in spec-Foc and spec-T)[23]. The next section therefore explores an alternative analysis which aims to overcome this problem.

5. A *syncretism-based* account

Miyagawa (2005) claims that topic/focus features are projected on T in languages like Japanese, but are projected on an independent superordinate functional head above T in languages like English (the relevant superordinate head corresponding to C in traditional analyses of clause structure, but to independent Topic/Focus heads under the split CP analysis of the clause periphery in Rizzi 1997). Miyagawa's idea can be related to the claim in Rizzi (1997) that adjacent peripheral heads are syncretised under certain conditions: for example, Rizzi maintains that Force and Finiteness heads are syncretised

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[22] It cannot be the null copy of *they* in spec-v intervening between T and v which blocks contraction, if the Visibility Condition (24) renders lower copies invisible.

[23] One way in which this problem could seemingly be overcome would be if we supposed that *who* moves into Foc rather than into spec-Foc. If the edge feature on Foc simply requires movement of a focused expression to the *edge* (i.e. specifier or head) position of FocP, this would appear possible. By virtue of being both a minimal and a maximal projection, *who* would be able to move either into the head or the specifier position of FocP, and we might suppose that economy considerations would favour movement to the head position where possible. However, whether adjoining a wh-word to Foc produces a structure which is in any objective sense more economical than one creating a new specifier position is debatable. Likewise, it is debatable whether a head which is not a clitic is able to adjoin to another head.

in English whenever immediately adjacent (i.e. when not separated by an intervening projection such a FocP or TopP). Let us therefore assume that UG allows for a focus or topic feature to be expressed either on a separate peripheral head, or to be syncretised with T. What I shall argue here is that syncretising focus on T only yields a grammatical outcome in root subject questions, and that focus must be projected on a separate head in other types of question in order to yield a grammatical outcome[24].

To see how the syncretism analysis works, consider first of all what happens if focus is syncretised with T (so that T carries both focus and agreement features). More particularly, consider what happens in a root subject structure such as that below (assuming that the verb *go* raises to v):

(46) $[_{CP}\ [_C\ \emptyset]\ [_{TP}\ [_T\ \emptyset_{TNS,\ AGR,\ FOC,\ EPP}]\ [_{vP}\ [_v\ go]\ [_{VP}\ who\ [_v\ \text{go}]\ there]]]]$

On the assumption that the EPP feature on T in English is selective and operates in tandem with other uninterpretable features carried by T to trigger movement of a single goal which matches the other sets of uninterpretable features carried by T, the EPP feature on T in (46) will operate in tandem with the agreement and focus features on the affix to attract a single goal which is (i) the closest goal agreeing with the affix, and (ii) the closest focused goal. Since *who* satisfies both criteria by virtue of being the closest (and only) focused goal and also the closest (and only) agreeing goal, *who* moves to spec-T, deriving (47) below:

(47) $[_{CP}\ [_C\ \emptyset]\ [_{TP}\ who\ [_T\ \emptyset_{TNS,\ AGR,\ FOC,\ EPP}]\ [_{vP}\ [_v\ go]\ [_{VP}\ \text{who}\ [_v\ \text{go}]\ there]]]]$

Since no visible constituent intervenes between T and v in (47), the (T-affix containing) the inflectional features on T will lower onto the verb *go* in the PF component, so eventually deriving *Who goes there?*

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[24] I shall implicitly assume here that syncretism is lexical in nature (in that UG allows features to be assembled into lexical items in a variety of ways), rather than the result of some syntactic operation by which (e.g.) Foc and T are initially projected as separate heads in the syntax, and then somehow subsequently conflated as a single head – perhaps via Head Movement, as hinted at in Rizzi (2006: 128, fn. 8).

Next consider what will happen if Foc is syncretised on T in a complement question structure such as the CP bracketed below:

(48)   I wonder [$_{CP}$ [$_C$ Ø] [$_{TP}$ [$_T$ Ø$_{TNS, AGR, FOC, EPP}$] [$_{vP}$ [$_v$ go] [$_{VP}$ he [$_v$ ~~go~~] where]]]]]

The resulting structure will crash because T has a single EPP feature which needs to attract a single goal which both agrees with the affix in T and is focused. However, since there is no single goal which can satisfy the requirements of both the agreement and focus features on of T (*he* being the closest agreeing goal and *who* being the closest focused goal), the derivation crashes. More generally, syncretism of Foc with T will only yield a convergent outcome in a root subject question.

Now consider what happens if focus is not syncretised with T and is instead projected on a separate Foc head. Let's look first at what happens in an (unsyncretised) complement question structure such as:

(49)   [$_{CP}$ [$_C$ Ø] [$_{FocP}$ [$_{Foc}$ Ø$_{FOC, EPP}$] [$_{TP}$ [$_T$ Ø$_{TNS, AGR, EPP}$] [$_{vP}$ [$_v$ go] [$_{VP}$ he [$_v$ ~~go~~] where]]]]]]

The EPP and agreement features on T will work in tandem to attract a copy of the closest expression which T agrees with (= *he*) to move to spec-T. Likewise, the EPP and focus features on Foc will work in tandem to attract a copy of the closest focused expression (= *who*) to move to spec-Foc, so deriving the structure:

(50)   [$_{CP}$ [$_C$ Ø] [$_{FocP}$ where [$_{Foc}$ Ø$_{FOC, EPP}$] [$_{TP}$ he [$_T$ Ø$_{TNS, AGR, EPP}$] [$_{vP}$ [$_v$ go]

[$_{VP}$ ~~he~~ [$_v$ ~~go~~] ~~where~~]]]]]

Given that English is a monosuffixal language, the (interrogative) inflectional feature on Foc will have to be united with the (tense-agreement) inflectional features on T in the PF component. Since the intervening pronoun *he* is visible (by virtue of being the highest copy in the associated A-movement chain), it blocks Foc-to-T lowering in accordance with (36), and so (the inflectional features on) T will raise to Foc as a last resort. This

will leave the relevant inflectional features stranded in Foc, so requiring *do*-support and thereby correctly specifying that *Where (doe)s he go?* is grammatical.

Finally, consider what happens in an (unsyncretised) root subject question structure such as:

(51) [CP [C Ø] [FocP [Foc ØFOC, EPP] [TP [T ØTNS, AGR, EPP] [vP [v go] [VP who [v go] there]]]]]

The EPP feature on T will attract a copy of the closest expression it agrees with (namely, *who*) to move to spec-T, and likewise the EPP feature on Foc will attract a separate copy of the closest focused expression (again, *who*) to move to spec-Foc, so deriving the structure:

(52) [CP [C Ø] [FocP <u>who</u> [Foc ØFOC, EPP] [TP *who* [T ØTNS, AGR, EPP] [vP [v go]

[VP who [v go] there]]]]]

Although the italicised copy of *who* intervenes between Foc and T, it is not the highest copy of *who* (the highest copy of *who* being the one which is underlined), and hence the Visibility Condition (24) renders the italicised copy of *who* invisible. Accordingly, there is no intervening visible constituent to prevent the inflectional FOC feature being lowered from Foc onto T, with the FOC and AGR features on T in turn being lowered onto the verb in v, ultimately deriving *Who goes there?*

What this would mean is that a root subject question like *Who goes there?* can either have a syncretised structure like (47), or an unsyncretised structure like (52). Since root subject questions do not appear to be structurally ambiguous, we might therefore wonder if one of the two structures is ruled out for independent reasons.

One possibility is that economy considerations favour a syncretised structure like (47) over an unsyncretised structure like (52): perhaps standard varieties of English prefer *do*-less structures over *do*-structures (wherever possible) for reasons of *morphological economy* (Freidin 2004: 119), or because they contain fewer lexical items (Schütze 2004), or because they contain fewer functional projections.

A second possibility is to suppose that the derivation of structures like (54) leads to violation of some UG principle(s). One way in which a structure like (54) might arise would be via successive movement of *who* from spec-V to spec-T, and then from spec-T to spec-Foc. However, movement from spec-T to spec-Foc is ruled out by the *Invisibility Condition* (23): this is because once *who* moves to spec-T, its uninterpretable case feature is valued, and it thereby becomes invisible to a higher probe. Movement from spec-T to spec-Foc is also ruled out by the Chain Uniformity Condition (24), in that it leads to the formation of a mixed chain with one link in an A-position (spec-T) and another in an A-bar position (spec-Foc)[25].

Another way of generating a structure like (52) would be (as in Chomsky 2005) for one copy of *who* to move from spec-V to spec-T (as shown by the dotted arrow below) thereby forming an A-chain, and for a separate copy of *who* to move from spec-V to spec-Foc (as shown by the solid arrow below) thereby forming a separate A-bar chain:

(53) $[_{CP} [_{C} \varnothing] [_{FocP} \underline{who} [_{Foc} \varnothing_{FOC, EPP}] [_{TP} \textit{who} [_{T} \varnothing_{TNS, AGR, EPP}]$
$[_{vP} [_{v} go] [_{VP} \textbf{who} [_{V} go] there]]]]]]$
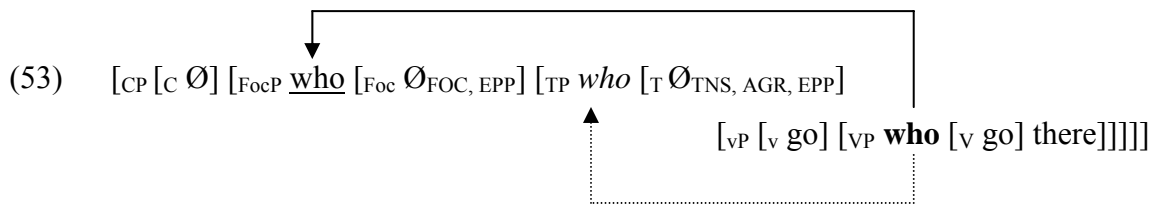
One way in which we could rule out a derivation such as (53) would be to suppose that the ban on *mixed* chains imposed by the Chain Uniformity Condition (25) subsumes a ban on *interlinked* chains (i.e. structures in which a given constituent is a link in two or more different movement chains). Such a condition would rule out a structure like (53) because the bold-printed occurrence of *who* in spec-V is a link at the foot of two different movement chains – namely (i) an A-chain whose head is in spec-T, and (ii) an A-bar chain whose head is in spec-Foc[26].

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[25] It is further ruled out by the *Freezing Constraint* of Rizzi (2006), one consequence of which is that a constituent moving into a subject position is thereafter frozen in place.

[26] A constraint against chain interlinking would have important repercussions for the analysis of more complex structures such as:

(i)　　$[_{CP}$ Who $[_{C}$ is$]$ it now thought $[_{CP} [_{C} \varnothing] [_{TP} [_{T}$ doesn't$]$ appear $[_{TP} [_{T}$ to$]$ have been chosen$]]]]$

Who would originate as the complement of *chosen*. If chain interlinking is barred, *who* would undergo A-bar movement to the embedded spec-C, followed by A-bar movement to the root spec-C. It would not be

The core claim of the *syncretism* analysis outlined in this section is that focus can either be syncretised on T head (yielding a grammatical outcome only in root subject questions without auxiliary inversion) or projected as a separate head (yielding a grammatical outcome in other types of questions with auxiliary inversion). The syncretism analysis provides a straightforward account of the possibility of *have*-cliticisation in root subject questions like (43) *Who've been your sternest critics?* If T and Foc are syncretised, (43) will have the structure shown in simplified form below:

(54)   [$_{CP}$ [$_C$ Ø] [$_{TP}$ who [$_T$ have] been your sternest critics]]

The strict adjacency between *have* and *who* in (54) allows *have* to cliticise onto *who*, so correctly specifying that (43) is grammatical.

Although the discussion in this section has considered the possibility of syncretising Focus with T, there is evidence (from contrasts like that below) that a null C/Force head can also be syncretised with T:

(55) a.   *Who do you think that is telling the truth?
      b.   Who do you think is telling the truth?

Let us suppose that the presence of the overt complementiser *that* in (55a) prevents C from being syncretised with T, with the consequence that C and T are projected as separate heads. This means that prior to (A/A-bar) movement, the complement clause in (55a) has the structure shown in simplified form below:

(56)   [$_{CP}$ [$_C$ that] [$_{TP}$ [$_T$ is] [$_{vP}$ who [$_v$ telling] the truth]]]

The constraint against chain interlinking prevents C and T from attracting the same goal *who*; and the Invisibility Condition (23) together with the ban on *mixed chains* imposed

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possible for *who* to also undergo A-movement, and thereby move to become the specifier of the TP headed by *to*, and thereafter to become the specifier of the TP headed by *doesn't*. The finite auxiliary *doesn't* would agree with the original copy of *who* which is the complement of *arrested*. I shall not attempt to explore the wider ramifications of a ban on chain interlinking here.

by the Chain Uniformity Condition (25) prevents *who* from moving first from spec-v to spec-T, and then from spec-T to spec-C (movement from spec-T to spec-C also being barred by Rizzi's 2006 *Freezing Constraint*). Accordingly, (56) has no grammatical outcome, and (55a) is correctly specified to be ungrammatical. Thus, the syncretism analysis accounts for *that-trace* effects[27] (though see Sobin 2002 and Rizzi and Schlonsky 2005 for discussion of complicating factors).

However, if a null C head can be syncretised with T, the complement clause in (55b) will have the structure shown below:

(57) $[_{TP} [_{T} \text{ is}] [_{vP} \text{who} [_{v} \text{ telling}] \text{ the truth}]]]$

Assuming that the syncretised C-T head carries both the kind of feature which enables C to attract an interrogative goal and the kind of feature which enables T to attract an agreeing goal, the syncretised C-T head will attract a goal which is the closest agreeing expression and which is also the closest interrogative expression, identifying *who* as the only such goal. Accordingly, *who* can move to the edge of TP, and will be able to be extracted out of TP to move into appropriate landing sites in the matrix clause at later stages of derivation[28].

The syncretism analysis also provides us with the basis for developing an account of the subject-object asymmetry found in relative clauses such as those bracketed below, in standard varieties of English:

(58)   a.   I met someone [that you know well]

        b.   I met someone [you know well]

(59)   a.   I met someone [that knows you well]

        b.   *I met someone [knows you well]

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[27] Rizzi (2006: 129, fn. 2) proposes a related *Truncation* analysis under which the complement clause in sentences like (55b) is said to have a truncated structures involving (inter alia) truncation of the CP layer.
[28] If uninterpretable features on a root head cannot be deleted (and so cause the derivation to crash), it follows that C cannot be syncretised with T in root clauses, since this results in the syncretised C-T head being a root head carrying uninterpretable agreement features.

In (58a, b), the bracketed relative clauses are unsyncretised CP constituents containing a relative operator on the edge of CP, and the operator is null (and the complementiser in 58b is likewise null) because relevant constituents on the edge of an unsyncretised CP can have a null spellout in English. In (59a), the bracketed relative clause is again an unsyncretised CP (the presence of the overt complementiser *that* blocking syncretism), and the operator on the edge of CP can again be given a null spellout. However, in (59b) the relative clause is a syncretised CP-TP structure, and if we suppose that a syncretised CP-TP constituent does not allow an operator on its edge to have a null spellout (perhaps for the same reason as T does not allow a null subject English), we can account for the contrast in (58)–(59) above[29].

The assumption that C/Force can be syncretised with T also has implications for the analysis of embedded subject questions such as that italicised below (given the arguments in §2 that a preposed interrogative expression becomes the specifier of a C/Force head in embedded clauses):

(60)   I wonder *who is telling the truth*

It implies that C can be syncretised with T in such cases. If so, the italicised complement clause in (60) will have the structure shown below prior to movement:

(61)   [TP [T be] [vP who [v telling] the truth]]

The syncretised C-T head will then attract the closest agreeing interrogative goal (namely *who*) to become its specifier, thereby ultimately deriving *who is telling the truth*. For reasons which should be apparent, the syncretism analysis also predicts that *have*-cliticisation should be possible in sentences such as:

(60)   I wonder *who've been his sternest critics*

And this prediction turns out to be correct.

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[29] Other subject-object asymmetries pointed out by Agbayani (2006: 73–74) can also be accommodated within the syncretism analysis, in ways I lack the space to explore here.

6.   Summary

This paper began by outlining Rizzi's (1997) view of auxiliary inversion as movement to the head of a Focus projection. It then turned to consider why root subject questions do not show inversion, highlighting theoretical and empirical shortcomings in Pesetsky and Torrego's (2001) tense-based analysis. It went on to propose a *percolation* analysis, under which (for economy reasons) the agreement features on C percolate down onto the closest subjacent head which will lead to convergence, with percolation onto Foc yielding convergence in root subject questions, and percolation onto T yielding convergence in other structures. However, the percolation analysis was seen to be based on some potentially problematic theoretical assumptions, and to face the empirical problem of failing to account for the possibility of *have*-cliticisation in sentences like *Who've been your sternest critics?* An alternative *syncretism* analysis was proposed under which focus can either be syncretised with T (yielding a convergent outcome in root subject questions) or projected as a separate head (yielding convergence in other structures). It was also argued that the syncretism analysis can provide a plausible account of *that-trace* effects if it is supposed that C and T can also be syncretised[30].

Acknowledgments

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[30] Numerous questions arise from the analyses presented above, to which I have not provided any satisfactory answer here. One concerns the relation between auxiliary inversion in wh-questions and in other types of structure (e.g. negatives, comparative correlatives etc: see Green and Morgan 1996, Culicover and Jackendoff 1999, Fillmore 1999, Sobin 2003 and Maekawa 2007). A second relates to the question of what exactly an *inflectional feature* is (and how such features differ from other kinds of feature). A third relates to the nature of *percolation* (and indeed *syncretism*) – e.g. under what conditions they are possible, impossible or required.

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Agreement Features, Indeterminacy and Disagreement

Louisa Sadler

Syntactic features such as CASE and NOUNCLASS show the property of indeterminacy, which means that an adequate account of their syntactic behaviour is elusive if such attributes have simple atomic values. Dalrymple and Kaplan (2000) proposed an approach in LFG in which the values of such features are closed sets. Subsequent work (Dalrymple et al., 2006) argues that the set-based approach should be replaced by a feature-based approach in which (potentially) indeterminate features take complex features (that is, f-structures) as values. This paper considers three further morphosyntactic phenomena which can be given a simple treatment using complex features: Rumanian gender, Jingulu disagreement and Hopi constructed duals.

1 Introduction

Forms that are indeterminately specified for the value of a feature can simultaneously satisfy conflicting requirements on that feature, and the treatment of such forms has generated large amounts of work in a variety of frameworks (Zaenen and Karttunen, 1984; Pullum and Zwicky, 1986; Ingria, 1990; Bayer, 1996; Dalrymple and Kaplan, 2000; Levy and Pollard, 2001; Blevins, 2001).

In recent work, Dalrymple et al. (2006) re-examine the formal encoding of indeterminacy of syntactic features in LFG focusing on case indeterminacy as an exemplar of the phenomenon. They argue that the existing set-based approach to indeterminacy of syntactic features should be replaced by a feature-based approach which is formally simple and does not require the postulation of special structures or objects in the representation of case or other indeterminate features. In the proposed approach, the value of a (potentially) indeterminate feature such as CASE is neither atomic nor a set (as in the previous approach of Dalrymple and Kaplan (2000)) but a complex feature, that is, an f-structure.

In subsequent work, Dalrymple et al. (2007) show how the feature-based approach can also provide an account of indeterminacy in a feature such as GEND, which introduces an additional complication because agreement features such as GEND undergo resolution. That is, in a coordinate structure, the GEND and PERS features of coordinate NP are "calculated" from the values of the GEND and PERS features of f-structures corresponding to the conjuncts. A set-based approach is

also introduced in Dalrymple and Kaplan (2000) for the representation of the non-distributive, resolving agreement features, and resolution is captured by set union. Dalrymple et al. (2007) provide an alternative to set-based resolution.

The current contribution extends this line of work showing how a complex feature-based approach can account for some other, non-standard agreement phenomena involving indeterminacy, underspecification and or apparent disagreement. Section 2 reviews the phenomenon of morphosyntactic indeterminacy, the existing set-based LFG treatment of it and the problems with it. In Section 3 I outline the alternative feature-based approach currently being developed. The three following sections discuss Rumanian gender, Jingulu disagreement and Hopi constructed duals, showing how an account of each can be given using a complex feature-based approach.

2 Feature indeterminacy by marker sets

Plural nouns in German provide a convenient and frequently used simple illustration of the syntactic ramifications of indeterminacy in morphosyntactic features, here the CASE feature. Indeterminate forms can appear in positions in which more than one different requirement is imposed, apparently satisfying conflicting requirement. The German masculine plural noun *Papageien* 'parrots' is indeterminate for case,[1] and can appear as the object of verbs in a coordinate construction in which one verb requires accusative case and another requires dative case, as in (1).

(1) *Er findet und hilft   Papageien.*
    he finds and helps parrots
        ACC   DAT ACC/DAT/NOM/GEN

    He finds and helps parrots.   (German)

Dyła (1984) shows that the Polish interrogative pronoun *kogo* 'who' is indeterminately accusative and genitive, and can be the object of an accusative-taking and a genitive-taking verb at the same time, as in (2), while a form such as *co* 'what', which is indeterminately NOM/ACC, cannot (3).

(2) *Kogo   Janek lubi a   Jerzy nienawidzi*
    who   Janek likes and Jerzy hates
    ACC/GEN   ACC   GEN
    Who does Janek like and Jerzy hate?   (Polish)

(3) **Co   Janek lubi a   Jerzy nienawidzi*
    what   Janek likes and Jerzy hates
    NOM/ACC   ACC   GEN
    What does Janek like and Jerzy hate?   (Polish)

---

[1]Only the indeterminacy between ACC and DAT is relevant to our exposition of the issue, but *Papageien* is completely case-indeterminate.

The syntactic effects of indeterminacy can also be observed outside of coordination. A much-discussed case concerns German free relative constructions, in which violations of the case-matching requirement are sanctioned just in case the wh-item displays the appropriate indeterminacy (see (4) below).

Dalrymple and Kaplan (2000) propose the use of (closed) sets as the value of features like CASE which show syntactic indeterminacy. The approach makes use of a notion of a *set designator* which indicates that the value of a feature is a set and also *exhaustively enumerates* the elements of the set. For example, the equation $(\uparrow \text{CASE}) = \{\text{NOM, ACC}\}$ (in which $\{\text{NOM, ACC}\}$ is a set designator) defines the value of CASE (for the f-structure in question) to be the set $\{\text{NOM, ACC}\}$, and the constraint $(\uparrow \text{SUBJ GEND}) =_c \{\text{M}\}$ requires the value to be the (singleton) set $\{\text{M}\}$ (Dalrymple and Kaplan, 2000).

The move to sets as values is a relatively simple one: fully determinate forms specify a singleton set as the value, indeterminate forms specify larger sets as values and agreement forms are associated with membership statements over the (closed) sets serving as the values of these features. Thus, as shown in the partial lexical specifications in (5) below, the verb *essen* 'eat' requires its OBJ to be ACC by stating that ACC must be an element of the (set-valued) OBJ CASE, while *ubrig* 'left' requires NOM to be an element of its SUBJ CASE.

(4) *Ich habe gegessen   was   übrig   war*
    I   have eaten   what   left   was
          ACC $\in$ OBJ CASE $\{\text{NOM,ACC}\}$ NOM $\in$ SUBJ CASE

I ate what was left.

(5)   *was*: $(\uparrow \text{CASE}) = \{\text{NOM,ACC}\}$
    gegessen: ACC $\in (\uparrow \text{OBJ CASE})$
      übrig: NOM $\in (\uparrow \text{SUBJ CASE})$

On this account, an indeterminate form (and only an indeterminate form) can satisfy apparently conflicting CASE requirements. There are, however, several problems with this approach. In particular, modifiers and predicates must impose **compatible** agreement requirements (Levy, 2001): a noun that is indeterminately accusative or dative must take a dative modifier if the predicate requires dative, and an accusative modifier for an accusative predicate (see (6)). This behaviour is not predicted by Dalrymple and Kaplan (2000), because agreeing forms never serve to restrict (or narrow down) the set-valued feature, thus the approach suffers from what we can call a transitivity problem.

(6) a. *Er hilft   die   Papageien.
       he helps   the.ACC   parrots-ACC/DAT
           DAT∈OBJ CASE ACC ∈ CASE {DAT,ACC}

   b. *Er hilft   den   Papageien.
       he helps   the.DAT   parrots-ACC/DAT
           DAT∈OBJ CASE DAT ∈ CASE {DAT,ACC}

   He helps the parrots.   (German)

A further problem, which we will not illustrate here, is that features that are indeterminate (and thus set-valued) may have indeterminate requirements placed on them. For example, some Russian verbs require objects that are either genitive or accusative, and CASE itself is a feature showing morphosyntactic indeterminacy. Such interactions require a non-null intersection between the set of case values specified by the noun and the set required by the verb, a requirement that is not possible to impose within the standard formal assumptions of LFG.

3   Feature-based approach to indeterminacy

Dalrymple et al. (2006) replace the set-based approach of Dalrymple and Kaplan (2000) by a return to the use of atomic values. Much previous work on the formal representation of indeterminacy has emphasised the inadequacy of simple atomic values for indeterminate features like case (e.g. CASE=NOM or CASE=ACC), since atomic values like NOM or ACC do not lend themselves easily to indeterminate specification (Bayer, 1996; Dalrymple and Kaplan, 2000; Blevins, 2001; Levy, 2001; Levy and Pollard, 2001; Sag, 2003): the proposal of Dalrymple et al. (2006) is to treat potentially indeterminate morphosyntactic features (such as CASE) as a feature structure containing atomic attribute-value paris. On this view, (i) the value of the CASE attribute is a feature structure which allows specification and differentiation of each (core) case by means of a separate attribute for each case possibility: NOM, ACC, DAT, and so forth; (ii) nouns and their modifiers specify negative values for the cases they do not express; and (iii) verbs specify positive values for the case(s) they require to be realised.

Consider first a case of indeterminacy and how this is handled in the complex feature approach. The verbs *hilft* and *findet* place positive constraints on certain case features of their OBJ, while the noun *Papageien* 'parrots' which is case-indeterminate, places no negative specifications on its case features.

(7) *hilft*:   (↑ OBJ CASE DAT) = +

(8) *findet:* (↑ OBJ CASE ACC) = +

The f-structure which results for the coordinate sentence in (1) is shown in (9).

(9)

$$
\left\{
\begin{array}{l}
\left[
\begin{array}{ll}
\text{SUBJ} & \left[\text{PRED} \quad \text{‘HE’}\right] \\
\text{PRED} & \text{‘FIND}\langle\text{SUBJ},\text{OBJ}\rangle\text{’} \\
\text{OBJ} & \left[
\begin{array}{ll}
\text{PRED} & \text{‘PARROTS’} \\
\text{CASE} & \left[
\begin{array}{ll}
\text{ACC} & + \\
\text{DAT} & + \\
\text{NOM} & - \\
\text{GEN} & -
\end{array}
\right]
\end{array}
\right]
\end{array}
\right] \\[6em]
\left[
\begin{array}{ll}
\text{SUBJ} & \\
\text{PRED} & \text{‘HELP}\langle\text{SUBJ},\text{OBJ}\rangle\text{’} \\
\text{OBJ} &
\end{array}
\right]
\end{array}
\right\}
$$

This approach takes care of the transitivity problem which arose in the previous set-based approach. Like nouns, adjectives and determiners state negative values for the case features which they do not express: since these are constraints over the f-structure of the NP, they combine with (and hence must be compatible with) those expressed by the noun itself. They may well serve to limit the indeterminacy by further constraining the case features of the noun they modify.[2] For example, the strong form dative *alten* ‘old’ has the agreement constraints shown in (10).

(10)   *alten*: $((\text{ADJ} \in \uparrow)\ \text{CASE NOM}) = -$

$((\text{ADJ} \in \uparrow)\ \text{CASE ACC}) = -$

$((\text{ADJ} \in \uparrow)\ \text{CASE GEN}) = -$

When it combines with an indeterminate (plural) noun, such as *Frauen* ‘women’ or *Papageien* ‘parrots’, the result is not indeterminate but a fully case-determinate NP which can only satisfy positive DAT requirements placed by verbs. The f-structure for *alten Papageien* is shown in (11).

(11)

$$
\left[
\begin{array}{ll}
\text{PRED} & \text{PARROTS} \\
\text{CASE} & \left[
\begin{array}{ll}
\text{NOM} & - \\
\text{ACC} & - \\
\text{GEN} & -
\end{array}
\right] \\
\text{ADJ} & \{\ [\text{PRED} \quad \text{OLD}]\ \}
\end{array}
\right]
$$

---

[2]Note that agreement is modelled in LFG in terms of co-specification of a feature in a single f-structure (here that headed by the noun), rather than in terms of matching or agreement between CASE features of the nominal f-structure with those of the adjectival f-structure. Similarly it is standard in LFG to treat finite verbs as placing (agreement) constraints directly over the SUBJ f-structure, and agreeing prepositions as placing constraints directly over the f-structure of their OBJ, and so on. This has no crucial formal consequences for the analysis developed here, which could be recoded, somewhat more verbosely, in terms of a feature matching approach.

(11) is not indeterminate, but unambiguously DAT; *alte Papageien* is likewise unambiguously ACC: this account captures the transitivity effect, as modifiers and verbs must place compatible requirements.

(12) a. *Er hilft   *alte/alten   Papageien.*
        he helps *old-ACC/old-DAT parrots-ACC/DAT

        He helps old parrots.   (German)

    b. *Er findet alte/*alten   Papageien.*
        he helps *old-ACC/old-DAT parrots-ACC/DAT

        He finds old parrots.   (German)

With this background in place, we now turn to some other agreement phenomena which can be simply modelled in LFG using the complex feature approach.

4   Rumanian gender without set-valued features

Rumanian displays an apparent mismatch between the number of nominal controller genders (three) and the number of target genders (two): nouns appear to make more gender distinctions than the elements which agree with them. This phenomenon, which is not unique to Rumanian, has engendered a number of analyses and on the face of it is a challenge to approaches to agreement by token identity or co-specification.

Rumanian nouns fall straightforwardly into three distinct gender classes when we consider their behaviour in construction with agreement targets such as adjectives, as illustrated in (13) to (18) below. In Rumanian, participles and predicate adjectives show predicate-argument agreement with the subject, and determiners and adjectives within NP agree with the head noun (head-modifier agreement), as shown in (13) to (16) for masculine and feminine nouns (examples from Farkas and Zec (1995), glosses slightly altered for consistency):

(13) *un copac   frumos*   (14) *doi   copaci   frumoşi*
     a.M tree.MSG beautiful.MSG   two.M trees.MPL beautiful.MPL
     a beautiful tree   two beautiful trees

(15) *o rochie   frumoasă*   (16) *două rochii   frumoase*
     a.F dress.FSG beautiful.FSG   two.F dresses.FPL beautifulFPL
     a beautiful dress   two beautiful dresses

There is a third class of nouns shown in (17) to (18) and glossed as neuter, which show a mixed behaviour:

(17) *un   scaun   frumos*
a.M chair.NSG beautiful.MSG

a beautiful chair

(18) *două scaune   frumoase*
two.F chairs.NPL beautiful.FPL

two beautiful chairs

Assignment to a gender class is partly driven by formal factors in Rumanian — nouns ending in [e] are MASC or FEM, those ending in any other vowel are FEM, and nouns ending in a consonant are MASC or NEUT (Farkas and Zec, 1995), but there is also a semantic dimension to syntactic gender assignment: nouns referring to males are MASC in gender while those referring to females are FEM. Nouns referring to inanimate objects may be in any of three classes.

Note that neuter is not an inquorate gender, that is, a marginal agreement class with a very small number of members (Corbett, 1991, 170), but rather is a class fully on a par with the MASC and the FEM genders. This third class of nouns controls agreement forms identical to the MASC in the singular, and forms identical to the FEM in the plural.

One theoretical possibility is that this (large) class of lexemes simply belongs to two different syntactic genders — they really are MASC in the singular and FEM in the plural (as found with Somali gender polarity), with the existence of this 'third' class being essentially a fact internal to the morphology. Such a proposal is found in recent work by Bateman and Polinsky (2005), who propose that Rumanian has just two noun classes in the singular and two in the plural, with membership determined on both formal and semantic grounds. A similar position is adopted in Wechsler and Zlatić (2003, 157): 'the so-called neuter is really a class of inquorate nouns that are masculine in the singular but feminine in the plural'.

On the other hand, in his wide-ranging study of gender as a morphosyntactic category, Corbett (1991) reasserts the traditional view and argues that the existence of three distinct agreement classes is itself enough to merit recognition of three genders in Rumanian, with a distinction emerging between controller and target genders. There is, furthermore, indication of a three-way syntagmatic distinction in the syntax, in the form of clear evidence from coordination that neuter singular nouns show a behaviour different than that of masculine singular nouns. The agreement pattern exemplified in the following data for coordinations of singular nouns shows that neuter should be recognised as a third gender in the syntax.

(19) a. *Podeaua   şi plafonul   sînt albe.*
       floor.DEF.FSG and ceiling.DEF.MSG are white.FPL

       The floor and the ceiling are white.

   b. *Scaunul   şi dulapul   sînt albe.*
       chair.DEF.NSG and cupboard.DEF.NSG are white.FPL

       The chair and the cupboard are white.

   c. *Peretele   şi scaunul   sînt albe.*
       wall.DEF.MSG and chair.DEF.NSG are white.FPL

       The wall and the chair are white.

   d. *Podeaua   şi scaunul   sînt albe.*
       floor.DEF.FSG and chair.DEF.NSG are white.FPL

       The floor and the chair are white.

   e. *Podeaua   şi uşa   sînt albe.*
       floor.DEF.FSG and door.DEF.FSG are white.FPL

       The floor and the ceiling are white.

   f. *Nucul   şi prunul   sînt uscaţi.*
       walnut.DEF.MSG and plum tree.DEF.MSG are dry.MPL

       The walnut tree and the plum tree are dry.   (Farkas and Zec 1995, 96)

These data highlight the difficulty for the view that neuter nouns are simply members of a class MSG/FPL. On this view, a coordination of two MSG nouns should be indistinguishable from a coordination of two NSG nouns, which is clearly not the case (Indeed, Bateman and Polinsky (2005) explicitly leave the resolution behaviour under coordination as a problem in their account.). The presence of a NSG noun in the coordinate structure is enough to trigger FPL agreement. The data above shows that FEM operates as the syntactic resolution gender for inanimate nouns.

Animate nouns show a different, semantically-based, resolution pattern. Coordinations of animate nouns determine masculine agreement if any of the conjuncts are male-denoting. Confirmation that the determining factor is semantic rather than grammatical gender assignment comes both from nominals which are not (semantic) gender specific, but which are feminine in form (*persoană* 'person'), and those which denote a male individual but are feminine in form (*popă* 'priest'). We shall not be concerned with the resolution pattern found with animate nouns here, but see Wechsler and Zlatić (2003) and Sadler (2006) for discussion.[3]

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[3]There is also evidence for the existence of a single conjunct strategy alongside syntactic resolution, which also falls outside the scope of the present discussion.

(20) *Maria   şi tata   au fost văzuti.*
Maria.FSG and father.MSG were   seen.MPL

Maria and father were seen.   (Moosally, 1998, 112)

(21) *Maria   şi mama   au fost văzute.*
Maria.FSG and mother.FSG were   seen.FPL

Maria and mother were seen.   (Farkas and Zec, 1995, 94)

(22) *Ion   şi tata   au fost văzuti.*
Ion.MSG and father.MSG were   seen.MPL

Ion and father were seen.   (ibid. 95)

(23) *un vizitator   şi o turistă   mult interesaţi*
a   visitor.MSG and a tourist.FSG very interested.MPL

a very interested (male) visitor and a very interested

(female) tourist   (Maurice, 2001, 237)

Such nouns control agreement of adjectives, determiners, participles and predicative adjectives in terms of their grammatical gender, but participate in semantically-based agreement in coordination. The pronominal anaphor referring back to nouns such as *persoană* also reflects the natural gender of the denotata.

(24) *Persoană   cu barbă a fost văzută.   El trebuie arestat*
person.DEF.FSG with beard was   seen.FSG he must   arrested.MSG
imediat.
immediately

The person with a beard was seen.

He must be arrested immediately.   (Farkas and Zec, 1995, 94)

(25) *Maria şi santinelă   au   fost căsătoriti   de catre protul*
Maria and sentry.DEF.FSG PST.PL were married.MPL by   priest.DEF
local.
local

Maria and the sentry were married by the local priest.   (Wechsler and Zlatić, 2003, 188)

Sadler (2006) provides an LFG analysis of Rumanian gender, agreement and resolution behaviour under coordination adopting the set-based framework of Dalrymple and Kaplan (2000). They propose that syntactically resolving agreement features (typically PERS and GEND) should also be treated as set-valued rather than atomic-valued, along the same lines as features such as CASE and NOUNCLASS, which show indeterminacy. On this approach, syntactic resolution reduces to the simple operation of set union. The value of the GEND feature of the coordinate structure as a whole is defined as the smallest set containing the values of the individual conjuncts, as in (26).

(26) NP $\longrightarrow$ 　　　NP　　　　CONJ　　　NP
　　　　　　　　　　$\downarrow \in \uparrow$　　　　　　　　　　$\downarrow \in \uparrow$
　　　　　　　($\downarrow$ GEND) $\subseteq$ ($\uparrow$ GEND)　　　($\downarrow$ GEND) $\subseteq$ ($\uparrow$ GEND)

(27) $x \cup y$ is the smallest set z such that $x \subseteq z \wedge y \subseteq z$

In the approach of Sadler (2006), Rumanian MASC nouns are ($\uparrow$ GEND) = $\{M\}$, FEM nouns are ($\uparrow$ GEND) = $\{M, N\}$ and NEUT nouns are ($\uparrow$ GEND) = $\{N\}$. This reflects the fact that FEM is the (syntactic) resolution gender. For example:

(28) *copac:*　($\uparrow$ GEND) = $\{M\}$　　*rochie:*　($\uparrow$ GEND) = $\{M, N\}$
　　　　　　　($\uparrow$ NUM) = SG　　　　　　　　($\uparrow$ NUM) = SG
　　　　　　　($\uparrow$ PRED) = 'TREE'　　　　　　($\uparrow$ PRED) = 'DRESS'
　　　scaun:　($\uparrow$ GEND) = $\{N\}$
　　　　　　　($\uparrow$ NUM) = SG
　　　　　　　($\uparrow$ PRED) = 'CHAIR'

Targets of agreement underspecify the agreement features of their controllers, capturing the mixed pattern of agreement illustrated in (13) – (18) and below. The lexical entries for predicative adjectives (as in the examples (29) – (32), from Farkas, 1999, 539-540) are along the lines shown in (33)– (36).

(29) *Un　　trandafir alb　　　e scump.*
　　　a.MSG rose.MSG white.MSG is expensive.MSG

　　　A white rose is expensive.

(30) *O　　garoafă　　albă　　e scumpă.*
　　　a.FSG carnation.FSG white.FSG is expensive.FSG

　　　A white carnation is expensive.

(31) *Un　　scaun　　confortabil　　e folósitor.*
　　　a.MSG chair.NSG comfortable.MSG is useful.MSG

　　　A comfortable chair is useful.

(32) *Nişte　　scaune　confortabile　e folositoare.*
　　　some.FPL chair.NPL comfortable.FPL are useful.FPL

　　　Some comfortable chairs are useful.

(33) *scumpă* (SUBJ GEND *must be* FEM)
　　　($\uparrow$ SUBJ GEND) $=_c$ $\{M, N\}$
　　　($\uparrow$ SUBJ NUM) = SG
　　　($\uparrow$ PRED) = 'EXPENSIVE'

(34) *scumpi* (SUBJ GEND *must be* MASC)

$(\uparrow \text{SUBJ GEND}) =_c \{\text{M}\}$

$(\uparrow \text{SUBJ NUM}) = \text{PL}$

$(\uparrow \text{PRED}) = \text{'EXPENSIVE'}$

(35) *scump* (SUBJ GEND *can't be* FEM)

$(\uparrow \text{SUBJ GEND}) \neg = \{\text{M, N}\}$

$(\uparrow \text{SUBJ NUM}) = \text{SG}$

$(\uparrow \text{PRED}) = \text{'EXPENSIVE'}$

(36) *scumpe* (SUBJ GEND *can't be* MASC)

$(\uparrow \text{SUBJ GEND}) \neg = \{\text{M}\}$

$(\uparrow \text{SUBJ NUM}) = \text{PL}$

$(\uparrow \text{PRED}) = \text{'EXPENSIVE'}$

For example, in (32) *scaune* is lexically specified $(f1\ \text{GEND}) = \{\text{N}\}$ and the FPL adjective *folositoare* (like *scumpe* in (36)) specifies $(f1\ \text{GEND}) \neg = \{\text{M}\}$, that is, requires the GEND value not to be the closed set containing the single element $\{\text{M}\}$, hence allowing the GEND value to be either $\{\text{N}\}$ or $\{\text{M, N}\}$. Given that there is a limited set of possibilities here, we can alternatively express this negative constraint as the equivalent:

(37) $\{\text{N}\} \in_c (\uparrow \text{SUBJ GEND})$

Attributive adjectives place constraints along the lines shown in (38) and (39), and other NP-internal modifers such as numerals, demonstratives and quantifers will be similar.

(38) *frumoasă* (FSG)   *frumoşi* (MPL)

$((\text{ADJ} \in \uparrow)\ \text{GEND}) =_c \{\text{M, N}\}$ $((\text{ADJ} \in \uparrow)\ \text{GEND}) =_c \{\text{M}\}$

$((\text{ADJ} \in \uparrow)\ \text{NUM}) = \text{SG}$ $((\text{ADJ} \in \uparrow)\ \text{NUM}) = \text{PL}$

(39) *frumos* (MSG)   *frumoase* (FPL)

$((\text{ADJ} \in \uparrow)\ \text{GEND}) \neg = \{\text{M, N}\}$ $((\text{ADJ} \in \uparrow)\ \text{GEND}) \neg = \{\text{M}\}$

$((\text{ADJ} \in \uparrow)\ \text{NUM}) = \text{SG}$ $((\text{ADJ} \in \uparrow)\ \text{NUM}) = \text{PL}$

This accounts for the pattern of syntactic resolution shown by inanimate NPs: the resolving GEND follows from set union as shown in (26); this is summarised in (40).

(40)

NP1		NP2		NPCoord	Target Morph
{M N}	(FSG)	{M}	(MSG)	{M N}	FPL
{M}	(MSG)	{N}	(NSG)	{M N}	FPL
{M N}	(FSG)	{N}	(NSG)	{M N}	FPL
{N}	(NSG)	{N}	(NSG)	{N}	FPL
{M N}	(FSG)	{M N}	(FSG)	{M N}	FPL
{M}	(MSG)	{M}	(MSG)	{M}	MPL

We now show how this account using set-valued features can be recast in terms of simple features by treating GEND as an f-structure. The three nominal genders are represented as follows:

(41) Rumanian Nominal Genders

MASC $[\, \text{M} +, \text{F} - \,]$

FEM $[\, \text{M} -, \text{F} + \,]$

NEUT $[\, \text{M} -, \text{F} - \,]$

Targets of agreement underspecify the agreement features of their controllers as shown below for the attributive adjectives.

(42) *frumoasă* (FSG)   *frumoşi* (MPL)

((ADJ $\in \uparrow$) GEND F) = +   ((ADJ $\in \uparrow$) GEND M) = +

((ADJ $\in \uparrow$) NUM) = SG   ((ADJ $\in \uparrow$) NUM) = PL

(43) *frumos* (MSG)   *frumoase* (FPL)

((ADJ $\in \uparrow$) GEND F) = −   ((ADJ $\in \uparrow$) GEND M) = −

((ADJ $\in \uparrow$) NUM) = SG   ((ADJ $\in \uparrow$) NUM) = PL

In the SG, it is the MASC agreeing forms which have the wider distribution, occurring with both MASC and NEUT controllers because they simply require the controller to be compatible with GEND F = −. In the PL, on the other hand, it is the FEM agreeing forms which have the wider distribution, occurring with FEM and NEUT controllers, which satisfy the GEND M = − constraint.

In the set-based approach, syntactic resolution in coordinate structures essentially comes for free: the annotation ($\uparrow$ GEND) $\subseteq$ ($\downarrow$GEND) on conjunct NPs specifies that the resolved GEND is the set union of the GEND values of the conjuncts. The question therefore arises as to how syntactic resolution will be specified in the feature-based approach. For Rumanian inaninate NPs, FEM is the resolution gender and results in all coordinate structures except those involving only MASC conjuncts. In the feature-based approach, there are several ways of doing this (and this is not necessarily a good sign!). Here is one approach. The resolution generalisations are as shown in (44): in the implemented grammar these are simply encoded into the relevant agreement templates.

(44)   • M: if all are + then +, else −

        • F: if any are + then +, else −

The result of this is summarised in the following:

(45)

Conj		Conj		Coord NP	Agr Target
(M)	M +, F −	(M)	M +, F −	M +, F −	MPL: (M +)
(F)	M −, F +	(F)	M −, F +	M −, F + −	FPL: (M −)
(N)	M −, F −	(N)	M −, F −	M −, F −	FPL: (M −)
(F)	M −, F +	(M)	M +, F −	M −, F +	FPL: (M −)
(F)	M −, F +	(N)	M −, F −	M −, F +	FPL: (M −)
(M)	M +, F −	(N)	M −, F −	M −, F −	FPL: (M −)

This is in some respects simpler than the original encoding in sets in Sadler (2006). The difference is that we need to formulate explicit resolution rules, whereas under the set-valued approach resolution is taken care of by set union.

In fact, there is further reason to wonder whether the original Dalrymple and Kaplan (2000) proposal for resolution by set union might need to be rethought. Dalrymple and Kaplan (2000) used set-valued features for two quite different purposes. In the case of indeterminacy, the set denotes a set of alternatives which can simultaneously satisfy otherwise imcompatible case requirements. In the case of agreement features, sets are essentially a notational device for representing what are conceptually single, discrete values: {M, N} is not an element which is indeterminately M or N, but a set-based characterisation of a gender (say FEM) which is the resolution gender for MASC, NEUT and FEM.

Dalrymple et al. (2006) show that two problems with the use of set-valued features to model indeterminacy (the transitivity problem and the problem of second-order indeterminacy) can be avoided if features with sets as values are replaced by features with complex (f-structure) values. The use of set-valued features to model syntactically resolving agreement features would also be in question if it turned out that such features may themselves show indeterminacy, contrary to the expectations of Dalrymple and Kaplan (2000). Dalrymple et al. (2007) look at a case of precisely this sort: written Bokmål standard Norwegian has three nominal inflectional classes — masculine, neuter, and indeterminately masculine or feminine. There are no nouns in Bokmål which are obligatorily feminine: the third class of nouns can control either FEM or MASC dependent forms (and inflections), and, like MASC nouns, can also occur with COMMON forms where agreement targets show only a NEUT/COMMON opposition (neutralising the distinction between MASC and FEM).

(46) a. *en   liten   hest*
     a.MSG small.MSG horse.MSG

   b. *ei   lita   bok*
      a.FSG small.FSG book.MFSG

   c. *en   liten   bok*
      a.MSG small.MSG book.MFSG

   d. *et   lite   hus*
      a.NSG small.NSG house.NSG

(47) a. *Hesten   er liten/hvit*
     horse.DEF.MSG is small.MSG/white.CSG

     The horse is small/white.

   b. *Hver bok   er lita/liten/hvit*
      every book.MFSG is small.FSG/small.MSG/white.CSG

      Every book is small/white.

   c. *Huset   er lite/hvitt*
      house.DEF.NSG is small.NSG/white.NSG

      The house is small/white.   (Norwegian)

Dalrymple et al. (2007) provide an account of the Norwegian data (which had resisted analysis under the set-based approach), using a complex feature representation of GEND, also formulating the appropriate resolution generalisations for the observed agreement patterns in disjunctions (Norwegian plural adjectives do not show gender distinctions and so gender resolution behaviour cannot be tracked under coordination). It seems therefore, that there is some evidence that the set-based approach to resolving features might be in question.

5   Optional disagreement in Jingulu

The phenomenon of so-called optional disagreement (or superclassing) occurring in a number of Australian languages is one for which an account in terms of a complex gender feature is appropriate. I discuss here the facts as they are described for Jingulu (non-Pama Nyungan) in Pensalfini (1999), but similar facts obtain in other Australian languages (Harvey and Reid, 1997). Jingulu has four genders (MASC, FEM, NEUT, VEG) and nominal heads control agreement forms on their modifiers (adjectives and demonstratives), which usually appear in the same gender as the head. However disagreement between head and modifiers can occur, and occurs as a possible option independent of grammatical and semantic context. Although gender disagreement

is always an option, only certain types of disagreement are possible and these are summarised in (48).

(48)

Nominal Gender	Possible Modifier Gender
MASC	MASC
FEM	FEM, MASC
NEUT	NEUT, MASC
VEG	VEG, NEUT, MASC

The assignment of nouns to class is basically semantic, with the MASC and FEM classes containing words for male and female animates respectively, and VEG containing words for edible plant forms. There are, however, many lexical exceptions so an entirely semantic assignment to noun class is not possible. Nouns in a given gender typically (but not always) show that gender's characteristic suffix; nominals which may occur in a number of genders, pronouns, demonstratives and adjectives take gender suffixes as inflections.

The following examples illustrate the phenomenon of optional disagreement in Jingulu. In (49) head nominals in the FEM gender class occur with MASC as well as FEM dependents, which may be combined in the same sentence.

(49) a. *Nyama-bili-rna-ni   nayu-wurlu kuwirinji-yurlu*
      DEM.M-DU-ERG-FOC woman-DU W.Mudburra.FEM-DU
      These are two Western Mudburra women.

    b. *Ngamulirni jalyamungka binjiya-ju, birnmirrini*
      girl.FEM   young.MASC grow-do   prepub.girl
      That little girl is growing up into a big girl.

    c. *Nginda-rni   wujuwujurni kurlukurli-ni, kurlungkurli-ni ngina-rniki*
      that.MASC-FOC parrot.FEM   small-FEM   small-FEM   this.FEM
      wujuwujurni-ni
      parrot.FEM-FOC
      The wujuwujurni parrot is small.   (Jingulu, Pensalfini 1999, 171)

In (50) NEUT heads combine with MASC dependents. (51a) illustrates a VEG head occurring with a MASC modifier and (51b) with NEUT) dependents.

(50) a. *Jama-rni   nyanyalu-ngkuju, darrangku kirdilyaku.*
      that.MASC-FOC leaf-HAVING   tree.NEUT bent.NEUT
      That bent tree is leafy.

    b. *Ngandirdi ngini-niki-rni   biyijala   bikirra-rni*
      grass-sp this.NEUT-FOC tall.MASC grass.NEUT-FOC
      Ngandirdi is this tall grass.   (Jingulu, Pensalfini 1999, 171)

(51) a. *Karrangayimi   nyama-niki langaningki-mindi-i,   dajbajalmi*
       yam-species.VEG this.MASC dig-1DU.INCL-will.go spicy.VEG
       nyama-niki marrimarri-mi.
       this.MASC cheeky-VEG

       The karrangayimi yam, which I'm going to dig up, will burn you.

   b. *Bilyingbiyaku ngini-rniki-rni ngurndungurndulbi-rni lilingbi-nga-ju*
      red.NEUT   this.NEUT-FOC throat.VEG-FOC   hurt-1SG-do

      My throat's red and sore.   (Jingulu, Pensalfini 1999, 172)

As Pensalfini observes, this behaviour suggests that MASC is some sort of default or underspecified gender, while NEUT and FEM are more specific subclasses and VEG operates as a more specific subclass of NEUT. Unsurprisingly, optional disagreement behaviour of this sort can be captured straightforwardly in an account which represents GENDER as a complex feature. One way of doing so is to assign the complex feature descriptions for GEND shown schematically in (52) to nominals, and the gender agreement constraints shown schematically in (53) to (potentially disagreeing) modifiers:

(52)

	Nominal
MASC	F −, N −, V −
FEM	F +, N −, V −
NEUT	F −, N +, V −
VEG	F −, N +, V +

(53)

	Adj Mod
MASC	places no agreement constraint (universally acceptable)
FEM	F = +
NEUT	N = +
VEG	V = +

So for example, a NEUT head noun such as *ngurndungurndulbi* 'throat' would have the gender specification shown in (54) and would combine with (55) or (56) but not with (57).

(54) *ngurndungurndulbi:* 'throat.VEG' $(\uparrow \text{GEND FEM}) = -$
                                         $(\uparrow \text{GEND NEUT}) = +$
                                         $(\uparrow \text{GEND VEG}) = +$

(55) *bilyingbiyaku:* 'red.NEUT' $((\text{ADJ} \in \uparrow) \text{GEND NEUT}) = +$

(56) *bardakurrimi:* 'good.VEG' ((ADJ ∈ ↑) GEND VEG) = +

(57) *bardakurrirni:* 'good.FEM' ((ADJ ∈ ↑) GEND FEM) = +

The phenomenon of optional disagreement in Jingulu is not limited to head-modifer gender agreement — (58) and (59) show both modifiers and verbs disagreeing (or agreeing only partially) in number with nominal agreement controllers (the fully agreeing forms are given in parentheses).

(58) a. *nyama-baji   imimikirni-bila   (nyama-bila)*
       DEM-PL(ANIM) old-woman-DU(ANIM)
       the two old women

   b. *jama bininja-yila   (jama-bila)*
      that man-DU(ANIM)
      those two men

   c. *nginda   juliji-darra (nginda-ala)*
      that.MASC bird-PLUR
      those birds   (Jingulu, Pensalfini 1999, 174)

(59) a. *Kunyirrirni dij bila-nya-mi kandirri!* (*dij bilikunyimi*)
       2DU-ERG   divide-2SG-IRR bread
       You two cut up the bread.

   b. *Kujarri-bila-rni   yurriy-urru-ju (yurriy-unyu-ju)*
      two-DU(ANIM)-FOC play-3PL-do
      Those two boys are playing.   (Jingulu, Pensalfini 1999, 174)

The agreement and disagreement possibilities are set out in (60).

(60)

Nominal	Agreeing Element
SING	SING
PLUR	PLUR, SING
DU	DU, PLUR, SING

The observed pattern of number (dis)agreement follows from the specifications shown schematically in (61).

(61)

	Nominal	Modifier/Verb
SING	PL −, DU −	places no agreement constraint: universally acceptable
PLUR	PL +, DU −	PL = +
DUAL	PL +, DU +	DU = +

On this view, a SING head specifies that the argument is neither PL nor DU, while a SING modifier (or agreeing predicate) places no constraints. A (partial) entry for a PL verb is shown in (62). This specifies that the SUBJ NUM PL is +, and thus is compatible with a DU or a PL SUBJ. On the other hand, the DU demonstrative in the partial lexical entry in (63) is compatible only with a DU nominal such as (64). Both are incompatible with a SG nominal as SUBJ (65).

(62) *yurriy-urru-ju:* 'play.3PL-DO'   ($\uparrow$ SUBJ NUM PL) = +

(63) *nyama-bila:* 'DEM-DU(ANIM)'   ($\uparrow$ NUM PL) = +
                                    ($\uparrow$ NUM DU) = +

(64) *imimikirni-bila:* 'old.woman.DU(ANIM)'   ($\uparrow$ NUM PL) = +
                                               ($\uparrow$ NUM DU) = +

(65) *ngamulirni:* 'girl'   ($\uparrow$ NUM PL) = −
                            ($\uparrow$ NUM DU) = −

Pensalfini (1999) suggests that the optional disagreement facts militate in favour of a model allowing feature erasure together with post-syntactic insertion of lexical items (Distributive Morphology, Halle and Marantz (1993)). On this view disagreement is basically a morphological process of feature erasure. A modifier to a syntactic head which is in the VEG class will *ipso facto* have the complete feature set of a member of the VEG gender, in Pensalfini's representation, [+N, Neuter: vegetable], in the syntax. A post-syntactic process of 'feature erasure' is then postulated, which must apply optionally, the morphology then spells out a (potentially) reduced set of features (for example, if 'vegetable' is erased from the feature bundle, the morphology will deliver up a NEUT form rather than an VEG form).

The phenomenon of agreement superclassing or optional disagreement, found in a number of Australian languages, can be captured straightforwardly in LFG by using a complex feature representation of agreement attributes such as GEND and NUM. The use of a feature-based approach does not appear to offer any particular advantage over a set-based approach here, because a very similar approach can be coded in terms of set-valued features. There are two aspects to the LFG architecture which enable both these approaches to optional disagreement. The first is the conception of agreement in the framework: agreement constraints associated with target and controller must be compatible (satisfiable by the same minimal f-structure) but they do not have to match — therefore is it not necessary to assume that the agreement targets are specified as (say) VEG when they are actually NEUT or MASC. The second, and perhaps less obvious, aspect is the flexible nature of the syntax-morphology interface

and the projection architecture of LFG. The morphological component states a bi-directional correspondence between strings and morphological descriptions (relating forms to lexemes and collections of morphological tags or features[4]). Morphological features are placed in correspondence with the syntactic information they contribute. Thus, for example, the morphological feature +Sg associated with a verb will express a constraint over the f-stucture of the SUBJ while a morphological feature +Sg associated with a noun will express a contraint over the f-structure of the noun itself. The correspondences between the atomic morphological tags +Du, +Sg, +Pl, +Veg, +Neut, and so on, and the collection of syntactic constraints over f-structure that they correspond to are stated in the morphology-syntax interface.

6   Constructed duals

In Hopi (Northern Uto-Aztecan), dual number is expressed constructively through the combination of a plural pronouns and a verb showing SG agreement.

(66) a. *Pam wari*
         3SG run.SG
         S/he ran.

      b. *Puma yuutu*
         SPL   run.PL
         They ran.

      c. *Pum wari*
         SPL run.SG
         They (two) ran.   (Kalectaca 1978, 52 cited in Bliss, 2005)

Constructed duals of the same sort are also found in Zuni (isolate, New Mexico) and Kawaiisu (Numic Uto-Aztecan). Constructed duals pose an interesting problem because on standard assumptions about feature matching, the values PL and SG for an atomic feature NUM are not compatible (i.e. unifiable) and nor do they 'combine' to produce a third value DU.

One suggestion in the literature which would sidestep the problem of constructed duals is the notion that the data in (66) do not illustrate subject-verb agreement at all, rather the number marking on the verb should be interpreted as indicating a number distinction inherent in the verb itself (and marking characteristics of the verbal event itself). On this view, verbal number is simply an inherent lexical property of particu-lar verbs (see Corbett (2000) for a suggestion along these lines). Bliss (2005) shows

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[4]LFG morphologies are usually implemented using finite state transducers using the Xerox finite state tools BF XFST and LEXC —an informal notation loosely based on that of **xfst** is used here to state the morphological side of the morphology-syntax correspondence.

that this cannot be true and establishes that constructed duals do indeed involve the noun and the verb co-specifying a 'constructed' NUM value for the subject. The two main diagnostics for verbal number are that it always has an ergative base and that distinctions of verbal number are preserved beyond the environments characteristic of verbal agreement (Durie, 1986). As Bliss demonstrates, in Hopi, number marking by suppletion (which involves a particular class of verbs) *is* organised on an ergative basis, but this is not true of number marking by suffixation, where the number suffix is always interpreted with respect to the SUBJ. Furthermore suppletive verbs retain number marking under agentive nominalisation and causativisation, while suffixing verbs do not. This indicates that only suppletive verbs have verbal number in Hopi. Constructed duals, on the other hand, occur equally with suffixing verbs, and do not operate on an ergative basis, being interpreted w.r.t. the SUBJ regardless of transitivity:

(67) a. *Itam peena* (PL = *peena-ya*)
    1PL write

    We (DU) write.   (Kalectaca 1978, 26 cited in Bliss, 2005)

b. *Itam sikwit nöösa*
    1PL meat eat.SG

    We (DU) ate meat.   (Kalectaca 1978, 52 cited in Bliss, 2005)

In summary, constructed duals in Hopi (similarly in Zuni and Kawaiisu) are instances of syntactic predicate arugment agreement, although they take the form of apparent disagreement in the NUM feature. Bliss argues (under Miminalist assumptions) that they cannot be accounted for in the syntax (assuming the options to be either checking or valuation), because the NUM features of controller and target must be identical, and proposes (two variants of) an account in which features are discarded in the morphology (Bliss's first account is closely modelled on that of Cowper (2005) for very similar data).

The syntactic feature-based account I propose, using underspecification, would be as follows. NUM is a complex feature with boolean valued attributes SG and PL: [SG +, PL −] is a fully determinate SG entity, [SG +, PL +] corresponds to a DU entity and [SG −, PL +] to a PL entity. Singular nouns and plural verbs are fully determinate in their lexical specification, while plural nouns and singular verbs are underspecified: plural nouns are lexically specified as PL + and thus can combine with either SG or PL marked verbs, similarly, singular verbs are lexically specified as SG + and can combine with singular or plural nouns. This is shown in (68).

(68)

Category	Features	Category	Features	Combined
N.SG	SG +, PL −	V SG	SG +	SG + PL −
V PL	SG −, PL +	N PL	PL +	SG − PL +
Combined: undefined			SG +, PL +	

The data above follow straightforwardly. Since singular nouns are fully determinate they always define SINGULAR f-structures, and are only compatible with verbs marked with singular agreement. Plural nouns, on the other hand, are partially indeterminate: because singular verbs only constrain the SG feature, plural nouns are compatible with both singular and plural verbal agreement forms.

Constructed duals have been the focus of some recent theoretical work. Cowper (2005) (for Zuni) and the analysis of the Hopi data in Bliss (2005) modelled on it assume that instances of constructed duals involve both verb and pronoun being represented as syntactically DU (a theory-internal requirement, to ensure that feature checking operates as required). In the morphology, plural pronouns are syncretic dual/plural forms and singular verb forms are syncretic singular/dual forms. An account appealing to syncreticism necessarily posits a paradigmatic distinction (here between singular and dual, and between dual and plural respectively) — but there is no morphological evidence at all that either the pronoun system or the verbal system motivates a three-way morphological distinction.

Bliss's alternative account is based on a slightly different (syntactic) feature representation of the number system, that of Harley and Ritter (2002). Again it is assumed that in examples such as (66c) above both verb and nominal are specified as DUAL in the syntax, but the assumptions about spell-out differ. Intrinsic (or in her terminology, interpretable) features spell out the largest possible subset of matching features, where there is no exact match, while contextual (constructional or valued) features (e.g. SUBJ NUM marked on a verb) take the default, rather than the largest match. Thus, a dual pronoun requirement spells out as a plural (under closest match) while a dual verbal agreement requirement spells out as a singular (under the resort to default). This avoids postulating a three-way number distinction for pronouns and verbs in the morphology. However, as Bliss herself observes, the proposed difference in spell-out strategies for intrinsic versus contextual features runs into difficulty with the fact that morphologically dual nouns take plural determiners, rather than the singular determiners predicted by resort to the default.

(69) *Puma   moosa-vit wari*
     DEM-PL cat-DU   run.SG
     Those (two) cats ran.   (Kalectaca 1978, 58 cited in Bliss, 2005)

On the LFG account, a dual nominal is fully determinate in the syntax, defining the features PL + and SG + in its f-structure. Plural determiners are parallel to plural

pronouns, they are essentially indeterminate between plural and dual, and define the feature PL + in the f-structure of the NP. Singular determiners, on the other hand, are fully determinate (like singular nouns and pronouns) and are therefore incompatible with morphologically dual marked nouns. The facts in (69) also therefore follow unproblematically.

The flexible interface between the morphology and the syntax in LFG means that morphological features generating the paradigm space are placed in correspondence with, but not necessarily identical to, the syntactic features space. The relevant correspondence for Hopi is as shown in (70).

(70)

V:Sg	$(\uparrow \text{SUBJ NUM SG}) = +$
V:Pl	$(\uparrow \text{SUBJ NUM SG}) = -$
	$(\uparrow \text{SUBJ NUM PL}) = +$
PrN:Sg	$(\uparrow \text{NUM SG}) = +$
	$(\uparrow \text{NUM PL}) = -$
PrN:Pl	$(\uparrow \text{NUM PL}) = +$
N:Sg	$(\uparrow \text{NUM SG}) = +$
	$(\uparrow \text{NUM PL}) = -$
N:Pl	$(\uparrow \text{NUM PL}) = +$
	$(\uparrow \text{NUM SG}) = -$
N:Du	$(\uparrow \text{NUM SG}) = +$
	$(\uparrow \text{NUM PL}) = +$

The account of constructed duals available under LFG assumptions treating NUM as a complex feature (f-structure) is extremely simple. It requires no subsidiary assumptions about particular features and it does not require SG verbs or PL pronouns to be classified as syntactically DU: the DU number arises in the syntax through the interaction of constraints associated with the nominal and verb respectively.

7 Conclusion

Recent work in LFG has suggested that the well-established approach to indeterminacy and resolution in morphosyntactic features should be replaced by an approach based on treated features exhibiting such behaviours as takign complex (f-structure) values. This paper has explored a number of additional areas of complexity in syntactic features which also appear to be straightforwardly accounted for under this proposal.

Acknowledgements

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The possessum-agreement construction

or

'Does Albanian have a genitive case?'

Andrew Spencer

I discuss the possessive construction in two languages said to have a 'genitive case', Albanian and Hindi. In both languages the possessed noun (possessum) in the construction agrees with the possessor in exactly the manner that an attributive adjective agrees with its head noun. I show that these constructions cannot sensibly be thought of as instantiations of a genitive case, and hence, that they do not require that a formal grammatical description appeal to an attribute or feature [Case: Genitive]. Rather, they are precisely homologous to the possessive construction of Bantu languages. This construction, the 'possessum-agreement' construction, marks the possessor noun or noun phrase with a formative which agrees with the possessor in the manner of an adjective, but the possessum itself is not categorially an adjective and the possessum phrase itself retains the internal syntax of a noun phrase. The possessum-agreement construction also highlights the unfairly neglected, but very close relationship between possessive constructions and attributive modification.

1. Introduction

Traditional grammars of Albanian and most of the theoretical discussions of Albanian morphosyntax that depend on those descriptions assume that the Albanian nominal system distinguishes a genitive case. Yet the morphosyntax of this genitive is extremely odd compared to that of other Indo-European languages and compared to the other, less controversial, case forms of Albanian. A genitive-marked noun is preceded by a clitic/prefix 'article' which agrees in number, gender, definiteness and case with the possessed noun. The genitive-marked noun itself is in an oblique-case form which on its own expresses the dative case (and ablative in most contexts). The clitic/prefix article on the genitive noun, moreover, is identical in form and function to the agreement clitic/prefix on the declinable class of adjectives. In other words a genitive-marked noun

shows exactly the kind of agreement with the head noun of the NP that an attributive adjective shows. This agreement pattern is the mirror image of the more familiar possessor agreement construction, in which it is the possessed noun (possessum, Pd) which agrees with the possessor (Px). Where we have a construction in which the possessor (Px) agrees with the possessum (Pd) I shall use the usual terminology, 'possessor agreement construction'. Where we have a construction in which it is the possessor which agrees with the possessum I shall speak of the 'possessum-agreement construction'. The two types are shown schematically in (1):

(1) a. possessor agreement

NP_i agr_i-N

Px   Pd

the girl   her-books   'the girl's books'

b. possessum agreement

NP-agr_i N_i

Px   Pd

the girl-AGR(PL)   books   'the girl's books'

Traditional grammars of Indo-Aryan languages outside the eastern region, such as Hindi, Punjabi, Marathi and others distinguish a series of cases marked by particles. These particles are clitics (phrasal affixes) appended to the right edge of the noun phrase. In Hindi, these case particles are ascribed functions of accusative (in imperfective tenses), ergative (in perfective tenses), dative, instrumental and various locatives. Quite separate from this phrasal marking, in many of these languages certain subclasses of nouns have two forms, 'direct' and 'oblique'. Where a noun distinguishes an oblique form from a direct form, the case particles invariably select the oblique form. In addition, these languages have a genitive case particle. However, this particle agrees with the possessed noun in number and gender, and also inflects for the direct/oblique status of the possessed noun. In Hindi the actual morphology of this inflecting 'genitive case particle' is identical to the morphology of a declinable adjective.

Apart from the fact that the word order of the Albanian construction is the mirror image of the Indo-Aryan constructions (as represented by Hindi), the two languages demonstrably have what to all intents and purposes is the same possessum-agreement construction. However, it is typologically unusual for genitive case to exhibit possessum agreement. Although cases do exist of genuine genitive case markers agreeing with possessed nouns (this is reported for certain Daghestan languages, Boguslavskaja 1995, Kibrik 1995; and Central Cushitic, Hetzron 1995), this is typologically unusual. The Albanian and Hindi contructions therefore raise interesting questions about the nature of grammatical categories such as 'case' and 'agreement'. What is the relationship between such constructions and more familiar types of case marking or attributive modifier agreement? More specifically, we can ask what is the minimal set of features, properties or attributes that need to be set up in order to capture all the relevant facts of the system without formal redundancy. In English, for instance, it would be entirely unnecessary to appeal to an attribute-value pair [Case: Genitive] in order to account for the distribution of the preposition *of*. What is the situation in languages like Albanian and Hindi: do we need a feature [Case: Genitive], as is implied in most traditional and most contemporary theoretical descriptions?

To help answer this question we can examine a construction which is more or less the morphosyntactic equivalent to the Albanian/Hindi system, but which has nothing to do with case, the Bantu 'connecting *a*' or '-A of Relationship' construction. Since Bantu languages do not have case, the '-A of Relationship' construction is not a kind of case. Therefore, unless the possessum-agreement construction of Albanian and Hindi can be shown to have additional case-like properties, we can conclude that the possessive construction is not mediated by genitive case in those languages either.

2. The Albanian case system

Albanian nouns inflect for number and definiteness, and fall into gender-based inflectional classes. Traditional grammars distinguish five cases – nominative, accusative, dative, ablative and genitive (Bokshi 1980) and this analysis is generally carried over into descriptions written in other languages (e.g. Buchholz and Fiedler 1987; Camaj 1969, 1984; Ejntrej 1982; Mann 1932; Newmark, Hubbard and Prifti 1982; Zymberi 1991). However, the grammatical synopsis in Newmark's (1998) dictionary

fails to list the genitive as a separate case and Newmark's (1957) structuralist grammar argues on the basis of distribution that there are only three cases – nominative, accusative and marginal (Newmark (1957: 56) speaks of the '"genitive" functions' of the marginal case in construction with the 'proclitics of concord', thereby distancing himself from an analysis which appeals to a genitive case). Whether it makes sense to distinguish dative and ablative is an interesting question, but it is irrelevant to the question of whether Albanian has a genitive case and what criteria we can deploy to answer such a question. We are therefore left with three uncontroversial cases – nom, acc, obl(ique).

The examples in Table 1 are taken from Zymberi (1991: 51f, 101) (omitting the marginal neuter class).

Table 1 Albanian basic noun declension

| djalë 'boy' | Masc sg | | Masc pl | |
	Indefinite	Definite	Indefinite	Definite
Nom	një djalë	djali	ca djem	djemtë
Acc	një djalë	djalin	ca djem	djemtë
Obl	një djali	djalit	ca djemve	djemve

| vajzë 'girl' | Fem sg | | Fem pl | |
	Indefinite	Definite	Indefinite	Definite
Nom	një vajzë	vajzja	ca vajza	vajzat
Acc	një vajzë	vajzën	ca vajza	vajzat
Obl	një vajze	vajzës	ca vajzave	vajzave

The preposed indefinite articles *një* (singular) and *ca* (plural) are in effect loosely bound prefixes.

Albanian adjectives fall into two broad groups: a group of declinable adjectives which are also accompanied by an inflecting preposed 'article', and a group of indeclinable adjectives lacking the article. In Table 2 we see the declinable adjective *mirë* 'good'.

Table 2 Albanian attributive modifiers: article-taking adjectives

	Indefinite	Definite
Masc sg	'a good boy'	'the good boy'
Nom	një djalë i mirë	djali i mirë
Acc	një djalë të mirë	djalin e mirë
Obl	një djali të mirë	djalit të mirë
Masc pl	'good boys'	'the good boys'
Nom	ca djem të mirë	djemtë e mirë
Acc	ca djem të mirë	djemtë e mirë
Obl	ca djemve të mirë	djemve të mirë
Fem sg	'a good girl'	'the good girl'
Nom	një vajzë e mirë	vajzja e mirë
Acc	një vajzë të mirë	vajzën e mirë
Obl	një vajze të mirë	vajzës së mirë
Fem pl	'good girls'	'the good girls'
Nom	ca vajza të mira	vajzat e mira
Acc	ca vajza të mira	vajzat e mira
Obl	ca vajzave të mira	vajzave të mira

From this table it can be seen that the adjective itself has fairly minimal inflection, changing only in the feminine plural form, while the article agrees with the head noun in number, gender, case and definiteness.

The traditional 'genitive case' is formed by taking the preposed article illustrated for adjectives in Table 2 and placing it before the oblique form of the noun (singular or plural, definite or indefinite). The article then agrees in number, gender, definiteness and case with the possessed noun (not the possessor noun to which it is preposed). This is shown in Table 3, where *fshatit* is the oblique definite singular form of the word *fshat* 'village'.

Table 3 The Albanian 'genitive case'

	Indefinite	Definite
Masc sg	'a boy of the village'	'the boy of the village'
Nom	një djalë i fshatit	djali i fshatit
Acc	një djalë të fshatit	djalin e fshatit
Obl	një djali të fshatit	djalit të fshatit
Masc pl	'boys of the village'	'the boys of the village'
Nom	ca djem të fshatit	djemtë e fshatit
Acc	ca djem të fshatit	djemtë e fshatit
Obl	ca djemve të fshatit	djemve të fshatit
Fem sg	'a girl of the village'	'the girl of the village'
Nom	një vajzë e fshatit	vajzja e
Acc	një vajzë të fshatit	vajzën e
Obl	një vajze të fshatit	vajzës së
Fem pl	'girls of the village'	'the girls of the village'
Nom	ca vajza të fshatit	vajzat e fshatit
Acc	ca vajza të fshatit	vajzat e fshatit
Obl	ca vajzave të fshatit	vajzave të fshatit

Exactly the same pattern would be found if we substituted *fshatit* with any other noun, singular or plural, masculine or feminine, definite or indefinite. I give a sampling of the relevant data in (2) (adapted from Zymberi 1991: 53f):

(2) a. një djalë   i   një   fshati

INDEF boy.NOM.SG.INDEF   ART   INDEF   village.OBL.SG.INDEF

'a boy of a village'

b. një vajze   të   një   qyteti

INDEF girl.OBLSG.INDEF   ART   INDEF   town.OBL.SG.INDEF

'(to) a girl of a town'

c. studentët   i   shkollës

student(MASC).NOM.SG.DEF(MASC) ART   school(FEM).OBL.SG.DEF

'the (male) student of the school'

	d.	studentes		së	kolegjit
		girl.OBL.SG.DEF		ART	college.OBL.SG.DEF

'(to) the (female) student of the college'

	e.	ca djem		të	ca	fshatrave
		INDEF.PL boy.NOM.PL.INDEF		ART	INDEF.PL	village.OBL.PL.INDEF

	f.	vajzave		të	qyteteve
		girl.OBL.PL.DEF		ART	city.OBL.PL.DEF

'(to) the girls of the towns'

As is clear from these examples the 'genitive article' marker instantiates the possessum-agreement construction shown in (1b). Moreover, the actual morphology of the 'genitive article' is identical to that of a declinable attributive adjective.

The question arises as to which constituent the article is associated with (if any), the attribute/possessor or the modified/possessed. The answer is unequivocal: the article is part of the adjective/possessor NP constituent (Morgan 1984): [djali [i mirë]] 'the good boy', [djali [i fshatit]] 'the boy of the village'. Crucial evidence comes from the facts of coordination. In (3) we see that each conjunct of a coordinated adjective has to repeat the article (Zymberi 1991: 104):

(3)   djalë   i   mirë   dhe   *(i)   sjellshëm
        boy   ART   good   and   ART   polite
        'a good and polite boy'

Plank (2002: 165) provides further evidence of for the constituent structure, given here in (4):

(4) a. Akademia   e   Shkenca-ve   të   Shqipëri-së
        academy.DEF   ART   sciences-OBL   ART.PL   Albania-OBL
        'the Academy of Albanian Sciences'

b. Akademia   e   Shkenca-ve   e   Shqipëri-së

academy.DEF ART sciences-OBL ART.SG Albania-OBL

'Albanian Academy of Sciences'

From these examples we can see that the article takes a different form depending on whether it is construed with 'sciences' or with 'Albania'. In (4a) the article *të* agrees with the plural noun *Shkenca* and the constituent structure is [Akademia [e Shkencave [të Shqipërisë]]]. In (4b) the second occurrence of the article *e* agrees with the head *akademia*, which is singular, so that the constituent structure is [[[Akademia [e Shkencave]] [e Shqipërisë]]]. Clearly, in (4a) we have an instance in which a modifying genitive is itself modified, forming a minimal pair with (4b). The point is that in (4b) the genitive article which agrees with the head noun *akademia* is not adjacent to that noun but appears as a prefix to the noun *Shqipërisë*.

Example (4) also illustrates the important point that the genitive-marked NP behaves like a noun phrase in the syntax and not like an adjective phrase. On both of the construals of (4) a genitive-marked noun is modified as a noun, namely by means of the possessive construction, and not as an adjective. Further examples can be found in Buchholz and Fiedler (1987: 418).

Clearly, the Albanian 'genitive case' is an unusual type of case, both from the point of view of the rest of the Albanian nominal inflectional system and typologically. The problem is in the morphosyntactic construction, not in its uses. Although the genitive is not used as the complement of a head (including a simplex preposition), it nevertheless has most of the other uses common with inflectionally realized genitives. Buchholz and Fiedler (1987: 219f) identify twenty-five uses for the adnominal genitive, including subject-like functions ('the answer of the pupil', 'the author of the article'), object-like functions ('the defence of the fatherland'), picture-noun constructions, partitive-type constructions ('a salad of tomatoes', 'the number of listeners') and so on. The genitive construction can also be used appositively, as in an example whose translation runs 'the principles of friendly relations: of equal rights, of sovereignty, of non-interference in internal affairs and of mutually advantageous exchange'. In addition the genitive NP can

be used predicatively in expressions such as (5, 6) from the nouns *mendim* 'thought, opinion' and *udhë* 'path, road':

(5)   Unë jam   i mendimit   se ti ke të drejtë
        I am   the.opinion.GEN   that you are right
        'I am of the opinion that you are right'

(6)   Më duket   e udhës   të mos përgjigjemi
        me seems   ART right.GEN   that we don't answer
        'It seems to me to be appropriate that we don't answer'

The noun *udhë* occurs as the genitive object of verbs such *sheh* 'see', *gje* 'find', *qua* 'call, consider', as in (7):

(7)   E gjen   të udhës
        he finds ART right.GEN
        'He finds it advisable'

Finally, a genitive-marked NP can be the complement of the copular verbs *është* 'be' and *bëhet* 'become':

(8)   Ky libër është i Agimit
        this book is   ART Agim.GEN
        'This book is Agimit's'

This behaviour makes it reasonable to regard the construction as a species of genitive case. However, all of these functions are also found with other types of morphosyntactic construction. Indeed, most of them are found with prepositions meaning 'of' in case-less Indo-European languages such as English.

    Before exploring these issues I turn to an alleged genitive case construction found in a variety of Indo-Aryan languages.

3.   The Hindi case system

3.1 Layer I and Layer II markers

The non-Eastern Indo-Aryan languages such as Hindi-Urdu, Punjabi, Marathi, Nepali, Romani and others make use of particles to express case relationships, commonly labelled 'absolutive', 'ergative', 'nominative', 'accusative', 'dative' and 'genitive'. In this section I base myself on Hindi, relying particularly on the detailed description of the case system found in Mohanan (1994b, ch 4). Mohanan (1994b: 66), following a long descriptive tradition, distinguishes the following cases:[1]

(9)   nominative   (zero-marked)
        ergative   ne
        accusative   ko
        dative   ko
        instrumental   se
        genitive   kaa
        locative1   mẽ
        locative2   par

In (10) we see a typical transitive (ergative) construction in the perfective aspect (Mohanan 1994b: 70):

(10) raam-ne   ravii-ko   piiTaa
      Raam-ERG   Ravi-ACC   beat
      'Ram beat Ravi'

The case markers are called clitics by Mohanan. They are not properly regarded as (canonical) affixes. They can only appear once in a given NP and they are restricted to final (rightmost) position on the NP. Moreover, a single case clitic can readily take scope over a coordinated NP.

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[1] In the transcriptions, capital T, D, N, R represent retroflex consonants, vowel doubling represents length and a tilde represents a nasalized vowel.

These case markers belong to what Zograf (1976) and, following him, Masica (1991) refer to as Layer II markers. However, Hindi nouns also show a form of inflection marked either by genuinely affixal formatives or by stem allomorphy, belonging to Zograf's Layer I. The Layer I affixes are true affixes in the sense that they show none of the clitic-like properties found with the case clitics and postpositions and indeed sometimes the forms are suppletive. There are three forms of interest to us.

First, nouns can be inflected for number (singular/plural). For instance, masculine nouns in *-aa* (other than those borrowed from Sanskrit and other languages) take the plural ending *-e*: *laRkaa* 'boy', *laRke*; *kamraa* 'room', *kamre*. Masculines in nasalized *-ãã* take *-ẽ*. Feminine nouns take the plural suffix *-ẽ*, except for those in *-i/ii*, which take the ending *-yãã*: *bahin* 'sister', *bahinẽ*; *maataa* 'mother', *maataaẽ*; *tithi* 'number', *tithiyãã*; *beTii* 'daughter', *beTiyãã*.

The second inflectional category is difficult to name. Historically it derives from the Sanskrit case system but there is now an entrenched tradition which uses the term 'case' for the Layer II clitics. For the present, let us distinguish a direct from an oblique form of the noun (following Mohanan (1994b: 61), who speaks of 'stem forms'). The oblique singular form of a native masculine noun in *-aa* is identical to the direct plural form. The oblique singular form of other masculine nouns and of feminine nouns is identical to the direct singular form. The plural oblique form ends in *-õ*.

The third Layer I case form is the vocative. In *-aa* masculines the (opional) ending is *-e,* while in the plural for all inflecting nouns it is *-o*. Recent discussion has tended to ignore the vocative on the grounds that it has no syntactic function, though this begs the question of how the vocative case is to be integrated into morphological descriptions and what relationship the morphological vocative bears to the other morphological cases.

Examples of all six inflected stem forms are shown in Table 4.

Table 4 Hindi 'case stem' forms

		'boy'	'girl'	'sister'
Sg	Direct	laRkaa	laRkii	bahin
	Oblique	laRke	laRkii	bahinẽ
	Vocative	laRkee	laRkii	bahin
Pl	Direct	laRke	laRkiyãã	bahin
	Oblique	laRkõ	laRkiyõ	bahinõ
	Vocative	laRko	laRkiyo	bahino

Even from this brief description it is evident that there is some unclarity in the case system. On the one hand, the case clitics *ne*, *ko*, *kaa* and so on have the usual functions of case markers, namely marking the grammatical functions of noun dependents. On the other hand, the case clitics themselves are not really affixes and tend to show properties of postpositions. In particular, the case clitics invariably select the oblique form of the head noun of the noun phrase. Moreover, in most Indo-Aryan languages the oblique form cannot be used on its own but is only found when governed by a case clitic or postposition (Masica 1991: 239). In order to avoid any possible terminological confusion I shall refer to the Layer I case inflections as 'm-cases' (for 'morphological cases'). To avoid commitment to any particular morphosyntactic analysis of the NP-case clitic formatives, I shall refer to them as 'case particles'. Thus, a word form such as *laRkõ* 'boys (oblique)' is an m-case oblique form (in the plural). The construction *laRkõ ko* 'boys (acc/dat)' is formed from the oblique m-case form of the lexeme LARKAA in construction with the case particle *ko* realizing what is generally referred to as accusative/dative case.

One important respect in which the oblique form has the morphosyntax of a case is found in adjective agreement. In Table 5 we see the declension of ACCHAA 'good' and the demonstrative YAH 'this' (McGregor 1995: 7f):

Table 5 Hindi adjective inflection

		Masc	Fem	Dem
		'good'		'this'
Sg	Dir	acchaa	acchii	yah
	Obl	acche	acchii	is
Pl	Dir	acche	acchii	ye
	Obl	acche	acchii	in

Notice that the demonstrative has suppletive inflected forms.

Adjectives agree not only in number/gender but also with respect to the direct/oblique m-case distinction. This is illustrated in Table 6 (Dymshits 1986: 78, 79):

Table 6 Examples of Hindi adjective agreement

Direct m-case forms:

Masc	Sg	acchaa laRkaa	'good boy'
	Pl	acche laRke	'good boys'
Fem	Sg	acchii laRkii	'good girls'
	Pl	acchii laRkiyãã	'good girls'

Oblique and vocative m-case forms before acc/dat case particle *ko*:

Masc	Sg	pyaare beTe ko	'favourite son'
		are pyaare beTe!	'O favourite son!'
	Pl	pyaare beTõ ko	'favourite sons'
		are pyaare beTo!	'O favourite sons!'
Fem	Sg	pyaarii beTii ko	'favourite daughter'
		are pyaarii beTii!	'O favourite daughter!'
	Pl	pyaarii beTiyoN ko	'favourite daughters'
		are pyaarii beTiyo	'O favourite daughters!'

Notice that the vocative plural form of masculines is treated like the oblique form with respect to agreement, not like the direct form.

No adjective or other modifier in Hindi agrees with respect to any Layer II property, in particular, the case particles do not trigger any kind of agreement.

As an interim summary we may say the following:

- Hindi nouns may inflect for number and m-case (direct, oblique, vocative).

- Hindi adjectives may inflect for number and m-case (direct, oblique, vocative), agreeing with the modified noun.

- Hindi NPs may be marked by case particles realizing (what have come to be called) ergative, accusative/dative, genitive cases and so on.

- The case particles serve solely to realize grammatical functions such as subject-of, object-of, possessor-of as well as various adverbial meanings. They do not participate in agreement relations.

3.2 The 'genitive'

The genitive case particle was given above as *kaa*. However, its morphosyntax is considerably more complex than that of the other case particles, as seen from the examples in (11) (McGregor 1995: 9; see also Payne 1995):

(11) a. us   strii   kaa   beTaa
          that   woman   KAA.M.SG   son
          'that woman's son'

   b. us   strii   ke   beTe
          that   woman   KAA.M.PL   son.PL
          'that woman's sons'

   c. us   strii   ke   beTe   kaa   makaan
          that   woman   KAA.M.OBL.SG son.OBL.SG   KAA.M.SG   house(M)
          'that woman's son's house'

d.	us	aadmii	kii	bahnõ	kaa	makaan
	that	man	KAA.F.PL	sister.F.OBL.PL	KAA.M.SG	house(M)

'that man's sisters' house'

e.	yah	makaan	us strii	kaa	hai
	this	house	that woman	KAA.M.SG	is

'this house is that woman's'

The case particle KAA agrees with the possessum in gender, number and m-case (even when the possessum is elided, as in (11e)). The pattern of agreement and its morphological realization is identical to that inflecting of adjectives. Clearly, Hindi instantiates the possessum-agreement construction. The only real difference between the Hindi and the Albanian constructions is that they are the mirror image of each other in their word order. In the next section we investigate to what extent the possessive formatives can be regarded as genitive case markers.

4. Does Albanian or Hindi have a genitive case?

In this section we investigate whether the Albanian and Hindi formatives can truly be called genitive cases, or, more precisely, whether the formal grammars of these languages ever need to appeal to an attribute-value pair [Case: Genitive].

Blake (1994: 1) speaks of case as 'a system of marking dependent nouns for the type of relationship they bear to their heads'. In the canonical possessive construction expressed by genitive case, the possessed noun functions as the head and the possessor noun functions as the dependent. In a language 'with genitive case' this dependency requires us to set up the [Case: Genitive] attribute. In order to investigate the notion of genitive case without admitting a whole host of typologically distinct constructions, it is desirable to distinguish canonical case markers such as the various forms of genitive in Latin, from markers which we would not wish to label as case markers, such as the English preposition *of*. We can therefore restrict the notion of case marking to individual words, that is, nouns as opposed to noun phrases. Beard (1995) proposes more rigorous criteria for casehood. He argues that it is only necessary to set up an attribute [Case] in languages in which one and the same case takes several different forms, a situation which regularly arises in a language such as Latin which has inflectional

classes. He claims that even an apparently paradigm example of a case language such as Turkish doesn't have a case system. There's no need to generalize across forms with a [Case] feature in Turkish, as all nouns have the same suffixes. We can generalize this by factoring in the effects of allomorphy due to cumulation of case with other features such as number, definiteness or possessor agreement. If, say, nouns have distinct affixes for one and the same case in singular and plural then again a [Case] feature is needed. Moreover, even with purely agglutinating languages, if the syntax imposes case agreement on modifiers then a [Case] feature will be needed in the syntax to generalize over the set of cases triggering that agreement. Spencer and Otoguro (2005: 121f) have expanded on this logic and propose what they call 'Beard's Criterion': a [Case] attribute is only warranted in the formal grammar of a language if it is needed to generalize over allomorphy due to inflectional classes or cumulation with other features, or in order to generalize over syntactic constructions, principally case agreement on modifiers. The question now reduces to that of whether the Albanian and Hindi 'genitives' satisfy Beard's Criterion. We investigate the relevant morphological and syntactic properties in turn.

The first morphosyntactic property is therefore a purely morphological one: a genitive case is an inflectional affix placed canonically on a possessor noun indicating that noun's grammatical relation to the possessed head noun.

If we adopt this (rather strict) morphological (affixal) criterion then it is clear that the Hindi 'genitive case' particle is not a case at all. All the case particles, including KAA, are clitics taking the whole of the NP in their scope, including coordinated NPs. This general standpoint on Indo-Aryan case particles is defended at some length in Spencer (2005). However, since this is still a controversial matter let us set aside those objections and continue to explore the idea that Hindi has a genitive case marker.[2]

The Albanian marker, somewhat ironically, is often referred to as a 'clitic', though it tends to show the properties of an affix, as we have seen. But this means that it might still make sense to regard the Albanian formative as an inflected form of the noun and

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[2] I shall continue to refer to the markers in both languages as 'genitive markers', even though I shall conclude that they are not cases at all.

hence, perhaps, a kind of case marker. To resolve this issue we need to look into the morphosyntax of the constructions in more detail.

We begin with the Albanian genitive, where we will see significant differences in morphosyntax of the genitive compared with the other (true) cases. The most transparent observation is that the Albanian genitive has completely different morphology from the other cases. First, it is a prefix (or perhaps a tightly-bound proclitic) and not a suffix. Second, it fails to cumulate the properties of definiteness, number and inflectional class like the other cases. At the same time the syntax of the genitive is completely different from that of the other cases. Attributive modifiers, including the genitive construction, agree with the head noun in definiteness, number, gender and case. However, these modifiers have no dedicated form indicating 'genitive case'. Rather, the modifier agrees in case with the oblique case-marked noun selected by the genitive 'article'. In terms of agreement the genitive is therefore invisible. Thus, the genitive construction fails to pattern like a case either in the morphology or in the syntax.

We have seen that the Hindi construction differs significantly from a canonical case form. Indeed, it has sometimes been claimed that the genitive-marked NPs are really a species of adjective, with the KAA formative being some kind of derivational morpheme. The relevant arguments are succinctly summarized in Payne (1995: 293f), in which he addresses this question explicitly by comparing the Indic languages (such as Hindi) with the Dardic language Kashmiri. He cites six reasons for not treating the Hindi KAA construction as a kind of adjectival derivation. The crucial point is that in their internal syntax the KAA-marked phrases retain nearly all the properties of NPs. The only adjectival property that they show is their external syntax: attributive modifier agreement with their head noun. Payne argues that the NP-internal syntax rules out an analysis under which KAA serves to convert the noun into an adjective, a conclusion we can concur with. From this, however, he concludes that the KAA marker is a genuine genitive case marker. However, the logic of the argument is flawed, in that the KAA marker can still be in construction with an NP without being a case.

The status of the case particles, and of the inflecting genitive particle in particular, has been the subject of some debate. If the KAA formative is treated on a par with the other case particles, and if the case particles are case markers, then we can treat KAA as

an inflecting genitive case marker, as Payne argues. The problem is that there is very little reason for treating the case particles as cases and very good reasons for not treating them as cases. As mentioned earlier these particles are clitics taking wide scope over coordinated NPs. It is ungrammatical to repeat a case particle within a coordinate structure (in this respect the Hindi case particles behave less like cases than the Albanian genitive article does). Moreover, there is no case agreement of any kind involving the case particles. There is only one sense in which the case particles behave like case markers proper:[3] they are often used to mark core grammatical functions of subject and direct object, as indicated in (9) above. Broadly speaking (see Butt and King 2004 for a more nuanced description, including the important role of agentivity), the ergative marker *ne* marks a transitive subject, though only in perfective aspect constructions. The *ko* marker is used canonically for indirect objects, but in addition can be used to mark a direct object depending on a complex set of factors including animacy and specificity/definiteness (see Mohanan 1993, 1994a, b for detailed discussion of these factors). The genitive can mark subjects of certain types of nominalized or infinitival (non-finite) subordinate clause. The other case particles mark locative or other 'semantic' functions.

Compare the Hindi case particles with the three inflected m-case forms of nouns illustrated above in Table 4. Inflecting nouns have singular and plural forms and three m-case forms. These forms cumulate number and inflectional class features with m-case. With respect to agreement it is these m-case forms that behave like true cases. Many modifiers are indeclinable, but an inflecting attributive modifier (including a genitive noun) agrees in number, gender and case (direct/oblique) with its head noun. Only a

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[3] Certain pronouns have special suppletive forms in free variation with the *ko*-marked forms. However, in Spencer (2005) I show that it would be a mistake to use this handful of forms as evidence for a full-blooded case system, in the same way that it would be inappropriate to argue that French prepositions are inflected for the definiteness, number and gender of their complements on the basis of portmanteau forms such as *du* 'of the.masc.sg'. The same pronouns in Hindi have portmanteau forms arising from fusion of the pronoun with the focus marker *hii*. This means that if the 'case'-portmanteaus force us to set up a case system for Hindi, by the same logic, the *hii*-portmanteaus would force us to claim that all nominals inflect for 'focus'.

noun in the direct-case stem form can trigger agreement on the predicate (which means that zero-marked intransitive subjects and zero-marked direct objects can trigger agreement, but not *ne*-marked transitive subjects or *ko*-marked direct objects). The case particles invariably select the oblique-case stem form of the noun. The oblique-case stem form is almost always found in conjunction with such a particle, but occasionally the bare oblique form is found, in which case it bears a locative meaning.

Moreover, there are further morphosyntactic reasons for withholding the label 'case' from the postpositional case particles. Sharma (2003) discusses the interaction between the case particles and emphasis or focus particles such as *hii*. For some speakers the focus particle can intervene between noun and the case particle (the '%' sign in (12b) indicates variation in speakers' judgement of acceptability; particles are separated from their hosts with the sign '='):

(12) a.   in   tiin   laRkõ=ko=hii   chot lagi

           these   three   boys=DAT=FOC   hurt got

           '(Only) these three boys got hurt'

     b.   (%)in   tiin   laRkõ=hii=ko   chot lagi

           '(Only) these three boys got hurt'

(13) a.   mai vahãã saikal=se=hii   pahũch saktii hũũ

           I   there   bicycle=LOC=FOC   reach   able am

           'I can get there only with a bike'

     b.   mai vahãã saikal=hii=se   pahũch   saktii hũũ

           I   there   bicycle=FOC=LOC   reach   able   am

           'I can get there with only a bike'

In (12) we see that the focus particle *hii* can optionally intervene between the case postposition *ko* and the noun phrase, while in (13) we see that difference in linear positioning between *hii* and the postposition *se* can give rise to scope effects. Sharma explicitly likens this behaviour to a similar, though more extensive interaction found in Japanese. Such behaviour in Japanese is part of a more widespread patterning which

severely undermines the treatment of the case particles of Japanese as case markers. Otoguro (2006) provides extensive argumentation to demonstrate that the case particles of Japanese and Hindi fail to behave like genuine cases in nearly all respects.

All these facts (and others detailed in Spencer 2005) show that it is the m-case forms and not the case particles that are the real case forms in Hindi. Although it is a convenient shorthand to refer to a *ne*-marked NP as 'ergative', this is strictly speaking an abuse of terminology, on a par with calling an English *of*-phrase a 'genitive'. In sum, the Hindi forms marked with the inflecting particle KAA are not really cases because none of the case particles are case markers.[4]

We have arrived at the conclusion that the Albanian and Hindi 'genitives' are neither adjectival derivational formatives nor true genitive case markers. Rather, they are markers which are attached to nouns (Albanian) or NPs (Hindi) and which give that NP (or the phrase headed by the marked noun) the external agreement morphosyntax of an adjective: the possessum-agreement construction. We now look at a 'pure' instantiation of that construction.

5.   The Bantu possessum-agreement construction

The typical possessive construction in Bantu languages is an unadorned version of the possessum-agreement construction, as illustrated from Swahili in (14), from Ashton

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[4] An example of the descriptive difficulties we get into when we try to treat the case particles as genuine cases is revealed in Masica's (1991) survey of the Indo-Aryan languages. Masica (1991: 239) points out that some descriptions regard the oblique form as an 'Oblique Base' rather than a case 'since it has no casal function', that it, is cannot be used on its own to signal argument structure relationships). He then adds in a footnote that '[t]he historically-minded conversely sometimes prefer to treat it as the *only* "case", very general in function, with specifying postpositions added (1991: 474, fn. 17, emphasis original)'. The jibe at the 'historically-minded' is aimed at those who stress the fact that the oblique forms generally reflect earlier inflectional cases in Sanskrit. However, Masica soon afterwards finds himself in a quandary when dealing with the morphosyntax of adjectives (p. 250), for he is obliged to describe that as agreement with Layer I case (sic).

(1944: 55f, 324) (though other languages of the group pattern in essentially the same way – see Welmers 1973, especially chapter ten for general discussion):

(14) a.   k-iti   ch-a   Hamisi
             CL7-chair   CL7-POSS   Hamisi
             'Hamisi's chair'

      b.   k-iti   ch-a   nani
             CL7-chair   CL7-POSS   who
             'whose chair'

      c.   v-iti   vy-a   nani
             CL8-chair   CL8-POSS   who
             'whose chairs'

      d.   k-iti   ch-a-ngu
             CL7-chair   CL7-POSS-1SG
             'my chair'

      e.   v-iti   vy-a-ngu
             CL8-chair   CL8-POSS-1SG
             'my chairs'

As can be seen, the possessive construction is mediated by an inflecting particle -*a*. This construction goes by a variety of names in Bantu linguistics, including associative -*a*, connecting -*a* and the a-binder, and, in Ashton's grammar of Swahili, '-A of Relationship'. The connecting element takes concord prefixes in agreement with the possessum. The concord markers are given their traditional labels here: Classes 7/8 are respectively the singular and plural classes for (broadly speaking) smallish artefacts. Before a vowel the /i/ of *ki/vi* undergoes gliding and the glide then triggers palatalization of /k/ to /č/, represented as 'ch' in the orthography.

Like the Albanian and Hindi 'genitive', the -A of Relationship has exactly the same agreement morphosyntax as attributive modification. Compare the possessive construction examples in (14) with the attributive modification examples in (15):

(15) a. k-iti   ch-ema   ki-moja
          CL7-chair   CL7-good   CL7-one
          'one good chair'

      b. v-iti   vy-ema   vi-tatu
          CL8-chair   CL8-good   CL8-three
          'three good chairs'

Schematically we can represent the Bantu possessive/attributive constructions as in (16):

(16) Possessed$_i$ AGR$_i$-a   Possessor
      Head$_i$ AGR$_i$-Attribute
      CL7-chair   CL7-a   Hamisi   'Hamisi's chair'
      CL7-chair   CL7-good   'good chair'

The -A of Relationship has a great variety of uses in addition to simple possessive constructions (Ashton 1944: 145). In particular, it can cooccur with verbs in the infinitive (17a) and adverbs (18b):

(17) a. chakula   ch-a   ku-tosha
          food   AGR-A   INF-suffice
          'sufficient food'

      b. w-a   kupigwa   wakapigwa   w-a   kukimbia   wakakimbia
          AGR-A beat.PASS.INF were.beaten   AGR-A get.away.INF   got away
          'Those who were to be beaten were beaten and those who were to get
          away, got away'

(18) a. chakula ch-a jana
         food   AGR-A yesterday
         'yesterday's food'

    b. vyombo   vy-a ji-koni
        things   AGR-A LOC-kitchen
        'kitchen utensils (lit. things of in-the-kitchen)'

It seems to be taken for granted by most commentators (for instance, Vitale 1981: 108) that the constituent structure of the -A of Relationship is Pd [AGR-a Px] (though I have not seen a formal demonstration of this). This is very clear where the possessor is a pronominal, as in (14d, e), in that the -*a* formative and the possessive pronominal stem fuse to form a single word. The AGR-A formative is always immediately to the left of (the head of) the possessor phrase. On the other hand, the possessed/modified noun can be elided altogether, as in (17b) and it can be separated from the possessor/modifier phrase by other modifiers, as in (19):

(19) ratli   mbili   z-a   sukari
     pound   two   AGR-A   sugar
     'two pounds of sugar'

In other Bantu languages the constituency may be more obvious than in Swahili. In Xhosa, for instance, the class agreement prefix coalesces with a following vowel within a word (see du Plessis and Visser 1992: 328f):

(20) ízi-njá   z-a-índoda   => zéndoda
     CL8-dog   CL8-A-man   'the man's dog'

This type of morphophonemic alternation does not occur between word boundaries, so that we can be confident that the constituent structure is as shown in (20).

The -A of Relationship construction is shown schematically in (21):

(21)   Swahili '-A of Relationship'

Possession

daughter$_i$ [AGR$_i$-A   man]

Pd$_i$ [AGR$_i$-A   Px]

'the daughter of the man'

Swahili attributive modification

daughter$_i$ AGR$_i$-beautiful

N$_i$ AGR$_i$-ADJ

'the beautiful daughter'

This can be regarded as the canonical form of the possessum-agreement construction.

Bantu languages lack case. They exhibit the possessum-agreement construction in its 'pure' form so to speak. The Albanian and Hindi 'genitive' constructions share all their important properties with the Bantu -A of Relationship possessum-agreement construction, including constituent structure. Therefore, either Albanian and Hindi lack a true genitive or Bantu languages have an (extremely aberrant, not to say unique) genitive case.

6.   Conclusions

Although the Albanian case system has figured rather sparsely in theoretical debate, systems such as that of Hindi have been widely discussed in the literature on case. This paper has shown that nearly all of that debate is misguided, since neither language has a genitive case. However, I do not wish to end on a negative note. Languages such as Albanian, Hindi and Swahili illustrate that there is a close relationship between the functions of possession and attributive modification. In the possessum-agreement construction what is actually happening is that a morphosyntactic construction that canonically is used for attributive modification has been seconded to express possession, a relationship that is typically expressed in morphology by case or by possessor agreement. At the same time many languages use genitive case marking or possessor agreement for expressing attributive modification. For instance, it is common for languages to express modification of a noun by a noun by using the genitive form of the

attributively used noun, as in English *children's party* or *men's room*. Similarly, many languages use possessor agreement as a way of expressing N N modification, giving constructions with the form 'bed its-room' for 'bedroom' (see the examples of Turkish izafet in Spencer 1991, for instance). Another strategy for modifying a noun by a noun is to transpose the modifying noun into a relative adjective without adding a semantic predicate. This is what happens with an expression such as prepositional phrase (and why this means the same as preposition phrase). Finally, many languages express possession by turning the possessor noun/pronoun into an adjective. Indo-European languages do this routinely with pronominal possessors, but many languages also do it with lexical nouns so that the meaning 'the president's palace/the palace of the president' is expressed using a possessive adjectival form of the noun, as in *the presidential palace*. (See Corbett 1995, for why this isn't a genitive in Slavic, and for general discussion see the work of Koptjevskaja-Tamm in this area, e.g. Koptjevskaja-Tamm 1995, 2002, 2003a, b, 2004.) Work currently in progress is investigating the formal, syntactic, semantic and typological aspects of this problem with a view to identifying a checklist of morphosyntactic devices and their canonical uses, and a set of universal implicational hierarchies on those devices (Nikolaeva and Spencer 2007a, b). Watch this space!

Acknowledgements

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