snippets

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Contents

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Note.
This issue consists exclusively of invited submissions, which were reviewed by a special review board constituted for this issue only. *Snippets* will return to its usual submission and review procedure as of the next issue.

0.

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A message from the editors of the special issue

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In 2006 a number of squibs were offered as birthday present to Manfred Krifka. The original collection, named *Between 40 and 60 Puzzles for Krifka*, edited by ourselves together with Regine Eckardt, Renate Musan, and Barbara Stiebels, can be accessed online at http://www.zas.gwz-berlin.de/publications/40-60-puzzles-for-krifka. From these "puzzles," approximately two thirds were submitted for this special issue of Snippets and 17 have been accepted for publication. We thank the reviewers for their help, the authors for their patience, and Orin Percus for both his help and his patience.

S.B. & H.M.G.

1.

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Positively comparative

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English comparative constructions typically include an item of comparison, a *than*-clause or phrase as in (1). But this is not necessarily so. We also find data like (2).

(1) a. Mr Darcy is richer **than Mr Bingley is**.
b. Mr Darcy is richer **than Mr Bingley**.
c. Mr Bingley has five thousand a year. Mr Darcy is richer **than that**.

(2) Mr Bingley has five thousand a year. Mr Darcy is richer.

In this case, we intuitively make an anaphoric connection between the first clause in (2) and the second clause in (2). The example is very similar to (1c). Let us assume that the interpretation of (1c) proceeds as sketched below (compare e.g. Stechow (1984), Heim (2000)).

(3) a. [[-er [than that]] [1[Mr Darcy is t1 rich]]]
b. [[[1[Mr Darcy is t1 rich]]]]^g = λd.Mr Darcy is d-rich
c. [[that]]^g = 5000£ p.a.
d. [[-er]](d)(D) = 1 iff max(D)>d

(3') a. max(λd.Mr Darcy is d-rich) > 5000£ p.a.
b. The degree d such that Mr Darcy is d-rich exceeds the degree of wealth measured by an annual income of 5000£.

It is natural to suppose that the only difference between (1c) and (2) is that the item of comparison is an overt anaphoric element 'that' in the case of the former and a covert anaphoric element in the case of the latter. Thus the interpretation of (2) follows the steps in (4) and derives the truth conditions in (3') just as before.

(4) a. [[-er c] [1[Mr Darcy is t1 rich]]]
b. [[-er c]]^g (λd.Mr Darcy is d-rich)
c. [[c]]^g = 5000£ p.a.

Let us compare this to examples in which the adjective shows up in the unmarked, positive form. It is well-known that the interpretation of such statements is context dependent. The individual to whom the property expressed by the adjective is

attributed has that property to an extent remarkable in the relevant comparison class. This is not expressed overtly in (5), but must be derived from the context. A simple semantics for (5) (following specifically the formulation in Heim & Kratzer (1998)) is given in (6).

(5) Mr Darcy is rich.

(6) a. $\exists$d[Mr Darcy is d-rich & d>c]
(where c is the wealth standard made salient by the utterance context)
b. $\text{rich}_{POS} = \lambda x.\exists d[x \text{ is d-rich} \ \& \ d>c]$
(where c is the wealth standard made salient by the utterance context)

If we do not know exactly what the context of (5) is, an intuition of vagueness arises: it is not completely clear what would count as rich (rich compared to the average member of the society we are considering? Rich for a gentleman?). It is interesting that the positive allows explicit information as to the intended standard. Beck et al. (2004) propose that the *compared to*-phrase in (7) interacts with the main clause in the following manner (the implicatures that arise in such examples are ignored here):

(7) **Compared to Mr Bennet**, Mr Darcy is rich.

(8) a. $\exists$d[Mr Darcy is d-rich & d>c]
(where c is the wealth standard made salient by the utterance context)
b. c := the standard of wealth made salient by comparison to Mr Bennet
:= Mr Bennet's degree of wealth

Thus the *compared to*-phrase serves to indirectly (contextually) fix the intended value for the comparison standard of the positive. The question I would like to raise is: what precisely is the difference between the positive and the comparative? It seems that there is no significant theoretical difference between (9a) and (9b) regarding how we identify the degree that Mr Darcy's wealth is compared to. Both times we suppose that the semantics provides a free variable whose value is contextually fixed. There is even some evidence in favour of such similarity in that (10) is acceptable as well.

(9) a. Mr Darcy is rich.
b. Mr Darcy is richer.

(10) **Compared to Mr Bennet**, Mr Darcy is richer.

(11) a. $[[\text{-er c}]]^g (\lambda d.\text{Mr Darcy is d-rich})$
b. c := the standard of wealth made salient by comparison to Mr Bennet
:= Mr Bennet's degree of wealth

However, there is a clear intuitive difference between (9a) and (9b) in terms of the discourse contexts in which they are acceptable. While (9b) requires that a particular degree of wealth has been made available to serve as an antecedent for the covert item of comparison in (9b), (9a) imposes no such requirement. This is immediately apparent in (12), where only the sentence with the adjective in the positive form is an acceptable reply by Charlotte. It can also be seen in (13): the comparative version (assuming that there is no further relevant preceding context) entails that Mr Darcy makes more money than the just mentioned 5000£ p.a. The positive version entails no such thing; the 5000£ p.a. could be a description of Mr Darcy's wealth.

(12) Lizzy: Tell me something about Mr Darcy.
Charlotte: Mr Darcy is rich./ # Mr Darcy is richer./
Mr Darcy is richer than that.

(13) 5000£ p.a. is a nice income. Mr Darcy is richer./ Mr Darcy is rich.

Our standard theories of comparison, and indeed our understanding of context dependency in general, would model both kinds of data with the help of a free variable whose value is assigned by context. The value of the variable is a degree of wealth in both (9a) and (9b). But the discourse behaviour is different in that it needs a clearly provided antecedent in (9b) but not in (9a). I do not know why that is. I also do not know how to model the empirical difference with the theoretical tools at my disposal. I further think that this is one instance of a general question about the status of various items of comparison that language can specify or neglect to specify overtly, that is, the question of how these items interact with the compositional semantics of comparison.

(14) a. He is rich **for a farmer**.
b. (**Of these men**,) he is the richest.

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2.

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Why Ambiguity?

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All human languages are ambiguous. Ambiguity is not restricted to some special constructions that linguists are fond of discussing, but is quite ubiquitous. It is hard, in fact, to find a sentence that is not ambiguous. This fact is all too familiar to computational linguists: "One often hears in computational linguistics about completely unremarkable sentences with hundreds of parses, and that is in fact no exaggeration" (Abney 1996). The question is, simply, why? Why is language ambiguous?

Krifka (2002) raises a similar question concerning vagueness. He shows that there are good reasons for language to allow, and even encourage vagueness. Vagueness, however, is not ambiguity. When we are told that the theater is far from here, we may be unsure as to the precise distance; but we know what the speaker intends, and we can draw inferences based on this, e.g., that we should take a cab instead of walking to the theater.

But with an ambiguous term, the intended meaning is not merely insufficiently specified; it is not known, until the term is disambiguated. When we hear that John has a kid, we draw very different inferences if John is a father or if he owns a young goat.

Thus, the advantages of vagueness do not seem to apply to ambiguity, and we are back to the question: why ambiguity?

Some may say that it doesn't matter. Humans possess very powerful mechanisms for disambiguation; these mechanisms resolve ambiguity so well, that we are often not even aware that an expression is ambiguous. But this is not really an answer: these powerful mechanisms are there *because* language is ambiguous. If it weren't, they wouldn't have developed. Moreover, these mechanisms must require extra processing time. We know that all senses of an ambiguous word are accessed first, and only then does disambiguation occur (Swinney 1979). If language were not ambiguous, we would need to access fewer meanings, and we would be saved the additional process of disambiguation. And, of course, powerful though they unquestionably are, these disambiguation mechanisms are not perfect, and misunderstandings do occasionally occur, with consequences ranging from the hilarious to the disastrous. Thus, although our capacity for disambiguation may attenuate some of the disadvantages of ambiguity, these disadvantages are still there. Why would language burden itself with such a handicap?

It might be felt that language, because of its complexity, *must* be ambiguous—that any naturally developing formal system of that level of complexity must be ambiguous. While artificial formal languages are designed not to be ambiguous, their

complexity is lower than that of natural language by orders of magnitude; and even with these relatively simple languages, lack of ambiguity is accomplished by cumbersome and somewhat unnatural methods (e.g., brackets in mathematics and logic). Perhaps it is very hard to design an unambiguous complex language, so that for languages that evolved naturally, not by design, there is no hope of avoiding ambiguity.

Perhaps there is some merit to this claim, perhaps not; but what *is* clear is that language could easily have been, if not completely unambiguous, then much less ambiguous than it actually is. We know that languages are subject to *some* pressure to eliminate ambiguity: the devices of word order, case markings, agreement, and the like do not seem to have any other function than the avoidance of ambiguity. And if a language loses one of these devices, e.g., case markings, then another device, e.g., word order, becomes more powerful and restrictive. With very simple modifications, such devices could have evolved to reduce ambiguity to a much greater extent than they actually do. Agreement with indirect objects, for example, would substantially reduce PP attachment ambiguities. And yet, such a device is relatively rare.

Another popular answer follows Zipf's (1949) Law. The idea is that the current state of language is the result of the interaction between two competing goals: the good of the speaker and the good of the hearer. From the point of view of the hearer, the ideal language would contain no ambiguities, so that there would be no misunderstandings. In contrast, the ideal language from the point of view of the speaker is maximally ambiguous, so that one word would mean everything. Thus, speakers would require no effort at all in constructing their utterances: every utterance will always be the same word. The current state of language (namely ambiguous, but not maximally so), it is claimed, is the result of a balance between these two opposing forces.

This idea sounds appealingly simple, but is ultimately problematic. It rests on the assumption that ambiguity reduces the number of expressions in a language, hence making the speaker's job, of choosing between them, easier. But there is no evidence that this is so: the fact that *kid* means *child* as well as *young goat* does not mean that the word *child* is about to disappear from the language. Quite the contrary, in fact: languages tend to be *redundant*, i.e. use several expressions to express the same meaning. This means that, if anything, there is pressure to *increase*, rather than *decrease*, the number of expressions in a language.

It might be argued that, while ambiguity may not reduce the total number of words in a language, it reduces the number of *long* words. It is well known that short words tend to be more ambiguous than long words. Perhaps, then, the role of ambiguity is to "recycle" the short words, so that long words would only be used infrequently. In this way, speakers' job is made easier, since the words they need are usually short, hence easy to produce.

However, as mentioned above, it appears that the driving force behind language is redundancy rather than economy, and this seems to be the case here as well. If there were substantial pressure on language users to reduce the number of long words, we would expect many more nonsense short words to be used up, before turning to ambiguity. Why go to the trouble of ambiguity, when there are many strings that do not even have a single interpretation? Why not, in English, hang some meaning onto, e.g., *nisk*, *nime*, *bish*, *nast*, etc., before attaching it to a word that is already used?

We seem to be forced to the conclusion that there is some pressure on languages to be ambiguous: there is some advantage to using an ambiguous language. But what could this advantage be?

Wasow *et al.* (2005) make some tentative suggestions. They consider the case of two distinct dialects spoken in adjacent regions, and of a people living close to the border between the regions. Then, if some expression *E* has different meanings in the two regions, it will be to the advantage of these people to use *E* ambiguously in their dialect. Another case considered is that of speakers who wish to mislead without actually lying, by using an expression that can be interpreted in more than one way.

While ambiguity may well be advantageous in such cases, it does not seem plausible that such a prevalent phenomenon in all of the world's languages is motivated by a handful of rather special cases, and that this is sufficient to override the substantial disadvantages of ambiguity.

As far as I can see, the reason for the ambiguity of language remains a puzzle—we simply don't know why language is ambiguous. An idea presented in a science fiction novel (Brin 1995,582-583) appears to provide as good an answer as any:

> 'Pray, what could language be for, if not to maintain a culture's cohesion and foster communication?' …
>
> 'There is another desirable thing,' Sara replied. 'Another product of language, just as important, in the long run, as cohesion.' 'And that is?'
>
> '*Creativity*. If I'm right, it calls for a different kind of grammar. A completely different way of looking at error.'
>
> 'One that *welcomes* error. Embraces it.' Dedinger nodded. 'This part of your paper I had trouble following. You say Anglic [a future descendant of English] is better [than the unambiguous alien languages] because … errors and ambiguity creep into every phrase or paragraph. But how can *chaos* engender inventiveness?'
>
> 'By shattering preconceptions. By allowing illogical, preposterous, even obviously *wrong* statements to parse in reasonable-sounding expressions …'
>
> 'This is good?' 'It's how creativity works.'

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3.

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The asymmetry of affixation

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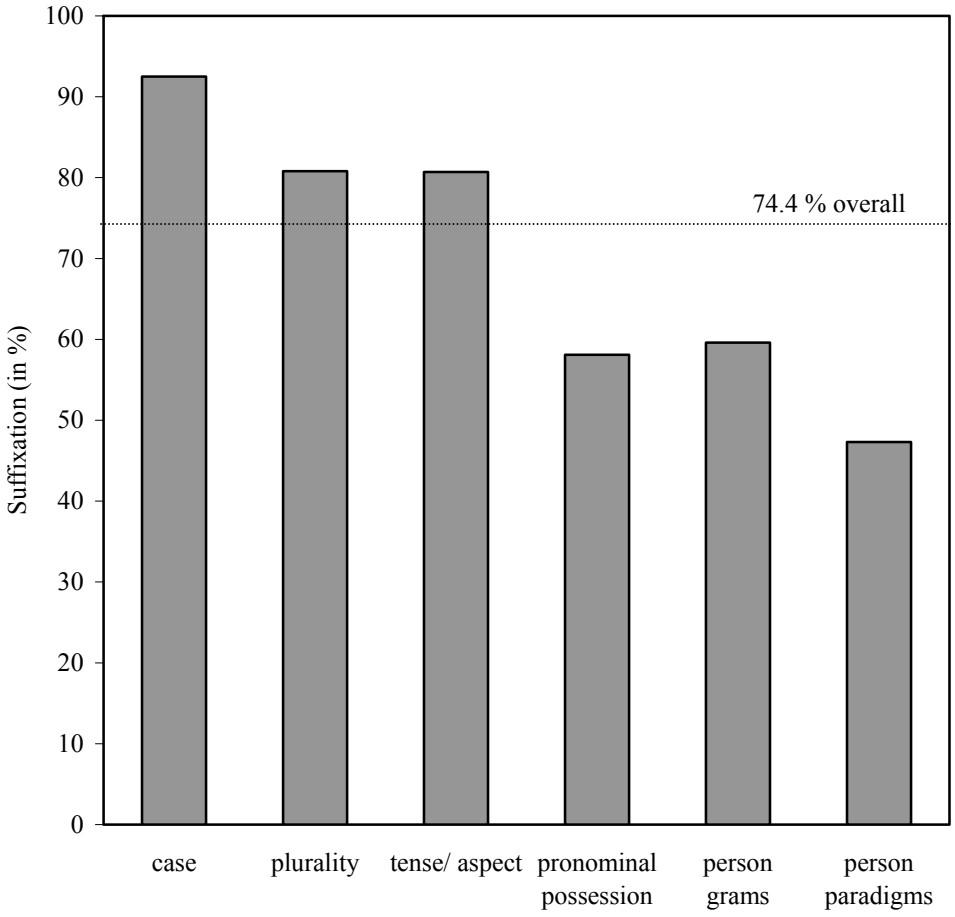
There is an old observation that, from a global perspective, there are more suffixes than prefixes in human languages. Probably the first to explicitly assert this was Edward Sapir: 'Of the three types of affixing – the use of prefixes, suffixes, and infixes – suffixing is much the commonest' (Sapir 1921: 67). Bybee *et al.* (1990: 4) provide some numbers showing this effect. In their cross-linguistic database of grammatical markers, they report to have an overall total of 1,236 suffixes and 426 prefixes (= 74.4% suffixes). There are a few attempts in the literature to explain this phenomenon (e.g. Greenberg 1957: 86-94; Cutler et al. 1985; Hawkins and Cutler 1988; Bybee et al. 1990). However, on closer inspection it turns out that the typological details of the suffixation preference are much more intricate than often assumed. In this squib, I will summarize a few observations concerning the asymmetry of affixation, based on data from recent typological surveys. There are considerable differences as to the extent to which a suffixation preference is attested, depending on which kind of marking is considered. There is even at least one phenomenon that shows a prefixation preference. The riddle is thus not so much the existence of an overall suffixation preference, but the extent of any asymmetries in affixation. The much more difficult, and unsolved, question is why a suffixation preference is attested for some categories, but not for others.

The percentage of languages having suffixes (as opposed to prefixes) is summarized in Figure 1, specified for a few different morphosyntactic categories. Nominal case marking (431 suffixes vs. 35 prefixes, data from Dryer 2005a), nominal plural marking (495 suffixes vs. 118 prefixes, data from Dryer 2005b), and tense/aspect marking (629 suffixes vs. 150 prefixes, data from Dryer 2005c) all show a clear suffixation preference. The percentage of languages with suffixes for these categories even surpasses the overall figure of suffixes from Bybee *et al.* (1990: 4). In contrast, person marking does not show a clear suffixation preference. In Figure 1, three different counts for person affixation are summarized. When looking only at paradigms to mark pronominal possession, i.e. the person marking in constructions like 'my book', there is only a slight tendency towards suffixation (330 suffixes vs. 238 prefixes, data from Dryer 2005d). Likewise, when summarizing over all person marking morphemes, a comparably small suffixation preference is attested (354 suffixes vs. 240 prefixes, data from Bybee *et al.* 1990: 9, 13, 15). However, in my own research of person marking I did not find any suffixation preference. In contrast, my results show even a slight tendency toward prefixation (80 suffixes vs. 89 prefixes, data from Cysouw 2003: 316)

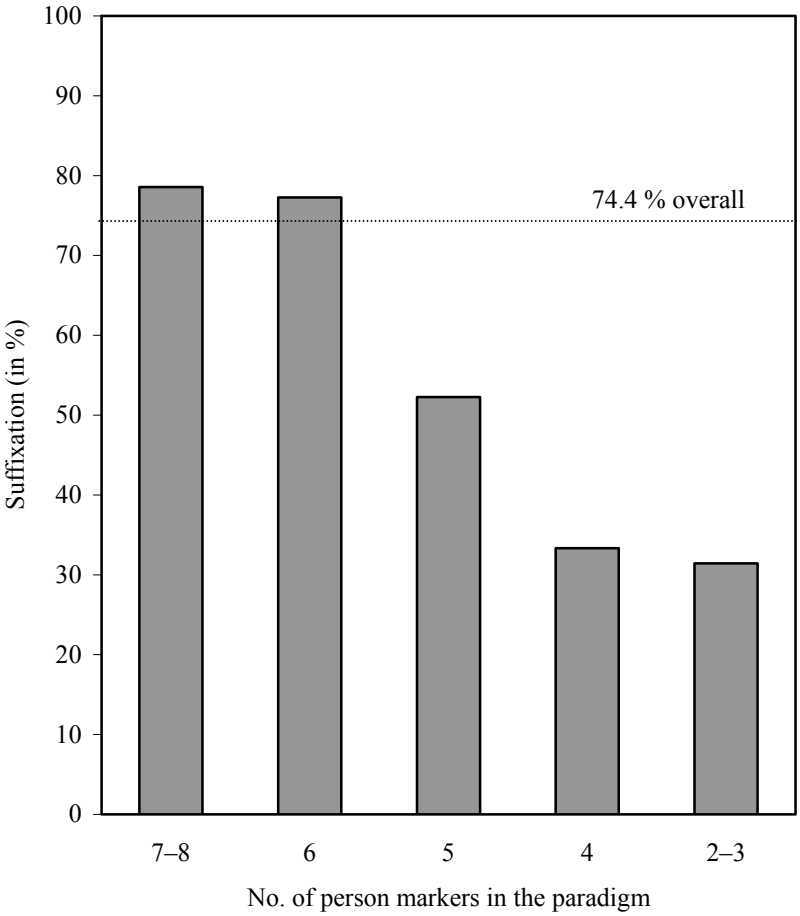
Figure 1. Differences in the suffixation preference.



The obvious explanation for the unexpectedly high number of person prefixes is that they arose from preverbal subject pronouns (cf. Givón 1976). However, there is more going on that does not fit in with this explanation. Looking somewhat more closely at the marking of person, a subsidiary effect is discernible. There is a relation between the size of the person marking paradigm and the suffixation preference. In Figure 2, different sizes of the person paradigms are distinguished, and for each size the fraction of languages that have suffixal paradigms is presented (data from Cysouw 2003: 316). Note that for these counts, I have only investigated person marking in intransitive constructions, and I ignored gender distinctions (these restrictions in the collection of data were purely of pragmatical nature). Further, I will ignore the marking of higher numbers (like dual, trial or paucal—this restriction is only added here to ease the presentation). There is a maximum of 8 different person markers because various

forms of clusivity are possible (see Cysouw 2003: Ch. 3 for details). As can be seen in Figure 2, the larger paradigms show a clear suffixation preference, roughly to the same extent as the overall suffixation preference from Bybee *et al.* (1990: 4). In contrast, the smaller paradigms show a clear *prefixation* preference, with only around 30% of the cases being suffixes. This effect shows that while there is clearly an overall suffixation preference among the world's languages, there are also specific situations in which prefixation is preferred. Note that explaining person prefixes as arisen from erstwhile preverbal pronouns does not at all help to understand why it is exactly small paradigms that have a prefixation preference.

Figure 2. Relation between paradigm size and suffixation.



Investigating somewhat more closely why small paradigms are much more often prefixal, two observations can be made that might help explain these curious statistics (cf. Cysouw 2001). First, languages with small prefixal person paradigms

very often have separatistic person/number marking with optional number marking, and, second, they appear to be areally overrepresented in the Americas. Let me explain these two observations in some more detail. Among small person paradigms, two different kinds of paradigms can be distinguished. Some paradigms do not have any obvious internal structure. The small size of these paradigms is mostly the result of random historical mergers. For example, in the German regular present verb inflection only four person markers are found, (*-e, -st, -t, -en*), which have synchronically a rather incoherent range of functions. Paradigms of this kind show a 'normal' suffixation preference. In contrast, a second group of small person paradigms consist of prefixes that are indifferent to number. Number can in some of these languages be marked as suffixes, but is mostly optional. When only such 'separatistic' person/number paradigms are considered, the percentage of suffixes falls below 20% (Cysouw 2001). The real riddle is thus why languages that separately mark person and number almost always mark person by prefixes.

One possible answer is that this is all just a historical coincidence. And indeed, when looking at the world-wide distribution of person prefixes, there is an inclination for them to occur in the Americas (cf. Cysouw 2001; Dryer 2005d). Currently, it is unclear how such a phenomenon has to be interpreted, though one possible interpretation is that the areal skewing is a founder-effect: the first humans to colonize the New World accidentally had a small prefixal person paradigm, and this phenomenon subsequently spread throughout the Americas. If this speculation contains any truth, then the prefixation preference for (small) paradigms could be a historical coincidence, messing up the statistics of a 'real' suffixation preference.

Finally, I would like to add one somewhat preliminary observation. As mentioned above, the counts in Figure 2 did not include the marking of gender, higher numbers, nor transitive constructions (with which I mean here cumulative morphemes combining subject and object reference). My impression is that the inclusion of these categories would not change the generalization that small paradigms have a prefixation preference. However, when these categories are also considered, there are a few languages that have exceptionally large paradigms of person. In particular, extremely large paradigms combining person, number, gender and case are found in the Bantu family (Africa), the Gunwingguan family (Northern Australia) and the Iroquoian family (North America). In the present context, the noteworthy characteristic shared by all these extremely large person paradigms is that they are *prefixal*. This observation is in need of more rigorous typological testing, but it suggests that there might also be a prefixation preference for very large person paradigms.

The suffixation preference has long been considered to be a riddle in need of an explanation. However, when considering the typological observations as presented in this squib, it does not seem to be a fruitful approach to consider the suffixation preference as a monolithic observation to be explained by one overarching theory of linguistic affixation. There are clear differences in the suffixation preference depending on the kind of morphosyntactic category considered. Some categories indeed have a strong preference for suffixation (e.g. case, plurality, tense/aspect), but others do not (e.g. person). Even within categories there are large differences as to the presence of any suffixation preference (e.g. depending on the size of the person paradigm). There are even linguistic phenomena that have a *prefixation* preference (e.g. person marking

in separatistic person/number paradigms). The big riddle of the suffixation preference thus actually consists of various smaller-scale riddles concerning different kinds of affixation asymmetry. The real riddle is not to explain the suffixation preference itself, but it is to explain why only a specific set of linguistic phenomena show a suffixation preference, while others do not.

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4.

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The puzzle of misleading focus clitic position and prosody

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Recent work on the prosody of focus like Rooth (1992, 1996), Selkirk (2004) and Truckenbrodt (1995, 1999) leads us to expect that the focused argument of a focus-related morpheme should be made prominent either phonologically, by having the same focus prosody as other focus constructions; or morphologically, by adjacency of the focusing morpheme and its argument. For example, in English, sentential accent marks all types of focus, including focus on the italicized argument of 'also' in (1c):

(1) Where are you going to eat dinner on Friday?
   a. We are going to *an Italian restaurant* for dinner on Friday.
   b. We are going to an *Italian* restaurant, not a *Thai* restaurant.
   c. We are <u>also</u> going to an Italian restaurant on *Saturday* night.

However, analogous focus particles in Chitumbuka, a Bantu language spoken in Malawi, do not conform to this proposal, as the position of the particle and/or prosody do not always highlight the focused argument.

The association-with-focus verbal enclitic, *-so* 'also; again' illustrates the problem most clearly. It attaches only to verbs and is followed by a phonological phrase boundary (indicated with parentheses). The verb host realizes the prosody – penult lengthening and contour tone – which motivate the phonological phrase boundary. Notice in the data in (2) - (5) that the verb is not always the argument of this clitic even though it is always the host. Further, a phonological phrase boundary consistently follows the clitic, not its argument – the constituent in focus. This leads to potential ambiguity about what is in focus. For example, in (4b), the subject, the verb, the verb phrase or the object could be interpreted as the argument of *-so* without the context in (4a) to disambiguate:

(2) a. (n-khu-limilíra ma-púuno).
   I-TAM-weed 6- tomatoes
   'I am weeding tomatoes.'
   b. (Ku-limiliráa-*so*) (ngóomá)?
   You/TAM-weed-also maize
   'Are you also weeding the maize?'

(3) The friend who killed the snake also brought father to the hospital.
(Mu-nya[β]o uyo wa-ka-yi-koma n-jô:ka)
1-friend 1.REL 1-TAM-9.OM-kill 9-snake
(ndiyo wa-k-izáa-*so*) (na [β]a-dada [β]-â:[β]o) (ku-chi-patâ:la).
is.who 1-TAM-bring-also with 2P-father 2P-their Loc-7-hospital

(4) a. Q- Is it only the doctor who helps the teacher?
(Ni [β]a-dokotala péera) (a[β]o [β]a-ku-vwíra [β]a-sambíizíi)?
COP 2P-doctor only 2P.REL 2P-TAM-help 2P-teacher
b. A- No, the chief also helps the teacher.
(Yâ:yí), ([β]a-fúmu [β]a-ku-vwiráa-*so*) ([β]a-sambíizi).
no 2P-chief 2P-TAM-help-also 2P-teacher

(5) a. Q- Are you going to Lilongwe today?
(Kâ:si), (mu-ku-luta ku-Lilóongwe) (mw-ahúunóo)?
Q you-TAM-go LOC-Lilongwe today
b. A- Yes, and I am also going to Salima.
(Ê:nya), (n-khu-lutáa-*so*) (ku-Salíima).
yes I-TAM-go-also LOC-Salima

To sum up the description of the data, focus-related enclitics attach only to the verb – most plausibly because it is the head of the VP – even though this is not the position that would fall out from either syntax or discourse function. Prosodically, they are systematically followed by a phonological phrase break, even if they themselves are not in focus.

Work by Rooth (1992) on focus-related morphemes has argued that focus particles like these should be morphologically and phonologically uninteresting. The focused argument of these morphemes should be made prominent either phonologically, by having the same focus prosody as other focus constructions, like Q/A pairs and in situ contrastive focus; or morphologically, by adjacency of the focusing morpheme and its argument. The proposal that all focus constructions – including focus-related morphemes – should have the same prosody is at least implicit in phonological theories of focus prosody, like Selkirk (1984, 1995, 2004) and Truckenbrodt (1995, 1999), as well as in syntactic and semantic work on focus like Reinhart (1995), Samek-Lodovici (2005) and Szendröi (2003), which generally assumes the STRESS-FOCUS constraint in (6):

(6) STRESS-FOCUS (Samek-Lodovici 2005: 697):
For any XP_f and YP in the focus domain of XP_f, XP_f is prosodically more prominent than YP.

The Chitumbuka data raises problems for these proposals, as the focus argument of enclitics is not always made prominent by either phonology or morphology. (Additional work demonstrates that Bantu languages in general do not support the STRESS-FOCUS constraint (6). See, e.g., Downing (2003, 2006), Downing et al. (2004) and Zerbian (2006).) In particular, data like (4b) shows that *-so* is cliticized

to the verb even if the entire VP or only the complement is focused. As a result, this particle does not make its focused argument morphologically prominent. A further problem is that the phonological phrasing found with focus-related morphemes does not always match the phonological phrasing found in other focus constructions, as work like Rooth (1992, 1996) and Truckenbrodt (1995, 1999) predicts. Phonological rephrasing is the most consistent cue to focus on answers to *Wh*-questions that fall within the VP: the focused constituent is always followed by a Phonological Phrase break. In contrast, it is the focus-related morphemes themselves which trigger phonological rephrasing. Their focused arguments are not consistently highlighted by any special prosody.

This, then, is the puzzle. The proposal that either phonology or morphology should consistently highlight the argument of a focusing morpheme is a logical one. Surely discourse prominent information should be made unambiguously salient by the grammar. Why should we find languages like Chitumbuka, with focus particles that make non-focused information morphologically and prosodically salient?

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5.

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Unrealised possibilities

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Linguists want simple, elegant, and convincing grammatical principles, but the degree to which actual linguistic constraints meet these criteria is often inversely correlated with the amount of their empirical coverage. The problem seems to be particularly strong in the domain of typological generalizations: there are only very few examples of crosslinguistic generalizations that hold without exceptions. Here, I would like to raise the question of whether some of the difficulties can be circumvented if we assume that linguistic communities do not always realize all the possibilities which their grammars allow.

What might constitute an unrealised possibility is perhaps best exemplified by prenominal genitives in German. Often, they are considered ungrammatical, or restricted to non-complex genitives, as (1a) vs. (1b) suggests. (1b) is certainly ungrammatical since it violates the adjacency condition for heads in the prenominal domain that is also exemplified by prenominal adjectives, as the contrast between (1c) and (1d) illustrates. The factor ruling out (1b) does not block complex prenominal genitives. While (1e) and (1f) sound fairly acceptable to a certain extent, slight lexical variation leads to a decrease in acceptability.

(1) a. Marias Mutter
'Mary's mother'

b. * der Frau mit dem Hut Buch
'the woman with the hat's book'

c. der auf Julia stolze Vater
the on Julia proud father
'the father proud of J.'

d. * der stolze auf Julia Vater

e. eines jeden Mannes grösster Traum
one each man biggest dream
'every man's biggest dream'

f. des Weines Geschmack
the wine taste
'the taste of wine'

g. ? jeder zweiten Frau kleines Geheimnis
every second woman small secret
'the small secret of every second woman'

h. ? des Potsdamer Bieres Farbe
the Potsdam beer colour
'the colour of Potsdam beer'

There is no convincing formulation of a grammar that rules in (1a), but excludes, say (1g) or (1h), in particular because the length of the prenominal DP plays a role, and such effects are very difficult to capture in the standard grammatical frameworks (Jäger & Rosenbach 2006). Since we want to avoid reference to length in our grammars, a more appealing description might be the following: all prenominal genitive DPs are *grammatical*, yet this grammatical possibility is not made use of by speakers of German, simply on the basis of a 'stylistic' constraint that may partially (but not completely) be grounded in the processing/production problem prenominal genitives may come with.

A similar explanation has been proposed by Barbiers (2005) for a different domain, and also by Fanselow, Kliegl and Schlesewsky (2005) for long *wh*-movement in German: there are regional and non-regional differences in German concerning the acceptability of extractions from *dass* "that"-clauses, but such differences can be influenced so easily by confronting speakers with relevant examples that no profound grammatical difference between the dialects can be involved. Rather, long movement again seems to be an "unrealised" option of the grammar of, say, Northern German that is normally blocked on stylistic grounds.

Many linguists may be willing to accept such explanations at the 'periphery' of grammar, but can (and must) it be applied in the core of the syntactic system, too? I think we have reason to give a positive answer, since it might, e.g., help to cope with a number of otherwise mysterious observations in the crosslinguistic description of movement. Consider discontinuous noun phrases (DNP) as exemplified by German (2).

(2) Bücher liest er viele
books reads he many
'He reads many books'

The construction is more frequent among the world's languages than its low prominence in the generative discussion suggests. While there are a number of rivalling grammatical descriptions for (2), there appears to be some convergence concerning the licensing conditions for DNPs. First, there is a pragmatic dimension: The two parts of a DNP always fulfill different informational functions. Thus, in (2), *Bücher* can be the contrastive topic, and *viele*, the focus, but *Bücher* can also be the narrow focus while *liest er viele* is given information. DNP seem possible in those languages only that allow the displacement of topics and/or foci.

In addition, formal properties of DP are equally important. DNP of the type exemplified in (2) arise only if both parts could figure as independent, complete, DPs on their own (see, e.g., Fanselow 1988). For the left part of the DNP in (2), the relevant property is that DPs need not have an overt determiner in the language in question, for the right part, it is crucial that the language does not require the presence of an overt noun in a DP.

Ideally, these two constraints on DNP (possibly complemented by one or the other further principle) already predict which languages have DNP, and which do not. In a principle-based grammar, the set of constructions generated by it is a function of the basic operations (such as movement, applied to phrases with a topic feature) and the setting of various parameters (in the lexicon?) which further determine well-

formedness. Once we have identified the pertinent parameters, the grammatical properties of a language are completely defined in the relevant domain.

The two constraints mentioned above predict the distribution of DNP in a fairly satisfactory way. Languages with a 'liberal' DP structure of the sort described above and with free constituent order driven by considerations of information structure such as the Slavic languages, German, Hungarian, Estonian, Finnish, Latin, Greek, Albanian, Turkish, Georgian, Greenlandic, Hindi and Warlpiri have DNP, while English lacks them, probably due to the ungrammaticality of sentences like **I do not buy an expensive* and the general low sensitivity of English clause structure to informational distinctions.

One major differentiating factor appears to be the status of nominalized adjectives. In many languages, adjectives have to be augmented by some (nominalizing?) morpheme if the DP they occur in contains no overt noun. Such languages may allow DNP (as Japanese does), or lack them (as Avar does), and we can try to capture this variation in terms of the status of a nominalized adjective relative to the question of whether there are truly nounless DPs in a given language.

While the combination of our pragmatic and our formal constraints work well in the case of many languages, it is not too difficult to find languages which cannot be captured in this way. First, there may be exceptions of a very systematic nature: Baker (1995) claims that polysynthetic languages (in the strict sense) do not have DNP on principled grounds. If correct, his generalization just forces us to augment the set of criteria in our grammar, and thus poses no principled problem for explaining DNP exclusively in terms of principle-based grammars. Unfortunately, there are less systematic (and therefore more problematic) exceptional cases as well.

Consider, e.g., Basque. Basque does not require that there be an overt noun in a DP, and it clearly belongs to those languages in which word order serves to express distinctions of information structure. Basque thus should possess the major ingredients for DNP, yet, DNP are ungrammatical in this language. At first glance, the observation may appear to be helpful that Basque DPs require determiners in considerably more circumstances than, e.g., their English counterparts do. Consequently, one half of the formal constraint on the well-formedness of DNP, viz., that determinerless DPs must be possible, might not be met in Basque. But the problem which Basque poses goes beyond DNP. It is not just the case that DPs cannot be split up in the way exemplified in (2). DPs are also islands for extraction (say, of PPs) in general, they must *always* be contiguous. Of course, Basque DPs can be declared absolute islands for movement in one way or the other, but thereby, we merely reformulate what we want to understand. So far, satisfactory solutions have not been found (Itziar Laka, p.c.).

A general ban against movement out of DPs would not be descriptively adequate for Icelandic. PPs can be extracted from DP objects. Icelandic DPs need no overt realization of a noun, and, just as in the other Germanic V/2 languages, we find informationally driven movement to Spec,CP in Icelandic. Again, all factors licensing DNP in other languages are operative in Icelandic as well, yet DNP are ungrammatical for what appears to be the majority of speakers, and those few speakers that accept it consider the construction 'oldfashioned'. It is hard to see what property of Icelandic could be blamed for this status of DNP.

Most of the languages with DNP are of the scrambling type, and many of

them have an underlying SOV order. While Icelandic differs from, e.g., German in terms of basic word order (SVO vs. SOV) and of the availability of scrambling (impossible in Icelandic, possible in German), we must not make either of the two properties responsible for the very low acceptability of DNP: like Icelandic, Swedish is an SVO language without scrambling, yet DNP are well-formed. That Old Icelandic allowed DNP, but did not differ dramatically from modern Icelandic in syntactic terms (Rögnvaldsson 1995) narrows down the options for linking the behavior of DNP in modern Icelandic to some other property even further. In particular, the differences between Old and Modern Icelandic that Rögnvaldsson identifies would also distinguish Swedish from Old Icelandic. Again, it seems that we can find no satisfying grammatical answer to the question of why Icelandic has no DNP in the idiolects of most speakers, so that DNP may just be blocked on extragrammatical, stylistic grounds.

There is more evidence that falls in line with this interpretation. Most speakers of Swedish who we consulted found DNP acceptable, but they were rejected by all our Norwegian informants but one, and by all our Danish informants. Often, Dutch is characterized as a language lacking DNP, and when we compare it to German, we certainly note that, e.g., scrambling is much more constrained in Dutch, but Swedish has already taught us that DNP do not presuppose scrambling. Furthermore, DNP *are* okay in Brabant Dutch (van Hoof 2005), and a survey among Dutch linguists has revealed that the acceptance of DNPs is not geographically bound: some Dutch speakers coming from other dialect areas tolerate DNPs, others do not. We have little reason to believe that there is an additional, major grammatical factor distinguishing the DNP-speakers from the others, both in the case of different versions of Dutch and different versions of Mainland Scandinavian.

German and Sorbian illustrate the same from a different angle. German is productive with respect to the DNP type (2), but sentences such as (3) are ungrammatical. In contrast to nearly all Slavic languages, only the lowest overt head of a DP can move in a DNP. I see no grammatical difference between Polish, Russian, and further languages such as Georgian on the one hand, and German on the other that might be able to account for the difference between (2) and (3). To a limited extent, dialects allow Slavic-like constructions (as in (4)), an observation that also militates against the view that (3) is excluded from Standard German for profound reasons.

(3) a. * Viele liest er Bücher
many reads he books
b. * Interessante las er Bücher
Interesting read he books

(4) wie viel habt Ihr Schweine
how many have you pigs

Sorbian behaves like German (dialects) in allowing (4) but forbidding (2), contrasting in that respect with the neighboring Slavic languages with which it otherwise has much in common.

Basque, Danish, Dutch, German, Icelandic, Norwegian, and Sorbian have

idiosyncratic properties in the domain of DNP, and one would prefer to not to have to explain them in terms of principle-based grammar. Can stylistic constraints be extended such that they can cover these facts instead?

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6.

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Postponed* auch: *Where does its accent come from?

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There are a few particles in German which change their meaning along with their accented status, *wieder* 'again' being the clearest example. Accented *wieder* has a repetitive meaning, while unaccented *wieder* has a restitutive meaning. In the case of *wieder*, the difference in meaning seems to be dependent on a difference in focus.

The particle *auch* 'also' appears in an accented and in an unaccented version, as well. But it is not immediately clear whether the accent associates with a shift in semantic interpretation, as with *wieder*. According to Reis & Rosengren (1997), there is just one *auch*, which they analyze as a scope particle. The material c-commanded by *auch* can be focused (new) or not. If it is new, it is also accented (and *auch* is not). If only known material (or nothing) appears in its syntactic scope, *auch* is accented by default. Compare the sentences in (1). At the time of B's reply, the verb *angerufen* 'called' is given and cannot be accented. Either *Stavros* or *auch* has the main accent, depending on their relative order. Both versions in (1B) are well formed and both mean that not only Sam has called, but Stavros has too. (1B′) shows that *auch* is obligatory. (1B′) is readily understood as a correction: it is not Sam who called, but Stavros.

(1) A. SAM hat ANGERUFEN.
'Sam called.'
B. Auch STAVROS hat angerufen. / STAVROS hat AUCH angerufen.
'Stavros called too.'
B′. * STAVROS hat angerufen.

We will see below that the two versions of (1B) may differ in interpretation. (Reis & Rosengren propose that accented and unaccented *auch* may mean 'in addition' and 'likewise', respectively. It is not clear how to reconcile this difference with their strong claim that there is only one *auch*.) For now, let us concentrate on the following aspect of Reis & Rosengren's proposal. The other 'degree' particles, *nur* 'only' and *sogar* 'even', remain unaccented when the material in their scope is given. Reis & Rosengren anchor this difference in the observation that *auch*, stressed or not, contributes a non-implicated, truth-relevant meaning element, called ADD (for 'in addition'). Thus, an *auch* clause corresponds to ADD(p), meaning roughly, 'in addition p'. ADD may be focused and negated. In contrast, *nur* and *sogar* lack such a meaning component. However, if the accent on postponed *auch* arises as a consequence of its position in the sentence, *nur* and *sogar* should be able to carry a default accent as well, regardless of their meaning.

Krifka (1999) proposes that preposed and postponed *auch* differ in interpretation: postponed *auch* is the focused part of a topic-focus pattern. The associated constituents of postponed *auch* are contrastive topics and *auch* gets its accent because it realizes an overt affirmative element, as can be seen in (2B), an answer to the polarity question in (2A). Additive particles contrast with the non-overt affirmative element AFF and hence express a particular emphasis. (2C) shows a sentence in which AFF is non-overt. The subscripted F stands for focus and FT stands for 'focused topic' or 'contrastive topic.' The first clause of (2B) answers part of this question (and can also be an answer to the more neutral question *'What did Peter and Pia eat?'*), but another part is still open, and the second clause answers the polarity question, and no other. The set of alternatives is {Pia ate Polenta, Pia did not eat Polenta}. The accented additive particles receive their stress because they realize an affirmative element explicitly, just like *did* and *certainly* in some cases.

(2) A. Haben Peter und Pia Polenta gegessen?
'Did Peter and Pia eat polenta?'
B. Peter_{FT} hat POLENTA_{F} gegessen, und PIA_{FT} AUCH_{AFF}.
'Peter ate polenta, and Pia too.'
C. Peter_{FT} hat PASTA gegessen AFF.
'Peter ate pasta.'

The first argument Krifka advances to support the interpretation of the fronted element as a topic correlates with the accent pattern. Since the associated constituent is accented in the same way as a contrastive topic and the particle gets a falling accent, they resemble a topic-focus structure. But this accent is not obligatory, as there are examples of postponed *auch* unaccompanied by a contrastive topic (3). Krifka refers to Kowalski (1992) for such examples. If the accent in a contrastive topic is realizing an embedded focus (inside the topic), the possibility of accentless topics is explained. This focus is just not obligatory, and the contrastive topic is implicit in the answer (3B).

(3) A. Du hast das Geschirr gespült. Und den Abfall?
'You did the dishes. And the garbage?'
B. Hab ich AUCH\ erledigt.
'I took care (of it) too.'

A convincing argument correlating with the first one is the ill-formedness of the sentence (4B′) in the context of (4A). *Griechenland* 'Greece' can only appear in the sentence initial position, as in (4B′), when it is clear from the context that it is one of the possible destinations, i.e., when it is a topic. Such a situation would arise if speaker B went to the Mediterranean region, leaving only a small set of countries as possible destinations. (4B) is a perfect answer to (4A) in a situation in which A has no idea where B spent their holidays.

(4) A. Ich hab gehört, ihr seid nach Itálien gefahren.
Seid ihr sonst noch wohin gefahren?
'I heard you went to Italy. Did you go anywhere else?'

B. Wir sind auch nach GRIECHENLAND\ gefahren.
'We also went to Greece.'
B′: ? Nach Griechenland/ sind wir AUCH\ gefahren.

Like Reis & Rosengren, Krifka also considers the meaning of *auch* to be additive: it expresses and presupposes that the predication holds for at least one alternative of the expression in focus. Krifka proposes the following formalization, where the presupposed part is in parentheses.

(5) $[\text{ADD}_1\ [...F_1...]]$: [...F...] $(\exists F' \neq F\ [...F'...])$

Now for the puzzle: It is not always the case that *auch* is additive and/or has the meaning of (5). To see the problem, consider the dialogue in (6), adapted from Heim (1992:209).

(6) A. Yukiko mag Sushi.
'Yukiko likes sushi.'
B. Shin glaubt, dass ich AUCH Sushi mag (but in fact I hate raw fish).
'Shin believes that I too like sushi.'
B′: ? Shin glaubt, dass auch ICH Sushi mag.
C: Shin glaubt, dass ICH Sushi mag.

What could be the additive meaning of *AUCH* in (6B)? It could introduce the presupposition that Shin believes that somebody other than 'I' (namely Yukiko) likes sushi, in which case *auch* would add 'I' to this set. But this is clearly not part of the assertion of C. Shin does not need to believe that Yukiko likes sushi. In fact he could ignore Yukiko's existence, and the dialogue in (6) would still be well formed. The alternative is that *AUCH* adds 'I' to the set of persons who like sushi, regardless of Shin's beliefs. However, the intention of (B) is to remind the protagonists of the fact that this person doesn't like sushi. The stress pattern indicated in (6B′) with an accent on *ich* is possible but introduces a contrastive meaning which is not intended in this exchange. The same is true if (6B) is replaced by (6C); see the discussion of (1).

Krifka's interpretation of accented *auch* as focus of the sentence's affirmation in need of a contrastive topic is difficult to hold. The problem is first that there is no constituent in the sentence which could play the role of a contrastive topic, except for *Shin* or *ich* but then the difficulties just discussed reappear.

The example (7) shows even more clearly that AUCH does not affirm what is asserted in the sentence, since Mary did not get the job. The dialogue (also from Heim 1992) must be set in a context in which John and Mary competed for a single job. (Heim does not propose a solution for these cases, and is only interested in the presupposition they introduce.)

(7) John: I got the job.
Mary: My parents think that I ALSO got it.

In (6) and (7) *auch* (or *also*) is the only place the accent can be realized,

without introducing an unwelcome contrast. To appreciate this claim, consider (8).

(8) A. Peter sagte, dass die Semantikprofessur gestrichen wird.
'Peter said that the semantics professorship will be eliminated.'
B: Und was sagte Pia über die Phonologieprofessur?
'And what did Pia say about the phonology professorship?'
A′: Sie sagte, dass sie AUCH gestrichen wird.
'She said that it will be eliminated, too.'

In (8A′) the DP *sie* or *die Phonologieprofessur* is given, in Schwarzschild's (1999) sense: it is entailed by the preceding context. The same is true of the verb *wird gestrichen*: that a professorship will be eliminated is also given (see Féry & Samek-Lodovici 2006 for observations along these lines). What is not entailed by the context is the assertion expressed by (8A′), and the word *auch*. *Auch* is the only place where the necessary accent can be located.

Returning to (6) and (7), *auch* (or *also*) in these dialogues does not have any additive meaning, but just serves as a place for the accent. In the absence of *auch* or *also*, the only place where accent can be assigned is on *I*, the only part of the embedded clause which is not entailed by the context. But accenting *I* conveys an undesired contrastive touch between *Yukiko* and *ich* (or *John* and *I*), implying that Shin not only believes that I like sushi, but also that Yukiko does not like sushi (see (1)). The conclusion that imposes itself is that a particle like *auch* or *also* does not necessarily have an additive meaning, but can play the role of an accent holder.

Reexamining now the role of postponed *auch* in the earlier examples, consider (9), which replicates (4) in different guises. (9B) has a narrow focus on *Griechenland* and is readily interpreted as an exhaustive list, whereas (9C) leaves it open whether other countries were visited as well. (9D) is incongruent, because the exhaustive narrow focus on *Griechenland* contradicts the preceding sentence, which claims that we were also in Italy.

(9) A. Welche Länder im Mittelmeerraum habt Ihr besucht?
'Which countries in the Mediterranean did you visit?'
B. Wir waren in Italien. Auch GRIECHENLAND\ haben wir besucht.
'We were in Italy. We also visited Greece.'
C. Wir waren in Italien. GRIECHENLAND/ haben wir AUCH\ besucht.
D. Wir waren in Italien. * GRIECHENLAND\ haben wir besucht.

In conclusion, to fully understand the role of postponed *auch* it is necessary to examine the other accents in the sentence, as Krifka proposes in his paper. However, to reduce its role to a single meaning may conceal other aspects which are still in need of analysis. Postponed *auch* often gets the final falling accent, which readily gets an interpretation of finality and exhaustivity. It may well be the case that the choice of putting this accent on *auch* simply reflects an avoidance of putting it on any other word in the sentence.

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7.

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Andrew amalgams, their properties, and a suggested analysis

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One of the things that Lakoff (1974) brought to the attention of the linguistic community was an 'amalgam', which he attributed to Avery Andrews, and which is illustrated in (1).

(1) John invited [you'll never *guess* [$_{DP}$ **how many people**]]
to [you can *imagine* [$_{DP}$ **what kind of a party**]]
at [it should *be obvious* [$_{DP}$ **which place**]]
with [God only *knows* [$_{DP}$ **what purpose in mind**]],
although he was [you can *guess*
{[$_{AP}$ **how tired**], [$_{PP}$ **under what kind of pressure**]}].

An A(ndrew) A(malgam) typically consists of a matrix sentence and one or more inserted expressions with the superficial appearance of independent sentences, which, however, function as arguments, predicates and/or adjuncts. These AAI(nsert)s are thus constitutive elements of the matrix, and their syntactic category and logical type are determined by a *wh*-phrase that they necessarily contain. Unlike various kinds of parentheticals, AAIs are prosodically integrated into the matrix, and are pronounced with the kind of intonation that characterizes 'ordinary' arguments, predicates, and adjuncts. This is prominently reflected in the fact that AAIs may appear in utterance-initial position (as illustrated by (2) – with the indicated bracketing, not with the *wh*-phrase as part of a subordinate interrogative clause!), something that is generally not possible with parentheticals.

(2) [You know very well **who**] means to kill you.

A fact widely recognized in the earlier literature is that the *wh*-phrase within an AAI is the overt 'remnant' of a sluiced interrogative clause. The interrogative status of the *wh*-phrase is revealed by the fact that it invariably occurs in the complement position of a verb that selects an interrogative complement (see the italicized verbs in (1)). The thesis that we are dealing with some form of Sluicing is reinforced by the observation that 'Swiping', i.e., the metathesis of a *wh*-pronoun with a preposition, which is possible in the 'standard' Sluicing constructions (SSCs) of certain languages (Merchant 2002), is allowed in AAIs as well, as shown in (3b)

(3) a. I heard John is involved with someone, I wonder **who with**. *[SSC]*
b. Bill has been involved [$_{PP}$ you will never guess **who with**]
since August. *[AA]*

The central goal of this snippet is to bring up certain distinctions between AAs and SSCs, which, to my knowledge, have not been noted in earlier literature, and for which I propose to offer an account.

A first distinction can be appreciated by first noting that P-stranding within the ellipsis of an SSC is allowed just in case the language and/or the specific prepositional construction allow this process (Merchant 2006); this is illustrated by the contrast between the English and Romanian data in (4), the latter, a language that disallows P-stranding. Surprisingly, however, this restriction seems to be suspended in Romanian AAIs, as may be seen in (5).

(4) a. Bill wants to play poker with someone, but I am not sure **who**
(he wants to play poker with).
b. * Ion a reuşit datorită cuiva, dar n-am
Ion has succeeded thanks-to someone.Dat but not-have.1
să-ți spun **cui** (a reuşit el datorită).
Subj.Prt.-you.Sg.Dat tell who.Dat has succeeded he thanks-to
'Ion succeeded thanks to someone,
but I won't tell you who (he succeeded thanks to).'

(5) Ion a reuşit datorită [ştii tu **cui**]
Ion has succeeded thanks-to know.2.Sg you.Sg who.Dat
la examenul de ieri.
at exam-the of yesterday
'Ion succeeded thanks to [you know who] at yesterday's examination.'

A second difference between SSCs and AAs is that ellipsis is optional in the former and obligatory in the latter. This is so regardless of whether the ellipsis is syntactically or pragmatically controlled (for these notions, see Hankamer and Sag 1976).

To see this, note first that Hankamer and Sag (op. cit.) proposed that SSCs allow syntactic control only, but the reduced version of (6) shows that pragmatic control is also possible under special circumstances (on which, see Hankamer's 1978 discussion of pragmatic control in VP-ellipsis).

(6) [Context: someone discovers a murdered relative]
My God, who (could have done this)?

The full versions of (4a) and (6) show that the intended import of the ellipsis may be overtly expressed in both cases. Returning now to AAs, note that while some constructions, e.g., (7), are consistent with the assumption of either syntactic or pragmatic control, others clearly require pragmatic control, as can be appreciated by examining the reduced versions of (2) and (8)). For example, the reduced version of (8)

does not purport to ask the tautological question "does someone who wants to kill me want to kill me?", but rather something like "does someone such that you can figure out who I am now thinking of want to kill me?"). Thus, the ungrammaticality of the full versions of (7)-(8) points to the conclusion that the import of ellipsis in AAs, whether syntactically or pragmatically recoverable, must remain unexpressed. – For completeness, I note that the full versions of (7)-(8) become grammatical if the expressions within square brackets receive the intonational contour and interpretation of parentheticals; what matters for our purposes, however, is that these sentences are ungrammatical if the expressions at issue are AAIs.

(7) Bob sent me [you can easily guess **what** (*he sent me)].

(8) Does [you know **who** (*I am thinking of)] want to kill me?

A third difference between SSCs and AAs is that in the former case, the morphological Case and the syntactic category of the *wh*-phrase are determined ellipsis-internally, while in AAs, they reflect the Case/category requirements of the matrix slot filled by the AAI. This cannot be shown on the basis of data like (7), but can be shown in relation to data like (8). Thus, consider the reduced version of (9), an approximate Romanian counterpart of the reduced version of (8).

(9) Vrea (cu adevărat) [ştii tu **(*la) cine**] să mă omoare?
wants with truth know.2.Sg you.Sg at who Subj.Prt. me kill
'Does [you know who] (really) want to kill me?'

Since we are dealing here with pragmatic control, there are a variety of imaginable ways of conveying the import of ellipsis. In particular, if we stay as close to (8) as the grammar of Romanian allows (recall that it disallows P-stranding), the full version of the interrogative clause in (9) will be ***la cine*** *mă gândesc* 'of whom I am thinking'. Now, the ungrammaticality of the full version of (9) shows that in addition to ellipsis, it is necessary for the Case/category properties of the *wh*-phrase to match those of the slot occupied by the AAI.

To deal with the facts noted above, I propose the following analysis: AAIs are not bare sentences, but complex XPs with a null head that satisfies the Case/category requirements of the matrix slot and takes the overt part of the AAI as its sister. The XP is existentially bound, so that the import of, say, (7) is essentially 'Bob sent me something whose content you can easily guess'. In view of the matching in Case/category between AAIs and their internal *wh*-phrase, some writers (e.g., van Riemsdijk 2006) have proposed analyses that make the *wh*-phrase (also) an external head of the AAI. However, such an analysis is problematic insofar as it entails Swiping effects *in situ* in the matrix, something that is otherwise disallowed (cf. (3b) with (10)).

(10) a. Who spoke {with whom, *who with} yesterday?
b. Napoleon shouted {at whom, *who at}
before the battle of Austerlitz?

To account for the grammaticality of (5) and the ungrammaticality of the full version of (9), I propose to assume that pragmatic control of a null IP anaphor is *the only option in AAs*, syntactic ellipsis and non-elliptical realization being both excluded. The exclusion of syntactic ellipsis implies that no interrogative IP with internal structure exists at any stage of the derivation, and thus that there is no P-Stranding by the *wh*-phrase, which is base-generated as sister of a null IP; this allows (5). Furthermore, the exclusion of an overt IP throughout the derivation makes it impossible for the *wh*-phrase to derive Case/category properties from its IP sister, leaving the AA's null head as the only possible source for such properties; this is, I submit, the reason for the Case/category matching effects. The full version of (9) is thus out because matching is not satisfied.

Why is the import of the null IP not overtly expressible (in contrast to (6))? I can think of two facts that arguably shed some light on this matter. First, compare (11a) with (11b).

(11) a. Bob swallowed [I won't tell anybody **what**] last Sunday.
b. Bob swallowed [I won't reveal **what** *to anybody*] last Sunday.

While the interrogative IP is elliptical in both cases, the bracketed expression in (11b) is much harder to construe as an AAI, the preferred construal (and prosody) of the former being that of a parenthetical. The factor responsible for this seems to be the italicized constituent in (11b), which prevents the *wh*-phrase from occurring at the AAI's right edge. If occurrence of the *wh*-phrase at the right edge is important (see below for a suggested reason), note that this can only be achieved if the interrogative IP within an AAI is elliptical.

A second potentially relevant fact emerges from a comparison of (1) with (12).

(12) John invited [you'll never *guess* [$_{DP}$ **how many people**]]
[you can *imagine* [$_{PP}$ **to what kind of a party**]]
[it should *be obvious* [$_{PP}$ **at which place**]]
[God only *knows* [$_{PP}$ **with what purpose in mind**]].

In contrast to (1), (12) seems to be interpretable only as a sequence of juxtaposed independent sentences, not as one sentence that includes AAIs. The factor responsible for this difference seems to be that in (1), but not in (12), the various AAIs are separated from each other by overt elements of the matrix.

I conjecture that the matrix elements that apparently need to intervene between multiple AAIs make salient their left edges, and that the necessarily final *wh*-phrases make salient their right edges. Conceivably, the edges of AAs need to be salient in order to facilitate their construal as constitutive elements of the matrix, rather than as independent sentences (as their appearance might suggest). If this conjecture is on the right track, the obligatory status of ellipsis follows.

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8.

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Constraints on the complexity of verb meaning and VP structure

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There is a sense in which possible verb meanings are unconstrained: a particular verb can pack numerous idiosyncratic entailments into its meaning. Nevertheless, as Carter (1976) and Grimshaw (2005:85) point out, there are constraints on how much information can be packaged into a verb meaning. We suggest that complexity is not determined by the number of entailments or implicatures associated with a verb; rather, it is determined by the presence of certain types of meaning components, whether or not they themselves are internally complex.

Levin and Rappaport Hovav (1991) suggest that there is a complementarity between manner and result lexicalized in verbs: only one can be lexicalized. We suggest that this complementarity reflects a constraint on the overall complexity of verb meanings. Only certain types of meaning components contribute toward lexical complexity, with each type having a multitude of instantiations, which themselves do not contribute to lexical complexity. A manner can be very complex, as in the set of steps encoded in the verb *waltz* or the subtle distinctions lexicalized in *frown*, *grimace*, and *scowl*. Nonetheless, no manner verb also lexicalizes a result meaning component: *crawl, swim*, and *walk* are all similar in this respect.

Talmy (1975) notices that languages tend to 'conflate' in their verbs either motion and path, as in English *ascend* and *cross*, or motion and manner, as in English *amble* and *jog*. *Ascend* specifies a path of motion, but not the manner in which it is traversed, while *jog* specifies a manner of motion, but is neutral as to the path of motion. While path/manner complementarity is reminiscent of manner/result complementarity, the question is what path and result have in common. (There is a potential counterexample to the manner/result constraint: English *climb*, which is said to express both manner and result in some uses, including *Kelly climbed (up) the tree*. Interestingly, as discussed by Fillmore (1982:32-33) and Jackendoff (1985), in most uses, *climb* expresses either a clambering manner of motion, as in *Kelly climbed down from the roof*, or an upwards direction, as in *The plane climbed to a cruising altitude,* consistent with the constraint. See Rappaport Hovav and Levin (2006) for discussion.) We suggest that changes of state and traversals of path are both changes along a scale, and it is scalar change that gives rise to the potential for a result. As all eventive verbs involve change, manner verbs also involve changes, but nonscalar, rather than scalar, changes. It is these two types of changes, then, that are in complementary distribution.

The constraint against lexicalizing both manner and result components is a constraint on what can be packaged into a verb's meaning. But as illustrated above, a clause can include more than one such meaning component. The English resultative

construction allows manner and result 'state' components to be combined (e.g., *wipe the table clean*), while PPs can be used to combine manner and path components (e.g., *stroll towards/to the pond*). Furthermore, there are languages where serial verb constructions (e.g., Emai, Mandarin) or compound verb constructions (e.g., Japanese, Korean) may express comparable meanings. These constructions allow multiple pieces of meaning to cooccur at a level above a single word.

However, some languages appear to have a constraint on how much meaning can be packaged into the smallest constituent that includes the verb and its complements, which mirrors the constraint on how much can be packaged into a verb in other languages. Thus, unlike English, manner of motion verbs in Romance languages cannot take telic path phrase complements (Aske 1989). Thus, as a translation stylistics book notes, the English sentence *An old woman hobbled in from the back* must be translated into French as in (1).

(1) Une vieille femme arriva en boitant de l'arrière-boutique.
an old woman arrived in limping from the back-store
(Vinay and Darbelnet 1958:105)

We suggest this difference reflects a constraint against a manner verb appearing with a complement expressing a result, a VP-level constraint reminiscent of the word-level constraint in English. (Romance languages, however, do allow atelic path phrases with manner of motion verbs (Aske 1989). In fact, *climb*, the potential exception to the constraint at the word level is also an atelic verb; manner apparently never combines with a telic path inside a verb. Thus, none of the telic path verbs in English include manner (e.g., *arrive, enter, exit, reach*). Manner tends to be in complementary distribution with a path-type result, but a telic path 'counts' more with respect to this constraint than an atelic path.) Romance languages also lack the resultative construction (e.g., Aske 1989, Green 1973, Talmy 1991), and use a result verb plus a manner adjunct or modifier to express the same content, so the English sentence *Marie sponged the table clean* has the French counterpart in (2).

(2) Marie a nettoyé la table avec une éponge.
'Marie has cleaned the table with a sponge.'

This property can be taken as another instantiation of the constraint against a manner verb appearing with a result complement. Romance languages show still another less known instantiation of this constraint. In English verbs like *dig* or *carve*, which describe activities often used to create objects, may take an effected object, as in *carve a statue* or *dig a hole*. Their Romance counterparts lack comparable uses (Levin and Rapoport 1988, Martínez Vázquez 1998). For example, as Martínez Vázquez (1998:259) points out, in Spanish effected objects are mainly found with verbs lexicalizing a notion of creation, i.e., a result; they are not possible with verbs that emphasize the activity over the creation, i.e., manner verbs.

(3) Escribió unas palabras.
'S/he wrote some words.'
(Martínez Vázquez 1998:259, (66))

(4) * Rayó/grabateó unas palabras.
'S/he scratched/scrawled some words.'
(Martínez Vázquez 1998:259, (68))

As the examples suggest, Romance languages lack a range of constructions which allow a manner verb and a result phrase to be combined in a VP in English. English and Romance languages, then, differ as to where the complementarity constraint operates: at the word level in English and at both this level and the VP level in Romance. The mystery is why languages should differ so. This mystery might be considered together with another mystery raised by Bohnemeyer et al. (2005), who show that in some languages a single motion verb can take source, goal, and route complements, as in the English *go/walk from the post office across the street to the bank*, while in a second type of language, represented by Japanese, a single verb can jointly take source and goal complements, but a route must be introduced by a separate verb, and in yet a third type of language, represented by Yukatek, a distinct verb must introduce each of these components of a motion event, roughly as in the English *leave the post office, cross the street, and go to the bank*. Even above the level of the word, then, some languages show stricter constraints than others on how much information can be combined with a single verb. Thus, we point to a mystery about whether VP meanings have the same constraints as verb meanings, while Bohnemeyer et al. raise the question of how many components of meaning a verb itself can combine with in a language.

We close by asking why some languages allow more material to be packaged into a VP than others. This mystery does not seem to have an obvious answer. It cannot just be attributed to the morphosyntactic means available for expressing certain meanings. French, for example, does allow APs inside a VP (Green 1973), so this cannot be why it lacks resultatives. Is there simply a constraint on how complex a meaning can be encoded in particular linguistic units with languages differing as to what this unit is?

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9.

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Do we need two basic types?

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In a provocative book (Carstairs-McCarthy 1999), Andrew Carstairs-McCarthy argues that the apparently universal distinction in human languages between sentences and noun phrases cannot be assumed to be inevitable for languages with the expressive power of human languages, but needs explaining. His work suggests, but does not explicitly state, that there is also no conceptual necessity for the distinction between basic types e and t, a distinction argued for by Frege and carried into formal semantics through the work of Montague (Montague 1970). Pragmatic distinctions among various kinds of speech acts, including asserting, questioning, commanding, and pointing things out are assumed in Carstairs-McCarthy's work, as are expressions of functional types; what is questioned is whether a syntacticized sentence-NP distinction is essential.

If I am asked why we take e and t as the two basic semantic types, I am ready to acknowledge that it is in part because of tradition♣, and in part because doing so has worked well. I would acknowledge that while the model-theoretic domain corresponding to type e has been fairly uncontroversial, modulo how big it gets when the products of nominalization are packed into it (Chierchia and Turner 1988, Cresswell 1973), proposals concerning the domain for type t have been more varied: truth-values, sets of assignment functions, functions from possible worlds to truth-values, propositions taken as primitives, probably others. In a certain sense Montague had a third basic type, the type of possible worlds; in Gallin's Ty2 (Gallin 1975) this is explicit. But that is not essential, since on some alternatives the basic type t is taken to be the type of propositions, inherently intensional. There have also been proposals for adding something like situations or eventualities as an additional basic type, and sometimes times. Arguments for or against various choices have usually been arguments from elegance of resulting analyses, not arguments claiming conceptual necessity.

But suppose we imagine neutralizing the syntactic distinction between NPs and S's, as in Carstairs-McCarthy's thought-experiment language Monocategoric. Here are two examples, with argument-takers written in small caps, and with alternative possible English meanings written below each example.

(1) a. [you snake SEE] YESTERDAY
(i) 'You saw a snake yesterday.'
(ii) 'your seeing a snake yesterday'
(iii) 'the snake you saw yesterday'♦
(iv) 'you who saw a snake yesterday'
b. John Mary [[you snake SEE] YESTERDAY] TELL
(Carstairs-McCarthy 1999, p.23)

Can we imagine a parallel neutralized basic semantic type? Since Carstairs-McCarthy countenances distinguishing 'argument-takers' from their arguments, we are not being asked to give up functional types, although we could imagine following Chierchia and Turner and not require functions to be of different types from their arguments and try to get along with just one type altogether.

This is a question I have only begun thinking about, but it seems to me that it might be possible to put together several lines of recent research to come up with a defense of the conceptual possibility of getting along without the e-t distinction without losing expressive power. I offer some preliminary sketchy notes in this direction, and invite everyone to help debate whether such an approach is workable (or to let me know if it has already been done!)

Ingredients:

1. Neo-Davidsonian semantics of event sentences. Sentences become similar to indefinite (existential) noun phrases, stating the existence of an event of a certain sort. (Bach 1986, Davidson 1967, Kratzer 1996, Parsons 1985)

2. Irene Heim's and Hans Kamp's semantics for indefinite noun phrases (Heim 1982, Heim 1983, Kamp 1981): removing the existential quantifier from the interpretation of the NP, making the NP more like an open sentence. On the Kamp-Heim theory, the semantic interpretation of (2) is (3).

(2) A cat walked in
(3) cat(x) & walk-in(x)

The free variable may be bound by a higher operator, e.g. an adverbial quantifier. In a simple sentence like (2), implicit existential quantification comes from the definition of what it is for a file (Heim) or a DRS (Kamp) to be true in a model: there must be some assignment of variables that satisfies it.

3. An open formula like (3) is easy to shift into expressions of various types: existential closure gives a proposition, a lambda operator gives a property of cats, an iota operator gives (if defined) the entity 'the cat that walked in', a choice function gives an indefinite cat that walked in, suitable interrogative operators could ask whether a cat walked in or what cat walked in. In terms of potential interpretations via such operators, the open formula is in a sense already neutral among a range of types, even though when understood as a formula of a familiar logic it has a definite type.

4. Exploit the similarities and shiftability among entities and events, remarked on by many authors (Bach 1986, Krifka 1989, Partee 1991). The notion of situation as

used by Kratzer is at some remove from classical notions of events or (Bach) eventualities; situations are parts of possible worlds, but they can be event-sized, person-sized, or larger or smaller. I don't know what would be the most ontologically neutral term, but 'situation' might not have to shift very far to be a good candidate.

5. Bach et al (1995a) suggest that common nouns are the basic sortal predicates for describing entities, and verbs the basic sortal predicates for describing eventualities. They also note that the distinction between proper names and common nouns does not seem nearly as basic in natural languages as it is taken to be by philosophers – natural languages are much more likely to syntacticize the distinction between nouns and verbs (both one-place predicates for the logician) than the NP-CNP distinction. So the distinction between entities and properties of entities does not seem crucial. And if the distinction between entities and eventualities might be just a sortal distinction, and the distinction between eventualities and their properties also need not be sharp, then we may be almost there.

6. Then a single basic type might be the type of situations, conceived of as neutral between entities and eventualities, or, probably a better choice, a type of properties of situations, since it seems easier to get from properties to individual situations (via a property analog of 'singleton set') than vice versa. We will need properties in any case, since we need argument-takers, and perhaps they are enough, if we follow the lead of Chierchia and Turner and exploit the dual nature of properties as potentially saturated or unsaturated.

7. Pragmatics can do a lot of the work. It already does in various places in contemporary semantic/pragmatic theories, and it already does in Carstairs-McCarthy's presentation of how to interpret Monocategoric. Given a typally as well as syntactically 'neutralized' expression, context and pragmatics might be enough to indicate whether something is being said to "hold" or "be instantiated", or is being "indicated" (identifying, labeling, etc. uses), or demanded, requested, queried, etc. – all possible independent speech acts -- or is being "considered" or "envisaged" or "mentioned" in various embedded contexts. Wittgenstein's primitive "slab" language (Wittgenstein 1953, Part I), while far short of the expressive power of natural languages, exploited context to allow one-word utterances to function as 'mands' ("Bring me X"), and context or overt operators could easily extend this to include offers, assertions, etc. Anecdotal feasibility arguments come from our ability to understand children's early utterances. Formally, it also helps that we know how to shift among properties, propositions, and terms via such operations as existential closure, existential disclosure, iota-operators, and other sorts of type-shifters (Partee 1986). Such operators could operate on a pragmatic level instead of or in addition to within the semantics.

How might the semantics of one part of one of Carstairs-McCarthy's examples go?

(1) a. [you snake SEE] YESTERDAY
  (i) 'You saw a snake yesterday.'
  (ii) 'your seeing a snake yesterday'

Let me use type p as the (basic) type for properties of entities/ eventualities/situations.

SEE: type $p \times p \rightarrow p$

Maps a pair of properties p1 and p2 of situations onto a new property p3 which holds of a situation s3 if s3 contains situations s1 and s2 that have properties p1 and p2 respectively and in s3 (something in) s1 sees (something in) s2.

YESTERDAY: type p

The type of a situation contained in the interval yesterday. On the readings in (i) and (ii) it is conjoined with the property denoted by 'you snake see'.

you: type p

The property a situation has if it's a minimal situation containing you. This should go proxy for "you" and the property "being you", neutralized as in Straits Salish (Jelinek 1995).

snake: type p

The property a situation has if it's a snake-containing situation.

[you snake SEE] YESTERDAY:

The property a situation has if it's within yesterday and in it a "you" situation (or its contents) sees a "snake" situation (or its contents). Covert or overt operators could then lead to 'asserting the existence of' such a situation (1a-i) or 'referring to' such a situation (1a-ii), without a syntacticized S-NP distinction nor a semanticized t-e distinction.

Is that plausible? What are the main problems to worry about? The absence of individual variables? But Polly Jacobson has shown us how not to worry about that (Jacobson 1999). Quantification can proceed by unselective adverbial quantification, the favored choice in various languages (Bach et al. 1995b). While Carstairs-McCarthy in some places emphasizes how often we get along perfectly well with expressions that are ambiguous or vague, and ontological distinctions that are far from sharp, he also notes that a language without an NP-S distinction can still have an arsenal of explicit operators with interpretations related to focus-marking, question-marking, sortal specification, and other semantico-pragmatic functions, to reduce vagueness and indeterminacy. In place of truth-conditions and conditions on reference he suggests 'applicability' conditions, which could apply equally to proposition-like interpretations and description-like interpretations. I would add that we shouldn't have to give up the centrality of entailment relations: an open formula like that in (3) can have entailments; Groenedijk and Stokhof showed how questions can have entailments (Groenendijk and Stokhof 1989); and I suppose one could just as well say that one description entails another if anything that satisfies the first description (to which the first description applies) satisfies the second.

So why not? Are we just following tradition or is there a deeper reason to build a semantics on two basic types rather than just one?

♣ Once when I was giving a talk on Montague Grammar in the early 70's, a logician asked me why I was using typed rather than untyped lambda calculus. I replied that the typed lambda calculus was what Montague gave us, and I was only a "consumer" and would be glad to be shown how semantics with the untyped lambda calculus would work. (Chierchia and Turner's

property theory work goes part way in that direction.)

♦ The possibility of the readings indicated in (iii) and (iv) with the given bracketing are argued for by appealing to the phenomenon of head-internal relative clauses. This may or may not be reasonable, but it is probably orthogonal to the issue of S and NP as basic categories and t and e as basic types.

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10.

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The thing about modal adverbs

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The class of modal adverbs includes adverbs such as *probably*, *possibly*, *evidently*, *certainly*, *surely*, etc. Bellert (1977, pp.343–347) was the first to point out two intriguing distributional facts about these adverbs. The one is they cannot be negated:

(1) a. Probably, the socialists won the elections.
b. # Improbably, the socialists won the elections.

(2) a. Possibly, the socialists won the elections.
b. # Impossibly, the socialists won the elections.

(3) a. Evidently, the socialists won the elections.
b. # Not evidently, the socialists won the elections.

More generally, they do not occur in the scope of negation (putting aside the case of metalinguistic negation, where the modal adverb receives a focal stress):

(4) a. The conservatives probably did not win the elections.
b. # The conservatives did not probably win the elections.

(5) a. The conservatives possibly did not win the elections.
b. # The conservatives did not possibly win the elections.

(6) a. The conservatives evidently did not win the elections.
b. # The conservatives did not evidently win the elections.

The other observation is that these adverbs do not occur in questions:

(7) # Did the socialists probably | possibly | evidently win the elections?

Bellert notes that if modal adverbs were synonymous with the corresponding modal adjectives (*probable*, *possible*, *evident*, *certain*, *sure*, etc.), the standard view since Jackendoff (1972) (see also Ernst 2002), then these distributional facts would be unexpected, for the modal adjectives do not exhibit these restrictions:

(8) a. It is probable that the socialists won the elections.
b. It is improbable that the socialists won the elections.

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(9) a. It is possible that the socialists won the elections.
b. It is impossible that the socialists won the elections.

(10) a. It is evident that the socialists won the elections.
b. It is not evident that the socialists won the elections.

(11) Is it probable | possible | evident that the socialists won the elections?

Bellert claims (p.345) that modal adverbs differ in meaning from the corresponding modal adjectives in that "modal adverbs should be interpreted as predicates over the truth of the proposition expressed by the respective sentence, and that sentences with modal adverbs express two propositions; whereas the corresponding modal adjectives are predicates over the fact, event, or state of affairs referred to by the sentence, and sentences with modal adjectives express one complex proposition." She cites the following kind of contrast in support of this particular difference:

(12) a. It is probably | possibly | evidently true
that the socialists won the elections.
b. # The truth that the socialists won the elections
is probable | possible | evident.

For Bellert, then, modal adverbs are metalinguistic because they qualify the truth of a proposition, in contrast to the corresponding modal adjectives, which do not.

Although Bellert's proposal is initially intuitively appealing, it is not worked out and suffers from a few problems. The first is that although (12b) is indeed awkward, replacing *truth* with *fact*, *event*, or *state of affairs* does not help, although it should (other things being equal) if her proposal were correct:

(13) # The fact | event | state of affairs that the socialists won the elections
is probable | possible | evident.

The second worry is that a variation on (12b) does improve things, as shown in (14).

(14) It is a probable | possible | evident truth
that the socialists won the elections.

Here it would be odd to claim, as Bellert would presumably be forced to, that the modal adjectives do not qualify the truth of the proposition expressed by the embedded sentence. Finally, the third issue is that it is not so clear what it means for "two propositions" to be expressed in sentences with modal adverbs (as opposed to "one complex proposition" in sentences with modal adjectives).

Nilsen (2004) offers a more explicit analysis of modal adverbs, which he regards as positive polarity items. Concentrating on *possibly* versus *possible*, he takes (p.823) the following kind of contrast to be indicative of the semantic difference

between modal adverbs and the corresponding modal adjectives:

(15) a. # The socialists will possibly win,
even though they certainly won't.
b. It's possible that the socialists will win,
even though they certainly won't.

Whereas (15a) is contradictory, (15b) is not. Nilsen concludes from this that *The socialists will possibly win* expresses a stronger statement than *It's possible that the socialists will win*.

Although Nilsen's formal analysis is technically involved (and therefore cannot be discussed in detail here for lack of space), the basic idea—simplifying things a bit—is to think of belief states relative to an agent as being partitioned three ways into a set of strongly believed (or highly plausible) propositions, a set of weakly believed (or lowly plausible) propositions, and a set of neither strongly nor weakly believed (i.e., mediumly plausible) propositions. There are two main constraints on propositions in belief states. The first constraint states that if a proposition p is highly plausible in a given belief state, then $\neg p$ (its negation) is lowly plausible, and vice versa, which entails that no proposition and its negation are at once either highly or lowly plausible. The second constraint specifies that if p is mediumly plausible, then $\neg p$ is mediumly plausible, and vice versa. The strategy is then to analyze (the epistemic sense of) *possible* when applied to a proposition p as stating that p is (at least) lowly plausible and to treat the corresponding sense of *possibly* as saying that p is (at least) mediumly plausible. Assuming that *certainly* only applies to highly plausible propositions, the result is that (15a) expresses a contradiction, whereas (15b) does not. (15a) is contradictory because it asserts of the proposition that the socialists will win that it is (at least) mediumly plausible and that its negation is highly plausible, but according to the constraints, if p is (at least) mediumly plausible, then $\neg p$ cannot be highly plausible—it must be no more than mediumly plausible. In contrast, (15b) is consistent because it asserts of the proposition that the socialists will win that it is (at least) lowly plausible and that its negation is highly plausible. This is consistent as long as the proposition that the socialists will win is lowly plausible. Technically, the meaning of *possibly* is derived from that of *possible* by a specific kind of domain narrowing (see Kadmon and Landman 1993 and Chierchia 2004 for the related notion of domain widening), which is intended to account for the observation that *possibly* (in contrast to *possible*) is a positive polarity item.

Although Nilsen's proposal is intriguing, the empirical justification for distinguishing *possibly* from *possible* (and, more generally, modal adverbs from the corresponding modal adjectives) in terms of semantic strength is rather weak, and yet his account crucially depends on the claim that modal adverbs are semantically stronger than the corresponding modal adjectives. The following texts are meant to test this claim:

(16) a. It's possible that the socialists won.
Indeed, it's even certain that they won.
b. The socialists possibly won. Indeed, they even certainly won.

(17) a. It's possible that the socialists won. Indeed, they even certainly won.
b. # It's possible that the socialists won. Indeed, they even possibly won.

In each of these texts, the force of *indeed…even* in the second sentence is to strengthen what is asserted in the first. If Nilsen's claim were correct that *possibly* is semantically stronger than *possible*, it would be a puzzle why *possible* cannot be sensibly strengthened by *possibly* in (17b). This strongly suggests that the difference between *possible* and *possibly* is not one of semantic strength.

For consistency, Nilsen should extend his strategy to all modal adverbs, though he does not actually do this. But the idea that (e.g.) *certainly* is semantically stronger than *certain* is also not very promising:

(18) # It's certain that the socialists won. Indeed, they even certainly won.

In sum, if modal adverbs are not semantically stronger than the corresponding modal adjectives, then Nilsen's account loses the motivation that it crucially depends on. But this, in turn, means that the contrast in (15) must have another explanation.

If the proposals of Bellert and Nilsen are not adopted, the next step would be to consider other approaches. Krifka (1995) presents a theory of polarity items that makes crucial use of (focus) alternatives introduced by polarity items and ordering relations of semantic specificity induced by such alternatives, but he does not discuss modal adverbs. At any rate, not all modal adverbs are positive polarity items in Krifka's sense (pp.240–241), because they are not all "exhaustive" in the sense of denoting the union of the set of their alternatives. Whereas *certainly* might be a candidate for a positive polarity item in his analysis, *possibly* would be much less so, but the issue deserves a closer look. However, Haegeman (2006, pp.1652–1653) points out that modal adverbs may also not appear in clauses that are not downward entailing, which casts further doubt on the claim that modal adverbs are positive polarity items in the usual sense. If correct, then Bellert's observations (see (1)–(11)) require a different account.

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11.

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Are there interrogative VPs?
Some observations on a pied-piping puzzle in German

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Cases like (1) show that German (non-Echo) interrogative clauses do not tolerate VPs containing a *+wh*-phrase in clause-initial position; all that can be fronted is the VP dominated *+wh*-phrase by itself, see for example (2). In other words, VP pied piping seems to be ruled out.

(1) a. * [Was gespielt] haben die Kinder? /
What played have the kids
* Ich weiß, [was gespielt] sie haben.
I know what played they have
b. * [Wann geschrieben] hast du das? /
When written have you this
* Ich weiß, [wann geschrieben] du das hast.
I know when written you this have
c. * [Wie geschrieben] hat er den Brief? /
how written has he the letter
* Ich weiß, [wie geschrieben] er den Brief hat.
I know how written he the letter has

(2) Was haben die Kinder [t gespielt]? / Ich weiß, was sie [t gespielt] haben.

Since there are no *+wh*-words of category VP either – a sentence like *Was haben die Kinder?* ('what have the kids') just cannot be used as a normal (non-Echo) question about what the kids did in the past – this seems to add up to the generalization (G1):

(G1) In German♣ interrogative VPs do not exist.

There is, however, a class of apparent counterexamples to (G1); cf. (3).

(3) a. [Wie schön geschrieben] muss man eigentlich haben,
How beautifully written must one actually have
um eine 1 zu bekommen?
for an A [=1] to get
Weißt du, [wie schön geschrieben] man eigentlich haben muss, . . . ?
Know you how beautif. written one actually have must

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b. [Wie hoch geschlagen] hätten sie werden dürfen,
How highly beaten would.have they be allowed.to
ohne den Titel zu riskieren?
without the title to risk
Weißt du, [wie hoch geschlagen] sie hätten werden dürfen, . . . ?
Know you how highly beaten they would.have be allowed.to

The essential difference between (1) and (3) is clear: (3) always involves quantifying *wie*(+adjective) phrases. But this is of no explanatory help for under standard assumptions these modal adverbials are VP adjuncts, hence the cases in (3) are *prima facie* interrogative VPs, and thus cases of VP pied piping violating (G1).

While these cases (first mentioned in Reis 1989:132) are dutifully cited in recent relevant studies (Trissler 2000, Heck 2004), there is no move whatever to integrate them into Pied Piping theory. This is strange for (3) is at odds not only with the older theories of Pied Piping (see Trissler 2000: §9.3.1/2) but also with central recent claims: Against Trissler, the interrogative phrases occupying SpecC in (3) are *prima facie* no functional projections, and against Heck, putative pied piping in (3) is clearly optional. In other words, cases like (3) are still an unsolved problem for Pied Piping theory. In the following, I will present a few facts (3.1-3.3) suggesting that the problem may be even more far-reaching than so far envisioned.

The contrast between (1) and (3) can be replicated with APs; cf. (4) vs. (5). ((5a) is, of course, only acceptable in the quantifying reading of *wie*, not the modal reading).

(4) a. * Auf wen stolz ist er schon lange?
of whom proud is he already long
b. * Wann zufrieden ist er eigentlich?
when content is he actually
c. * Wo wohnhaft ist dein Sohn?
where living is your son
d. * Auf welche Weise berühmt wurde er?
in which way famous became he

(5) a. Wie berühmt ist er geworden?
how famous is he become
b. Wie ungeheuer überlegen muss man denn sein?
how incredibly superior must one MOD.PART be?
c. Wie schlecht verständlich das alles war, sah er erst später.
how badly understandable this all was, saw he only later
d. Um wie viel besser verstehbar würde das durch Lautsprecher sein?
by how much better understandable would this through loudspeakers be

Inasmuch as constructions with true Deg elements like *wie* are given a DegP analysis, cases like (5) pose no problem for Pied Piping theory (*pace* optionality), which, apart from (3), conforms to generalization (G2), thus correctly excluding 'bare' +*wh*-VPs as in (1) (see also Trissler 2000).

(G2) +*Wh*-phrases are functional projections.♦

Given the pied piping parallels between (3) and (5), this suggests giving (3) a DegP analysis as well. But then VPs would have to be admitted as Deg complements, an option that is universally possible (Rijkhoek 1998) but generally excluded for German and related languages.

At first glance, this objection seems easy to overcome: Why not (re)analyze the pied-piped VP parts, in (3) always past participles, as adjectives? The fact that VP expansions forcing the verbal character of the participle are usually absent, in any case never pied-piped along, see (6), could be considered as an argument in favor of this move.

(6) a. * Wie schön den Brief geschrieben müssen wir haben . . . ?
how beautifully the letter written must we have
b. *? Wie schön auf die Tafel geschrieben müssen wir haben . . . ?
how beautifully on the blackboard written must we have

However, VP pied piping is not confined to past participles (as assumed by Trissler 2000:146 and taken over by Heck 2004) but occurs with (certain) coherent bare and *zu* infinitives as well, for which adjectival reanalysis is impossible.

(7) a. Wie gut zu sehen muss ein Verkehrsschild sein, damit es gilt?
how good to see must a traffic.sign be so.that it is.valid
b. Wie oft zu spät kommen darf man eigentlich,
how often too late come may one actually
bevor man abgemahnt wird?
before one reprimanded is
c. ? Wie schrecklich laut singen haben Sie ihn denn gehört?
how terribly loud sing have you him MOD.PART heard

Hence, a DegP analysis for (3) must stipulate that Deg admits VP complements in German, and this just in case that the VP meaning is gradable *by virtue of a gradable AP- or AdvP-modifier*. The italicized part of the licensing condition makes the DegP analysis empirically vacuous but without it, the condition would not work; cf. *lieben* ('love'): Although gradable by itself (8a), and admitting the Deg element *so* (8b), the corresponding *wie*-construction (8c) is ungrammatical – unlike (8d), which fulfills the italicized part as well.

(8) a. Weil er sie (nicht/kaum/sehr/unglaublich/schrecklich ...) geliebt hatte
because he her (not/hardly/very much/incredibly/terribly ...) loved had
b. weil er sie so geliebt hatte
because he her so loved had
c. * Wie geliebt hatte er sie denn?
how loved had he her MOD.PART

d. Wie schrecklich geliebt hat er sie denn?
how terribly loved had he her MOD.PART

In addition, there is a constituency problem. According to the DegP analysis, the interrogative phrases in (3) should have the structure [*wie* [VP AP+V]] but the possible splits by movement in (9) show that the structure must be [VP [*wie*+AP] ...]. (Not that splitting off just the *wie*-phrase is acceptable in exclamative clauses; cf. *Wie hat er schön geschrieben!* ('How has he beautifully written'). There are however good reasons to consider this as a quasi-idiomatic construction pattern, which does not even extend to embedded contexts such as **Erstaunlich, wie er schön geschrieben hat!* ('amazing how he beautifully written has').)

(9) a. * Wie muss man schön geschrieben haben, um . . . ?
how must one beautifully written have, in.order.to
b. Wie schön muss man geschrieben haben, um . . . ?
how beautifully must one written have, in.order.to ...

Hence, a DegP analysis for (3) seems untenable from the start, which also shows that (G2) cannot be the last or only principled word on the matter. This is confirmed by *wie*-constructions like (10) in which quantifying *wie*(+adjective) in modifier position is likewise able to interrogativize the entire phrase. But what makes this exceptional behavior of quantifying *wie*(+adjective) possible?

(10) a. Der wievielte Versuch war das?
the how.manieth attempt was this
b. Ein wie großes Vermögen hat er in dieser Zeit angehäuft?
a how big fortune has he in this time amassed

Heck (2004) thought it safe to neglect exceptions like (3) to his claim that Pied Piping is always obligatory because he assumed this participle pied piping to be a mere idiosyncrasy of German. But it is perhaps not exclusively German,♥ and it is certainly not an idiosyncrasy pure and simple:

First, optional pied piping related to quantifying *wie*-phrases is not just participle pied-piping but includes VPs with infinitival forms, cf. above (7), as well as adjectives, cf. (5b-d) above with (11).

(11) a. Wie ungeheuer müsste man denn überlegen sein, um . . .
b. Wie schlecht das alles verständlich war, sah man . . .
c. Um wie viel besser ist das mit Lautsprechern verstehbar?

Second, even with pied-piped CP-infinitives, according to the literature an exclusively German phenomenon indeed, the putative pied piping of the sentential *zu*-infinitival is optional (see ♣).

Third, there is optional pied-piping of infinite VP parts of VP beyond the *wie*-cases in question, see in particular cases of split topicalization like (12). This again seems to be an exclusively German phenomenon,♠ and just like the *wie*-cases it

includes not only cases of past participle pied-piping (mentioned already in Haider 1987, albeit in a different context) but also VP pied piping of infinitival and adjectival forms (12b-e).

(12) a. Briefe (geschrieben) hat sie mir nur drei traurige (geschrieben).
letters (written) has she me only three sad [ones] (written)
b. Briefe (zu schreiben) sind heute vor allem formale (zu schreiben).
letters (to write) are today in particular formal [ones] (to write)
c. Briefe (schreiben) möchte ich heute nur fröhliche/keine (schreiben).
letters (write) want I today only merry[ones]/none (write)
d. Luxus (gewohnt) ist der keinen (gewohnt).
luxury (used.to) is this.one none (used.to)
e. Geld (los) wurde er keines (los).
money (rid.of) became he none (rid.of)

In other words, (i) optional pied-piping includes in principle all predicative non-finite forms, not only the past participle, (ii) there are remarkably many cases of non-finite pied-piping in German. Thus, we do not deal with a marginal idiosyncrasy but with a constructional option used often enough in German to require some kind of systematic explanation. This is what lends additional interest to cases like (3) no matter whether or not the regularity of optional VP pied-piping contributes to elucidating the special status of quantifying *wie*(+adjective) phrases.

A final point: The special pied-piping behavior of quantifying *wie*(+adjective) phrases recurs with its non-interrogative operator counterparts *so/je-desto*(+adjective):

(13) a. So schön (schreiben) er auch (schreiben) kann,
so beautifully (write) he MOD.PART (write) can,
er hat als Kalligraph keine Chance.
he has as calligrapher no chance
b. So schön (geschrieben) er das auch (geschrieben) haben will, . . .
so beautifully (written) he that MOD.PART (written) have wants
c. So leicht (verständlich) das auch (verständlich) ist, . . .
so easily (understandable) this MOD.PART (understandable) is

(14) a. Je leichter (zu verdauen) der Vorschlag
the easier (to digest) the proposal
für die Oberen (zu verdauen) sein wird, desto schwerer
for the upper [ones] (to digest) be will the more.difficult
(umzusetzen) wird er für uns Untere (umzusetzen) sein.
(to.implement) will it for us lower [ones] (to.implement) be
b. Je schöner (geschrieben) man (geschrieben) haben muss,
the more.beautifully (written) one (written) have must
um eine 1 zu bekommen, desto saurer (verdient)
for an A to get the harder (earned)
hat man sich die Belohnung dafür (verdient).
has one himself the reward for.this (earned)

This suggests strongly that the specific pied-piping behavior we observe with *wie-/so-/je-desto*(+adjective) phrases has no purely syntactic basis but is related to the semantic operator properties these phrases have in common. Likewise, it offers additional support for what I claimed in the previous section: Rather than being a mere idiosyncratic quirk to be safely put aside, interrogative *wie*-VP constructions like (3) seem to result from regularities which interfere (viz. 'interact') with the usual pied-piping regularities in systematic ways that deserve the linguist's attention.

♣ Cross-linguistically, interrogative *+wh*-VPs seem to be exceedingly rare as well. However, since *+wh*-VPs seem to exist in the Austronesian languages Seediq and Tagalog (thanks to H.M.Gärtner for pointing this out), (G1) cannot be strengthened to a universal generalization.

♦ (G2) predicts that *+wh*-CPs should be possible. This may be true in German for infinitival CPs such as *Ich weiß, wen hier zu finden du nicht gedacht hättest*, ('I know who here to find you not thought would-have') and their V2-counterpart (?)*Wen hier zu finden hättest du nicht gedacht?* ('Who here to find would-have you not thought'). For one thing, however, their analysis as just one infinitival *+wh*-phrase in SpecC (which would again involve optional pied-piping) is controversial. More importantly, there remains the notorious problem of finite *+wh*-CPs, which never function as *+wh*-phrases: *Was er tun wird, weißt du.* ('what he do will, know you') is a declarative, not an interrogative sentence. – Whether viz. to what extent *+wh*-PPs constitute a problem for (G2) as well is a question I will leave open here.

♥ Heck relied on informants, according to whom there were no counterparts to (3) in other Germanic languages (2004:161n.). This may be wrong; cf. the Swedish Google example (pointed out to me by H.M. Gärtner) *...känna hur bra gjort albumet är.* '[they will] realize how well made the album is'.

♠ The phenomenon seems also to occur in reverse form: As pointed out by Büring (1997:46-47, 72-73), who in turn cites Krifka (1994), there are cases of VP focus (or VP topic respectively) where the focus exponent appears in the pre-field, and the non-finite parts of the VP are only optionally pied-piped along.

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12.

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Telicity at the dinner table: Do I have to eat it all?

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A problem which Manfred Krifka (along with the author of this squib) is no doubt familiar with is the child who doesn't want to eat her dinner. Instructions to the child to stop playing with her spoon and get on with the job in hand, at least in the author's home, have led to an interesting observation about the interaction between telicity and mood.

It is a well known fact (which Krifka himself has brought to the centre of linguistic discussion over the last 25 years) that the direct object or theme of an accomplishment verb determines whether the VP headed by the verb is telic or not. Thus we have the familiar contrasts in (1):

(1) a. Lydia ate her soup in record time.
b. # Theresa ate her soup for five minutes.
c. Dafna ate soup for half an hour.
d. * The girls ate soup in half an hour.

When the theme/direct object is what Krifka (1989, 1992) called quantized as in (1a/b), the accomplishment headed VP is telic, and can be modified by standard *in α time* PPs, but not by *for α time*. When the theme is cumulative as in (1c/d), the converse is the case, and *for α time* is acceptable but not *in α time*.

The reason why the direct object/theme has this effect is the subject of some debate. There is some general agreement that telic verb phrases denote events which can be measured in some kind of way, and that this measurement takes place via a homomorphic function whose values form a partition on the temporal parts of the event (sometimes with added constraints). Beyond this there are different theories as to how the homomorphism works. Krifka, in a number of very influential papers (Krifka 1989, 1992,1998) argues that the event can be measured because of a thematic property of accomplishment verbs. The relation between the event and the direct object is mediated by the thematic role which determines the nature of the participation of the theme in the event. In the case of accomplishment verbs the thematic relation is **incremental**, which is to say that it determines that the involvement of the theme in the event is on a part-by-part basis in such a way that there is a homomorphism from the part-of structure of the theme to the part-of structure of the event. Since allotted parts of the theme are assigned to parts of the event on a once-only basis, when the theme has been 'used up' the event must be over. Quantized nominals are those where the quantity or size of the nominal are determined. When a nominal denoting the theme is quantized, the incremental homomorphism determines an event of a specified size since it is

possible to specify completely the function from parts of the theme to temporal parts of the event. This means that we can determine when every part of the theme has been 'used'. When every part of the theme is used, the event must be over, and since we can determine when the event is over, the VP comes out telic.

A crucial element of Krifka's theory is that it explains not only what telicity is, but also how the direct object is involved in determining the telicity of the VP, and indeed any decent theory of telicity must explain this dependence. I have argued elsewhere (Rothstein 2004) that there is good reason to suggest that telicity is in fact not determined by a homomorphism from the part structure of the theme onto parts of the event. Instead, assuming that accomplishment headed VPs denote complex events consisting of an activity and an event of change, the measurement of the whole event takes place via a homomorphism from the parts of the event of change onto the event as a whole. But in this approach, the direct object DP still plays a crucial role in determining the telicity of the VP, since it is this DP which individuates the relevant events of change, and only quantized, or countable, DPs can have this individuating role. So the interdependence between the telicity of the VP and the properties of the theme DP is maintained here too.

The relation between the determination of telicity and the properties of the DP expressing the theme is common to all theories of telicity, and the point that I want to make in this squib bypasses the debate about which the correct theory is. Instead I want to make an observation which poses a question to all theories of telicity that I know about.

Whatever the explanantion for the facts in (1), and no matter how crucial the role of the DP is in determining the telicity of the VP, the dependency disappears (or is severely weakened) in the imperative. Look at the example in (2):

(2) Dafna, eat your soup!

It seems to me, and to my informants, that this instruction is clearly an instruction to Dafna to get on with eating her soup, and not an instruction to finish it. If I issue the instruction in (2), and my daughter picks up her spoon and begins to eat, she is clearly complying with my instructions, and she is entitled (semantically) to ask "Do I have to finish it all?"

An obvious suggestion is that the contrast between (2) and the earlier examples is because of the effect of tense. However, as the examples in (3) show, the contrast cannot be because of tense, since we get the same telicity effects that we saw in (1) in the bare VPs in complements of perception and causative verbs.

(3) a. I made Theresa eat her soup in five minutes.
b. I saw Lydia eat her soup in five minutes.
c. # I made Dafna eat soup in five minutes
d. I made the girls eat soup for five minutes.

Similarly, (4a) strongly implies that Dafna finished eating her soup, and if I want to indicate that I saw the event going on, rather than that I saw the whole event, I will use the participial form in (4b):

(4) a. I saw Dafna eat her soup.
b. I saw Dafna eating her soup.

(4b) is pragmatically compatible with the question "Did she finish her soup?" while (4a) makes the question redundant.

I have no answer to the question why the imperative mood should weaken the telicity effects in this way, since, as we have seen, the bare VP with quantized theme denotes completed events and is telic.

Telicity doesn't disappear in (2), since (5a) is infelicitous, but (5b) is odder than one would expect in a 'normal' telic sentence.

(5) a. # Eat your soup for five minutes!
b. Eat your soup in five minutes!

Similary (6) is odd:

(6) # I saw/heard Dafna eat her soup for five minutes.

A plausible suggestions is that it is mood, rather than tense, which is playing a role here. Thus telicity effects seems to be weakened in the same way in (7):

(7) I insist that you eat your soup!

To show that these effects are not dependent on a particular choice of verb, look at a clearly telic predicate like *build a house*. We get the same effects:

(8) a. Build your house tomorrow and stop wasting time!
b. I suggest that you build your house tomorrow.

Both the examples in (8) can be taken as an instruction or suggestion that you get on with the work of building your house tomorrow, whereas (9) strongly implies completion:

(9) John will build his house tomorrow.

A consequence of the weakened telicity effects in non-indicative mood is that we find a clear case in English where 'telic particles' have an explicit semantic contribution to the sentence. An instruction, whether couched in the imperative or the subjunctive, to eat the whole plate of soup has to use the particle form as in (10a) or (10b), and thus we find an instance where *eat* and *eat up* are not synonymous.

(10) a. Eat your soup up!
b. I insist that you eat your soup up!

I have no idea why we should get weakened telicity effects in (2) nor why mood should apparently be the factor which leads to these effects. But this is a puzzle,

not a paper, and maybe Manfred will enjoy thinking about a solution!

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13.

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A relative clause puzzle

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This relative clause puzzle concerns a systematic and not yet understood difference between finite and infinitival relatives in English. Earlier work on the topic (see, for example, Chomsky and Lasnik 1977, Rizzi 1990, Déprez 1995 for discussion) has not yet provided a satisfactory account of the pattern illustrated here.

In infinitival subject (1) and non-subject (2)-(3) relative clauses an overt relative pronoun is excluded. Only the empty operator is possible.

(1) a. The man [*Op* [__ to solve the problem]] is in your office.
b. * The man [*who* [__ to solve the problem]] is in your office.

(2) a. The book [*Op* [to read __]] is in your office.
b. * The book [*which* [to read __]] is in your office.

(3) a. A linguist [*Op* [to work with __] is in your office.
b. * A linguist [*whom* [to work with __] is in your office.

This restriction does not hold for finite relative clauses. As shown in the (b)-examples below, an overt *wh*-element can be used in all corresponding finite relative sentences:

(4) a. * The man [*Op* [__ should solve the problem]] is in your office.
b. The man [*who* [__ should solve the problem]] is in your office.

(5) a. The book [*Op* [you should read __]] is in your office.
b. The book [*which* [you should read __]] is in your office.

(6) a. A linguist [*Op* [you should work with __]] is in your office.
b. A linguist [*whom* [you should work with __]] is in your office.

(4a) has been ruled out for independent reasons in the relevant literature, for example, because of a failure of proper government of the subject trace by the empty operator (Rizzi 1990) (Rizzi suggests that insertion of *that* in (4a) turns C^0 into a proper governor for the subject trace, see also Déprez (1995) for an alternative suggestion). Likewise, the ungrammaticality of (1b) seems to have independent i.e., case-theoretic reasons (cf. the Case filter in Chomsky (1981, 1986)). Obviously, we have a systematic difference here between finite and infinitival relative clauses. The puzzle can be

formulated as follows:

Puzzle: *Why can't overt relative pronouns appear in infinitival relatives while they appear in finite relatives?*

A systematic account of this puzzle has to take into account two additional facts. Firstly, PPs can appear as relative operators in infinitival relatives (compare (3b) vs. (7)):

(7) A linguist [*with whom* [to work __]] is in your office.

And secondly, overt *wh*-elements may appear fronted in infinitival interrogatives, as in (8), the counterparts of (2b) and (3b):

(8) a. I don't know [*which book* [to read __]].
b. I don't know [*whom* [to work with __]].

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14.

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Quantification therapy?

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Broca's aphasia is a term that refers to language disturbance characterized by halting, disfluent, effortful speech. Historically, this type of impairment is associated with damage to the superior frontal convolutions of the left temporal lobe. This area is generally described as Broca's area, after Pierre Broca, who first associated this language impairment with damage to the left temporal lobe. Broca's research was first presented to the French Academy of Anthropology in 1861. Subsequent to Broca's early descriptions it was reported, first by Pitres (1898), that there was a type of Broca's aphasia in which parts of speech were selectively omitted. In this condition, in addition to effortful, disfluent speech, verbs are almost always uttered in progressive form with no apparent use of inflectional morphology, derivational morphology appears to be retained but only in nominal or adjectival form, and functional elements - determiners, complementizers, modals, prepositions and quantifiers - are noticeably lacking. This style of speech was described as agrammatic or telegrammatic because the utterances seem to be formed without regard to the niceties of the grammar and are reminiscent of the economy of style used in telegrams. The term 'agrammatism' has come to be associated with this condition.

It was generally believed that while speech production in agrammatic Broca's aphasics was impaired, their comprehension of speech was intact, although in 1914 Salomon proposed a comprehension disorder coincident to the expressive disorder generally recognized at the time. In research reported in 1976, Caramazza and Zurif investigated comprehension deficits associated with agrammatism. They reported that the Broca's aphasic subjects they tested could not understand thematically reversible object relative constructions on a sentence picture verification paradigm. That is, (potentially) reversible object relatives of the form 'the girl the boy is chasing is tall' were incorrectly associated with pictures that corresponded to 'the girl who is chasing the boy is tall'. Non-reversible object relatives, on the other hand were correctly comprehended. Thus sentences like 'the dog the boy is patting is brown' were correctly identified.

On the basis of this performance, Caramazza and Zurif proposed that these individuals were 'asyntactic'. They did not generate a syntactic representation associated with the sentence they heard at all but rather relied on extralinguistic heuristic devices, such as canonical word order and plausibility, to guess at the meaning of sentences. In the case of reversible object relatives, the application of heuristics resulted in incorrect comprehension. The canonical word order approach determined that the first mentioned NP would be the agent and, as plausibility did not contradict this conclusion, they misinterpreted the sentences. In the case of the non-

reversible object relatives, the canonical word order approach would assign agent to the first mentioned NP, 'the dog' in the above example, but the implausibility of a dog doing the patting rather than being patted overrode the canonical word order and a correct interpretation resulted. This proposed account of a comprehension deficit in agrammatic patients set the tone for many subsequent approaches. The comprehension deficit is explained in terms of an impaired, in this case non-existent, syntactic representation.

Further research demonstrated what appeared to be a correspondence between the lack of functional terms in the speech of agrammatic patients and their comprehension deficits. Heilman and Scholes (1976) demonstrated that agrammatics could not distinguish between sentences of the type 'Mary showed her the baby pictures' and 'Mary showed her baby the pictures'. Since the distinction between the two sentences is marked by the position of the determiner 'the', the impaired ability to distinguish between these two sentences suggested that the lack of determiners in the agrammatic's speech was accompanied by an inability to attend to determiners in comprehension. It was further demonstrated that, along with their problems in interpreting relative constructions, agrammatics also had difficulty with reversible passive constructions. Various proposals to account for the comprehension deficits were advanced. Safron et al. (1980) proposed that agrammatics do not map thematic roles onto word order. Bradly, Garrett and Zurif (1980) suggested that the normal access route to the functional vocabulary might be absent in the agrammatic. Caplan (1983) suggested that the syntactic representation generated by agrammatics was impaired and that the use of a complex heuristic that was sensitive to thematic role assignment could account for the pattern of impaired comprehension. Caplan and Futter (1986) took a similar approach. They suggested that the syntactic representation constructed by agrammatics might consist only of projections of lexical heads.

The major difficulty encountered by most of these approaches is that they predict uniform performance on the part of the brain damaged individuals. If no functional elements are represented then all reversible passives should be consistently interpreted as active. This is because the agrammatics understanding of *the boy was kissed by the girl* will be *the boy...kissed...the girl*. The canonical word order heuristic will always determine that the first NP is agent. However, the actual agrammatic performance on these constructions is generally chance. Similarly, the interpretation of object relatives is predicted to be systematically incorrect: *the boy who the girl kissed held a book* becomes *the boy...the girl...kissed held a book*. Once again, the canonical word order heuristic will always determine that the first NP is agent whereas the actual agrammatic performance is chance.

Grodzinsky (1984) and subsequent work offers a different angle on the problem of accounting for comprehension deficits in agrammatism. Grodzinsky notes that the constructions that agrammatics have difficulty understanding all involve moved constituents. Grodzinsky proposes an account that relies on the traces of movement being lacking from the linguistic representation available to the agrammatics. This approach does not exploit the open class/closed class distinction apparent in English agrammatic behavior but rather asserts that the agrammatic is incapable of representing the antecedent-trace relation. If this is so, he argues, then the thematic roles transmitted from the trace to its moved antecedent will not be retrievable

by the agrammatic.

Grodzinsky invokes a heuristic to account for the thematic roles that are assigned. He suggests that thematic roles are available in hierarchical order - agent, patient, theme, goal - and that when an agrammatic encounters a linguistic representation in which a referring expression is not associated with a thematic role, then he provides it a default interpretation by assigning a thematic role, taken in order, from the hierarchy. Grodzinsky's account of passive interpretation is as follows. The sentence *the boy was kissed by the girl* encodes the displacement of the object via a movement chain: *the boy$_i$ was kissed t$_i$ by the girl*

However, for the agrammatic, the antecedent trace relation is not represented. The agrammatic has access to *the boy was kissed ... by the girl*.

The agrammatic's representation and grammar are otherwise intact. Since the first NP, 'the boy', is not associated with a thematic position, the heuristic provides a thematic role, agent, from the top of the list. The next NP is 'the girl'. However, the preposition 'by' is recognized as assigning agent theta to its complement so 'the girl' is assigned agent. Now the agrammatic has a problem. There is one representation with two agent theta roles. How can this be interpreted? Grodzinsky suggests that it cannot, and so the agrammatic must guess at the correct interpretation. This results in chance performance for tests of comprehension of reversible passives.

Other approaches that tie the agrammatic comprehension deficits to modification of their derived syntactic representations include the tree pruning hypothesis advanced by Naama Friedman (2005) and the complexity approach advanced by Herman Kolk (2000).

In the late 1980's other researchers became interested in the proposal that syntactic representations could be impaired and asked how non-thematic aspects of meaning like scope were handled by agrammatics. The agrammatic's systematic problem of comprehending passive and other constructions containing displaced constituents would seem to be, at root, a problem of complexity of processing. It was therefore expected that agrammatics would have access to only one of the potential readings associated with common existential-universal interaction. However, investigations into agrammatic interpretation of sentences like 'a boy photographed every girl' showed that agrammatics easily retrieved both potential readings. Subsequent investigations into the interpretation of quantified expressions and *Wh* dependencies revealed that these were generally retained (Saddy 1992, Hickok and Avrutin 1995, Saddy 1995). Such results were difficult to explain for the approaches outlined above. More problematic, however, was the observation that when universally quantified NPs were substituted into the simple reversible passive constructions, comprehension performance improved from chance in constructions like (1a) to normal in constructions like (1b) and (1c).

(1) a. a boy was kissed by a girl →
    b. every boy was kissed by a girl; or
    c. a boy was kissed by every girl

That is, not only did the agrammatic understand that there was both wide and narrow scope readings available in (1b) and (1c), the agrammatic also no longer

showed any confusion about the thematic interpretation of the reversible passive.

This observation suggests that an account of agrammatic comprehension based purely upon syntactic representation is unlikely to succeed. From the psycho- and neuro-linguistic perspectives this observation raises many questions about the relation between syntactic and semantic representations and the mechanisms of comprehension available to speakers of English and similar languages. It appears that, in these cases, the use of universal quantifiers may be therapeutic! Why this might be true remains elusive.

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15.

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Decomposing questions acts

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German *wieder* ('again') has several uses that have not been discussed in much detail as Klein (2001) points out. One puzzling use that I have not seen discussed even by Klein is that of *wieder* in questions (as far as I can tell, *nochmal* displays the same range of uses). Consider example (1):

(1) Wie war wieder ihr Name?
how was again your name

It seems that *wieder* in (1) has a reading, that is absent from the corresponding declarative in (2). (2) has two interpretations: One interpretation requires a namechange: my name used to be Uli at some point in the past, then I had a different name for a while, but now I went back to Uli. Secondly, (2) has an interpretation, where it states that somebody earlier in some salient sequence (not necessarily temporal), also had the name Uli. Focus disambiguates between these two interpretations. While the former requires focus on 'Uli', the latter requires focus on 'mein'.

(2) Mein Name ist wieder Uli.
my name is again Uli

The question in (1) allows an interpretation that corresponds to neither of these interpretations of (2): (1) does not presuppose that the person addressed has changed his name or that there is another person with the same name. I suspect that this interpretation involves an interpretation of *wieder* at the speech act level.

One initial indication of this speech-act nature comes from its cooccurrence with *denn*. Krifka (2001) discusses the German particle *denn* as a speech act particle. *Denn* naturally co-occurs with *wieder* as in (3).

(3) Wie war denn wieder ihr Name?
how was DENN again your name

Secondly the use of *wieder* in (1) and (3) seems to indicate prior knowledge of the answer to the question. It would not be natural to ask (1) or (3) to someone who you meet for the first time.

However, it is also inaccurate to scope *wieder* over the question act as in (4): If anything, this would require that the speaker have asked the addressee the same question before (I adopt here the question operator *Quest* from Krifka 2001).

(4) wieder(Quest(Wie war Ihr Name))

Rather it seems sufficient that the speaker just knew the answer to the question at some time in the past. (1) does not presuppose a prior speech act.

Interestingly, the question act patterns with the complex *want to know* in (5) rather than *ask* with (6): (5) like (1) doesn't require a prior question act, while (6) does.

(5) Er möchte wieder wissen, wie Sie heißen.
he wants again know how you are.named

(6) Er fragt wieder, wie Sie heißen.
he asks again how you are.named

For questions acts, the facts seem to indicate the following: The reading of *wieder* in questions involves the speaker, but it does not presuppose a prior speech act. Therefore, examples like (2) seem to require a decomposition of the question act into two parts. I would still like to know, though, which of the various proposals (Truckenbrodt (2004) and references there) are actually compatible with facts like (2) and how other properties of speech acts interact with such a proposal and with other properties of questions.

In particular, the data in (7) are still puzzling: In a situation that brings out the interpretation of *wieder* mentioned above, *wieder* prefers to occur in front of the subject quantifier rather than following it.

(7) Situation: A waiter forgot the order of each person at the table. He asks:
a. Was hat nochmal/wieder jeder bestellt?
what has again everyone ordered
b. ?? Was hat jeder nochmal/wieder bestellt?
what has everyone again ordered

The control in (8) shows that with a non-quantificational subject either order is acceptable.

(8) a. Was hat nochmal/wieder Manfred bestellt?
what has again Manfred ordered
b. Was hat Manfred nochmal/wieder bestellt?
what has Manfred again ordered

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16.

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Do free relative clauses have quantificational force?

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Consider the German Free Relative Clauses (FRC) in (1):

(1) Wer nimmt, was ihm nicht gehört, ist ein Dieb
who takes what to.him not belongs is a thief

The meaning of (1) can clearly be paraphrased as (2), so that the subject FRC gets a universal (or generic) meaning:

(2) Jeder, der (everyone who) nimmt, was ihm nicht gehört, ist ein Dieb

But now, the meaning of the embedded object FRC in both (1) and (2) can be paraphrased as in (3):

(3) Jeder, der etwas nimmt, das ihm nicht gehört, ist ein Dieb
everyone who something takes that to.him not belongs is a thief
'Everyone who takes anything that does not belong to him is a thief'

The point I want to make is that, given the meaning of *thief*, the embedded FRC can (and must) be semantically interpreted as an indefinite existential quantification with a free choice reading. It is not the case that only those who steal *everything* that does not belong to them are judged to be thieves.

Consulting the literature on the subject, this comes as a surprise. The standard analysis seems to be that FRCs either have a universal (sometimes) generic reading, or a definite reading (cf. e.g. Wilder (1998), Jacobson (1995), or Grosu (2003)); the existential reading FRCs exhibit hitherto has been gone largely unnoticed. As has kindly pointed out to me by Ralf Vogel, there is the exception of Wiltschko (1999). However, her examples of indefiniteness allow for reinterpretations that make them less straightforward than (1). To exemplify, Wiltschko argues that, given that the set of subjects studied by a student may vary from student to student, the FRC in (4) cannot have a definite reading.

(4) every student studies what(ever) (subject) is useful for society

However, this indefiniteness might well be the result of some hidden, unexpressed variability that is located inside the FRC, as exemplified by an overt expression like *he thinks* in (5):

(5) every student studies what(ever) he thinks is useful for society

But (5) undoubtedly has a definite (universal) interpretation. Likewise, the variability may also be induced by different temporal relativizations of *useful*; whatever is useful at a certain time may then nevertheless be interpreted as definite, but relativized to the student at various times. (5) then reads as:

(6) (the) students studied the subjects that were considered useful (at a certain time)

As pointed out to me by Sam Featherston, some such additional implicit parameter seems to be necessary for the proper understanding of (4).

Jacobson, in assuming that the universal reading is a special case of a definite plural reading, tries to unite the ambiguity between universal and definite reading by a mechanism that picks out the *maximal* set of entities that satisfy the RC. The latter is interpreted by the Hamblin meaning of the question corresponding to the FRC. The same line of analysis is taken by Grosu. Apart from a technical problem, namely that I do not see why maximality is essential to the analysis (given that the lambda term already is (or represents) a maximal set, so that there is no need for additional maximality) the more important problem is that intuitively there still remains an ambiguity that is not accounted for, and this problem becomes even more severe when taking into consideration the additional variability of interpretation illustrated in (1)-(3).

Given that an existential indefinite reading cannot be obtained from a definite one, the above observation not only refutes the attempted unification, it also invites the conclusion that no lexical ambiguity at all (supposedly of an empty D-head to which the FRC is attached) can be involved. If there were, (7) will be added to the list of problems:

(7) Ich kaufe nur, was mir gefällt
I buy only what to.me pleases

In calculating the truth conditions we first consider the set of things that please me, then chose any alternative disjoint set, so that the meaning of *only* gives us:

(8) If *x* is something that does not please me, I don't buy *x*.

(8) is compatible with a definite/universal interpretation of the FRC but is clearly incompatible with an existential interpretation. But what about the presupposition of *only* in (7)? Is there any, over and above what we've already got in (8)? In order make the problem more transparent, consider:

(9) Ich kaufte nur, was mir gefiel
I bought only what to.me pleased

(9) clearly does have a presupposition, namely that I bought **something** that

pleased me; we do **not** get as presupposition the definite/universal interpretation of (9) without *only*:

(10) a. Ich kaufte, was mir gefiel
b. If *x* is something that pleased me, then I bought *x*

Nonetheless, this definite/universal meaning was required to calculate the meaning of (9). How can this result be achieved in a compositional way?

If this is correct, a number of questions arise for which I do not have a definite solution. What is it that determines the interpretation of the FRCs in (1) and (9)? Even if the resolution of multiple ambiguities is guided completely by pragmatics, we would like to know which principles are responsible for the fact that in most cases the interpretation is unambiguous. If the quantificational force is completely context dependent, how does the pragmatics work that resolves ambiguity? Are there word order effects? Is there a subject/object asymmetry? How does processing influence the interpretation, if it does? To answer these questions, much more empirical work needs to be done. On the other hand, the variability itself seems to challenge any theory that stipulates that the meaning of FRC is quantificational.

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17.

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Why are the lazy so agreeable?

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Serbo-Croatian allows a pronoun to show either grammatical or semantic agreement with its antecedent. That is, a pronoun may share the formal features of its antecedent, or it may display features according to its intended referent. But there is a puzzling exception: 'genuine pronouns of laziness' (Geach 1962, Evans 1977), also called 'paycheck pronouns', permit only grammatical agreement. Why?

Pronouns typically make reference to previous linguistic material in the discourse, using a variety of construal modes, including coreference, bound-variable interpretations, 'donkey' anaphora, and so on. The pronouns Evans 1977 called 'genuine pronouns of laziness', later called 'paycheck pronouns', are not coreferential but can usually be interpreted by substituting the antecedent noun phrase for the pronoun. Some examples follow (examples based on Karttunen 1969, Cooper 1979):

(1) a. The woman who gave *her paycheck* to her daughter was wiser than the woman who used *it* to bail her husband out of jail.
b. John spent *his paycheck* on beer. Everyone else put *it* in the bank.

On the relevant (and most plausible) readings of the sentences in (1), the pronoun *it* does not corefer with its italicized antecedent. But substituting the antecedent for the pronoun yields a sentence with the relevant reading, where the possessive pronoun is bound by its local subject.

Serbo-Croatian has several types of agreement: within a nominal, determiners and adjectives agree in gender, number and case with the head noun; a nominative subject NP triggers person and number agreement on finite auxiliaries and finite verbs, and triggers number and gender agreement on participles; and pronouns agree in person, number, and gender with their antecedents. Each of these types is illustrated in (2) below. Wechsler and Zlatić (1997, 2003) argue that nouns have two grammatical agreement feature sets: 'Concord' features that are grammaticalizations of declension features; and 'Index' features that are grammaticalizations of semantic features. NP-internal modifiers (determiners and adjectives) show *concord* with the head noun; finite verbal elements (auxiliaries and verbs) show *index agreement* with the subject; pronouns show *index agreement* with their antecedents. Among our evidence for this split is the mixed agreement with certain collective nouns such as *deca* 'children' (as in example 2), *braća* 'brothers', *gospoda* 'gentlemen', *vlastela* 'landowners'. Modifiers of such nouns are feminine singular while finite verbs and pronouns show neuter plural agreement, the latter illustrated by the continuation of sentence (2) shown in (i).

In addition to the two sets of grammatical agreement features, an NP or pronoun denotation can have semantic features such as 'male' and 'female'. Masculine plural is the default form for pronouns referring to collections of mixed or unknown sex or gender. When the antecedent of a pronoun is *deca* 'children', we find variation between neuter plural and masculine plural:

(2) Posmatrali smo ovu dobru decu$_i$.
watched.1.PL AUX this.F.SG good.F.SG children.ACC
(i) Ona$_i$ su se lepo igrala.
they.**NT.PL** AUX.**3**.**PL** REFL nicely played.**NT.PL**
(ii) Oni$_i$ su se lepo igrali.
they.**M.PL** AUX.**3**.**PL** REFL nicely played.**M. PL**
'We watched these good children$_i$. They$_i$ played well.'

Summarizing, the neuter plural pronoun in (i) registers the grammatical ('Index') person, number, and gender features of the antecedent noun *deca* 'children', while the masculine plural pronoun in (ii) registers the semantic features of the antecedent.

Wechsler and Zlatić (1999, 2003) explored the question of how this alternation between grammatical and semantic agreement on pronouns is affected by the mode of pronoun construal: coreference, bound variable, e-type, paycheck, etc. Our results are as follows.

A coreferential pronoun in a separate sentence from its antecedent alternates between grammatical and semantic agreement, as shown already in (2). However, if the antecedent is in the same sentence, semantic agreement is strongly preferred, as shown in the variant of (3) without *svako* 'every'. This example uses the neuter diminutive *devojče* 'girl'. Similarly, with pronouns whose antecedents are quantifiers like *every girl,* which are interpreted as variables bound by the antecedent quantifier, semantic agreement is strongly preferred:

(3) a. (Svako) devojče$_i$ misli da je$_i$/*ga$_i$ Jovan voli.
every.NOM.NT.SG girl.NT.SG thinks that PRON.ACC.SG.F/*NT John likes
'Every/The girl thinks that John loves her.'
b. Jovan je rekao (svakom) devojčetu da je$_i$/*ga$_i$ voli.
John AUX told every girl.DAT.NT.SG that PRON.ACC.SG.F/*NT likes
'John told every/the girl that he loves her.'

As shown by this example, semantic gender is observed regardless of whether the antecedent is the subject (3a) or object (3b) and regardless of whether it is a definite NP (*devojče* 'the girl') or a quantifier (*svako devojče* 'every girl').

Reflexive pronouns are split: those with nominative case antecedents require index agreement, while those with non-nominative antecedents require semantic agreement. Whether the antecedent is a quantifier or referential NP does not appear to affect this pattern:

(4) a. (Svako) devojče je volelo
every.NT.SG girl.NOM.NT.SG AUX liked.NT.SG
samo/?*samu sebe.
own.ACC.NT.SG/?*.ACC.F.SG self.ACC
'Every/The girl liked herself.'

b. (Svakom) devojčetu je bilo žao
every.DAT.NT.SG girl.DAT.NT.SG AUX be.NT.SG sorry
same/*samog sebe.
own.GEN.F.SG/*NT.SG self
'Every/The girl felt pity for herself.'

Next we turn to 'E-type' or 'donkey' pronouns, pronouns that have a quantifier as antecedent, but are not semantically bound by that quantifier, at least under some analyses (see Geach 1962, Evans 1977, 1980). Serbian/Croatian donkey pronouns allow either index or pragmatic agreement, i.e. either neuter plural or masculine plural is permitted.

(5) a. Svaki čovek koji ima decu misli
every man who has children thinks
da su ona najpametnija.
that AUX.3PL they.NT.PL smartest.NT.PL

b. Svaki čovek koji ima decu misli
every man who has children thinks
da su oni najpametniji.
that AUX.3PL they.M.PL smartest.M.PL
'Every man who has children$_i$ thinks they$_i$ are the smartest.'

To summarize: Semantic versus grammatical agreement depends on locality and other factors, but not on the mode of construal. Certain local domains seem to favor semantic agreement for ordinary pronouns, while grammatical agreement is required for nominative-bound reflexives. Outside of the local domain, pronouns alternate. These patterns appear to be orthogonal to the construal process such as bound variable, e-type, and coreference.

With that as background, let us now consider paycheck pronouns:

(6) Otac koji je insistirao da mu deca idu na studije je bio pametniji od
father who AUX insisted that his children go to college AUX was smarter from
onog oca koji je insistirao da se ona/oni odmah zaposle.
that father who AUX insisted that REFL they.NT.PL/they.M.PL immed. employed
'The father who insisted that *his children* go to college was smarter than the father who insisted that *they* immediately get a job.'
ona (they.NT.PL) => strict or sloppy reading
oni (they.M.PL) => ONLY strict reading

Interestingly, in order to yield the pragmatically plausible 'sloppy' interpretation, the pronoun must appear in neuter plural form, showing grammatical agreement with the antecedent *mu deca* 'his children'. The masculine plural pronoun

forces the rather implausible 'strict' reading in which the second (less smart) father has the audacity to insist that the children of the first (smarter) father get a job. This judgment has been confirmed by several native speakers.

Why do pronouns of laziness require grammatical agreement? We do not know the answer, but we made some comments that may point towards one (Wechsler and Zlatić 2003:212ff). A paycheck pronoun is interpreted exactly as if it were replaced with a copy (or near-copy) of its antecedent NP— it is a 'lazy' pronunciation of a full NP. (This stands in contrast to other types of construal, cp. 'Every girl thinks that John loves her' ≠ 'Every girl thinks that John loves every girl'.) Perhaps paycheck pronouns substitute for full NPs under a condition of formal consistency, i.e. the grammatical features must match. Why should there be such a condition for laziness/paycheck pronouns? If Manfred can solve this puzzle, he will simultaneously earn his paycheck and reward my laziness.

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