Everything's Indefinite

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1 Introduction

There exists in the literature an approach to QP interpretation that we might refer to as the UCLA syntactic school of scope; we find it represented by, among others, Beghelli (1995); Beghelli & Stowell (1997); Szabolcsi (1997); Brody & Szabolcsi (2003). The notion it puts forward is essentially that there is no Quantifier Raising (QR), or any such (more or less) free raising process to assign scope to QPs: rather, particular types of QPs undergo (LF) Spec–head agreement with quantificational functional heads of the same type situated within the structure of the clause. These intra-clausal heads serve to provide scope positions for QPs in their specifiers: the QPs themselves are taken to be interpreted as GQs, if Milsark-strong (Milsark 1977), or closed off, if Milsark-weak (Kamp 1981; Heim 1982, etc.).

A somewhat different implementation of the same intuition also exists; it is championed by Butler (2001, 2004b); Kratzer & Shimoyama (2002); Kratzer (to appear); and also, in a slightly different perspective, by Sportiche (2002, 2005); Hallman (2000). The idea put forward in this version is again that there is no QR, but rather that all QPs function *à la* Heim (1982) indefinites, introducing restricted variables into the structure,[1] which are then closed off by quantificational functional heads situated in the clause. Both approaches assume a closure analysis is appropriate for (at least some) indefinites: the main difference between them is whether a GQ interpretation is also taken to be available for other, non-indefinite, QPs, or not. In that the latter approach says not, and thus provides a uniform approach to QP interpretation, it seems more minimal in spirit. This paper proposes a syntactic formalization of this kind of approach, with a sketch of how the semantics works out, along the lines of Kratzer and Shimoyama's work; in a sense, it is like a syntactic companion paper to their work, which is a semantic formalization with a sketch of how the syntax works out.

[1] Or perhaps choice functions: Reinhart (1997); Winter (1997); Kratzer (1998); etc. etc. for indefinites; Sauerland (1998); von Stechow (2000) for extension to all QPs.

1.1 Implementation

From the point of view of the minimalist framework (Chomsky 1995; *et seq*), a uniform closure analysis seems worth pursuing. Supposing we pursue it, the question is: how do we make it work? Given the wide currency enjoyed by a Heim/Kamp-style closure analysis for indefinites, it is surprising how difficult it is to implement under the more orthodox current minimalist assumptions.

The main barrier to a straightforward implementation of closure in minimalist syntax is Chomsky's (2000) INCLUSIVENESS CONDITION (3), according to (the usual understanding of) which we can't have indices in the grammar (for reasons I return to shortly), and that makes it somewhat harder to have variables, at least as standardly conceived. But if we go after a uniform closure analysis of QPs, we need ensure that the right variables are closed off by the right operators: with variables and indices, that's easy; without them, it isn't immediately obvious how we are supposed to achieve it. This paper proposes that we can do it simply and successfully by reconceiving of variables and indices in terms of features and their values, and of binding in terms of the operation Agree.

2 Indices and inclusiveness

Traditionally, anaphoric and referential dependencies involving (pro)nominal elements have been represented by means of indexation; this is true both for the syntactic and the semantic literature. So in (1–2), coindexing, or the lack of it, indicates the dependencies we take to obtain.

(1) a. i. George$_i$ injured himself$_i$
ii. George$_j$ injured him$_{*j/m}$
iii. George$_i$ said that David injured him$_i$

(2) a. i. Every dog$_j$ has it$_j$s day
ii. All the men$_k$ took off their$_{k/i}$ hats
iii. Someone injured George

b. i. $\forall x_n$: dog(x_i). has(x_i)(x_i's day)
ii. $\forall x_i$: man(x_k). took_off(x_k)(x_k's hat)
iii. $\forall x_i$: man(x_k). took_off(x_k)(x_i's hat)
iv. $\exists x_j$: person(x_j). injured(x_j)(G)

The sentences in (1) illustrate binding-theoretic dependencies, which I don't consider in this paper.[2] However, it is clear that some binding dependencies are licit, whereas others are

[2]For an approach to Binding Theoretic dependencies that is largely compatible with the approach to operator–variable binding presented here, see Hicks (to appear).

ruled out (for example the good versus bad readings of 1a-ii), and indexation has given us a straightforward way of capturing this.

(2) illustrates operator–variable dependencies. In traditional philosophical work on coreference/anaphora, binding and operator dependencies have been treated equivalently; more recently in the linguistics literature they have been dissociated (see e.g. Reinhart 1987; Büring to appear for discussion). However, whatever the exact mechanisms employed for each of the two dependency types, indices are commonly taken to play a role. So in (2a), pronouns are viewed as variables that can be quantified over by the QPs, giving a bound reading; or not, giving an independent reading — see the ambiguity of (2a-ii), captured by distinct indexing. (2b-ii) and (2b-iii) represent the readings semantically, again by means of distinct variables.[3] The map between the syntax and the semantics isn't always as straightforward as in these examples though: compare (2a-iii) and (2b-iv), where the semantic representation makes use of indices the syntax doesn't on the face of it seem to need.[4]

Presenting a few examples where variables and indices are used isn't by any means indefeasible proof that they are and must be an intrinsic part of the grammar, but it does at least remind us that they are useful things, and they have done and continue to do good work for us. The reason for making this somewhat sentimental statement is that in recent minimalist syntax, indices in particular, and so by association variables, standardly conceived, have come under fire from Chomsky's (2000) Inclusiveness Condition (3).

(3) The *Inclusiveness Condition*: No new features are introduced by C_{HL}.
(Chomsky 2000: 113)

C_{HL} is the computational procedure for human language: we may reasonably conceive of it simply as syntax here. What the Inclusiveness Condition says is that all the syntax can do is take whatever it gets from the lexicon, and do stuff with it: what it can't do is add anything extra. On the view that indices are assigned by one element to another in particular structural configurations, syntax is in fact introducing new features, and so the Inclusiveness Condition is violated. Therefore (the argument goes) we can't have indices.

There are of course problems with this. One big one is that the Inclusiveness Condition is entirely conjectural, and as such simply may not hold. If it doesn't, then it doesn't rule out indices nor anything else. A second problem is the assumption that indices aren't present at the point of lexical selection. This too could be wrong: indices could be present at such a point in a number of ways. For example, indices could be distinct lexical items that are merged

[3] It could be objected here that coreference in semantic formulæ is often represented as $x \ldots x$, and non-coreference as $x \ldots y$: i.e. by different variables rather than different indices. The two notations are formally equivalent though: 'x' and 'y' are not in fact themselves variables, but letters of the alphabet that function as indices for variables just as much as any subscript or superscript on a pronoun does (see Fiengo & May 1994 for related discussion).

[4] Though again, see Fiengo & May (1994) for arguments that indexing is necessary in the syntax too here.

with (pro)nominals internally to NP, say. In this case they aren't added by the syntax and the Inclusiveness Condition isn't an issue, whether it in fact obtains or not. As far as I know no-one has proposed such a treatment (probably for good reason), but at least it escapes the argument presented.

Alternatively, indices could be added to lexical items before they are handed over to syntax, either inside the lexicon or at some intermediate point. Whether this would violate the Inclusiveness Condition would depend on the exact conception of C_{HL} — i.e. at what point in the derivation you take it actually to kick in — but certainly some version of this should be formulable that again escapes the standard Inclusiveness argument.

This latter idea is pretty similar to what was assumed in pre-Inclusiveness Condition GB theory. The story then was that indices were added freely to lexical items at the point of selection and then some kind of filter threw the bad indexings out and let the nice ones through, meaning that in principle an infinite number of possible indexings for the sentence would be generated, but only the relevant one used. This kind of generate-and-test model is obviously problematic in its own right, since ideally we would want a grammar that didn't have to do an infinite amount of work before it could give us anything useful, but that just gave us what we needed, as needed, because we needed it. This problem might well apply to whatever version of Inclusiveness-escaping indexing in the above paragraph we could formulate too.

So we have a problem: the common former view of indexation isn't great anyway, requiring infinite possible indexings to get the particular interpretation we need; and if the Inclusiveness Condition holds, it is difficult to see how to reformulate it in a more economical derivational way, so that, say, we have index-assignment via the syntax.

One solution, of course, is just to reject the Inclusiveness Condition; this would be a quite reasonable line to take. An interesting exercise, though, would be to assume Inclusiveness holds, and see if we can get the effects of indexation without violating it. This is what I propose to do in this paper, and what I will end up with is a perfectly licit, non-Inclusiveness violating minimalist system that derives the same kind of effects as indexation in a much cleaner and simpler way.

3 Indefinites

While there are many cases in which variables are used in analysing syntactic/semantic phenomena, the one I will concentrate on here is the well-known and widely discussed treatment of indefinites based on the work of Kamp (1981) and Heim (1982). Kamp and Heim independently proposed that indefinite DPs/QPs[5] like (4a) don't have inherent quantificational force, but are rather interpreted as restricted variables along the lines of (4b), which are then 'closed off' by independent quantificational operators or operations. Kamp proposed that this closure

[5] I use DP and QP interchangeably for what the works cited refer to as NP.

occurs as part of the semantic computation procedure itself; Heim proposed that the closure is to some degree syntactically represented, in that a quantificational operator of some sort must be there in the representation that the semantic computation procedure interprets.

(4) a. $[_{\text{DP}}$ a $[_{\text{NP}}$ hat]]

b. hat(x)

The analysis of variables I will develop here will bear closest resemblance to Heim's theory, so below I give a brief and somewhat simplistic runthrough of how that theory works.[6]

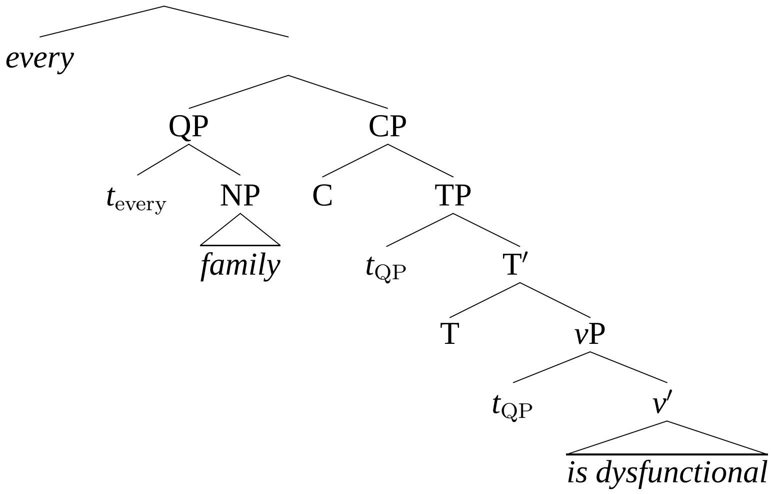
Heim assumes a number of construal processes to obtain the tripartite structures she takes to be necessary for the proper interpretation of quantification. The first process she assumes, she calls NP-PREFIXING, which she takes to apply to all types of QP, and which 'Adjoin[s] every non-pronominal NP [=QP] to S, leaving behind a coindexed empty NP [=QP]' (Heim 1982: 132) — essentially equivalent to standard Quantifier Raising (QR) (May 1977, 1985). But because Heim is assuming uniformly a tripartite structure for quantification, this by itself is insufficient to give us an interpretation: 'Since I ... assume that quantifiers can be interpreted only when they are the immediate leftmost constituents of S ... no NP in an interpretable logical form will ever have a quantifying determiner' (Heim 1982: 134–135) — i.e. we won't ever get a straight generalized quantifier (GQ) interpretation (see Barwise & Cooper 1981 for discussion of GQs) of a QP. Heim therefore takes a second process also to take place, which she labels QUANTIFIER CONSTRUAL: 'Attach every quantifier as a leftmost constituent of S' (Heim 1982: 133). That is, Quantifier Construal takes a quantifier out of its QP, and adjoin it in a c-commanding position. This process, and Heim's justifications for it, aren't much cited in the literature; although it is adopted almost wholesale in Aoun & Li's (1993) reinterpretation of QR, and it is also strongly reflected in many current minimalist approaches to QR where all that 'moves' (if anything) is the Q or its features (e.g. Chomsky 1995; etc.).

For a strongly quantified QP (in the sense of Milsark 1977), then, what we end up with (under updated assumptions about tree structures etc.) on this view is something like (5b). This only applies to strongly quantified QPs (broadly, non-indefinite QPs) because Heim treats indefinite determiners *a*, *some*, etc., as having no quantificational force — so applying or not applying Quantifier Construal to an indefinite would have no consequences. I'll come back to indefinites immediately below.

[6] Of course Heim 1982 by no means represents the cutting edge of Heim's views on quantification, but I use it here since it remains frequently cited, and has been used as a baseline for many subsequent analyses of indefinites and quantification generally.

(5) a. Every family is dysfunctional

b.



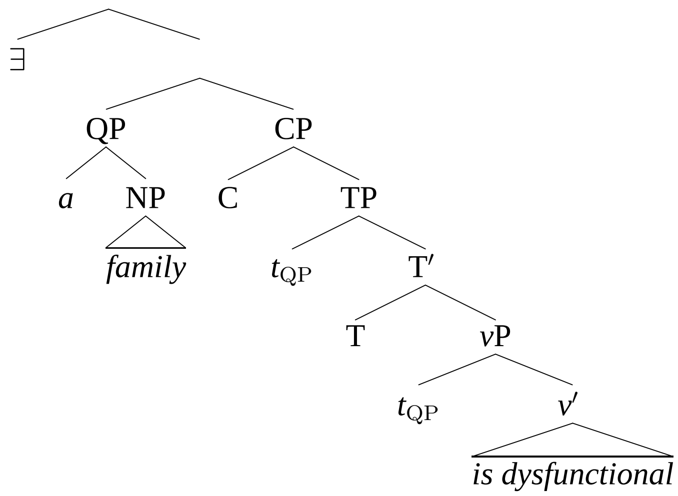
c. ∀x: family(x). dysfunctional(x)

Trace positions are then interpreted as variables, which are bound by the c-commanding quantifier. The interpretation of (5b) comes out something like (5c). (We can ignore the fact that we have two traces of the QP in (5b); we could represent this in (5c) but it would make no substantive difference.)

So the syntax covertly derives tripartite structures for (strong) QPs which are directly readable by the semantics. Since, in Heim's framework, this doesn't go for indefinites (for reasons that are well known, and that I will come back to in §6), something else is required to get the requisite tripartite structure there. For this something else, Heim introduces the rule of EXISTENTIAL CLOSURE, which can insert an existential quantifier ∃ at various points in the structure. Heim's rule of ∃-closure is in fact a little different from what is often assumed now, since the influential proposal of Diesing (1992) that ∃ actually just sits on top of VP (=*v*P). In general, though, the function of ∃-closure is to put an ∃ in a position that c-commands and binds variables that aren't bound by strong quantifiers under quantifier construal. For now, let's just say that in something like (6), ∃ is adjoined at the S level like a strong quantifier under quantifier construal. This will give (6b). ∃ binds otherwise free variables, so this will be interpreted as in (6c).

(6) a. A family is dysfunctional

b.



c. ∃x: family(x). dysfunctional(x)

Indefinites and strong QPs are dealt with differently by Heim, then, but nevertheless essentially the same kind of representation is derived for each by the different construal processes. As mentioned, Heim had good reasons for treating the two things differently (they behave differently), but it could be considered suspicious that the two very different processes of Quantifier Construal and ∃-closure do essentially the same thing, and it is tempting to reduce them to one and locate the relevant distinction elsewhere. Of course this suggestion has been made before; I will put forward a version of it here.

The question now with regard to the discussion in §2 is how the quantificational binding actually works. Above, I have avoided discussing this, simply stating that traces are interpreted as variables and these are bound by the relevant operators. Obviously this is insufficiently explicit. For Heim, the binding was achieved by indices: QPs were assigned 'referential indices' at some point prior to the syntax being read by the semantics. These indices were inherited by any trace(s) of QP, as in standard trace theory, so QPs and their traces were linked inherently. For Quantifier Construal, the assumption was that the index of QP was carried along by Q when it raised. Again, then, the link is direct here. For indefinites, Heim proposed that the referential index of a QP was copied up to the next c-commanding operator, so that the index the QP would have in (6b) would be copied up to the ∃.[7] However we choose to do the indexing, though, it's pretty clear that it's crucial: we can't obviously carry the analysis over to an approach that eschews any kind of indexing and retain the same effects in as straightforward a way.

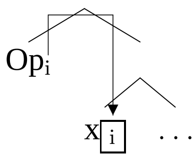
So what do we do?

[7]Heim notes that in fact this could be extended to the strong QP cases too, meaning it isn't actually necessary for Q to carry up QP's index — which makes the two processes Quantifier Construal and ∃-closure even more similar.

4 What we can have

If we accept Chomsky's Inclusiveness Condition — and as noted, we may have no reason actually to do so, but let's say we do — we can't do variable binding with indexing. Though we already saw what this is, for concreteness let's define it: variable binding introduces underspecifed elements into the structure that require a dependency to be formed with some c-commanding element, which then gives the underspecified element an interpretation (7).

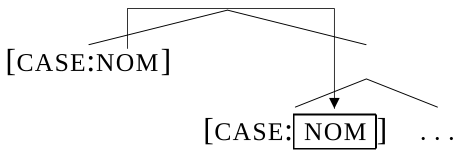
(7)



One way to approach the problem is to ask what we *can* have in minimalism. Something we can certainly have is features: in fact these form the basis of current minimalist syntax. Ideally, features are responsible for (virtually) everything, so one possibility would be to reinterpret variables and indexing in featural terms. This move has been taken by Adger & Ramchand (to appear), who use attribute–value features to capture the same effects, as discussed below.

A fairly standard use of attribute–value features in minimalism is to deal with case. We assume that DPs enter the derivation with a feature [CASE:] — i.e. the attribute of the feature (before the colon) is CASE, and the value of the feature (after the colon) is empty. The point of the attribute–value system, though, is that features should have both an attribute and a value, so DPs need their value filling in — this is how we formalize the case filter. Values are acquired by Agree with some c-commanding, specified case feature: here, say, with [CASE:NOM] on T.

(8)



Note now that exactly like (7), this 'introduces underspecifed elements into the structure that require a dependency to be formed with some c-commanding element, which then gives the underspecified element an interpretation'; unlike (7), though, it is uncontoversially licit minimalist syntax: the Inclusiveness Condition has nothing to say about (8), since no new features are being introduced. Rather, values for features are being filled in by Agree with features that already have values.

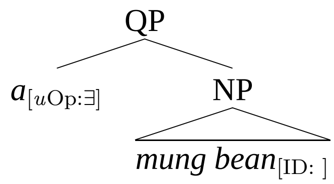
Following Adger & Ramchand (A&R), we can treat variables as features needing a value. A&R label these [ID:] features, to invoke the notion of identification. The constructions they deal with involve relativization, so their analysis involves valuation by λ operators, but it is straightforward to extend the analysis to other operators too so that [ID:]s can take different values.

5 ∃-closure reinterpreted

Recent minimalist literature makes much of feature (un)interpretability: syntactic features can be either interpretable or not at the interfaces. For example ϕ-features are taken to be interpretable at the semantic interface when they show up on DPs, since they clearly contribute something to, or constitute part of, the meaning of a DP. But ϕ-features when they show up on T are taken to be uninterpretable (notated [$u\phi$] following Pesetsky & Torrego 2001), since they make no contribution to the semantics of T itself.[8]

The idea of feature uninterpretability suggests an updated account for Heim's general treatment of indefinites where the indefinite determiner hosts uninterpretable quantificational features (cf. Kratzer to appear). I will assume this, and notate them as [*u*Op:∃]: uninterpretable Op features valued as existential. As syntactic features, these will behave like any other syntactic features; we don't need to say anything new — semantically, though, since they are uninterpretable, they superficially at least as well might not be there. A QP like (9), then, will by itself be interpreted just as the embedded NP *mung bean*. That is, the indefinite Q doesn't do anything here, as Heim proposed.

(9)



But we still need to sayn how the NP is interpreted. We want it to be a variable, so we'll say it has an [ID:] feature. (9) is then a variable over individuals with the property 'mung bean'. Specifically, as in Butler (2004a,b), I will treat [ID:] as a metavariable δ, i.e. an underspecified variable across variable types, whose denotation in context will be determined by the kind of property the head it appears on denotes. So because 'mung bean' is a property that relates to individuals, we get the interpretation of a standard x variable here. For cases where treating [ID:] features in this underspecified way does a little more work than this, see §§5.3;8.[9]

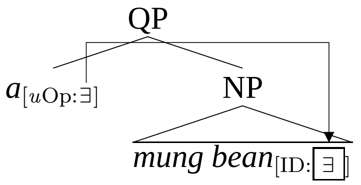
As seen above, the standard story then goes that the variable is closed off by a {VP/*v*P/clause/discourse/text/...}-level ∃-closure operator. Here I will suggest something a little different goes on: the variable is actually 'closed off' syntactically by the [*u*Op:∃] feature

[8]More recently, Chomsky (2001; 2004) has tried to reduce the notion uninterpretable just to the notion unvalued. What this gets us, if anything, is arguable (see Pesetsky & Torrego 2004 for relevant discussion), but I will come back to the notion of uninterpretability, and the use I make of it here, in §5.2.

[9]In fact we might want the NP to denote not a variable over individuals even in this underspecified way, but rather choice functions on sets of individuals, following the work of Reinhart (1997); Winter (1997); Kratzer (1998); etc, and it should be trivial to define the [ID:] feature in these terms instead. Note that if we do this, and the conclusions reached later in this paper are correct, then all QPs should be analysed in terms of quantification over choice functions, and we can do away with any notion of QR; for just this proposal see Sauerland (1998); von Stechow (2000). I won't go into this question in this paper; for concreteness and ease of exposition I will talk in terms of standard variables.

of the indefinite determiner. That is, I take closure at this point to correspond to valuation via Agree, as we saw for case features in (8): the [Op:∃] and [ID:] features Agree, and [ID:] is valued [ID:∃] (10).[10]

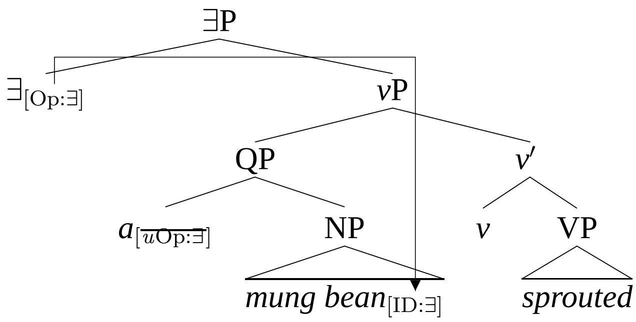
(10)



This step is crucially different from the standard story. Although the indefinite Q itself doesn't get interpreted, and so seems on the face of it not to do anything, in fact it does have an indirect but important influence on the subsequent derivation. Because the [ID:] is now valued [ID:∃], it must eventually Agree with an interpretable [Op:∃] feature: i.e. it must be eventually bound by an interpretable existential Op. This means it will be selectively, not unselectively, bound. I'll come back to the importance of this immediately below.

For the next step, assume an ∃ head above *v*P (Diesing 1992 etc.); say this hosts the relevant feature and Agree takes place (11).

(11)

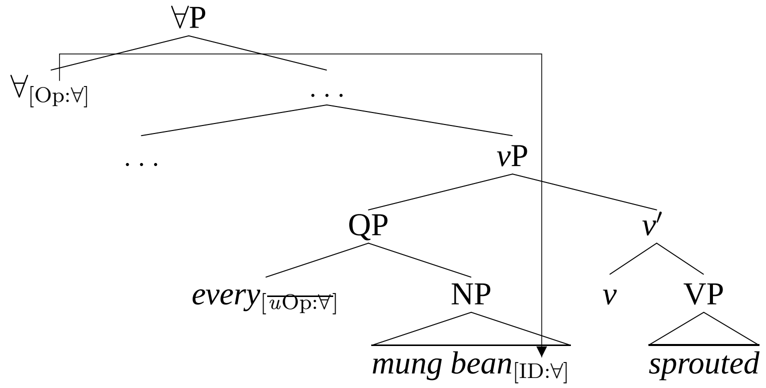


The intra-clausal ∃ head quantifies over the variable in the QP; the head of QP itself has its [*u*Op:] feature deleted, and as for standard closure stories plays no direct role in the quantification. But unlike standard stories, it does play an indirect role, in that it derives a kind of selectivity of binding. If we only want to do ∃-closure this really isn't much to shout about: we already had ∃-closure, and it just gives us a particular formalization of that notion.

But what it also lets us do straightforwardly, if we want to, is treat other QPs in terms of closure too. Say (very plausibly) we weren't sprouting just one mung bean but our whole bag: in that case we can treat universal QPs the same way. (12) is just like (11) except instead of an

[10] A number of ways exist to formalize the Agree relation here, depending whether we take it to be mediated by other matching features, by the semantic properties of the features in question, or whatever — see Manzini & Roussou (2000); Pesetsky & Torrego (2001); Adger & Ramchand (to appear) for discussion of various possibilities. Here I assume that the relation is direct, since we have a two-way dependency to resolve — the [ID:] needs valuing, or we'll have an unbound variable, and the [Op:] feature needs to Agree with something or we'd have something like vacuous quantification — which can be both ways resolved by the Agree relation described; this is similar to Manzini & Roussou's approach. It could easily be restated in different, more standard terms by those uncomfortable with this level of semantic influence on the derivation.

intra-clausal ∃ head binding the variable, there's a ∀ head, with the [*u*Op:∀] feature on *every* mediating the relation and again deriving selective binding.

(12)


If we also make the uncontroversial assumption that a GEN operator can be around in the syntax, then we get to deal with generically interpreted QPs too (we still need to say something about the difference between generic and existential interpretation of some indefinites, too: see §7). If we assume that a definite or referential operator also obtains in the structure (cf. e.g. Beghelli & Stowell's 1997 Ref head), we can also deal with definite DPs the same way.[11] Second order quantification, less straightforward, will be considered in §5.3.

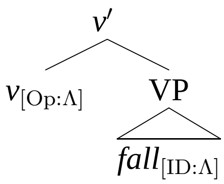
First, though, a question comes up regarding the structures in (11–12). I have represented the variable in the QP as an [ID:] feature on N. This feature replaces the referential index that Heim's system assigned to all DPs. However, the tripartite structures that Heim assumed were necessary also required a variable in the nuclear scope of the quantifier (which I take here to be the sister of QP). In Heim's system, this variable was again provided by a referential index, namely the one left behind on the trace of the moved QP, as noted above. Since I'm not using referential indices, this move is unavailable to me. It may be noted that in (11–12) the QP is actually represented in its base position in *v*P. We might then try to exploit the copy theory of movement, and say that when the QP moves, both the 'moved' and the lower copies contain the same [ID:] feature, so there is in fact a variable lower down in the structure that we could use, seemingly similar to the referential index on the trace in Heim's system. This won't do though, because the variable in the lower copy of the QP will range over whatever the NP denotes, just like the variable in the higher copy. What Heim had in the nuclear scope, though, was a variable ranging over whatever the predicate denotes, i.e. somewhere inside the *v*P, but outside QP. If variables are treated as [ID:] features, then that means we need [ID:] features in *v*P somewhere. In Butler (2004a), I argued that we have exactly that, and I will exploit this here, though I will conclude that they don't in fact provide exactly the kind of tripartite structure so far assumed. Given the semantic framework I couch this in, this won't be a problem.

[11] As to exactly where the various Ops might be located in the clause, I remain agnostic here; see Butler (2003, 2004b) for justification of one particular hierarchy. Alternatively, we could directly import a hierarchy suggested elsewhere, such as those of Beghelli & Stowell (1997); Hallman (2000); Brody & Szabolcsi (2003). The purpose of the current paper, though, is to give an analysis of how the system works, wherever the heads in question may sit in the clause.

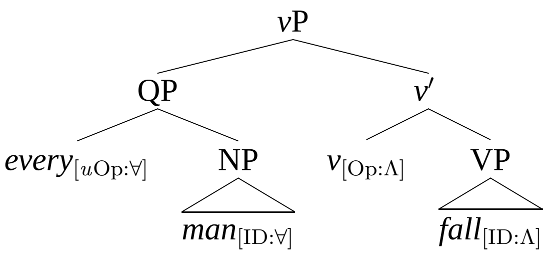
In Butler (2004a), I treated the verb's argument slots formally as [ID:] features on V. This gives us a featural reinterpretation of θ-roles (cf. Hornstein 1999; Manzini & Roussou 2000), such that θ-roles are variables ranging across the property denoted by V, exactly equivalent to the variables introduced on Ns (cf. Williams 1994, who also takes the two things to be fundamentally the same). I then took these [ID:]s to Agree with (= be bound by) [EPP] features introduced on *v* heads.

A simple intransitive V like *fall*, then, would have one [ID:] feature for its one argument.[12] I assume this is introduced unvalued, similar to the [ID:]s on N heads. A θ-role needs to be somehow associated with an argument, and I take it this happens via predication.

Current minimalism, where it is explicit on the matter, takes arguments to be introduced by [EPP] features on *v* heads. I make this assumption too, but I reanalyse [EPP] features as [Op:Λ] features (cf. Adger & Ramchand to appear), which map to the semantics as predicate abstraction operators λ. EPP and predication are then directly linked (see also Williams 1980; Rothstein 1983; Chomsky 1986; Heycock 1991; Åfarli & Eide 2001; a.o.). V then projects its VP; this is selected by a *v* head hosting an [Op:Λ], and the [Op:Λ] values V's [ID:] as [ID:Λ], i.e. a variable bound by a λ operator (13a). This maps to the semantics as a λ-abstract over individuals with the property 'fall' (13b), and as such it needs to be satisfied by an argument, so an argument is merged as [Spec, *v*P] (14).

(13) a.


b. = λ. fall(δ)

(14)


The syntax–semantics mapping looks idiosyncratic here: a λ-abstract, which basically wants an atomic argument, is being satisfied by *man(δ)*, which looks like it should be a predicate. On the view espoused here, though, predication is achieved by the use of [Op:Λ] features binding variables, and since there is no such feature in the QP, it's actually not a predicate *per se*. Rather, I follow the Hamblin-treatment (i.e. based on the treatment of *wh*-phrases in Hamblin 1973) of such things proposed by Kratzer & Shimoyama (2002); Kratzer (to appear), in which they introduce into the representation a set of individual 'alternatives' — i.e. in this

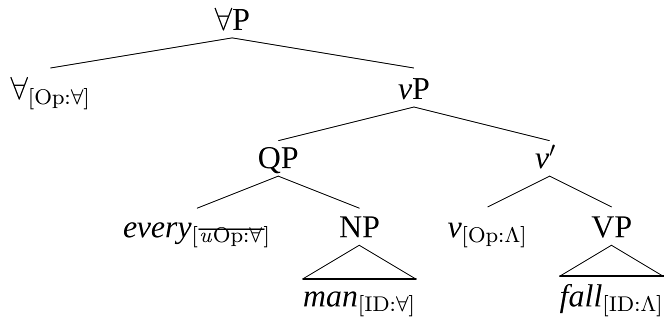
[12]In fact I assume it would have a second [ID:] for an 'event' argument also; this isn't relevant for now so I leave it aside till §8 where it will be briefly discussed. See Butler (2004a,b) for more details.

case, a set of men. For Kratzer, indefinites[13] ‘denote sets of individuals. We should not think of those sets as properties, though. They are individual alternatives, alternatives of type e, that is’ (Kratzer to appear: 10). These alternative individuals then saturate the verbal predicate one by one, or in Kratzer’s terms by ‘pointwise functional application’, so that we end up with a set of alternative propositions, one for each individual.[14] The intra-clausal Q then quantifies over these propositions, not over the individuals themselves. Within this framework *man(δ)* can be considered type e, and so is a licit satisfier of a λ-abstract wanting an atomic argument.[15]

The argument in (14) is a universal QP, whose Q head hosts a [*u*Op:∀] feature. This feature needs to Agree with an interpretable equivalent, as described above, so again, that’s what I assume happens. We end up with (15). As discussed, *v*P denotes a set of propositional alternatives, and the clausal ∀ tells us all these propositions are true.[16]

So this gives us the structure we need, interpretable straightforwardly by the semantics; but without the use of indices. Instead, we have feature attributes corresponding to variables, feature values corresponding to indices, and Agree corresponding to variable binding or index assignment. Note that in this system the relationship between the quantificational [Op:] features and the V’s [ID:] is’nt direct, as with Heim’s story, but rather mediated by predication. However, we end up with the right interpretation, so this does us no damage.

(15)



5.1 Empirical evidence

It is possible, then, to treat QPs generally, not just indefinites, in terms of a generalized quantificational closure mechanism. Theoretically we have good reasons to do this. One is that it reduces the machinery required by the theory considerably: we don’t need to postulate a split between construing indefinites and construing strong QPs; and we also can deal with other

[13] In fact Kratzer refers to indeterminates, but for my purposes here we can treat them equivalently.

[14] One can think of it this way: an indefinite like *man(δ)* introduces something like an unspecified value for the property ‘man’ — i.e. some (unspecified) member of the set of men.

[15] We can also do the [EPP] stuff this way: the verb denotes a set, as commonly assumed, and the [Λ] feature abstracts over this set. Once we have this level of generality of application, treating indefinites like this becomes much less specialized.

[16] For clarity: if we had an ∃ head instead, it would tell us that (at least) one of the propositions was true; if we had a Def head, we would presumably have a single member set introduced by NP, containing the unique relevant referent, and clausal Def would tell us that the unique proposition thus derived was true; etc.

quantificationally relevant phenomena by the same means — see §8, and Kratzer (to appear), for discussion of just some.

Two pieces of empirical evidence also favour the view that strong QPs needn't receive a GQ construal. One very straightforward one comes from Chinese, where we see overtly a case in which what looks uncontroversially like a universal QP actually gets an interpretation as a restricted variable. (16a) gives the data, (16b) gives a version of the same sentence with the QP replaced by a reflexive, with the interpretation the same. Here, there doesn't seem any way we could assign the second occurence of *meiyiben shu* a GQ construal and retain the correct interpretation for (16a). Matthewson (2001) makes a similar claim about this kind of data, suggesting that *dou*, usually glossed as some kind of distibutivity-related element, is an overt instantiation of a clausal quantificational head of the type posited here.

(16) a. Meiyiben shu dou you meiyiben shu de fengmian
every book PRT has every book its cover
= ∀x: book(x). book(x) has book(x)'s cover

b. Meiyiben shu dou you zhiji de fengmian
every book PRT has itself its cover

The second relevant piece of evidence comes from cases of control. It is well known that in control environments where the controller is a QP, we don't want PRO to receive an equivalently quantified interpretation. So in (17), for example, we want (17a) to be interpreted something like (17b), not (17c). If we assume that QPs receive a GQ interpretation, then we have to say something special about how *everyone* imparts an interpretation to PRO in these cases, without imparting anything quantificational. But if we instead assume that QPs are always interpreted as restricted variables, not GQs, then we correctly predict the data: *everyone*, lacking inherent quantificational force, will give PRO the same restricted variable meaning it has itself, and the clausal Q will bind both.

(17) a. Everyone$_i$ expected PRO$_i$ to wint

b. Every person x expected person x to win

c. Every person x expected every person x to win

5.2 Uninterpretability

As noted in fn. 8, recently there have been moves to get rid of the notion of purely uninterpretable features, in the sense that I have used them here, essentially on the grounds that they are introduced into the derivation with the sole purpose of being deleted. Chomsky (2001) suggests that the notion of uninterpretability actually reduces to the notion of unvaluedness: an unvalued feature is clearly uninterpretable at the interface; in order to receive an interpretation valuation is required. But once an unvalued feature has received a value, it doesn't get deleted, as was assumed for purely uninterpretable features once checked — rather, it becomes interpretable.[17] If this is right, it has strong ramifications for the story presented above, since what we would have in the structures in (11–12) would no longer be one interpretable quantificational feature located on the clausal head, but rather two interpretable features, one on the head of the QP and one on the clausal head. This is problematic, because we then seem to end up with a situation where we have two quantificational elements in a single tripartite structure:[18] one where we need it — on the clausal head — and one where we apparently don't — on the head of QP.

One way to deal with this is to retain the interpretable/uninterpretable distinction after all (see Pesetsky & Torrego 2004 for some independent arguments in favour of doing this). Then things would work exactly as described above, and there would be no problem. I think this would actually be more justifiable for this particular piece of analysis than in some cases: in general, the objection that uninterpretable features are introduced solely to get deleted is pretty much valid. The features may have some mediating effect on particular agreement relations, but such effects can be achieved without the mediating uninterpretable features — see Brody (1997); Manzini & Roussou (2000) for strong arguments to this effect; also fn. 10. In this case, though, the features on Q actually have an important derivational function, which is to derive the selectivity of the binding relationship, and it isn't as clear that this could be achieved without the features being there at all — we would rather expect unselective binding in these cases. For arguments that parameterization exists in this area, and that some languages do in fact lack mediating 'selectivizing' features and therefore display unselectivity, see Kratzer (to appear).

Another way of dealing with it is taken by A&R, who follow Chomsky in assuming that there are indeed no purely uninterpretable features, and propose the principle in (18).

(18) *Interpret Once Under Agree (IOA)*: Just as the PF interface only pronounces one phonological 'copy' of a MOVED item, the interpretational interface only interprets *one* version of features that are identical, when connected under an AGREE relation.[19]

(Adger & Ramchand to appear)

[17] In fact, Chomsky 2001 suggests that such features may be deleted at spellout anyway, if spellout operates 'shortly' after valuation, and so can somehow remember that the feature was until recently unvalued. This idea is clearly incoherent, though.

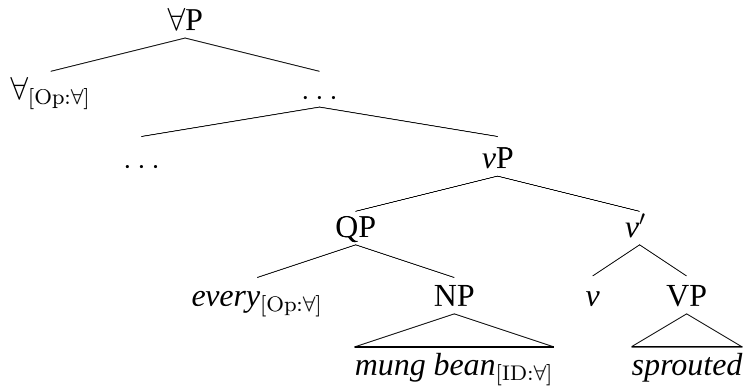
[18] Although in fact I have ended up with structures that aren't tripartite in the same way that Heim's were, because of the Hamblin-type semantics I adopt, I will continue to use the term 'tripartite' to distinguish these from bipartite GQ structures.

[19] Whether we really want only ever to interpret one instance of features is debatable: in cases where interpreting more than one would give us a problem with morphological or semantic interpretation, then certainly we wouldn't want to do that. However, in other cases, it may not be necessary to interpret only one, and indeed sometimes it may be necessary to interpret more than one. In the current system, this would probably be relevant for whether or not we have reconstruction: if we want to 'reconstruct' that would seem to ential ignoring (= not interpreting) some

This principle capitalizes on the 'possibility' component of the *-able* suffix, drawing a distinction between 'interpretable' and 'interpreted' features: it may be possible for a feature to be interpreted at the interface, but that doesn't mean it will be. IOA acts as a filter on representations, then, such that when the interpretive interface is handed a structure it tries to make sense of it, and this might include getting rid of or ignoring some parts that aren't necessary at that point.

Its effect on the discussion in the previous section is this: if we hand a structure like (19), with both sets of quantificational features interpretable, to the interface, it's going to crash unless we assume something like IOA.

(19)



If we really don't have uninterpretable features, then, IOA seems to make sense. But note that it doesn't tell us *which* occurence of the feature(s) to interpret. In principle, then, for a situation like (19) where we have two occurences of the relevant feature, either one could be interpreted. Since I'm talking specifically about QP interpretation, what that means is that in principle, we should expect for any QP a potential ambiguity between a generalized closure and a generalized quantifier interpretation. This is a very interesting prediction, since if we can confirm or disconfirm it we may be able to learn something important about the nature of syntactic features: whether they come in uninterpretable versions or not.

Unfortunately, it isn't that easy a prediction to test. For a start, in some situations the ambiguity simply won't come about since the structure will be such that only one choice of which feature to interpret will give us a coherent output. This is in fact the case for the examples A&R discuss.

In other situations, the 'ambiguity' may show up in the syntax, in the sense that either choice is licit, but the outputs may be truth-conditionally non-distinct. This is likely to often be the case for the examples discussed here: if GQ interpretations are as licit as closed-off interpretations for QPs (which isn't in fact clear to me, under the Hamblin-type semantics I

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higher [Λ] (= [EPP]) feature(s) and occurrence(s) of a nominal (possibly this is the *only* way reconstruction could be achieved under this approach). If we don't reconstruct, though, it isn't necessary to ignore lower occurrences, and indeed sometimes we need to take them into account, as argued in some detail by Sportiche (2005), among others. We might then need a modification of IOA that, rather than restricting interpretation to a single copy, restricts it to ignoring those copies that are problematic. This retains the spirit of IOA, while giving it broader coverage.

adopt, though Shimoyama (2001) retains GQ intepretations even for that system), then either choice of feature will be okay in (19), but in most cases, at least, the choice taken isn't going to make a detectable difference; the same point is made by Kratzer (to appear).

Presumably, though, there could still be cases where neither of these situations holds: that is, where the interface does have a choice which occurence of a feature to interpret, and the choice taken does has truth-conditional effects. It has been claimed that something very like this is the case for indefinites, by Kratzer (1998). Precisely, Kratzer suggested that indefinites could be interpreted either as 'quantificational', by which she meant GQ-style (the same, under her assumptions there, as any other QP); or via choice function variables, which she took in that paper to be left free and valued by context, and which for my purposes here can be seen as similar to a closure analysis.[20] Under Kratzer's more recent proposals (Kratzer to appear), though, quantification is handled along the lines proposed here, and it isn't clear where this leaves the 1998 story. Kratzer's recent presentations of her work (Kratzer 2003, 2004) suggest she takes a line where the variable behaviour of indefinites is to be considered instead a reflex of implicit domain restriction possibilities, following lines set out by Schwarzschild (2002); Breheny (2003). If this line of argument is right — and I will argue in §6 that it is — then the facts set out in Kratzer (1998) are elsewise dealt with, and we don't get to see an ambiguity between closure and GQ interpretations here after all.[21]

Another place where the distinction may show up is in cases of second order quantification.

5.3 Second order quantification

With second order quantification, things become more complicated. Definite, existential, universal and generic quantification can all be argued to have effects in the clause regardless of QP interpretation (in the domains of modality, habitual predicates, 'event' closure, stativity vs. eventivity of the predicate, etc.), and so the presence of operators performing these kinds of quantification can be justified independently (see §8 for some discussion). The presence within the clause of second-order operators like *most*, *five hundred and six*, *around half of*, and the like, can't really be justified on the same grounds; and since the number of 'second order quantifiers' is limitless, we run the risk of expanding our clause structure to infinite size if we try to put operators corresponding to them in there, which is less than ideal.

[20] See Kratzer's paper and Matthewson (1999) for discussion of the consequences of leaving choice function variables free versus closing them off.

[21] Arguments are presented in Matthewson (1999) that the ambiguity Kratzer posits is overtly encoded in the St'át'imcets (Lillooet Salish) determiner system. The ambiguity Matthewson discusses involves what she labels a 'polarity determiner', which is to say a determiner that must be licensed by a c-commanding polarity element such as negation, a question operator, or a modal. Kratzer notes that the same alternatives semantics that she adopts has also been used by Krifka (1995); Lahiri (1998) to account for various polarity phenomena. I have nothing to say here about the St'át'imcets polarity data at issue, but it seems therefore probable that Matthewson's data can be brought under Kratzer's umbrella.

This means we need to say something extra to cover these cases. On the face of it, to stick to the story so far, we seem to need to say that second-order quantifiers aren't in fact quantifiers at all, in the same sense that $\exists$, $\forall$, GEN, or D are. This is by no means a radical thing to say anyway, not least for the reasons given above: we can justify the quite general quantificational status of those elements based on factors from numerous areas, whereas second order quantification seems specific to the nominal domain. A second reason is that we can get away pretty much with quantification over individuals for (nominal) first order quantification, but once we get into second order quantification, we start to need additional possibilities; in particular, we need to quantify over sets of individuals[22].

The question then is where those set variables come from. Actually the answer to this should be straightforward: I am assuming that variables are introduced syntactically as [ID:] features on lexical heads. I am also assuming that they map to the semantics as metavariables δ, the actual range of δ in context being set by the kind of property the head it appears on denotes. So I took the variable introduced by 'mung bean' to be interpreted as ranging over individuals, just because mung-beanhood is typically a property relating to individuals. If we need a variable that ranges over sets instead, then the difference is going to be in the head the variable appears on: it will be a head that denotes a property of sets, not individuals.

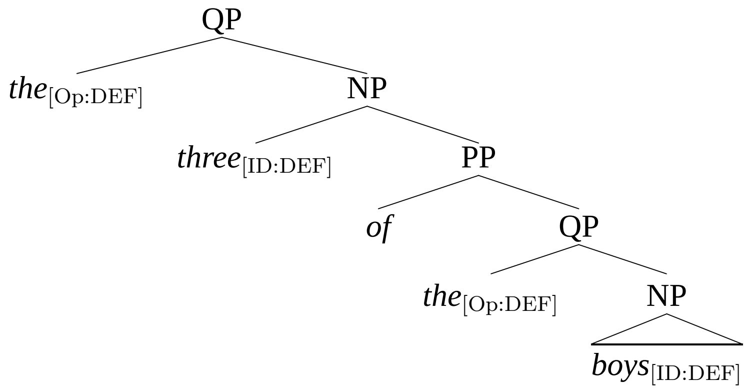
Since second order quantifiers don't exclusively select nominals that denote set properties, we have to assume something other than the [ID:] on the NP contributes the set interpretation. Here, I will analyse the 'quantificational' element itself to contribute this. For simplicity, I will use numerals to represent second order quantifiers generally, recognizing that they won't tell us everything. However, I do think they allow the relevant point to be made.

The reason for the scare quotes in the above paragraph is that I am not going to treat the numeral as a quantifier in the sense that I have been using that term so far. In fact, as can be inferred from the discussion so far, since I am taking them to contribute the set variable, and since I take [ID:]s to be contributed by lexical heads, I am going to treat them as lexical heads. To deal with the possibility of examples like (20a), I assign second order QPs a partitive-like structure as in (20b). Where we don't see all the elements we can see in (20a) overtly, I still assume we have the the same structure, though the features appearing in that structure may well be different — for example, [Op:GEN] or [Op:$\exists$], rather than [Op:DEF].

(20) a. The three of the boys

[22] Or choice functions that pick out a set of individuals from a set of sets of individuals: see Reinhart (1997).

b.



Two points: first, I purposely haven't overtly indicated (un)interpretability status on the [Op:] features in (20b). They might or might not be uninterpretable, and I will come back to this immediately below.

Second, I have assumed the partitive element — labelled P, but with no real commitment to whether it should be distinguished from other preposition-like elements such as locatives, particles, etc. — is the topmost element of the embedded nominals. That (some) Ps are really the topmost heads in the extended nominal projection isn't a new idea: see among others van Riemsdijk (1978); Grimshaw (2003); Starke (2003). This general approach to partitive Ps seems justified by the existence of partitive determiners in French, Italian, Dutch, etc., where the partitive determiner can be considered a realization of P, or D incorporated into P. See Chierchia (1998) for an analysis of the structure of Italian partitives very similar to (20b).

In (20b), the embedded nominal introduces one [ID:] feature, which because of the denotation of the N *boys* will be interpreted as a variable ranging across individuals. The embedding nominal introduces a second [ID:], which because of the denotation of the N *three* will be interpreted as a variable ranging across sets of three things. The partitive element ties these two things together, further specifying that the sets consist not just of three things, but of three boys. The whole, then, denotes a unique discourse relevant set of three elements from within a unique discourse relevant set of boys. This is a good interpretation for (20a), suggesting this is on the right track. We run into a question with regard to closure of the embedded variable, though.

The variable on the outer nominal is fine and can be dealt with in the way described so far, closed off by clausal closure heads — this will deal with scope facts we find with this kind of QP, so that if we didn't have an [Op:DEF] but rather an [Op:∃] (e.g. *three of the boys*) or [Op:∀] (e.g. *all five of the boys*) feature at the top, we will get the same kind of scope facts as we would with a straight existential or universal QP. The variable on the embedded nominal, though, has to be closed off internally to the whole partitive structure. If it wasn't, we would get an incoherent structure where the embedded QP too took clausal scope, potentially outscoping its own selector. We don't find this happening, and in fact the embedded nominals in these structures don't have clause-level scope properties at all. That means there must be something internal to the partitive as a whole that performs this closure. If we want to stick to the story that QPs themselves always host uninterpretable quantificational features, with something else closing off the variable on N, then we have to posit some distinct operator inside the structure.

Interestingly, Zamparelli (2002) has suggested a structure for partitives similar to that in (20b), and he suggests that the P element is, or hosts, a 'partitive operator'. If this is true then it may be possible to formulate some story wherein this operator closes off the embedded variable. However, to do this it would have to be a very powerful operator since the embedded QP allows for most if not all kinds of quantification (in addition to the example in (20a) with a definite, we can have at least embedded universals — *most of all boys* — and generics — *(the) five boys*); and moreover it is unlikely that an operator dealing in partitivity would have anything to say about quantificational closure.

The alternative seems to be allowing that in fact quantification isn't uniformly a clausal phenomenon, as discussed in the previous section: it would be quite straightforward here, if the operator feature on the embedded Q isn't uninterpretable, to say that the embedded QP gets a GQ interpretation, whereas the external one may not. This may, then, be the kind of case we were looking for to show that we can't just say that uniformly, quantificational features in QPs are uninterpretable, and that being so, we may have a case to make that the notion of uninterpretability isn't a valid one, as suggested by Chomsky etc. This is tentative, since it rests on the assumption that the structure for second order quantification is the right one, though that assumption is promising.

It then becomes problematic, however, to account for why, in cases like (16) and (17) in §5.1, we only see a restricted variable reading: there doesn't seem any obvious reason a GQ reading, if available at all, would be ruled out in these cases, and yet empirically it is — for example, the Chinese example (16a), and a pretty straight transliteration into English 'every book has every book's cover', don't, and indeed can't, mean the same thing. The fact that the Chinese example overtly encodes distributivity via the particle *dou*, whereas English doesn't do anything similar, may have a bearing on this, but it isn't clear exactly how the crosslinguistic variation we see here should be viewed: does encoding clause-level quantification overtly mean we don't get GQ readings, for example? If so, why? Would this then extend to the control cases? And so on.

The data, then, are highly indecisive on this point: we have suggestive evidence that sometimes only the restricted variable reading for a QP is available (as in the Chinese cases), but we also have reason to suppose something like IOA may hold. These two things *are* compatible, but we need a way of explaining why the GQ reading is ruled out where it is, and unfortunatley that isn't obvious.

6 Indefinite scope freedom

If we do decide to treat indefinites and other QPs equivalently, whether that is along the lines of Heim/Kamp indefinites, or as GQs, or even as uniformly ambiguous between the two, as seems possible based on the discussion up to now, then we appear to lose out in a big way in terms of the reason Heim, Kamp, and others proposed that we treat indefinites differently in the first place. Although there were a number of reasons for this, one of the foremost has been the apparent '(relative) scope freedom' of indefinites, by which is meant the ability of indefinites to apparently scope out of islands, take exceptionally wide scope, etc.; as distinct from the more restricted scope taking abilities allowed for other quantified expressions (often taken to be conditions on QR). If indefinites aren't in themselves quantificational, so the story goes, then their apparent ability to escape conditions on scope that affect other QPs is not unexpected — we can explain the distinction by assuming that ∃-closure isn't subject to conditions on QR. Once we propose that a closure story can be extended to all QPs though, we seem to lose an explanation for this distinction. But since empirically some such distinction seems to be real, we do actually want an explanation.

We could perhaps retain a version of the usual story in the theory offered here by proposing that ∃-closure heads are freer than other quantificational closure heads in terms of their placement in the clause stucture. This would be pure stipulation, though, and in fact seems to be in conflict with the empirical data: all the proposals that such quantificational heads exist in the clause and that are specific on the position of those heads (e.g. Beghelli & Stowell 1997; Hallman 2000; Butler 2004b; Brody & Szabolcsi 2003) agree that they conform to some hierarchy (even if they don't agree on what that hierarchy is), so that the scope possibilities enjoyed by particular quantificational elements are syntactically constrained. Evidence for this notion comes from several discrete domains, including QP scope (Beghelli & Stowell), modal scope (Butler), and intensionality facts (Hallman). These arguments all take existentiality into account, so a separate operation of free ∃-closure conflicts with this strongly.

An alternative way to go is to claim that though some such distinction is real, the closure/GQ split isn't the right way of capturing it. This has in fact been independently proposed, notably, recently, by Schwarzschild (2002) and Breheny (2003). Schwarszchild and Breheny show that whatever claims we might make about scope, and particularly about the ability of indefinites to apparently scope more freely than other QPs, we still need to say something more, since the scope proposal alone doesn't buy the facts. Schwarzschild's paper is more relevant to the discussion here, so I will concentrate on that; a few examples demonstrating the incompleteness of a wide-scope account that Schwarzschild discusses in his paper, along with the paraphrases provided by the references cited, are in (21).

(21) a. John gave an A to every student who recited a difficult poem by Pindar (Farkas 1981)

= $\exists$x [diff-Pindar-poem(x) & $\forall$y [(student(y) & y read x) $\rightarrow$ John gave y an A]

b. Nobody believes that I have seen a certain Buñuel movie (Cresti 1995: 130)

= There is an entity x_3 such that: it is presupposed that x_3 is a Buñuel movie in the utterance world, and it is asserted that nobody believes that I have seen x_3

c. Every gambler will be surprised if one horse wins (Abusch 1994: 94)

= Every gambler$_x$ $\exists_y$ horse(y) & will [if y wins] [x be surprised]

For (21a), the intended reading is that 'there is a particular poem whose recital yields a perfect score. But ... [Farkas's translation] would be true in the likely circumstance that there is some difficult Pindar peom that no student recited, regardless of what grades John assigned' (Schwarzschild 2002: 301). Of course, this doesn't capture the intended reading. The same kind of consideration holds for the other examples: (21b), for instance, is made true by the existence of any Buñuel movie, whether or not anyone has seen it, and whatever anyone believes about other people having seen it; (21c) is made true by the existence of any horse, winning, losing, or irrelevant — 'Candidate horses would include those that have not entered the race, dead horses and maimed horses' (Schwarzschild 2002: 302). Again, these cases don't correspond to the intended readings, nor even to reasonably salient readings.

6.1 Scope neutralization

This being so, it is necessary to assume that something else, either in addition to or instead of wide-scope, is involved in deriving the relevant readings for cases like those in (21). One old suggestion, due to Fodor & Sag (1982), is that the indefinites in these cases are understood as 'referential' indefinites: that is, they are distinct from what Fodor & Sag refer to as 'quantificational' indefinites in that they are genuinely referential descriptions, rather than quantificational expressions, whether we choose to understand that under an $\exists$-closed variable or a GQ analysis. If these readings of indefinites are actually referential, the story goes, then it isn't that they take unusually wide scope: it is rather that they are scope neutral, like other referential expressions (definites, proper names, etc.). There are a number of well-known problems with this notion which I won't go over here, but Schwarzschild's solution to the problem he sets out does come back to it in one sense, which is that he argues that specific readings of indefinites aren't in fact obtained simply by free, or unusually wide, scope, but reflect scope neutrality, or more precisely scope neutralization.

As seen from the discussion of the examples in (21), simple wide scope existentials suffer from the fatal problem that the existence of any x satisfying the nominal restriction can suffice to — inappropriately — fulfill their truth conditions. As Schwarzschild points out, even dead horses may be considered in (21c), although plainly dead horses oughtn't to be considered when calculating the contextually relevant truth conditions for that sentence. What is required, then, is a restriction on the set of horses under consideration that will exclude, among others, dead horses. All this tells us is that existentials sometimes require contextual domain restriction like any other quantificational expression — indeed, any other contextually sensitive expression — which is not in itself a surprising or controversial claim.

For the cases at issue, though, we need in fact to be more explicit about this: we have to assume that the implicit domain restriction carried by specific indefinites picks us out single unique element with the property denoted by the overt nominal restriction (cf. Hintikka 1986 on *a certain* indefinites, and Kratzer 1998; Breheny 2003 for more recent discussion of Hintikka's work). That is, (21c) doesn't just want us to consider the horses that actually raced, nor even the horse that in fact won if that information is available to us, but a single, particular horse whose winning in the relevant context would surprise all the relevant gamblers. The restriction we need, then, is a singleton set, a set of one horse in this case: Schwarzschild labels these readings of indefinites 'singleton indefinites', accordingly.

Although indefinites contextually restricted down to singleton sets in this way aren't referential, in the way that definite DPs or proper names are referential,[23] as far as scope is concerned they are going to behave the same way, which is to say they aren't going to interact scopally with with other elements in the structure in a way that makes any truth-conditional difference. The horse that will surprise all the gamblers if it wins is the same horse regardless of whether we scope it above or below the universal; likewise for the Pindar poem and Buñuel movie and their scope beraing clause mates. This is what Schwarzschild means by scope neutralization: restricting the domain of quantification to a singleton set robs the indefinite of its normal scope interactional properties.

The crucial point of this conclusion is that, if it is true — and it seems hard to escape it — in fact assigning wide scope to the indefinites in such cases isn't actually going to do anything for us after all, just as assigning 'wide scope' to a definite wouldn't. This means we no longer have to say anything special about the scope taking abilities of indefinites, since the relevant 'special' properties of indefinites are actually not to do with scope taking, but rather scopelessness. We can then treat indefinites and 'strong' QPs in the same way as far as the syntax/semantics of scope taking is concerned, which is exactly the claim made in the current paper. Schwarzschild (and also Breheny, in his work) takes the line that we should then treat them like other QPs, as GQs; I take the line that we should treat other QPs like them, as closed off by clausal heads (or maybe posit a general closure/GQ ambiguity, as in §5.2) — but the crucial point is that as soon as we encode the relevant properties of indefinites in terms of domain restriction, we don't then need to make any further distinction.[24]

One question that comes up is why indefinites seem particularly prone to this kind of scope neutralization. A partial answer to this is that, in a sense, they aren't: (singular) definites and proper names are also, presumably, restricted in exactly the same way, which is why they too

[23] E.g. if someone says (21c), there is no assumption that the hearer knows which horse is under discussion, whereas using *the horse* or *Red Rum* assumes that they do.

[24] Actually we do need to consider one more thing, which is the well-known 'intermediate scope' problem discussed by, among others, Reinhart (1997); Kratzer (1998). Space precludes me going into this also here — again I go along with Schwarzschild's treatment, which is essentially that, as proposed by others, the covert restriction of an indefinite can contain a bound variable; this then makes that indefinite dependent on whatever binds the variable, so that the scope of the indefinite is neutralized only with respect to the binder of the variable. Because of the relation between the indefinite and the binder of the variable, how the indefinite interacts with other scope bearing elements will be affected by how the binder interacts with them, and restricted by whatever scope restrictions, etc., affect that binder.

are scope neutral. The difference is the presupposition of uniqueness that comes along with these kinds of expressions — indefinites don't have this uniqueness presupposition, so they don't require a singleton restriction.

Another part of the answer is discussed by Schwarzschild, who points out that, for other QPs that don't carry a uniqueness presupposition, a singleton restriction is pragmatically unlikely — that is, there is what he calls a 'non-singletonness implicature' with other QPs. He encodes this as an implicature rather than, say, a presupposition, since it is defeasible in the right contexts. Some of his examples are in (22): (22a) is an example where the non-singletonness implicature is cancelled; (22b) is a case where it is deliberately flouted for effect.

(22) a. Everyone in the Italian department is happy with Cipriano's proposal since there is just one person in the Italian department and that is Cipriano

b. I do not know about you, but everyone I voted for in the last election was white

(Schwarzschild 2002: 304)

In fact, then, all that is 'special' about indefinites in this case is that they don't require a singleton restriction, as definites do, and nor do they pragmatically resist one, as other QPs do — and neither of these properties is really very special at all.

7 Generics

One final question remains with regard to indefinites vs. other QPs, and that is the generic reading available to some indefinites, and unavailable to others.[25] The probem is basically that while (23a) is ambiguous, (23b) isn't.

(23) a. A man (always) parts his hair on the left

b. Some man (always) parts his hair on the right

(23a) can be taken either as a statement about a specific man — call him Gary Crant — or as a statement about men in general. (23b) lacks this generic reading, however, being understandable only as about a certain man — say Cary Grant. Note that even when we insert the adverb *always*, which strengthens the generic reading in (23a), it doesn't make any difference to (23b).

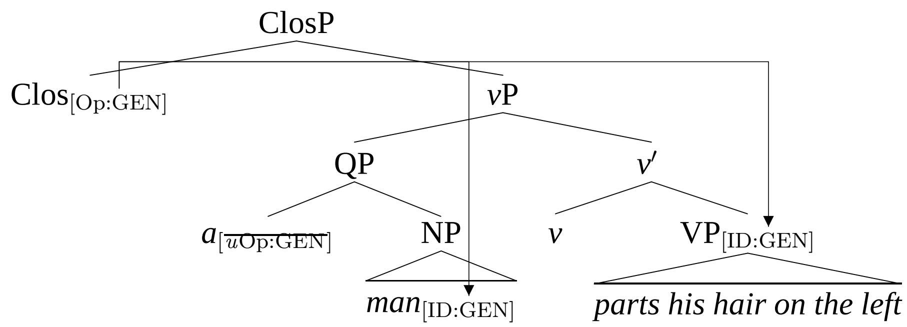
[25] A similar effect is found with other DPs too, e.g. definites: (1) can be talking about the monkey under discussion, e.g. the particular one visible from a safari vehicle; or else about monkeys generally, as a species. This may have more to do with reference to kinds than with the kind of ambiguity we see with indefinites, though; I don't discuss it here.

(1) The monkey has feet a bit like hands

To explain this, we need to first of all give an explanation for why the ambiguity comes about at all, and then for why it only comes about in certain cases. As far as the former is concerned, I encode it simply as an ambiguity of the head hosting the indefinite: it can introduce either an [Op:∃] or an [Op:GEN] feature. This is pretty much a formalization of the general notion in current Discourse Representation Theory that ∃ and GEN are just different ways of closing of DRSs. In Butler (2004b), I refer to the relevant level as a C(losure)P(hrase), and make exactly this claim with regard to the ambiguity of its interpretation. As it stands, simply stipulating this buys us the ambiguity, and we could leave it at that. One very good evidence that it is correct, however, comes from the interaction of subject interpretation and indvidual-level predication.

As noted, I assume that generic interpretations of QPs arise when the Clos head[26] is valued [Op:GEN]. Since, in the system developed here, Q heads receive their values from their intra-clausal equivalents, the corresponding clausal Clos head must also be valued [Op:GEN]. As in (11), I assume that the head in question sits on top of *v*P. The relevant structure is in (24).[27]

(24)



The other point about (24) is that the predicate is interpreted also as generic; basically as an ILP. The question is how this comes about. Somewhat along the lines of Chierchia (1995), I treat ILPs as arising from generic quantification over the situation variable of the verb, which like other variables I treat as an [ID:] feature, introduced, unsurprisingly, on V. This binding is indicated in (24) Since this [ID:] is introduced on V not N, it will range across things with the property denoted by V, i.e. situations rather than individuals. The notion is that the generic reading of the subject and the ILP reading of the predicate are derived by exactly the same process: closure of their associated variable by the clausal [Op:GEN] valued Clos head.

A common claim about indefinite subjects with ILPs (e.g. Diesing 1992; Kratzer 1995; Chierchia 1995) is that the subject can't reconstruct into its *v*P-internal base position, since in that case it should be able to receive an existential reading, and it can't. This argument is based on the assumption that indefinites get existential readings only when interpreted within *v*P, while generic and specific readings are associated with interpretation outside *v*P. To capture this, some stipulative claims have to be made about ILPs and subject positions: Diesing and

[26] I call it Clos rather than C here to avoid confusion with the standard C head; but see Butler (2004b) for arguments that it is in fact a C-level head.

[27] For concreteness, since the discussion in §5.2 wasn't especially conclusive, I continue to assume [*u*Q] features on the heads of QPs in (24).

Kratzer claim that the subject of an ILP is in fact generated outside the *v*P to begin with, and therefore reconstruction is by definition impossible. This requires them to attribute unusual (control-like) properties to T in ILPs. Chierchia on the other hand claims that the subject does originate inside *v*P, but that when it moves it leaves a special kind of trace that disallows reconstruction, quite distinct from traces in all other cases.

Here, I haven't made the assumption that indefinites can't be interpreted inside *v*P with ILPs: in fact, that is exactly where I take the subject to be interpreted in (24). However, the ILP is an ILP only because the clausal Clos head has a generic value, rather than existential. In that case, it is clear that the subject will be unable to receive an existential reading here, since it too is subject to closure by the same generically valued head. This deals with the lack of an existential reading in these cases straightforwardly, and since I deal with specific readings of indefinites in non-scopal terms, as in §6, nothing more needs to be said. This is much more elegant than the Diesing/Kratzer/Chierchia approach to ILP subjects, and it provides strong evidence for the posited lexical ∃/GEN ambiguity of the Clos head.

The second question is why this ambiguity in (23a) isn't available with all indefinites; again, the answer I give to this is pretty straightforward. I have taken the ambiguity to be lexical: that is, it is encoded on the head Clos (both in QP and in the clause). The point about the lack of ambiguity sometimes is that we find it with particular lexical realizations of Clos: e.g. *a* vs. *some* in (23). Depending how we think about morphology, we can, then, say either that *some* is lexically specified as [Op:∃], whereas *a* can receive either value; or that [Op:GEN] can never be realized as *some*, whereas both [Op:∃] and [Op:GEN] can be realized as *a*. Either way, the lack of ambiguity is captured simply in terms of the different specifications of the different lexical items; the same kind of lexical specification approach is taken by Chierchia (2001).[28]

8 Non-QP quantification

We already briefly saw in the previous section that positing intra-clausal quantificational heads can buy us more than just a particular way of encoding QP scope: there, it was taken also to be instrumental in deriving ILP interpretations of predicates, and their interactions with indefinite subjects. This in itself is a valuable result. Other data can also be explained in terms of such heads operating over variables introduced by elements other than QPs — in particular, over situation variables.

Firstly, we get a strong parallelism between D-quantification and A-quantification (Partee et al. 1987). A-quantification is standardly taken to be closure of situation variables by intra-clausal operators. Taking these operators to be the same ones that bind into QPs gives us a very

[28] Another instance of this approach to (lack of) ambiguity may be the English generic pronoun *one* or its German equivalent *Man*: we can treat this along similar lines, assuming that unlike other pronouns, it introduces an [ID:] feature lexically specifed as [ID:GEN] and therefore must be bound by an [Op:GEN], rather than by any other binder that might be available if it were a different pronoun.

nice reduction of theoretical apparatus. Taken alongside the more general suggestions offered here, we are able to reduce what have been considered three discrete quantificational processes — A-quantification, D-quantification (basically GQs), and ∃-closure — to a single, generalized process of closure by clausal heads.

One place where this is of particular value use is in the field of modality. A treatment of modality as quantification over possible situations (as in Portner 1992, building on the classical world-quantification analysis of Kratzer 1977, 1991, etc.[29]) is easy to capture here, since situation variables I take to be encoded by [ID:] features on V, as discussed briefly for ILPs in the previous section, and the quantification is provided directly by the clause structure. Relativizing the relation between situation variables and their binders to possibility (which we can do straightforwardly by adopting Kratzer's/Portner's semantics), we can then derive modal effects direct from the structures we already have.

A strong (necessity) modal then derives from a structure where a situation-related [ID:] associates with a clausal ∀ operator, giving us the classical 'all possible relevant situations (= worlds) are like s'; a weak (possibility) modal derives from a structure where a situation-related [ID:] associates with a clausal ∃ operator, giving us the classical 'some possible relevant situation (= world) is like s'. Subdistinctions within the basic necessity–possibility split can be aligned to independent syntactic or semantic distinctions, such as the position of the quantificational head in the clause, or the specification of just what situations are taken to be relevant for evaluating the modal claim. See Butler (2004b: ch.6) for discussion.

This approach to modality buys us numerous effects, both directly in terms of modality, and in terms of how modality so derived interacts with other scope bearing elements in the structure — QPs, negation, *wh*-elements — and with the interpretation of tense, finiteness, and (outer) aspect. For detailed discussion of these effects, see Butler (2003, 2004b,c,d).

9 Conclusion

I have shown that the commonly accepted method of dealing with indefinites in terms of closure by intra-clausal quantificational heads can be extended to all QPs with no loss of explanatory power, and considerable gain in terms of theoretical parsimony, with regard both to QP interpretation and, though with less discussion, to other quantificationally relevant phenomena, including I-level predication and modality.

The system proposed assumes that the Inclusiveness Condition (3) of Chomsky (2000) is correct, and therefore that indices proper aren't licit ways of encoding quantificational binding. It thus replaces indexing with a system of feature based Agree, which not only allows the effects standardly captured by indices to be retained, but does so in a considerably more elegant way, deriving, for example, selectivity directly from the way the system itself works.

[29]Portner moves from worlds to situations based on the notion put forward by Kratzer (1989) that worlds are just very big situations; or equivalently situations are just small worlds.

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