Quirky Case and 'Co-generative' LFG+Glue

Avery D. Andrews
ANU, May 2010

Andrews (1982, 1990a) presented an analysis of the phenomenon of 'Quirky Case' in Modern Icelandic that does a good job of accommodating quite a range of exotic phenomena, but arguably suffers from excessive stipulativeness in certain areas. Here I will argue that some of the problems can be cleared up with a modification of LFG architecture based on 'glue semantics' (Dalrymple (2001), Asudeh (2004), and numerous other sources), in which the glue semantics is adapted as described in Andrews (2007a), so as to provide an equal basis with the c-structure for producing the f-structures ('generative', as opposed to 'interpretive' glue, one might say). This, in combination with some other ideas (especially from Andrews (2008b)), will provide a basis for improving several aspects of the original analysis.

'Quirky case' is the name that has emerged for the phenomenon whereby several hundred Icelandic verbs specify 'non-canonical' cases on their subjects, objects and 'second objects',[1] in various combinations.[2] The default case for subjects is nominative, but subjects can also be dative, accusative or genitive. Objects, first or second, are arguably accusative by default (although it can be argued that the default case of first objects is dative, when a second object is present), but, again, the other three cases can appear as well, although not in all combinations. See Yip et al. (1987) for a presentation of the possible combinations.

Much of the interest of quirky case (QC) comes from its interaction with agreement, and the traditional 'movement' and 'deletion' rules such as Passive, Subject-Raising, and EQUI(valent Noun Phrase Deletion; nowadays usually known as control of PRO). In a nutshell, QC usually:

(1) a. Fails to trigger agreement.

b. Is preserved under 'Movement.'

c. But is lost under 'Deletion'.

But there are some important caveats, in that QC:

(2) a. Does not inhibit agreement in secondary predication, floated quantifiers, or control of a non-QC argument by a QC controller (Andrews 1990a, Sigurðsson 2008).

b. Is lost under some arguable movement-rules, such as the 'middle' (Svenonius, 2006).

The term 'Quirky Case' has acquired some diversity over the time in which it has been employed, in particular, it is often used to describe case whose distribution can't be predicted from anything, but must be a fully idiosyncratic specification of a lexical item. On this usage, there is probably rather less Quirky Case in Icelandic than was originally thought. van Valin (1991) showed that many instances of the dative were semantically predictable, and therefore not Quirky, while Jónsson (2003) notes many other semantic regularities. But here, I will use the term strictly to refer to NPs that arguably have the two layers proposed in Andrews (1982, 1990a).

[1] The term we will use for the second bare NPs after the verb, in SVO sentences.

[2] Obeying various restrictions, the most important of which is that truly Agentive subjects can never by Quirky. See Jónsson (2003) for a discussion of the semantic concomitants of Quirky Case.

First, I will introduce the analysis and how it describes certain things and arguably even explains some of them, but will also present the problems of stipulativeness that I want to address. Then I will show how the glue-based proposal addresses them, and increases the level of explanation attained. Along the way, I will discuss various other analyses that have been proposed, since there's no point trying to refurbish the 1982 analysis if something more recent really does a better job. A significant point is that I won't try to explain the phenomena under (2b), on the basis that they appear to be in general rather inconsistent. So for example Ancient Greek almost always 'strips' QC under passive (Feldman, 1978), while Faroese seems to do so sometimes (Thráinsson et al., 2004). So a degree of stipulation seems reasonable here in the synchronic grammar, with the real explanations being historical.

1 The 1982 Analysis

Relative to its competitors, the most distinctive characteristic of the Andrews (1982) analysis (henceforth A1982) is the use of a 'two layer' structure for QC NPs, whereby the PRED-value and the agreement features reside on the inner layer, and the outer layer is for the most part empty, except for taking the inner layer as value of an attribute (the CASE-name, primarily for reasons of formal convenience).

So for example the sentence (2a) gets the structure (2b):

(3) a. Okkur rak á land
us(A.PL) drifted(3SG) to shore
We drifted to shore

b.
$$\begin{bmatrix} \text{SUBJ} & \begin{bmatrix} \text{ACC} & \begin{bmatrix} \text{PRED} & \text{'Pro'} \\ \text{NUM} & \text{PL} \\ \text{PERS} & \text{I} \end{bmatrix} \end{bmatrix} \\ \text{TENSE} & \text{PAST} \\ \text{PRED} & \text{'Reka(SUBJ ACC, PCOMP)'} \\ \text{PCOMP} & \begin{bmatrix} \text{PRED} & \text{'Á'} \\ \text{OBJ} & \begin{bmatrix} \text{PRED} & \text{'Land'} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

The two-layer structure is supposed to account for, and, ideally, even explain, the fact that the verb form is third person singular rather than first person plural.

The resistance of QC NPs to agreement is one of their most striking properties, also preserved, along with the QC marking itself, under the traditional 'Movement' rules. This includes both finite verb agreement in person and number, and passive participle agreement in gender and number. Rather than appearing in the expected gender/number form, participles appear in a neuter singular form neutral between nominative and accusative (NA), while finite verbs appear to be 3SG:

(4) a. Okkur rak/*rákum á land
us(A.PL) drifted(3SG/*1PL) to shore
We drifted to shore

b. Strákana virðist/*virðast hafa rekið á land
the boys(A.PL.M) seem(SG/*PL) to have drifted to shore
The boys seem to have drifted to shore

c. Strákana er/*eru sagt/*sagða hafa rekið á land
the boys(A.PL.M) is/*are said(NA.SG.N/*A.PL.M) to have drifted to shore
The boys are said to have drifted to shore

The preservation of QC under 'Movement', illustrated in (b-c), is managed well in a variety of grammatical frameworks (obviously in those that actually use Movement or something similar; see Sag et al. (1992) for an HPSG analysis), but resistance to agreement is more challenging. For example, neither Yip et al. (1987) nor Sag et al. (1992) attempt to account for it at all.

The obvious thing to try is to suppose that these QC forms carry some peculiar feature which the 'default', 'nonagreeing' forms are actually manifesting agreement with, but this idea runs afoul of the fact that various items such as secondary predicates, floating quantifiers and adjectival complements with QC controllers all do manage to agree with QC controllers, as discussed in Andrews (1990a), Sigurðsson (1991, 2008) and elsewhere:

(5) a. Strákana rak þyrsta og svanga á land
the boys(A.PL.M) drifted(SG) thirsty(A.PL.M) and hungry(A.PL.M) to shore

b. Strákana vantaði alla í skólann
the boys(A.PL.M) lacked(SG) all(A.PL.M) in school
The boys were all absent from school

c. Þeir lýstu glæpamanninum sem stórhættulegum
They described the criminal(D.SG.M) as very dangerous(D.SG.M)

d. Hann heldur tönnunum sínum hvítum og hreinum
He keeps teeth his(D.PL) white(D.PL) and clean(D.PL)

The problem is that if the 'nonagreeing' forms are really just agreement with a special feature, we would expect this feature to cause these forms to appear in the examples of (5), but this doesn't happen. The examples of (c,d) (from Andrews (1990a)) are particularly puzzling, because these do not appear to be an arguably special structure like the others, but rather to be merely ordinary control structures in which the controller happens to assign QC, but not the controllee. If apparent nonagreement were just wierd agreement, one would certainly expect to see it here.

Most people who try to say anything at all about this[3] appear to produce a collection of verbal formulations, amongst which the assertions (6) are fundamental:

(6) a. Finite verbs never agree with QC NP

b. Predicate adjectives and participles that assign QC to an NP never agree with it.

Then there must appear a collection of statements concerning what sorts of things can agree with the QC (and the passive participles that QC NP move into subject position of without agreeing with are sometimes not covered by the formulation). This is clearly observation, not explanation, or even a good technical implementation that might help lead towards an explanation.

But the A1982 accounts for this material in a reasonably clear way, approaching explanation in certain respects, albeit with some substantial issues. The analysis assumes that singular

[3] Such as Boeckx and Hornstein (2006), Sigurðsson (2008).

number, third person, neuter gender, and, in Icelandic, nominative case, are all associated with the absence of any value for the associated feature.[4] Agreement forms are then supposed to specify the attributes found at the value of some grammatical relation, for example an agreement form for plural number of the subject would say that the value of the NUM attribute of the value of the SUBJ attribute is PL (by means of the equation (↑SUBJ NUM) = PL, in the usual LFG notation).

Returning to (3b), we see that under these assumptions the verb form is in fact showing the features of the outer layer, as expected.[5] Indeed, the two-layer idea can be seen as a somewhat special implementation of the 'wierd agreement feature' idea, which will, as we shall see, overcomes its main problems. But we aren't quite done yet, because why can't a 1PL form be used on the verb so as to produce a structure like this:

(7)
$$\begin{bmatrix} \text{SUBJ} & \begin{bmatrix} \text{ACC} & \begin{bmatrix} \text{PRED} & \text{'Pro'} \\ \text{NUM} & \text{PL} \\ \text{PERS} & \text{I} \end{bmatrix} \\ \text{PERS} & \text{I} \\ \text{NUM} & \text{PL} \end{bmatrix} \\ \text{TENSE} & \text{PAST} \\ \text{PRED} & \text{'Reka(SUBJ ACC, PCOMP)'} \\ \text{PCOMP} & \begin{bmatrix} \text{PRED} & \text{'Á} \\ \text{OBJ} & \begin{bmatrix} \text{PRED} & \text{'Land'} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

In classic LFG, the answer is that we have to use a bit of stipulation: Icelandic agreement equations, both for PN agreement of finite verbs, and GNC agreement of adjectives and passive participles, have to be 'constraining' rather than 'defining', which mean that they impose a requirement that something be there in the structure, without being able to put it there themselves:

(8) $(\uparrow \text{SUBJ NUM}) =_c \text{PL}$

Although this is somewhat reminiscent of the Minimalist concept of 'unvalued features', in the LFG context it appears to be undesirably stipulative, because no coherent principles have ever been devised to predict which equations should be defining and which constraining. Agreement equations for example appear to have to be able to be defining under various circumstances where the NP to be agreed with is covert (e.g. *andiamo* 'go(I.PL)' in Italian, meaning 'we go' or 'lets go'). To upgrade this aspect of the account from description to explanation, we need to either have some principle that will require the agreement specifications to be constraining rather than defining, or come up with some modification of the architecture that will make this distinction unnecessary, at least here.

A further explanatory problem is: why would language learners go for the two-layer structure in the first place? There are languages, such as Warlpiri, where lexically determined case-

[4] Languages differ in what case is unmarked; so that for example in English, Irish and Ancient Greek, accusative appears to be unmarked, while Icelandic is similar to Turkish and Hindi, with nominative unmarked. See Legate (2008) for relevant recent discussion of case-markedness.

[5] This depends on a very strict 'geometrical' interpretation of the formalism, but if one doesn't interpret formalisms in this way, what is the point of having them in the first place? For some discussion, see the critique of Williams (1984) in Andrews (1990a).

marking has minimal effects on agreement,[6] so that just attaching noncanonical case-values to ordinary NP structures would seem to be the appropriate solution. Why isn't Icelandic like this? The two-layer analysis can be seen as a rather complicated trick to get Icelandic QC to subvert agreement, but it's not really explanatory unless we can come up with a plausible story about what kinds of circumstances push language-learners into adopting it.

We'll leave this for later, and briefly review how the analysis accounts for, and indeed can be said to explain, the behavior under movement/control by a non-theta position. In LFG, QC preservation under 'Movement' is accomplished by the fact that 'Movement' is implemented by multiattachment, that is, the same f-substructure serves as value for two GF's. In the case of classic 'Subject Raising', the complement is treated as value of a GF XCOMP, and the multiattachment is implemented by this equation, associated with the matrix verb (in general, apparent Subject Raising seems to be limited to a relatively small number of verbs and adjectives, in those languages where it occurs at all):

(9) (↑SUBJ) = (↑ XCOMP SUBJ)

In LFG, this configuration is called 'functional control'.

The structure of (4b) will therefore be something like this (treating the TA marking verb *hafa* as well as *virðast* as a 'subject-raising' verb):

(10)
$$\begin{bmatrix} \text{SUBJ} & \begin{bmatrix} \text{ACC} & \begin{bmatrix} \text{PRED} & \text{'Strákur'} \\ \text{NUM} & \text{PL} \\ \text{CASE} & \text{ACC} \\ \text{GEND} & \text{MASC} \end{bmatrix} \end{bmatrix} \\ \text{PRED} & \text{'Virðast(XCOMP)'} \\ \text{XCOMP} & \begin{bmatrix} \text{SUBJ} & [\ ] \\ \text{PRED} & \text{'Hafa(XCOMP)'} \\ \text{XCOMP} & \begin{bmatrix} \text{SUBJ} & [\ ] \\ \text{PRED} & \text{'Reka(SUBJ ACC, PCOMP)'} \\ \text{PCOMP} & \begin{bmatrix} \text{PRED} & \text{'Á} \\ \text{OBJ} & \begin{bmatrix} \text{PRED} & \text{'Land'} \end{bmatrix} \end{bmatrix} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

The key thing is that the 'functional control' equations on the matrix and TA verbs say that the entire SUBJ-value, that is, the outer layer and its contents, are what is shared between the two clauses. Therefore the upstairs verb is no more capable of undergoing agreement than the downstairs one.

On the other hand, a secondary predicate will be expected to show agreement with a QC NP, as long as we can assume that such predicates will have to take the inner rather than the outer layer of as their SUBJ-values, which seems quite reasonable on the basis of how we would expect semantic interpretation to work. For example (11a) below would get the structure (11b):

(11) a. Strákana rak þyrsta á land
the boys(A.PL.M) drifted(SG) thirsty(A.PL.M) to shore

[6] As discussed for example in Hale (1973), Simpson (1991) and Legate (2008), among many other works.

b.
$$\begin{bmatrix} \text{SUBJ} & \begin{bmatrix} \text{ACC} & \begin{bmatrix} \text{PRED} & \text{`Strákur} \\ \text{NUM} & \text{PL} \\ \text{CASE} & \text{ACC} \\ \text{GEND} & \text{MASC} \end{bmatrix} \end{bmatrix} \\ \text{XADJUNCT} & \left\{ \begin{bmatrix} \text{SUBJ} & [\ ] \\ \text{PRED} & \text{`Þyrstur'} \end{bmatrix} \right\} \\ \text{TENSE} & \text{PAST} \\ \text{PRED} & \text{`Reka(SUBJ ACC, PCOMP)'} \\ \text{PCOMP} & \begin{bmatrix} \text{PRED} & \text{`Á} \\ \text{OBJ} & \begin{bmatrix} \text{PRED} & \text{`Land'} \end{bmatrix} \end{bmatrix} \end{bmatrix}$$

The idea here is that since the secondary predicate is giving information about the boys, its SUBJ value must be the inner layer, where the PRED-value and the agreement features are, rather than the outer layer. Therefore we get agreement here.

The same point is made perhaps a bit more strongly by certain instances of what appears to be functional control by QC NP, illustrated in (5c,d). The reason this is a bit stronger is that there is a tradition of suggestions that the floated quantifiers and secondary predicates might originate from a position inside the NP (Rögnvaldsson (1984), Sigurðsson (2008)), thereby explaining their agreement properties (in ways whose details don't seem entirely clear to me), but this is completely unworkable for the 'control by QC' examples.

Here, as discussed in Andrews (1990a), it is fully evident that in order to get the Completeness and Coherence Conditions satisfied for the adjectival complements, the control equation for the matrix verbs must be:

(12) $(\uparrow \text{OBJ DAT}) = (\uparrow \text{XCOMP SUBJ})$

leaving no possibility for the complement predicate adjective but to agree with its QC controller. Once again, the detailed geometry of the structures is crucial.

A conceivable way around this might be to suggest that these constructions involve PRO or anaphoric control rather than functional control/multiattachment, but the trouble with this is that the 'transmission' of case from a controller to a predicate adjective modifying a PRO appears to be in general optional, and for many speakers, impossible for the dative (Sigurðsson, 2008), but seems to be obligatory here.[7]

At this point, given the preceeding, we are near to real explanation, although, to pin it down fully, we perhaps really need an explicit theory of semantic interpretation that will entail that merely presenting one of this adjectives with the outer layer of its controller (which would produce null agreement) would produce uninterpretable results. This was assumed in A1982/90, but not connected to any specific account of semantic interpretation.

But there is one final thing of an explanatory nature to discuss, which is the apparent ungrammaticality that arises when an oblique functional controller tries to control QC subject predicate. Amongst the QC subject predicates in Icelandic are a substantial number of adjectives, mostly taking dative subjects, such as this:

[7] But I don't have much data on this, so it would be a good thing for somebody to check carefully.

(13) Glæpamanninum var (mjög) hlýtt til fjölskyldunnar
criminal-the(D) was (very) warm towards the family
The criminal was (very) warm towards his family

Under a standard 'wierd case feature' theory of QC, such as for example that of Sag et al. (1992), we would expect these to have no difficulty in being controlled by a QC dative controller. But in fact they cannot be. As noted in Andrews (1990a), examples such as (14) appear to be bad:[8]

(14) *Þeir lýstu glæpamanninum sem (mjög) hlýtt til fjölskyldunnar
They described criminal-the(D) as (very) warm towards the family
They described the criminal as (very) warm towards his family

At the time, Andrews (p. 210) didn't draw strong conclusions from this, on the basis that he wasn't really sure that the expressions were actually AP that would be generated as complements in these constructions.

But in retrospect, this would appear to be an excessively tentative conclusion: 20 years have gone by during which these constructions have been written about by various people, especially Sigurðsson, and nobody has ever suggested that they are anything but adjectives that take QC subjects. And, after all, what else would they be? And furthermore, some of them, such as the one above in particular, take typical stigmata of adjectivehood such as degree modification by *mjög*. So supposing that the attempted complement APs are what they appear to be, the two-layer hypothesis straightforwardly explains their ungrammaticality: the control equation of the matrix verb equates the inner layer of the matrix object to the complement's subject, but the complement predicate wants a two-layer quirky dative structure, so that Completeness fails to be satisfied (or, in terms of our glue-based account to come, semantic interpretation 'crashes').

The explanatory aspects of the two-layer analysis can be seen by contrasting what we get from the HPSG account of Sag et al. (1992). The basic idea of this account is that 'case' is split up into the morphologically realized attribute CASE, and the structurally assigned attribute SCASE, whose value is set to NOM on subjects and ACC on objects. Verbs select structural case to be manifested on an argument by imposing the constraint that CASE = SCASE, quirky case by simply setting the value of CASE to whatever the QC-value is to be, DAT for the cases we're interested in here (and not caring about whatever value SCASE might get). The result is that examples like (4c-d) are predicted to be ungrammatical, since the CASE of the object will be DAT but the SCASE ACC, but the non-QC adjective will impose the requirement that CASE = SCASE, but (14) should be fine, since both the matrix verb and the adjective say that this NPs CASE should be DAT (its SCASE-value will be ACC, but this causes no problem).

So the HPSG analysis gets these phenomena exactly backwards, which reveals that it is not so easy to come up with a consistent formal system that gets them right. But our attempted explanation has some questionable aspects to its underpinnings.

And there is a further issue to consider, which is how 'EQUI' (anaphoric control, binding of PRO) is supposed to work. Much of the interest of Icelandic QC NPs has come from the fact that they are subject to the obligatorily ellipsis found with Icelandic infinitives, being

[8] Of nine respondents to a questionnaire distributed for me in 1986 by Höskuldur Þráinsson, 7 found it completely ungrammatical, one a bit unnatural, and one doubtful. Other similar examples making the same point produced similar results.

controlled by a nominative NP while having a secondary predicate in their own clause agree with them (example from Sigurðsson (2008)):

(15) Ólafur hafði ekki gaman af að leiðast einum í veislunni
Olafur(N) had not pleasure from to be bored alone(D) at the party
Olaf didn't have the pleasure of being bored at the party alone

Leiðast is a verb taking a dative QC subject, which is here non-overt or moved, as required for all Icelandic infinitives. But its underlying presence and dative case has since Andrews (1976) been taken to be evidenced by the dative case on a secondary/semi- predicate modifying it.

A1982 relied on Bresnan's 1982 rule of 'anaphoric control' to produce the f-structure for the non-overt subject here, but there is a problem in that the structure of a QC NP is reasonably complicated, involving a substructure whose attribute is the same as the value of CASE in its value, and with gender and number values to boot. In order to produce these structures, Andrews needed an ad-hoc elaboration of either the anaphoric control rule, or the rules producing *að* infinitives; neither approach is fully satisfactory.[9] What we actually want is something that can produce these structures automatically, but also give them the somewhat degraded acceptability that they seem to have (see Andrews (1990a) for one study; other investigators report similar results).

Summing up, we find the following issues that we want to address here:

(16) a. How do the two-level structures of non-overt QC NPs get constructed?

b. Why can't agreement rules dump features randomly into the f-structure?

c. Why, exactly, do secondary predicates and floated quantifiers manage to agree with QC NPs?

d. What causes language learners to adopt the two level structure rather than something else, such as just letting lexical specifications deposit case-markers directly onto the bearers of certain GF's?

My claim is that these questions can be answered by means of a modification of the LFG+glue architecture, along lines presented in Andrews (2007a, 2008b), albeit with certain modifications.

In the next section, I'll briefly introduce the basic aspects of glue that we need, and in the one after, apply it to deal with issues (a-c) above. Then I will move on to case, and the more complex issue (16d).

2 'Co-generative' Glue

I'll take issue (16a) as providing the most fundamental guidance on what is to be done. LFG diverges strongly from TG-based grammatical theories, and many of the popular intuitions about the nature of language, in being strongly 'surface driven'. Rather than having a 'semantic structure' that is 'expressed', or 'externalized', the grammar starts by generating an overt PS tree (c-structure), and then builds the more abstract levels from that. This is clearly where our problem with (15) and (16a) comes from: where there is nothing in the overt structure,

[9] Note that the elaborations required to produce the agreement features will be required for many more languages than Icelandic, such as, for example, Ancient Greek (Andrews 1971, Quicoli 1982).

it can be expected to sometimes be difficult to manufacture anything somewhat complicated in the covert structure.

However a possible solution emerges from the moves Andrews (2007a) made in order to combine glue semantics with OT-LFG. Glue has standardly been conceptualized as a method (based on intuitionistic linear logic) for (non-deterministically) integrating 'meaning-constructors' produced as a by-product of normal LFG structure generation into a semantic representation for the structure generated. Standard LFG+glue could then be described as 'interpretive/model-theoretic off of overt PS', in the conceptual framework of Pullum and Scholz (2001).

But this conception is clearly unworkable for OT-LFG, which had been conceptualized as a system of principles for externalizing f-structures, which were supposed to have meanings (Kuhn, 2003). But the issue of exactly where they got these meanings from was not addressed in any coherent way. Andrews proposed that the glue framework could be adapted so as to produce directly a pairing of f-structures and assembled meanings, without involving the PS-rules. And there are actually some independent reasons for doing this,[10] namely, the fact that most grammatical features do one of the following two things (or either, in different circumstances):

(17) a. Take one of a number of interpretations provided by the grammar, on their own (e.g. NUM PL signifying semantic plurality

b. Take an 'idiomatic' interpretation jointly with a specific lexical item (*pluralia tantum*, grammatical gender).

His proposal was to split the standard unitary LFG lexicon into two components, a 'Morphological Lexicon' that connects overt word forms to f-structural information, almost the same as the standard LFG lexicon, and a 'Semantic Lexicon' that connects f-structure information to semantic information. The Morphological Lexicon works just like the regular LFG lexicon (except that PRED-features don't have argument lists, and the Completeness and Coherence constraints don't exist), but explaining how the Semantic Lexicon works requires a basic presentation of certain aspects of glue, to which we now turn.

'Explaining glue' appears to be a somewhat difficult enterprise;[11] where I will start here is with the 'semantic types' of the basic units of semantic composition, which will typically be 'Lexemes' and semantically interpreted grammatical features. The semantic types are specified in terms of a 'simple' type theory,[12] in a way that has been reasonably familiar since the rise of Montague Grammar and Formal Semantics in the early 70s. Most people start out by assuming two basic types, one for entities (e) and another for propositions, usually symbolized as t (alluding to the notion of a truth-value), but here represented as p. It is then common to entertain more basic types (paths, events, locations, manners, outcomes, ...), and we should also consider the possibility that there is actually only one (Partee, 2006).

But regardless of how this works out, the next step is to in essence follow Frege, and suppose that from the basic types can be derived 'functional types'. For example, a type $e \rightarrow p$ of things such that, if you feed them an entity as an argument, you get a proposition as a result. That is, a one-place predicate. $\rightarrow$ here is the 'implicational type constructor', usually represented as angle-brackets in introductory formal semantics. The type of a negation operator, or an epistemic adverb such as *obviously*, would then be expected to be $p \rightarrow p$ (feed a proposition

[10] Unbeknownst to Andrews, the glue implementation in the XLE LFG system worked directly off f-structure, but for pragmatic rather than theoretically motivated reasons (Richard Crouch, p.c.).

[11] For a more extended and leisurely presentation of the approach I take here, see Andrews (2009).

[12] 'Simple' meaning that glue has not yet found any use for elaborations such as dependent types.

in, get one back). For multi-place predicates, the standard trick is 'currying':[13] a 2-place predicate such as *Like* is treated as being of type $e \rightarrow (e \rightarrow p)$. That is, something of a type such that, if you feed it an entity, you get back a value such that, if you feed that a entity, you get back a proposition. The system of types can be interpreted in at least two ways, either as a system of ontological categories that existing things fall into, or as a system of grammatical categories for expressions in a formal language for expressing meanings. The latter interpretation is what we want here.

In introductory Formal Semantics, people usually stop with the implicational type constructor, but in glue (and sometimes in Type Logical Grammar) it is useful go a bit further and add a 'pairing' constructor, so that something of type $e \times e$ is something whose type is a pair of things of type e. We won't need this in the main body of the text, but we will need it in the more developed analysis of PRO presented in the appendix. (Note that when there is more than one type-constructor, a connective such as $\rightarrow$ becomes more convenient than angle-brackets.) For multi-place predicates, it is useful to follow a convention of omitting rightmost parentheses, so that $p \rightarrow e \rightarrow p$ might be the type of a 'propositional attitude' verb such as *believe*, $p \rightarrow e \rightarrow e \rightarrow p$ of a 'communication verb' such as *tell*.

Which leads to the problem of connecting actual semantic roles to argument positions in the semantic types: for *believe*, should we feed in the proposition first, or the supposed believer? (In which latter case the type would be $e \rightarrow p \rightarrow p$.) There does not appear to be a truly settled convention on this, but I think there's a lot to be said in favor of following the general guidance of Marantz (1984) and Jackendoff (1973), and supplying the 'least active' participants first, since, in various ways, they tend to act as if they were more tightly bonded to the verbal meaning.

Here is a typical collection of 'typed meanings'; what can we do with it?

(18) $\begin{aligned} \mathit{Like} &: e \rightarrow e \rightarrow p \\ \mathit{Bert} &: e \\ \mathit{Ernie} &: e \end{aligned}$

One useful task would be to put them together into a coherent proposition, which, intuitively, can be done in two different ways, which can be expressed as in (18) in the usual parenthesis-based notation $f(a)$ for applying a function f to an argument a, with understood grouping to the left:

(19) a. $\mathit{Like}(\mathit{Bert})(\mathit{Ernie})$

b. $\mathit{Like}(\mathit{Ernie})(\mathit{Bert})$

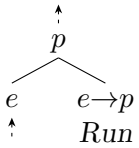
But something a bit more solid than just intuition is needed for complex cases involving quantifiers, control predicates, etc.

The presentation I'll follow here is to first expand the typed meanings into pieces of the structures that the meanings would fit into (including the meanings), following some rules presented in the appendex (the results for the simple cases considered here are hopefully self-explanatory) and then apply some further rules to assemble those pieces. This is really a variant presentation of proof-nets from linear logic (see Andrews (2008a, 2009) for a full discussion; the source of the ideas is de Groote (1999), which draws heavily on Lamarche (1994)).

[13] Or 'Schönfinkelization'; Schönfinkel appears to have been the original discoverer, while Curry did a lot of the work that made it important.

So to start out, if something is of type $e \rightarrow p$, we'd want a piece of tree structure into which we are prepared to plug in something of type e and get out something of type p. We could notate that like this (with the meaningful element as the right daughter, which seems to be the most convenient way to arrange these things):

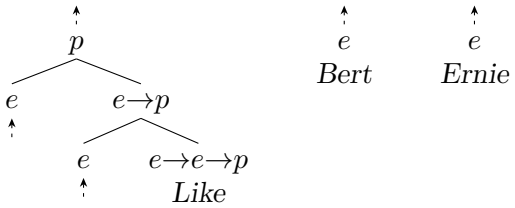
(20)



The idea being that since *Run* is of type $e \rightarrow p$, we are going to use it in structures where something of type e is stuck together with it to produce something of type p, the inbound arrows representing places where an argument is to be plugged in, the outbound ones places where the structure is supposed plug into something else.[14] Rules for expanding a typed meaning to a structure are given in the appendix.

For the example (18) with a two-place predicate, the structures will be:

(21)



To assemble, we plug output arrows into inbound ones, subject to these rules:

(22) a. Compatibility: an input arrow can only be connected to an output arrow of the same semantic type.

b. Monogamy: an input arrow can be connected to only one output arrow and vice-versa.

c. Completeness: every input arrow must be connected to an output arrow.

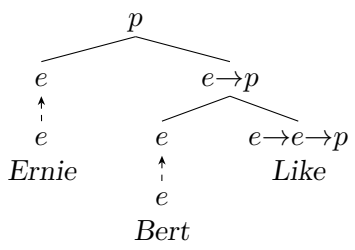
d. Coherence (Only one left over): every output arrow but one must be connected to an input arrow.

e. Correctness: the structure must be connected.

(e) needs to be reformulated when the structures get more sophisticated, but this over-simplified version will do for now. (c,d) are named after the LFG principles that they subsume/replace.

One possible result is:

(23)



[14] Technically the inbound and outbound arrows are the positive and negative polarities, respectively, of the theory of proof-nets.

It should be obvious what the other is.

This system of structures and assembly rules, when completed properly, is equivalent to the deductive linear logic normally used for presenting glue semantics, but more immediately comprehensible for at least some linguists, I believe. It also has significant antecedents in the logical literature, being closely related to C. Meredith's rule of 'Condensed Detachment' (Hindley, 1993).

So we have now put together (part of) a system of 'type driven semantic assembly',[15] whereby, given a collection of typed meanings, we can produce the various assembled meanings that these might yield. The next step is to connect these to f-structures. We do this by connecting the typed meanings to pieces of f-structure, via a function mapping atomic type-labelled nodes in the typed meanings (whether or not they are expanded into structures) to parts of the f-structure pieces. Such a combination of a typed meaning and a partial f-structure is an Semantic Lexicon Entry (SLE); here are some examples with unexpanded typed meanings, where the input and output arrows are omitted, on the basis that they are intuitively obvious, and can be supplied by following the rules in the appendix:

(24) a. $\left[\text{PRED} \quad \text{'Bert'}\right] \quad \Leftrightarrow \quad \mathit{Bert} : e$

b. $\left[\text{PRED} \quad \text{'Ernie'}\right] \quad \Leftrightarrow \quad \mathit{Ernie} : e$

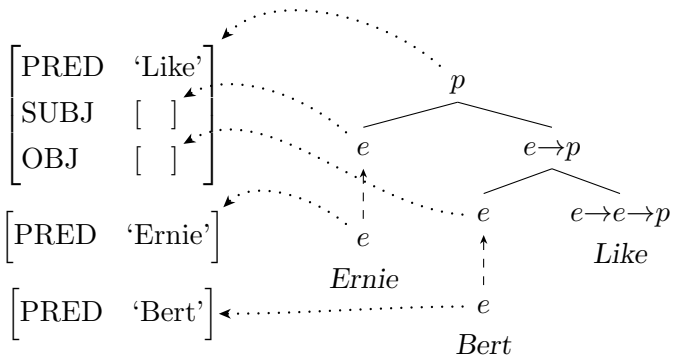
c. $\begin{bmatrix}\text{PRED} & \text{'Like'} \\ \text{SUBJ} & [\ ] \\ \text{OBJ} & [\ ]\end{bmatrix} \quad \Leftrightarrow \quad \mathit{Like} : e \rightarrow e \rightarrow p$

You can see that some of the substructures correspond to argument nodes, while the entire structure tends to correspond to the top 'output' node in the semantic structure. A variety of possible constraints on SLEs are discussed in Andrews (2008b). People familiar with standard glue will note that the SLEs combine the information provided by 'meaning-constructors' (the grammatical functions associated with the arguments) with other information provided by the lexical entry a meaning-constructor appears in (the grammatical feature-values, especially PRED).

The idea is that we're going to grab a bunch of the 'Semantic Lexicon Entries' (SLEs), taking multiple copies of any that we want to use more than once, and then assemble their typed meanings, if possible. There may of course be no legitimate assembly (in which one might say that the attempted semantic computation crashes), or more than one. The selection step would appear to be essentially the same thing as forming a Numeration in the MP. The assembly step however is not very much like the Minimalist 'computation', because it is much simpler, most of the work of the 'computation' being done by other components. These steps produce a structure where there is an assembled meaning, plus various pieces of f- structure connected to that meaning:

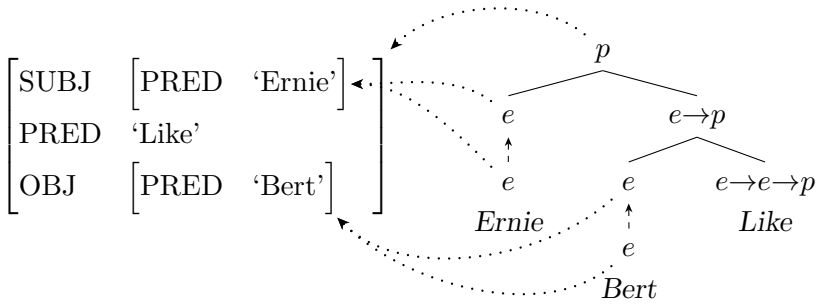
[15] With significant resemblances to the system of Jackendoff (2002), although more restricted, since it doesn't do Jackendoff's 'modification'.

(25)



Suppose now that the semantic nodes connected by the dashed arrows are 'really the same', and that the relationship designated by the dotted lines is really a function, that is, single-valued on the pairs of nodes connected by the dashed lines. Then the destinations of the dotted arrows will have to be merged, and the result will be the assembly of the pieces into a single f-structure:[16]

(26)



We've now got a scheme whereby a semantic assembly can produce an f-structure, conceptually somewhat similar to the way in which the production of a PS tree produces an f-structure in classic LFG.

There are however some important differences, one of which is that in building the meaning-driven f-structure, feature-values cannot be allowed to merge. The necessity for this can be seen by considering what could be done with an SLE like this:

(27) $\left[\text{POLARITY} \quad \text{NEG}\right] \Leftrightarrow \mathit{Not} : p{\rightarrow}p$

The problem is that if merger of feature-values was allowed, we could choose this twice somewhere where both POLARITY-values would appear at a single level of the f-structure, and thereby produce a single f-structure with one negative feature, but two instances of the operator in the semantics, an undesired effect (it would for example, allow confessions to be packaged as denials). This can be blocked by not allowing feature-values to merge when an f-structure is being produced.

An additional constraint on the production of the f-structure is that its topmost structure must correspond to the unconnected ('final output') node of the semantic assembly. This

[16] Andrews (2007a) gives a clumsier description of what this process is supposed to achieve, while Andrews (2008b, 2010) describes a reverse version, which starts with the f-structure and using SLE's to build the semantic structure. It would be good to have a proof that both define the same association between meaning assemblies and f-structures, for a given collection of SLEs.

might be plausibly connected to some kind of principle of hierarchical compatibility between the f-structure and the semantic assembly, but I don't presently have any specific proposal about that. As an exercise in the 'logic of assembly', you can try to work out what kind of assembly is blocked by (22e) (Correctness) but not the others, once SLEs like (27) appear in a Numeration.

We now have a notion of well-formed pairing of f-structures with meanings relative to an Numeration, i.e. collection of SLEs, from which we can get a collection of meanings associated with a given f-structure, and vice-versa, given a Semantic Lexicon.

The next step is to use a regular LFG to connect c-structure to f-structures, and thereby to meanings, via a requirement that the two f-structures 'match' well enough, according to some kind of criteria, such as, for example, those discussed in Andrews (2007a). Two criteria are rather plausible:

(28) a. The two f-structures should not disagree on the value of any attribute

b. The f-structure connected to the c-structure (by regular LFG, using the Morphological Lexicon) should not provide a value for any attribute that the one connected to the semantic structure (by the assembly method for SLEs) doesn't.

The motivation for these principles is for example that if the c-structure and lexical items put, say, a plural feature on the f-structure of an NP, that feature will also have to be interpreted in the semantics. It should be evident that regular LFG will provide *Ernie likes Bert*, but not *Bert likes Ernie*, with an f-structure compatible with that of (26), thereby explaining why the first but not the second of these examples is grammatical with that meaning.

These principles prevent the 'syntactically' generated phrase-structure from producing material not provided by the semantically generated one, but so far impose no constraints in the other direction. Saying that there are no such constraints would obviously be going too far, but it is not so clear exactly what they should be. A reasonable first proposal would be an adaptation of Panini's blocking principle to the general effect that a c-structure f-structure pair is blocked if the grammar provides another one that matches more of the f-structure. This would be equivalent to an OT-style application of a MAX-constraint.

But languages plausibly differ in what kinds of features they allow to be unexpressed. In Ancient Greek, for example, one can make a case that the finite irrealis moods (subjunctive and optative) require their mood features to be expressed overtly in the morphology: the person-marked perfect passive-middle forms, in which the position in which the irrealis markers occur is absent form the morphological template, cannot be used in these irrealis moods, and a periphrastic construction consisting of a participle and irrealis finite copula must be used instead. In effect, something seems to make the 'externalization' of the mood marker obligatory here.

Similarly, variations in the kinds of 'Pro-drop' that are allowed or required could be treated as variations in what kinds of pronominal features need to be expressed in the syntactically generated f-structure, and under what circumstances. This is part of the role of the lexical rule of 'anaphoric control' in classic LFG, but the phenomenon appears to be more suitable for a structural treatment, rather than lexical one. People with an interest OT might note that what I have proposed is asymmetric, in that (28) implies that DEP constraints hold absolutely, while a reasonable amount wiggle-room is left available for MAX. The absoluteness of DEP could well turn out to be a crutch for the feeble-minded, but it seems reasonable to assume it, for now.

On the other hand, allowing the syntactically generated structure to omit things that are present in the semantically generated one (MAX violations) can potentially produce problems of indecidability; it's basically the old issue of 'Recoverability of Deletion' from Chomsky (1965). I won't attempt to say anything specific about how to address it here.

The resulting architecture is significantly different from anything else that I am aware of. In terms of the conceptual framework of Pullum and Scholz (2001), we could describe it as 'co-generative', with two 'generative-enumerative' components, PS rules and semantic structures, each interpreted 'model-theoretically' at the level of f-structure, with a requirement on a well-formed structure that the two f-structures match, well enough.

Before moving on, we should point out one significant difference with standard LFG and that which is to be used with the Morphological Lexicon, which is, in the latter, we won't be using argument lists on PRED-features, and the associated Completeness and Coherence Constraints. The reason is, as first discussed by Kuhn (2001), the glue assembly principles do the basic work of these constraints, so that they should be retired until or unless some evidence appears that they are still really required. PRED-features on the other hand should arguably be retained, as discussed in Andrews (2008b).

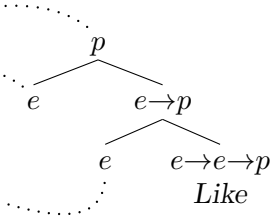
3 Applications

Our next task is to examine LFG+co-generative glue can help manage the issues of (16), taking (a, b, c) in order.

3.1. Producing Covert QC NPs

An important facility that the present proposal provides that isn't available in classic LFG is to allow the semantics, without cumbersome stipulations, to produce things that don't get expressed in the syntax, which is the kind of facility we need to manage 'control of Quirky PRO. Consider first how we can get an SLE to produce the two-layer QC case-structures, like this:

(29)
$$\begin{bmatrix} \text{PRED} & \text{'Líka'} \\ \text{SUBJ} & \begin{bmatrix} \text{DAT} & [\ ] \end{bmatrix} \\ \text{OBJ} & [\ ] \end{bmatrix}$$

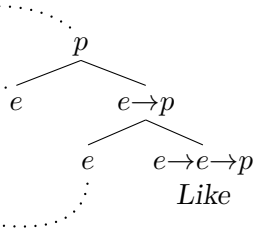


This is no different from our previous SLE for English *like*, except that the Experiencer argument is found at the end of a 2-GF path rather than a 1-GF one. This will have no effect on the ability of this SLE to assemble with nominal constructors, but the f-structure results will be a bit different, in that they will have a two-layer structure for the Experiencer. Likewise for other appearances of QC, on various GFs.

However, there is an issue of detail to consider here. A1982 used CASE-values as GF attributes, in emulation of the analysis of 'idiomatic' PP presented in Kaplan and Bresnan (1982). But this has not proved to be one of the more popular features of LFG, and some issues we will consider below are problematic for it. Furthermore, Andrews (2008b) motivates a constraint to the effect that every layer of structure (at least those that lead to substructures with PRED-features) must have a PRED-feature. This suggests that we discriminate the QC NPs with different values of a PRED-feature, for which the case-name in quotes would seem a

reasonable choice of value, and then produce the inner layer as value of some single GF such as OBL. (29) will then be revised to:

(30)
$$\begin{bmatrix} \text{PRED} & \text{'Líka'} \\ \text{SUBJ} & \begin{bmatrix} \text{PRED} & \text{DAT} \\ \text{OBL} & [\ ] \end{bmatrix} \\ \text{OBJ} & [\ ] \end{bmatrix}$$



Observe that we are *not* using the lexical entry to specify the CASE-value of the inner layer, because doing this would be inconsistent with the explanation we will eventually propose for why Icelandic learners adopt the two-layer hypothesis. We will discuss where the CASE-values come from below.

The problem we're concerned with now is that in certain environments, the SUBJ-value of a clause can be the unpronounced pronoun PRO. Fortunately, in the present 'generative glue' framework, PRO can have an SLE without a morphological lexicon entry. As a preliminary sketch theory of how this works, we can suppose that this has gender and number values that are fixed by some kind of antecedent relation that also fixes the meaning; we can symbolize it like this:

(31)
$$\begin{bmatrix} \text{PRED} & \text{'PRO'} \\ \text{GEND} & \alpha \\ \text{NUM} & \beta \end{bmatrix} \quad \Leftrightarrow \quad \mathit{PRO} : e$$

For a more thorough formulation using the 'tensor'-based analysis of anaphora presented in Asudeh (2004), see the appendix.

Our semantics-driven assembly will then be able to produce f-structures like this (meaning omitted), even if there's nothing overt to build the complex two-layer structure of the subject:

(32)
$$\begin{bmatrix} \text{SUBJ} & \begin{bmatrix} \text{PRED} & \text{'Dat'} \\ \text{OBL} & \begin{bmatrix} \text{PRED} & \text{'PRO'} \\ \text{GEND} & \alpha \\ \text{NUM} & \beta \end{bmatrix} \end{bmatrix} \\ \text{PRED} & \text{"Líka'} \\ \text{OBJ} & \begin{bmatrix} \ldots \end{bmatrix} \end{bmatrix}$$

Except for the problem of producing the actual CASE-values, which we will deal with later, this basically manages (16a), but there are a few more things to be said.

One pleasant consequence of this revision of the structure is that the c-structure rules for QC NP can be simplified. All we need to do as add to the regular NP rule this additional expansion:

(33) $\text{NP} \rightarrow \underset{(\uparrow \text{OBL}) = \downarrow}{\text{NP}}$

We don't need the extra bar level found in A1982, since the semantic f-structure generation will prevent (33) from effectively recursing, and we especially don't need the unpopular $(\uparrow(\downarrow \text{CASE})) = \downarrow$ annotation. The proposals about CASE to be made later will allow this rule to be used for semantically case-marked NPs, including, modulo issues of XBAR-level, in Warlpiri as analysed by Simpson (1991).

Another issue to discuss is that QC PRO, although apparently grammatical, is of somewhat degraded acceptability. Something worth pointing out here is the contrast between Icelandic, where QC PRO has been considered essentially grammatical since the mid 1970s, and German, where it has been generally regarded as quite ungrammatical, at least until, quite recently, Barðdal and Eyþorsson (2005, 2008) used computers and the web to find a reasonable number of examples. The appearance of these examples would seem to indicate, as Barðdal and Eyþorsson claim, that QC subjects are a 'common Germanic inheritance', but there still a striking difference in status: about 25 years elapsed during which any one of presumably thousands of German students of linguistics could have built up some reputation by showing that QC subjects existed in German as well as Icelandic, but none of them did it: it took a pair of Icelanders processing huge amounts of text with computers to find the examples. What I would suggest causes the difference is that, as discussed in Andrews (2001), the basic word order facts of Icelandic make QC subject analyses compulsory for young learners of Icelandic, but those of German make such analyses only available, not required for young learners of German.

But we still have the matter of degraded acceptability to deal with, and I think it is reasonable to suggest an answer along the following lines: producing a QC PRO requires producing some f-structure in the semantic part of the derivation that is not matched by anything produced in the syntax. In general, as discussed at the end of the previous section, this must be possible, but it also appears to be limited in various ways. One of the possibilities would appear to be that it can be somewhat questionable, without being truly ungrammatical.

A further consideration is that these examples seem to get even more questionable when something agrees with the PRO: this suggests a principle of Hypocrisy whereby, when something somewhat dodgy is happening, it's worse when there's more overt evidence that it really is happening.

3.2. Suppressing Feature Dumping

The next problem we'll consider is how the proposal suppresses the random dumping of feature-values by agreement markers. The basic reason is that so-dumped features will not normally be able to get a semantic interpretation.

To show this for NUM(ber), for example, we need to push glue analysis a bit further than we have taken it so far, into certain aspects of NP structure. Glue has so far accepted the standard Formal Semantics idea that noun, like adjectives and intransitive verbs, are of type $e \rightarrow p$. As discussed in some detail in Dalrymple (2001) and Andrews (2008a, 2010),[17] Common Nouns can be analysed as taking an e-type argument located at their f-structure correspondent, and returning a p-type result there. Quantifiers, etc, then pick put up the 'property' so-produced, and integrate it with the rest of the sentence-meaning.

On such an approach, the obvious thing to do with plurals is to follow Link (1998) and treat them as operators that convert a property $e \rightarrow p$ of singular count entities into a property of type $e \rightarrow p$ of 'collective' entity. I.e. *boy* is a predicate that is true or false of individual

[17] There are some technical differences between Dalrymple and Andrews, but they don't matter here.

entities, *boys* of certain kinds of collections of entities (i-sums, in Link's terminology). SLE's for a plural feature and a noun would then look more or less like this:

(34) a. $\left[\text{NUM} \quad \text{PL}\right] \quad \Leftrightarrow \quad Pl : (e{\rightarrow}p){\rightarrow}e{\rightarrow}p$

b. $\left[\text{PRED} \quad \text{`Boy'}\right] \quad \Leftrightarrow \quad Pl : e{\rightarrow}p$

(Since all the semantics-f-structure links go to the same place, they are omitted here.) SLEs like these can happily assemble to produce results like this (see the appendix for details on how this kind of assembly works):

(35) $\begin{bmatrix}\text{PRED} & \text{`Boy'}\\ \text{NUM} & \text{PL}\end{bmatrix} \qquad PL(Boy) : e{\rightarrow}p$

But assembly won't produce results where the PL-values are dumped into the upper-levels of QC NPs, since in such places, their meanings can't integrate properly with anything else.

And, unless something special happens, principle (28b) will therefore block agreement from dumping features into these positions, since such features won't be matched by the semantically generated structure.

We can tell a similar story for gender, but with the difference is that gender is normally 'co-introduced' with the PRED-value (especially in Icelandic, where the connection between grammatical gender and real world sex appears to be unusually weak for a European language), by SLEs that look more or less like this:

(36) $\begin{bmatrix}\text{PRED} & \text{`Strákur'}\\ \text{GEND} & \text{MASC}\end{bmatrix} \quad \Leftrightarrow \quad Boy : e{\rightarrow}p$

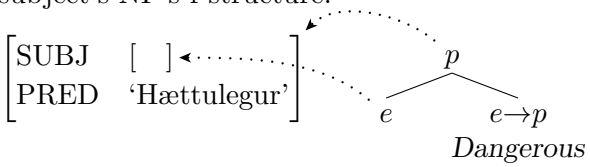
As with number, a randomly dumped gender feature won't wind up matching anything produced by the semantics, under normal circumstances, and so will be blocked by (28b).

There is of course one kind of feature-value for which this kind of story won't work at all, namely, structural case, since this is a kind of feature which appears to have no semantic interpretation, but rather appears to be a relatively pure reflection of the syntax. We clearly have to say something about case before we can push on to our attempt to explain why two-level structures are required, so we do this in the next section.

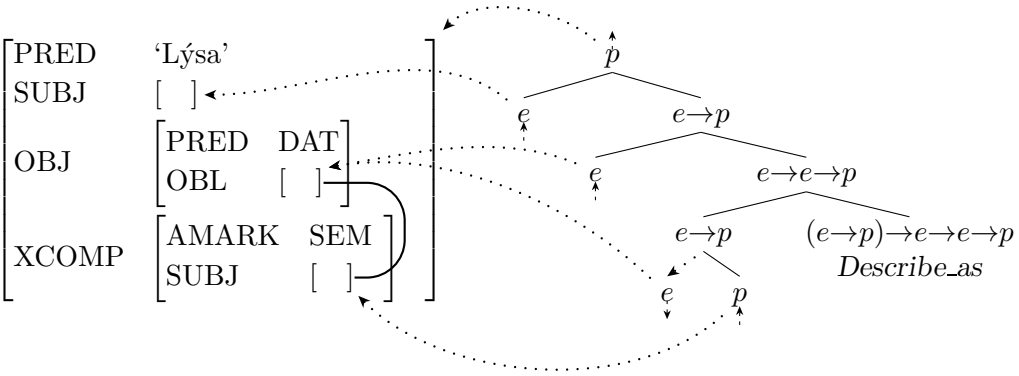
Another general issue is that we've suppressed this instance where a distinction between constraining and defining equations needs to be stipulated, but how general will this effect be? It might not be such a bargain if we made such a big change in the architecture to get only a bit less stipulation in some parts of some grammars. One can't be sure before working through a comprehensive range of analyses, but I think it's likely that *all* cases where a constraining vs. defining distinction needs to be made for agreement are eliminated by the present architectural change. This is basically because the agreement equations that need to be specified as constraining typically require this specification because otherwise they will dump features into locations where they don't make sense semantically, a concept which has no real status under the classic LFG architecture, but does under this one.

3.3. Secondary Predicates, etc.

So what now about secondary predicates and QC-controlled complements? This is quite straightforward, given reasonable analyses of the surrounding phenomena. Since Andrews (1990a) already proposes an explicit analysis of the QC-controlled complements of (5c,d), we'll discuss them first. The complements with which these constructions occur are ordinary NPs, taking non-QC subjects, whose SLEs would therefore take input form the upper layer of a subject's NP's f-structure:

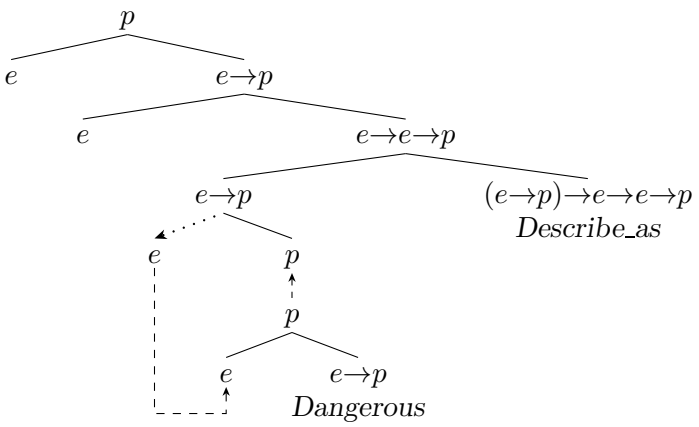
(37)


A verb such as *lýsa sem* on the other hand would probably best be treated as involving functional control by a theta-role bearer, as analysed by Asudeh (2005)"

(38)


This constructor involves a semantic configuration we haven't seen before, a 'complex argument' of type e→p. The expansion of such arguments is discussed in the appendix, note that the left daughter has an output arrow and a dotted line connecting to its mother, the right daughter an input arrow; due to the extra complexity here, the input and output arrows are all included. The QC objects inner layer and the complement subject are equated by functional control, so it doesn't matter which of the two the associated *e*'s in the semantic structure get connected to. There are some surprises in the treatment of this kind of structure, discussed in Andrews (2007b), but they're not relevant here.

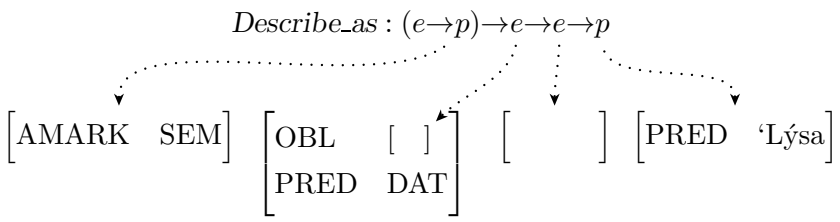
To assemble (38) and (37), we need to feed the subordinate output *e* of (38) into the input *e* of (37), and the output *p* of the latter into the input *p* of (38), producing this:

(39)


The constraints on this kind of assembly are given in the appendix; here we need merely note that if the complement subject isn't equated with the inner layer of the matrix QC dative object, then the assembly can't go through, and the structure can't be interpreted. And it should also be evident that if the complement adjective itself requires a two-layer subject, then no workable assembly can be produced with the SLEs at hand.

A further point is that in a project whose goal is to reduce stipulation, (39) looks like the kind of thing we would like to see less of; fortunately, the LFG literature suggests a variety of principles whereby (39) might be produced from a more reduced form of specification, such as perhaps this:

(40)



Andrews (2007b) discusses issues relevant to expanding something like (40) to a full SLE, drawing heavily on LFG argument-structure literature such as Alsina (1996).

The remaining cases are floating quantifiers and circumstantial predicates, which I won't develop explicitly here, but refer to analysis in Andrews (2007b) for floating quantifiers (in Catalan), and Haug (2008) for XADJUNCT secondary predicates. Neither of these analyses deal with Icelandic, but both would require that the agreeing elements get access to inner layer of a two-layer in order for interpretation to go through.

4 Case

Given the extent to which analysis of languages with rich case-marking systems have provided motivation for LFG, it is surprising how difficult it has been to come up with a stable and widely agreed on account of how structural case works (compare for example the treatments of Icelandic (Andrews, 1982), Russian (Neidle, 1982), Malayalam (Mohanan, 1982) and Warlpiri (Simpson, 1991)). In the first subsection we will propose and account of structure case, and in the second consider Quirky and Semantic case.

4.1. Structural Case

It seems to me that overall, the least problematic account of structural case is that provided by Nordlinger (1998), but even this has some substantial problems. The main one being that, given how Nordlinger's proposal works, case features are not conceptually necessary, but apparently present anyway.

This is because Nordlinger uses standard LFG architecture, supplemented by the idea of 'inside-out functional uncertainty' (iofu), whereby grammatical items can carry constraints on things found on paths leading up from their f-structure correspondent's location (inside-out), rather than only down (standard outside-in). So for example an ergative case form can carry the specification ((SUBJ ↑) OBJ), which is supposed to mean '↑ (my f-structure location) is the value of SUBJ in a structure that also has an OBJ-value'.

The problem is that in standard LFG architecture, there's no reason at all why the lexical items of morphological forms shouldn't just introduce the equations without the CASE-values which they are traditionally supposed (with some evidence offered by Nordlinger) to be

associated with. For example, why shouldn't an ergatively case-marked nominal just carry the specification (a), rather than (a) together with the CASE-setting equation (b):

(41) a. ((SUBJ ↑) OBJ)

b. (↑CASE) = ERG

One possibility that we need to consider is that CASE-features really are unnecessary, and should be dropped.

There are various reasons why this would probably not be a good idea. One is the kinds of interactions that sometimes occur between case and agreement. In Hindi, for example, verbs can only agree with a 'nominative' NP that arguably just lacks any case-value (preferably with the subject unless this is ergative, in which case the object is the next choice, unless this is accusative, in which case, no agreement). It is hard to see why the presence or absence of an iofu annotation like (41a), on one item, would affect the agreement possibilities for another item. But presence of a CASE-value, as either an inhibitor (Hindi) or facilitator (Maithili, as discussed in Butt and Sadler (2003)) is straightforward (to describe; the typology of such effects seems to be extremely variable, so that there is perhaps no real scope for explanation).

A more subtle indication can be gotten from the well-known fact that the usage of the traditional case forms in languages is quite varied, in spite of the tendency to re-use names such as 'nominative', 'ergative', 'dative', etc. across different languages. Furthermore, single traditional case-forms, such as 'accusative', tend to have multiple traditionally recognized uses. So Icelandic accusative forms can be structural, quirky, or to a limited extent, semantic (see Sigurðsson (2003) for a very extensive survey of uses of Icelandic case forms). Suppose there were no CASE-features, but only statements similar to (41a) directly connecting the morphological forms to their conditions of use.[18] We then might expect to find different parts of speech, such as Noun, Adjective and Determiner, making radically different choices as to what kinds of distinctions their case-forms marked, since they would require different iofu paths (those for adjectives starting with (ADJUNCTS $\in$↑), for example), and so might perfectly well specify different circumstances overall. But this isn't what normally happens: if agreement happens at all, one typically finds either the full inventory of distinctions, or a substantial reduction (along the lines of 'direct' vs. 'oblique'), but not something just different. What we find is just what we expect if agreement is marking or recapitulation of the value of a feature such as CASE, with a limited range of values.

We can implement this idea by allowing certain attributes, such as in particular CASE, to not require (or perhaps even allow) semantic interpretation, as long as they are 'licensed' by a separate mechanism, for which Nordlinger's iofu constraints would appear to be an appropriate formulation. We might for example have a constraint like this for ergative case:

(42) $\left[\text{CASE} \quad \text{ERG}\right]$: ((SUBJ ↑) OBJ)

This is supposed to apply to the results of the semantically driven f-structure production, and tack on the specified CASE-values whenever the stated conditions are met, '↑' designating here the f-substructure matching the material in brackets on the left of the ':'.

Mathematically, this is a 'Schönfinkel-Bernays' conditional as described by Johnson (1991), which has a somewhat degrading effect on computational complexity, but doesn't render LFG-type grammars undecidable (because the rules merely tack on additional structures chosen

[18] A possible interpretation of the idea of Sigurðsson (2008) that case is 'morphological' rather than 'syntactic', although I don't really know if this is what he intends.

from a finite number of possibilities onto a predetermined and finite number of locations, without producing the possibility of infinitely iterative application). With the case-feature so added, we can use either the Morphological Blocking principle of Andrews (1990b) or one of its more recent relatives (e.g. Blevins (2000)) to require the case-marked forms to be used when a CASE-value has been deposited into the semantically generated f-structure.

So now we can proceed to produce some case-marking rules, with the first obvious choice being the structural accusative case-marking rule, for which a reasonable-looking preliminary formulation might be:

(43) $\left[\text{CASE} \quad \text{ACC}\right] \;:\; ((\text{OBJ/OBJ2}\uparrow)\,\text{SUBJ PRED})$

This characterizes the Icelandic structural accusative as an 'anti-ergative' which is attached to an OBJ or OBJ2-value when the SUBJ-value has a PRED. This would work reasonably well with the 1982/90 version of the two-layer analysis, but needs modification for the present one, where the upper layer of a QC NP also has a PRED-value (although, arguably, not really a normal one).

We want something like this to explain the active-passive relationship of ditransitive verbs with dative Recipient and accusative Theme:

(44) a. Þeir gáfu honum peningana
They(N) gave him(D.SG) the money(A.PL)

b. Honum voru gefnir peningarnir
him(D) were(PL) given(N.PL) the money(N.PL)

c. Peningarnir voru gefnir honum
the money(N.PL) were(PL) given(N.PL) him(D)

As standardly discussed in the literature, either the (dative) Recipient or the Theme can appear in subject position in the passive; whichever does, the Theme becomes nominative.

To get the analysis to work in the present framework, we need some property that distinguishes outer layers of QC NP (and 'empty' expletive pronouns, such *það*) from 'interpreted' NPs, and in the present framework, the most obvious such property is that of being a correspondent of the semantic tree-to-f-structure relation that is set up by the SLEs, which is called ϵ in Andrews (2008a). This would appear to be a workable reformulation:

(45) $\left[\text{CASE} \quad \text{ACC}\right] \;:\; ((\text{OBJ/OBJ2}\uparrow)\,\text{SUBJ}\ \epsilon^{-1})$

A further issue is whether this rule applies to all values of OBJ/OBJ2, or only to those that are in the image of ϵ.

Andrews (1982) thought that the rule should only apply to 'interpreted' f-structures on the basis of his account of the agreement in examples like (44b), which was that the gender and number features of the (second) object would be copied onto the outer layer of the dative QC subject, where ordinary finite verb and passive participle agreement would pick them up. But if this outer layer could also pick up accusative case, then we'd expect to see it somewhere when these constructions are embedded as 'functional control by object'/ACI constructions. But mostly, we don't, as long as the Theme stays after the complement verb:

(46) a. Þeir segja honum hafa verið gefnir hestarnir
They say him(D) to have been given(N.PL) the horses(N.PL)
They say that he was given the horses

b. Þeir segja hestana hafa verið gefna honum
They say the horses(A.PL) to have been given(A.PL) him(D)
They say that he was given the horses

However, the accusative case on the horses and the items that agree with it in examples like (46a) seems to have some degree of acceptability for a small number of speakers. Furthermore, Sigurðsson and Holmberg (2008) have done substantial work on the 'agreement with nominative object' phenomena, which is quite complex, so it seems like a good idea to not try to pursue the matter here, and to leave for the future the question of further restrictions on (45).[19]

So what, then, about QC NPs? We have alluded to principles requiring their outer layer to have a PRED-value, but, for agreement, etc. to work out, we want their inner layer to wind up having a CASE-attribute that interfaces with the morphology in the same general manner as the structural accusative case, and in *exactly* the same manner in the case of the Quirky Accusative, where all the morphological forms are exactly the same. What I suggest as a notationally 'clever' rule like this:

(47) $\left[\text{CASE} \quad \alpha\right] \;:\; ((\text{OBL}\uparrow)\,\text{PRED}) = \alpha$
for $\alpha \in \{\text{ACC}, \text{GEN}, \text{DAT}\}$

This supposes that the PRED-values in the upper level are the same as the CASE-values in the lower level. Because it is merely adding additional links for already extant feature-values, it too is Schönfinkel-Bernays.

One potential problem with this is that PRED-values are supposed to be uniquely indexed, so that they don't merge (in the production of the syntactically determined f-structure). But such merger is clearly necessary for CASE-values, to manage agreement (especially if we're not going to make use of the notion of 'constraining specification'). However, Andrews (1990b) suggests that this should be something that can be overridden for certain specified PRED-values, such as, for example, our inventory of CASE-values.

A point worth noting is that although the things generally recognized as CASE-values often do merge, there are languages such as Gooniyandi (McGregor, 1990) where it can be strongly argued that they don't. Furthermore, if LFG grammars are taken to include declarations of what the possible values of grammatical features other than PRED are, then those declarations would obviate the need for the '*for*' restriction in (47).[20]

A further question is how semantic case-marking fits in. This topic is better left for the next section, where we consider the question of what might force Icelandic learners to the two-layer structure that is needed to deliver our results.

[19] Phenomena involving non-object-promoting passives discussed in Sigurðsson (1991, p. 347) seem especially relevant.

[20] It is noteworthy that LFG analyses in the literature never use feature declarations, while all serious computational grammars using the XLE system do.

4.2. Forcing the Two-Layer Structure

A preliminary observation is that the two-layer structure, although perhaps exotic, is not unique to Icelandic, because the morphological form and agreement behavior of locative NPs in certain Bantu languages motivates such a structure rather strongly (e.g. Bresnan and Mchombo (1995) for Chichewa). In these languages, there clearly appears to be an inner layer, containing the regular noun-class (gender and number) features, along with an outer layer, reflected in external agreement, expressing various locative relationships (example (50a), p. 209):

(48) Pa-mu-dzi w-áthú p-ó-chítítsa chĭdwi pá-ma-sangaláts-á alĕndo
16-3-village 3-us 16-NONFIN-attract interest 16-PRS HAB-please-IND 1-visitor
Our interesting village pleases visitors

But in Icelandic, there is no overt marking, and the putative outer layer is semantically empty and inert for agreement, with the possible exception of the dative-nominative verbs (where the features aren't semantically interpreted in that location, but might be involved in agreement, as discussed in connection with example (45)).

So what is supposed to drive language-learners to the two-layer analysis of Icelandic? One possibility is that in fact, nothing does, and the agreement inhibitions that the two-level hypothesis accounts for are essentially learned stipulations. From the point of generative linguistics, is it quite unfortunate that the Norse colonies, in Greenland, Vineland etc. didn't prosper, and thereby seed the world with Norse Varieties that would demonstrate the ways in which the Icelandic QC system might develop: as it happens, the only Norse Variety which has preserved a reasonable amount of case and agreement phenomena is Faroese (Thráinsson et al., 2004), which on the whole seems quite similar (some baleful influence seems to have destroyed the case and agreement systems of continental North Germanic).

So we don't really know whether, in principle, there could be languages with case-marking and agreement on the whole quite similar to Icelandic, but where QC subjects triggered agreement in a normal way without losing their Quirky Case. But I think there are some indications that this wouldn't have happened. Although no languages other than Icelandic and Faroese now seem to have the QC phenomenon in its full glory, more reduced versions of it are in fact quite widespread in the Indo-European family. It is for example quite common for semantically transitive and ditransitive verbs to take objects in various combinations of cases other than just the accusative. In Ancient Greek, for example, there are reasonable numbers of verbs taking dative and genitive objects, in various combinations, e.g. *phthoneo:* 'envy', with dative (or accusative) of the person envied, genitive for the object of envy:

(49) Me: moi phthone:se:is tou mathe:matos
Not me(DAT) begrudge.SUBJCT.2sg the(GEN) knowledge(GEN)
Do not begrudge me the knowledge (Plato *Euthyd.* 207 b, cited in Goodwin and Gulick (1930, p. 237)).

But it appears to be a strong regularity of these languages that verbs never agree with these 'irregularly' case-marked NPs. Under passive, for example, either the irregular case is retained, and no agreement happens, or the case is stripped in favor of the nominative, and case-marking proceeds normally (see Feldman 1978 for Greek; the case-conversion indicates that the dative or genitive putative objects really are objects, since they are subject to the effects of Passive). The case gets significantly stronger if we accept Barðdal and Eyþorsson's results on various Germanic languages, where some kind of case can be made that some of

the QC NP's are really subjects (whereas, in Greek or Latin, we only have various kinds of 'impersonal' sentences which might perfectly well not have any full NP as a SUBJ-value).

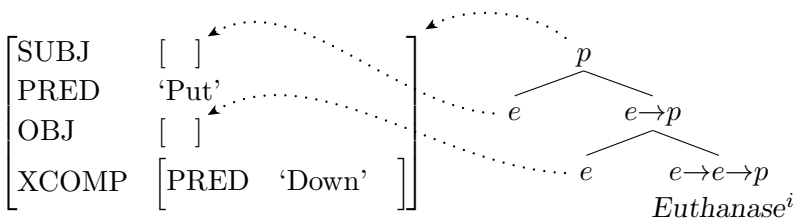
I therefore hypothesize that there is something about these irregularly case-marked NPs that has a principled effect of inhibiting agreement. This is a striking contrast with what we find, for example, in Warlpiri, where subject NPs can be ergative or absolutive, and objects NPs absolutive or dative, with minimal or zero effects on agreement (Hale 1973, Simpson 1991, Legate 2008). What could cause such a difference?

I will suggest that it is the possibility of more than one lexically determined case appearing on a single GF (that is, more than two possible case-markings for a single GF), especially in combination with others. In Warlpiri it is a rather striking regularity that subjects can be either absolutive or ergative, and objects absolutive or dative, with no further possibilities. This could be plausibly accommodated by one clause-level feature triggering ergative on the subject (and not quite only when the verb is transitive), and other triggering dative on the object (usually but not quite always when the subject is absolutive). This seems reasonable, especially if independently motivated properties of argument-structure could be used to trigger the case-marking variations, as seems conceivable (but not really worked out adequately, it seems to me, at least in LFG.[21])

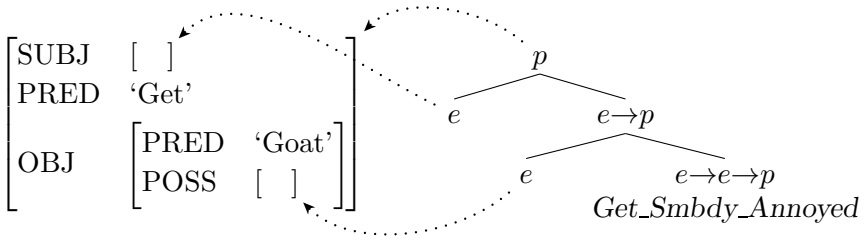
But in Greek, Icelandic, etc. we find a different situation, where for example objects can be accusative, dative or genitive, so that more than one binary feature is needed to determine the possible cases of a single GF. And for Icelandic subjects, it's even worse, since there are four possibilities. And furthermore, there are various combinations of irregular case to account for, so the total number of features required would become rather large. I suggest that this is the situation that triggers the two-level analysis, so now all we need is a mechanism which will cause this to happen.

A constraint that can be suggested on the basis of the treatment of idioms in Andrews (2008b) provide a possible answer. The basic idea that is presented there is that at least some idioms are a rather extreme case of the treatment we proposed for grammatical gender (and *pluralia tantum*) whereby f-structural attributes scattered across various levels of the f-structure are collectively taken by an SLE to be connected to a single basic meaning. Here for example are proposed SLEs for the idioms *put down*, in the sense of 'euthanase', and *get X's goat*, in the sense of 'annoy, make angry':

(50) a.



b.

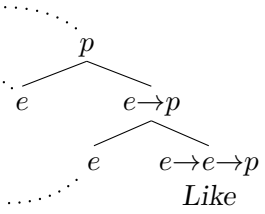


[21] But note the rather complex analysis of Bittner and Hale (1996).

Although one can say some rather interesting things about the role of metaphor in the interpretation of idioms (Egan, 2008), many of these expressions really do seem to depend on certain word-forms being dragged into certain configurations, in ways which are hard to explain without given them a certain amount of grammatical structure.

So what are the options, in an approach like this, for having an SLE specify a particular case on a given grammatical relation? The obvious thing to do is to have the SLE specify some particular case for some GF of the verb in question, for example a modification such as this for the f-structural portion of the entry we proposed for *Líka* in (30):

(51)
$$\begin{bmatrix} \text{PRED} & \text{'Líka'} \\ \text{SUBJ} & \begin{bmatrix} \text{CASE} & \text{DAT} \end{bmatrix} \\ \text{OBJ} & [\ ] \end{bmatrix}$$



This is basically what Simpson (1991) proposes for 'syntactic' (structural) case in Warlpiri, and what, as far as I can tell, almost everybody who tries to analyse QC in Icelandic in LFG thinks they ought to do (and in other frameworks, as well, to the extent that they produce explicit proposals at all).

In Warlpiri, it will work, because the case-features don't appear to disrupt agreement very much, but in Icelandic it doesn't, due to the phenomena discussed in section 1. So what I propose is that there's some kind of principle that rules out (51) as an SLE (although there's no problem with CASE-values winding up distributed as indicated in the SLE, as long as something other than an SLE is doing the work).

A first pass at such a constraint is:

(52) An SLE can't connect a semantic argument position to an f-substructure for which it specifies the value of an attribute.

This constraint will for example rule out SLEs whereby an idiom could specify the grammatical number of an argument, but leave other properties of the argument free to vary:

(53) John galked the dogs
meaning: 'John walked the dog'

These seems like an extremely unlikely kind of idiom, so the constraint would appear to have some plausibility. The absence of verbs in IE languages imposing 'quirky gender' on their arguments is further evidence for this constraint.

On the other hand, verbs taking 'control complements' of type $e \rightarrow p$ often seem to specify 'verbal form; features for these complements on a fairly arbitrary basis. But the forms of complements of type p seem to be considerably more tied to the semantics. So I'll suggest that (52) only applies to basic types, not functional ones.

Given (53), SLEs such as (51) are excluded, and we need a two-layer structure, which the principles of Andrews (2008b) will require to have a PRED in the outer layer. Now the fact that QC is being spelled out in the same manner as structural case indicates that determines a CASE-value in the inner layer, but due to the constraints, the only way that a verbal SLE can influence the morphological form of its argument is by involving something in the outer layer, for which the PRED-value is the most obvious candidate. So if a CASE-marking like (47) is possible, it is plausibly the best solution.

One problem with this is that it involves rather more in the way of 'problem solving' than one would like to see deployed to acquire a grammatical construction. But a simpler way to produce the evident behavior would be needed in order to give real force to this objection.

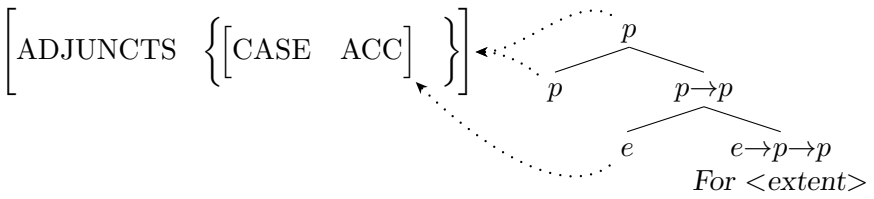
A more straightforwardly empirical problem is with the constraint (52) that makes the solving necessary. This is the way in which animacy appears to function in Algongkian languages: in these languages, transitive verbs select for 'animate' vs. 'inanimate' objects, so that if animacy is represented as a feature, (52) will probably be violated. A possible solution might be to put the animate/inanimate distinction into the type system.

4.3. Semantic Case

A final topic to discuss here is how 'semantic case' might fit in. Not the least of the problems here is that the range of what can be defensibly called 'semantic case' is quite wide, including 'modal case' in languages such as Kayardild (Evans 1995a, 1995b) and Hindi (Mohanan, 1995), marking tense, mood and modal concepts associated with the entire clause, the definiteness and animacy-specific 'accusative' case-marking found on objects in Hindi, Spanish, Turkish, etc, and various other phenomena discussed in Butt and King (2003).

But what people usually seem to mean by 'semantic case' is the only variety of this phenomenon that is found (to only a rather limited extent) in Icelandic, when a case-form is used to essentially convert an *e*-type meaning into some sort of adjunct (locative, temporal, instrumental, proprietive or whatever).

A treatment of these that would be nicely congruent to what we've done for AC would be to use the exact same PS rule and PRED-to-CASE-copying rule that we've proposed for QC, but let the PRED-value instances be standalone f-structural portions of the SLE. Although this seems to work (and produces structures essentially identical to what Simpson (1991) proposes for semantic case in Warlpiri), I unfortunately don't have any principle that would require it, by ruling out an SLE like this, that directly interprets an ACC value of CASE as an temporal adjunct:

(54)


Fortunately, so far at least, I haven't found any reasons for rejecting structures like (54) as wrong, so the relationships between QC and 'adjunct-creating' semantic case is something that can be left for later.

5 Conclusion

In this paper I have been pursuing two basic ideas. One is that the only known way to account for the agreement and certain other properties of QC NPs in Icelandic is to give them a two-layer structure. The other is that this can be best accomplished (within the current range of theoretical ideas) in a modified LFG architecture in which both semantics and overt phrase-structure function as semi-autonomous generative-enumerative levels, whose results must be sufficiently congruent in order to produce a well-formed sentence-structure.

The first idea is the primary one, which might in principle be realizable in various other architectures. Indeed, my original idea about Quirky Case, based on Siegel (1974), was that

QC NP had an underlyingly filled Case position in an outer layer of the X-bar structure, while structurally case-marked NP had an empty one, filled in later by the case-marking rules (Jackendoff's ideas about the preposition *of* were also supposed to play a role). But it was LFG that actually provided an implementation mechanism whereby the filled (and now extra) layer managed to exert the desired effects on agreement, etc, and the co-generative glue version of LFG that I'm now suggesting resolves a number of problems with the original analysis.

However this effect would be effectively neutralized if the architectural change disrupted the existing body of LFG analyses to an extreme extent, but this seems unlikely, at least in the presence of conditions requiring a considerable degree of 'Recoverability of Omissions' (i.e. limitations on MAX-violations). The effect of letting glue be co-generative, and allowing the syntactically generated f-structure to fail to match all of the semantically generated one, is to make it unnecessary to include in the PS rules and lexical entries a certain amount of clumsy additions to express things that are arguably there in the abstract structure but not explicitly marked by anything. This appears to permit some streamlining of analysis, and improvement of some of their explanatory properties, I believe, but not to motivate completely redoing everything.

Appendix

A The Glue Framework

In this section I will fill in some aspects of the glue framework that are not covered in the text. Although everything will in a sense be here, the presentation will at times be a bit brisk, so that readers without a basic familiarity with glue might want to consult more relaxed presentations such as Andrews (2010, 2009), as well as Asudeh (2004) for a very different kind of presentation.[22] Some other aspects of this approach are discussed in Andrews (2008a).

The first thing we need to do is give the rules for expanding a semantic type into a 'prefab' piece of structure. To describe this relationship with full generality, we need a notion of 'polarity', with two values, positive and negative. The polarity concept is derived from the interpretation of the semantic types as premises in a logical deduction, where the assumptions are negative, the conclusion positive. The result is that the entire semantic types are negative, with polarities then being assigned to subformulas by rules included in (55) below.

We start with a typed meaning, of negative polarity, with a type-label consisting of the semantic type, and then build a structure (in the simplest cases, a tree, but not a tree in general) whose nodes have polarities and semantic type labels derived as follows:

(55) a. Starter: the starter node has negative polarity, its semantic type label is the semantic type of the typed meaning. It also has a semantic label, which is the meaning of the typed meaning.

b. Atom: if the current (initially, starter) node has atomic semantic type, the construction is finished.

c. Negative Implication: if the current node has negative polarity and semantic type $a \rightarrow b$, make it the right-daughter of new node of negative polarity and semantic type b, with a new left-daughter with positive polarity and semantic type a. Then repeat (b-c) with both the new mother and the new daughter as current (neither new node has

[22] I suspect that glue is one of those things, which, if you don't understand it in at least two rather different-looking ways, you actually don't really understand it at all.

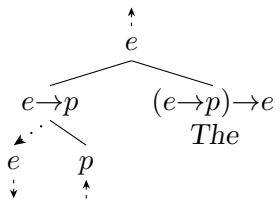
a meaning label, although there is a way to assign meaning-labels to all nodes of fully assembled structures). This produces substructures representing function-application.

d. Positive Implication: if the current node has positive polarity and semantic type $a \rightarrow b$, make it the mother of a new 'left pseudo-daughter' of negative polarity and semantic type a, and a new right-daughter with positive polarity and semantic type b. This creates a structure representing lambda-abstraction.

A notational point is that in linear logic, the symbol $\multimap$ is often but not always used instead of $\rightarrow$ for implication.

A result of the application of these rules is that a possible meaning-constructor for a definiteness (or generic) operator of type $(e \rightarrow p) \rightarrow e$ would be as in (56), where we've represented the polarities of atom-labelled nodes with in- and out-bound arrows (positive and negative, respectively, and left the others implicit. The left pseudo-daughter is connected to its mother by a dotted arrow, oriented downward:

(56)



To effect assembly, we then plug inputs into outputs in accordance with the rules of (22), forming what are called 'axiom links' (represented by dashed arrows in the diagrams), except that, once the positive implication (abstraction) nodes are introduced, we need to revise Correctness. The revision requires a notion of 'dynamic graph' (due to Lamarche (1994) via de Groote (1999)), which can be defined as follows:

(57) The dynamic graph consists of the axiom-links, oriented in the direction of their arrows, and the solid mother-daughter links (not the left-pseudo-daughter ones), oriented from daughter to mother (upward in the visual presentation).

Then we revise (22e), and add an additional condition:

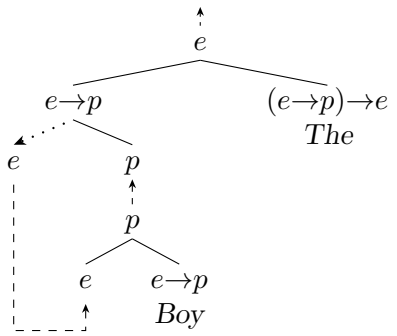
(22) e'. The dynamic graph must be connected.

f. From every target of a left pseudo-daughter link, there is a dynamic graph path to the source of that link.

This is one of many formulations of the so-called 'Correctness Criterion' of Proof-net theory. Andrews (2008a, 2009) discuss how the present notation is a modification of the standard notation for proof-nets (hopefully more intuitive for some linguists, but devoid of mathematical import), with the latter providing substantial material on the relationships between deductive and proof-net based formulations. Moot (2002) provides a more general linguistically oriented introduction to proof-nets.

Given the rules, we can combine structures for *The* and *Boy* into an expression of type e, like this:

(58)



It should be somewhat evident from this how the positive implications function to represent lambda-abstraction (but of one and only one variable, making these lambdas 'linear'). More complex assemblies involving plurals, adjectives etc. can be produced in a similar manner (see Andrews (2010) for some examples involving various kinds of adjectives).

This deals with the basic glue system, but we need one more thing to deal with most popular analyses of anaphora and related phenomena. This is 'tensors', extensively discussed in Asudeh (2004) (and various more recent work). The problem can be described this way. Consider a sentence such as *John loves his mother*. In some sense we want *his* to 'get its meaning' from its antecedent *John*. But the one-to-one connection principles enforced by linear logic mean that we cannot as it were just run a link from *John* to *his* and expect that things will work, because if we use the subject meaning at the possessor-of-object location, it will be gone from the subject location and not available to do its job there.

The answer is an additional type-constructor called 'tensor' in linear logic, and usually symbolized as '$\otimes$', but sometimes also symbolized as '$*$' (presumably because this what is used to symbolize the 'fusion' connective in relevant logic, which can be regarded as really being the same thing as tensor, from the point of view of the logical rules applying specifically to it). Explaining what tensoring really is and why it is called that would take us rather far afield,[23] so I'll just present the rules and a bit of motivation. A formula $a \otimes b$ represents a and b taken together, in such a way that they can both be used (in different parts of your structure), but not in such a way that either can be erased, dropped or forgotten. The result is that a formula of the form $a \rightarrow b \otimes c$ means 'if you give me an a, I can give you a b and a c, but you really have to keep and use both'.

The result is that $e \rightarrow e \otimes e$ (tensor bonding more tightly than implication) is the type of something that can solve our 'resource deficit' problem with anaphora: we can take something from the antecedent position, put something back there, and also put something into the pronoun position.

To work this into our system, we need new polarity and proof-net rules. For glue, the only polarity we need is negative, and the rule is that the components of a negative tensor are negative. The effect is that if the entire $e \rightarrow e \otimes e$ is negative, so will be the rightmost two e's, so, in the linkups, each of them will have an axiom-link coming out of them. We also turn out to have to make the Correctness Criterion a bit more elaborate, forbidding cycles in the dynamic graph (de Groote, 1999), which can be engineered into (22e) by stating that the dynamic graph must form a tree, with the links oriented towards the unconnected (Final Output) node.

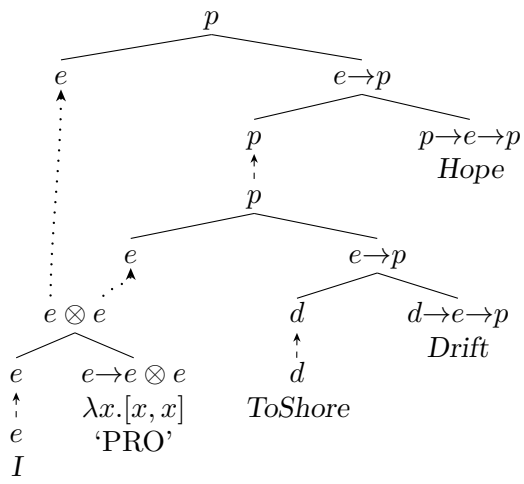
So the general form of meaning-assembly we're looking at for something such as:

[23] People who would enjoy a scenic tour through seriously nonlinguistic territory might have a look at Baez and Stay (2008).

(59) Ég vonast til að reka á land
I hope towards to drift to shore
I hope to drift to shore

will be as below, where I've introduced a new type d for 'direction':

(60)

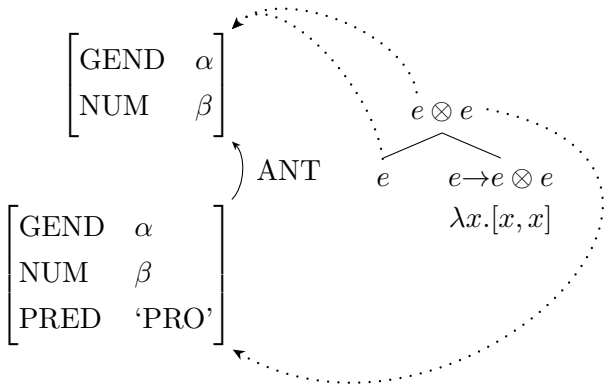


The interesting item is the one labelled 'PRO', which takes input from I (representing the matrix subject/controller of PRO, ignoring all questions about how the first person is really interpreted), and providing type e content to both *Drift* and *Hope*. This corresponds to the idea that PRO takes the matrix subject as its antecedent, and occupies complement subject position.

The λ-expression labelling PRO says what it does with the content gets from the matrix subject position, namely, make two copies of it, one going to each component of the tensor. This is discussed in great detail by Asudeh (2004), and, in terms of the present diagramming system, by Andrews (2009).

What we want to do next is indicate how this is connected to f-structure. One idea is that the 'PRO' element takes an antecedent, which we will follow Dalrymple (1993) in taking to be the value of an ANT(ECEDENT) function. Note that the entry makes no attempt to specify that the PRO must be a SUBJ-value (more on this shortly), and that by specifying a PRED-value of 'PRO', it can, if the rest of the grammar is set up appropriately, prevent (by feature-clash) any overt item from being inserted into the PRO position. On the other hand, no features are placed into the antecedent, only information to the effect that their values should be shared by the PRO (the notation used should be seen as an informal abbreviation for equations).

(61)



The linked gender and number features are not to be construed as interpreted by (61), but also to be interpreted only once, in the antecedent position.

One big unsolved problem here is why PRO is restricted to subject position. We could engineer this into the formulation, but then it would have to be tweaked to allow for the two-layer QC structures, which would tend to contradict what this paper is trying to do.

A more serious consideration is that we would like to predict the tendency of PRO to be restricted to nonfinite subject position, which cannot be done by putting restrictions into an SLE. Rather there are some problems of principle here, which are actually quite difficult, especially when Icelandic is taken into consideration (Sigurðsson, 2008). In fact, the entire issue of the distribution of 'covert NPs' seems quite complicated, with even the basic typology not very well established, so I propose here to leave it as an unsolved problem.

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