Phase structure, Phrase structure, and Quantification

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Abstract

I combine two areas of investigation in current syntactic literature: (1) the structure of the clause contains layers of hierarchically ordered quantificational heads, situated above the temporal and verbal fields (Beghelli & Stowell 1997); (2) the clause is built in PHASES — subclausal building blocks with parallel properties.

I claim these two threads should be tied together, in that phases should be defined in terms of such quantificational layers. Precisely, I claim a phase consists of some property denoting head H, topped by some associated 'little' head(s) h (as v over V) — this, the phase DOMAIN — surmounted by a CP layer — the phase EDGE — that encodes quantificational information to close off domain-internal variables. This derives a V phase, C $>$ v $>$ V (corresponding to the standard vP phase); a T phase, C $>$ t $>$ T, (the standard CP phase); also an N phase (DP/QP), C $>$ n $>$ N, where C = D. Treating phases in these terms derives the major facets of orthodox phase theory, but in a much more elegant, less stipulative way. Cyclicity, for example, is captured as an expected property of the system, rather than by the stipulated Phase Impenetrability Condition of Chomsky (2000).

Evidence for this reinterpretation of phase theory is adduced from:

(1) The interpretation of QPs, treated uniformly like Heim (1982) indefinites: so any QP introduces a restricted variable, subject to closure by intra-clausal quantificational heads analogous to Heim's $\exists$-closure operator.

(2) The structure and interpretation of temporal predicates, T/Perf/Prog, treated as embedding situation-denoting phases as internal argument, and introducing a situation variable, closed off by their own CP, as external argument.

(3) The interpretation and scope behaviour of modality, defined as quantification over possible situations: syntactically expressed as CP-/edge-level quantificational heads operating over domain-level situation variables.

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Preface

This thesis is basically an explication of how I think clauses are structured, and why. This is a pretty big subject, and I will be covering a lot of ground — in fact, I want to cover as much as possible, since the point I am trying to make is that a lot of facts about clausal architecture and the interpretation thereof can be explained by just a few principles concerning how predication/selection and quantification work.

It seems to me there are two ways to go about writing this kind of thesis. One is to go over the literature regarding each set of data — or really each theoretical claim about it — that I want to look at; then evaluate that claim, and show why the data is better dealt with some other way. In this way, the theory is justified largely by how well it compares to other competing theories. The other way is to start by setting out the theory, then to show everything it can do. In this way, the theory is justifed less by comparison with its competition, and more by its theoretical and empirical coverage. The idea is that a broad enough theory should to a certain extent justify itself, just by its broadness.

If theses in York were allowed to be, say, five times longer (and the time and funding available were scaled up equivalently), then the two methods might be compatible. But they're not. So I will go with the second method here, since I think this *is* the kind of theory that is justifiable by reference to its theoretical scope, covering as it does notions of quantification (over individuals and events), temporal, aspectual, and infinitival interpretation, modality, and more general notions such as selection, point of spellout, and the interface between the syntax and the semantics.

Sorry if you like the other kind.

Acknowledgements

To start at the start:

When I first came to York University, I was taught everything I ever learnt about how to do syntax and semantics by three people (since then, I've been making it up as I go along): whom being, alphabetically, David Adger, Siobhán Cottell, and George Tsoulas. A better triumvirate of first teachers in such matters surely cannot obtain anywhere, and if it weren't for them I *maybe* would have written one of those novels instead, but could equally well now be flipping (vege)burgers in a greasy spoon somewhere. I'm so very happy that I'm not. They have subsequently continued to be excellent supervisors and more generally people. Thank you kindly to them.

Very many thanks also go to Tim Stowell: this dissertation owes an obvious intellectual debt to his work, but even more to some most interesting virtual discussions about tense and things during its writing, and valuable feedback form his external examinership.

Three other major sources of academic influence need mentioning too, which being: (1) a discussion with Ian Roberts on the plausibility of the iterative clause structure proposed here, which pretty much set me off on the track I ended up following; (2) a very enlightening extended discussion with Liliane Haegeman on the same subject, with specific reference to exactly what goes exactly where in each of the levels, which set me thinking particularly about the best way of representing tense information in such structure; and (3) a discussion with Øystein Nilsen about whether my treatment of QP scope and scope interactions was adequate (it wasn't then; now I hope it is).

Non-academically, thanks to my office mate Gareth Walker, for his excellent taste in music (as I write, courtesy of one of his CDs which I have illicitly taken from his desk while he isn't here, Mississippi John Hurt is reporting quite beautifully on his glory since he laid his burden down — although I strongly suspect he isn't referring to finishing off a PhD thesis, I feel very much the same way), his skill in finding out how to do particular bits of LaTeXery, even if he didn't know himself, and just for generally being an amusing person to share an office with.

Also, thanks to everyone else in the department, good taste in music or otherwise, for making it such a good place to do a PhD (and a BA and an MA for that matter).

Back to LaTeXery: Donald E. Knuth and Leslie Lamport, and their followers, deserve obeisance of the highest order for creating such a perfect typesetting system. Having used an utterly enraging wysiwig processor for my MA, I know just what a difference it has made. Thanks to them, my thesis is beautiful; and just think — it could all have looked as silly as this with pretty much no extra effort. Astonishing! Not just that, though: using LaTeX has also had a surprisingly large and beneficial effect on the thesis organization and content. It honestly wouldn't have been anything like so good without it.

Without my mam and dad, I would not of course be here. Personally I think it's a good thing that I am, so thanks to them, and also to my brothers and sisters and nanas and so on, all for looking after me when I was little. It was a laugh, and there's not much better than that. My mam deserves special mention here as the degree in Linguistics she did those many moons ago undoubtedly influenced my doing one too; and she taught me while I was still in my critical period how great Chomsky was,* which I think is pretty crucial.

Last — but the word *least* could in no way be said to apply to them in any context — thank you to the wondrous Emma, Susan, Arthur, and baby Butler (to appear) for giving me such a good time when I'm not just sitting in an office thinking about stuff. I don't know where I'd be now without them, but even if it was right here, it wouldn't be anything like such a nice place.

(That *surely* can't be all? Sorry if I've forgotten anyone...)

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*We had a cat called Chomsky, who could get the lid off the biscuit tin with consummate ease, while our other cats sat and watched in admiration, then reaped the spoils. There's a metaphor there somewhere...

Author's Declaration

This thesis has not previously been accepted in substance for any degree and is not being concurrently submitted in candidature for any degree other than Doctor of Philosophy of the University of York. This thesis is the result of my own investigations, except where otherwise stated. Other sources are acknowledged by explicit references.

I hereby give consent for my thesis, if accepted, to be made available for photocopying and for inter-library loan, and for the title and summary to be made available to outside organizations.

Signed . . . . (candidate)

Date . . . .

In addition to presentation at a number of conferences, parts of the following parts of this thesis, or related pieces of work, have undergone previous dissemination: bits of chapter 3 have appeared as Butler (2004); bits of chapter 5 as Butler (to appear); elements of chapter 6 are based on Butler (2003a); Butler (2003a) in turn was based on Butler (2001); and §2.2.4 also appeared in a mildly different form in Butler (2001).

Chapter 1

Intro

Most broadly speaking, this thesis is about clause structure: what the structure of the clause is, and why it should be that way. It picks up on two major strands in the current syntactic literature.

The first, growing out of Chomsky (2000), is that a basic clause, whose structure is standardly taken to be something like (1), builds up derivationally in lumps,[1] or PHASES which are taken to be well-definable if not well-defined, and which are extensionally stated to be vP and CP. These phases are taken to derive various syntactic relations such as cyclicity (so that v and C are cyclic nodes), argument structure, etc. As it stands, the basic notion of the phase, and the basic characterization of phases as vP and CP, seem pretty stable and coherent. There are various questions of detail that aren't obviously dealt with once this level of generality is left: for example, should NegP in (1) be taken to be a low part of the CP phase or high part of the vP phase — or something else? How should we deal with aspectual auxiliaries? Assuming, quite uncontroversially, they project something like an AspP, again should this be considered part of the CP or the vP phase, or something else? Ditto for other auxiliaries (modals, copula *be*, periphrastic *do*). One aim of the dissertation is to deal with these kinds of questions concretely.

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[1]The basic idea is of course also statable in representational terms, so that rather than building up derivationally in lumps, the clause is made up of the same lumps without any reference to derivation. I have no strong feeling either way on this; for concreteness I happen to state things in terms of derivation here.

(1)

```
                 CP
            ╱         ╲
         Spec          C′
                   ╱        ╲
                  C           TP
                         ╱        ╲
                      Spec          T′
                                ╱       ╲
                               T         NegP
                                     ╱        ╲
                                  Spec          Neg′
                                            ╱        ╲
                                          Neg          vP
                                                   ╱       ╲
                                                Spec          v′
                                                          ╱       ╲
                                                         v          VP
                                                               ╱        ╲
                                                            Spec          V′
                                                                      ╱      ╲
                                                                     V        …
```
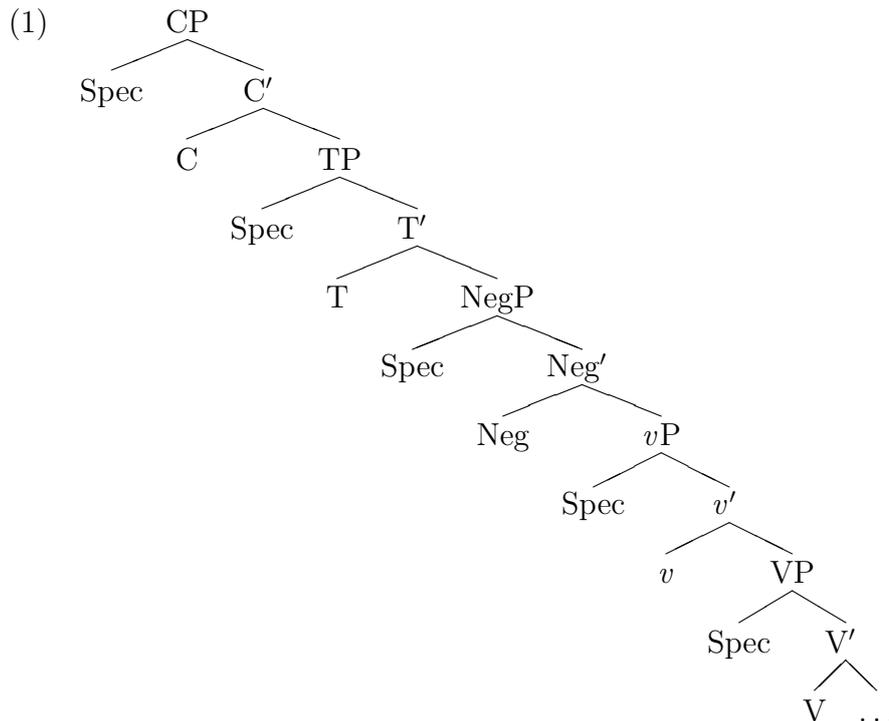
The second strand I pick up on can, and I argue should, be tied in to the first: it is the idea that the clause structure in (1) is actually too basic, in a particular way. A number of authors (Jayaseelan 2001; Starke 1993, 2001; Hallman 1997, 2000; Beghelli & Stowell 1997; Sportiche 2002; Belletti 2003; Jelinek & Carnie 2003; Brody & Szabolcsi 2003; Butler 2003a) have argued that some layer of functional structure that sits on the top of the clause is iterated lower down in the clause, at least on the top of the extended verbal layer (vP in (1)) (this will be discussed in detail in chapter 2). They identify this structure differently, and in different ways: Belletti and Jayaseelan identify elements akin to those posited to reside in a Rizzi (1997)-style extended CP, specifically Focus and Topic, so they propose that such a Rizzi CP appears internal to the clause, directly above the verbal layer. Starke makes a similar proposal, though he also identifies some broadly quantificational functions of the CP levels. Hallman identifies the iteration once by the general parallel A′ -properties the two layers display (Hallman 1997), and once, following the work of Sportiche, by similar quantificational properties associated with them (Hallman 2000). Beghelli & Stowell also look at quantification, and although they don't focus directly on the parallelism between the two quantificationally relevant areas of the clause they identify, it is certainly there to see. Jelinek & Carnie look at the interactions of case and specificity in ergative languages, and suggest that the iteration of the relevant layer of structure is considerably more than just above TP and vP. Brody & Szabolcsi, working on Hungarian along similar lines

to Beghelli & Stowell, also suggest the iteration is greater than this. Butler bases his claims on the behaviour of modality with regard to various other scope-bearing elements in the clause, arguing that it is elegantly captured under an iterative quantificational CP analysis. The major relevant claims of some of these analyses are reviewed in more detail in chapter 2.

As a few of the above authors note, if we take the idea that vP and TP are topped by a similar CP structure at face value, then we automatically derive a version of the phase-based analysis of the clause: the basic parallelism between the two phases falls directly out of their equivalent categorial status. The extensional definition of a phase, then, changes from CP and vP to just CP, which from a reductionist/minimalist point of view is clearly a nice outcome. If we also take into account the well-known DP–clause parallelism (Szabolcsi 1983, 1989, 1994; Siloni 1990; Cardinaletti & Starke 1999; Radford 2000; Bernstein 2001; Ogawa 2001; Pesetsky & Torrego 2001; etc.), then it becomes possible to claim that if these CP layers are tied in to quantification, as a number of the cited works above claim, then DPs are also really these same quantificational CPs. This suggests that DPs too are phases, a suggestion that has been floated (e.g. Chomsky 1999; though see Matushansky 2003a).

A second important feature of phase theory is that phases are taken to split into two distinct fields: a DOMAIN and an EDGE. The domain of a phase is its core: broadly, that lower part dealing with things like selection, predication, etc. The edge is the higher part, functioning largely as a space for movement. Again, this is not very well-defined in current work on phases. In the standard version of phase theory, the edge of the CP phase is C and its Spec(s), the edge of the vP phase is v and its Spec(s). Here, we can again reduce this so that the edge of a phase is the CP of a phase. This is interesting because I argue that the CP layer and the lower parts of the phase do fundamentally different things: the lower parts, i.e. the phase domain, deal with argument structure (verbal or temporal) and predication (chapters 4, 5). The CP layer, i.e. the edge, deals with quantification over elements introduced in the domain (chapters 3, 6). This allows us to define the notions of edge and domain much more precisely than they are ususally defined; it also captures the fact that the major diagnostics for (movement through) phase edges relate to quantification in one way or another: possibility of QR, possibility of reconstruction, possibility of parasitic gaps (see Legate 2003 for application of these diagnostics).

This, then, is what I set out to do here: to reduce the notion of the phase to this notion of manifold CPs, focusing on their quantificational nature. The fundamental idea is, as above, that the clause is made of a number of phases — CP elements with a predicative core. More precisely, a predicative Root; an l-syntactic structure on top of the Root encoding the situation (event) structure of that predicate, and any arguments, introduced as the specifiers of the l-syntax heads; and a (decomposed) quantification encoding CP that operates over variables introduced in its domain. This is also argued to be the structure for DPs, where D = C, but with uninterpretable quantificational features (chapter 3). (2) shows the general format, where H = a lexical head, and h its l-syntactic associated 'little' head (as 'little' v over V).

(2)   CP
         /   \
       C hP
              /   \
            h HP
                   /   \
                  H   ...
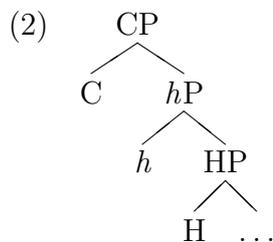
1.1   Motivation for phases

In this section, I examine two of the primary justifications given for the existence of phases. Before I do this, though, a note is in order: it has been observed in a few places that phases are taken to serve two distinct purposes in the derivation, though in general this is rarely taken to have much import if it is mentioned at all.

Their first purpose is straightforwardly a structure-building one: phases, i.e. vP and CP, are taken to be the basic syntactic 'building blocks' of the derivation, for whatever reason. This boils down to the claim that the lexical items v and C uniformly have particular effects on the 'dumb' syntactic derivation of the clause, deriving syntactic cyclicity/locality effects with regard to movement, agreement, etc.

The second function attributed to phases is more far reaching: they are taken to have a direct effect at the interfaces, in that not just syntactic structure building but also structure *interpretation* is taken to be cyclic: so we have the notion of cyclic spellout (cf. Epstein et al. 1998; Uriagereka 1999), relativized to the phase:

phases are taken to be the relevant elements for determining cyclicity in this domain also. This is explored at length in Chomsky (1999), and in much subsequent minimalist work. The distinction between the two is discussed in Atkinson (2000), where he labels the first, narrow-syntactic notion the LEXICAL SELECTION PHASE, and the second, more general notion the DERIVATIONAL PHASE. As these labels aren't particularly perspicuous, I will instead refer to syntactic vs. semantic (functions of) phases.

The distinction seems to be more than just a notional one, since it is quite generally assumed that what gets submitted to cyclic spellout, i.e. the semantic phase, is different from the syntactic phase. So when a phase is described as being sent to spellout, what is usually[2] meant is that actually the *domain* of that phase goes to spellout, with the edge remaining accessible till some later point.

As soon as this distinction is made, it becomes possible — and therefore (virtually) necessary — to ask whether it matters; that is, whether the two notions of phase should be considered distinct. Atkinson persuasively argues that they should, and to a certain extent when I return to this in chapter 7 I follow him, arguing that the second function in fact needn't be considered a particular property of phases at all.

However, in the literature generally, the two notions are tacitly to be taken to be the same: the syntactic phases that define the way the clause gets built are uniformly the same phases relevant for submission to spellout (even if the actual candidate for spellout is only a subpart). In discussion of standard assumptions below, then, I also adopt this assumption: I discuss further in §7.3.

The first standard justifications for phases that I go through below, relating to expletives, is largely a justification of syntactic phases; the second, to do with computational load, relates more to semantic phases. Perhaps surprisingly, I will argue that neither of them provides very strong evidence for phases, if we adopt certain assumptions that I will justify throughout this thesis.

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[2]Though not always; see Svenonius (2004) for some relevant discussion and references.

1.1.1   Expletives and Merge-over-Move

Standard phase-based minimalism assumes that clauses are built essentially in the following way: a lexical selection process selects a NUMERATION from the lexicon, this numeration being a set of lexical elements that will be used in building a structure.[3] A numeration is something like a very small, *ad hoc* lexicon, then.

Each numeration defines one clause; typically there will also be a series of small numerations, or SUBARRAYS, that define the phases of that clause as it builds up. The reason subarrays are posited involves examples like (3). Suppose we made a (super)numeration for the whole clause, and started building without phases. Once we get as far as (3a), the numeration will have reduced to (3b). Minimalism in general requires that simpler operations preclude more complex ones — saliently here, this means the availablity of Merge as an option in a particular case should preclude the possibility of Move, since Move is taken to be a more complex operation.[4] Under standard assumptions about expletives,

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[3]Whether this makes sense or not is a moot point. Certainly if we make sure we only select the exact elements we need to build a specific clause, this seems like an absolutely fundamental case of lookahead, something the minimalist program tries, and in many cases manages, to get rid of. On the other hand if we don't select numerations with some degree of intention regarding the final structure, this amounts pretty much to the 'generate and test' model of early GB, with selection of the numeration as essentially random, which seems outright implausible.

The notion of a numeration is supposed to reduce computational complexity; however, as with the discussion in the next section it isn't clear that it really does any work in this regard, nor that it really needs to. For the illustrative purposes of this section, though, I will proceed on the standard assumption that there is a numeration, and that it isn't random.

[4]This is because Move is taken to consist of Merge + something else. Whether it does, and whether this really matters, is again moot. Say we wanted to Move 'proofs' in (3a); as indeed we do if we want to end up with (3). Say, standardly, we aren't really 'moving' it, but just Merging a 'copy', which is to say another instantiation of the same lexical item. This copy isn't taken to be made during the derivation (this would violate Chomsky's inclusiveness condition (159)), so it must be in some way available in the numeration: it could just be there twice, or (this may or may not be distinct) it could be there once with an index to show how many times it is usable — so if we want it twice it would appear in the numeration initially as *proofs*[2], then when we had used it once, the index would decrease by one and we would have *proofs*[1]. That being so, Remerging *proofs* seems to amount to just the same thing as Merging anything else that is available in the numeration. Maybe the two instances of *proofs* would Agree, which might be deemed 'more complex' than pure Merge, at least if we consider it part of the same operation, but it seems to me this Agree relation would crucially have to happen *after* Merge, unless we allow Agree between elements already in the structure and elements still in the numeration, which is far from a standard assumption. Further, assuming a strictly local economy there is

then, the optimal next step would be to Merge *there* as [Spec, TP], giving us
(3c). While this is a perfectly licit step, it isn't the one we want to make; but
under the assumption that the whole supernumeration is available throughout
the derivation, it isn't one we can easily rule out.

(3)   There is a possibility that proofs will be discovered

    a. will be discovered proofs

    b. {there, is, a, possibility, that}

    c. there will be discovered proofs

If we work on the basis that the derivation is chunked into phases, however, we
can avoid this problem.[5] The embedded verb in (3) is passive, and passive v by
assumption isn't a phase defining head (actually it doesn't matter if it is in this
case or not), so the first phase in (3) will be built of the subarray (4a). If this is
so, then the only element suitable for Merge at the stage (3a) will be the second
occurrence of *proofs*. We end up with (4b), and then the next subarray can build
on this, with the expletive only being an issue in the phase where the relevant
subarray makes it available.

(4)   a. {that, will, be, discovered, proofs^2}

    b. that proofs will be discovered <proofs>

Apart from the problems with some of the general aspects of the account here
flagged up in the footnotes, there is another one which is that it rests entirely on
the particular treatment of expletives adopted. That there are problems with this
general treatment of expletives has long been recognized, and in §4.3.1 I will show,
following Higginbotham (1987); Stowell (1991); Ramchand (1996); Felser & Rupp
(2001), that an alternative treatment of expletives is available and well-motivated,
where they are treated not as non-semantic place-fillers, but overt instantiations

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no reason why the syntax should be able to see a difference between Merge (with subsequent
Agree) and Merge (without). If it could see a difference, and it mattered in terms of economy,
then exactly the same considerations should apply to the first occurrence of *proofs*: it is being
Merged into the structure with an Agree relation predetermined. This means we shouldn't be
able even to Merge this first occurrence, if we have any other suitable candidate for Merge in
the numeration. Again, I leave this aside here and go with the orthodox flow for expository
purposes.

[5]We retain the problems alluded to in the previous footnotes, though.

of Kratzer's (1995) event argument. Once this analysis of expletives is brought into account, we simply don't run into problems regarding whether to Merge an expletive or Move an argument, with or without phases. Under such a treatment, *there* will be Merged in the higher phase not because it is only made available in a sub-numeration at that point, but because it is the event argument of the higher phase's verb. This means that as far as expletive insertion goes, we don't need to have numerations at all in this system, sub or super[6] — we could in fact have constant lexical access, which intuitively seems reasonable. It is important to note here that just because this argument for (syntactic) phases doesn't run once we modify our assumptions, that *isn't* in itself an argument against phases: phases could still exist independently of any such considerations, whether they be the syntactic or the semantic variety.

1.1.2   Computational load

A justification for phases often floated is the intuitively appealing one that building up derivations in discrete smaller chunks like this lessens computational load, in the sense that once we have submitted a phase to spellout, the grammar can 'forget' about it, under some understanding of that phrase, and get on with the next stage of the derivation. Phases supposedly lessen what we have to deal with in our derivational workspace, then. In fact, it isn't clear that this is a very good argument after all. First of all, even assuming that spellout is phasal-cyclic, and so is able to reduce the load in the way described, as noted by Matushansky (2003a) it isn't obvious that it is very helpful, since it doesn't necessarily reduce the load very much; in some cases it seems not at all.

One example of this Matushansky gives is adjunction: within any given phase, we can in principle continue adding adjuncts indefinitely. The notion of phasal-cyclic spellout offers no way to stop us from doing so. Of course, at some point we become incomprehensible if we actually try to do this, but it isn't clear that phase theory has anything useful or interesting to say about that: all it shows us is that at some point we become unable to build/spellout/process certain elements, with or without phase theory trying to lessen our load. This is not a knock-down argument by any means, but it is puzzling why a system that has a

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[6]Though this doesn't mean we couldn't, and if some other evidence were produced that we really do, that would be equally compatible with the assumptions I make.

way of lessening computational load doesn't have a way of implementing it when it does actually seem useful to do so.

Another example, more damaging for the idea at issue, is raising. Just as it is in principle possible to adjoin indefinitely, it is also possible to embed raising infinitives indefinitely. Under standard assumptions, clauses out of which there is raising aren't phases; raising verbs shouldn't be either.[7] This being so, a clause like (5) consists of only two phases: the most deeply embedded vP and the matrix CP. Again under standard assumptions, (the domain of) a phase is sent to spellout once the next phase up is complete. This means nothing will be sent to spellout in this case till the matrix CP is built. There is thus no reduction of computational load in these cases, and yet they don't seem any more difficult to produce/process than examples with equivalent levels of embedding where we do assume we have phases, such as (6).

(5)   [$_{CP}$ Emma [$_{v$P}$ seems [$_{TP}$ to [$_{v$P}$ appear [$_{TP}$ to [$_{v$P}$ be unlikely [$_{TP}$ to [$_{v$P}$ happen [$_{TP}$ to [$_{v$P}$ arrive soon]]]]]]]]]]

(6)   [$_{CP}$ I [$_{v$P}$ said [$_{CP}$ that Susan [$_{v$P}$ expected [$_{CP}$ that Arthur [$_{v$P}$ thought [$_{CP}$ that Emma would [$_{v$P}$ need [$_{CP}$ to [$_{v$P}$ arrive soon]]]]]]]]]]

Additionally, as Chomsky (2000: 111) notes, it isn't clear why it should be true that computational complexity matters for a cognitive system anyway. If it doesn't, then this motivation for phases isn't really a motivation at all; if it does, and the system has a way of dealing with that — i.e. phases — then it is a reasonable question to ask why it isn't more efficient in this regard, i.e. why it allows in the kind of recursion discussed.

1.1.3   Phases anyway

The above two considerations are frequently cited as evidence for the existence of phases. However, neither is as straightforward as it seems, and even if we take them at face value there are still some poorly defined aspects that require elaboration.

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[7]Chomsky defines two kinds of little v: one, v^*, is exemplified by transitive or experiencer v and defines a phase; the other, v without a star, is exemplified by passive or unaccusative v, and doesn't define a phase. Raising verbs fall squarely into the latter class.

As noted, though, this doesn't count as an argument against the existence of phases. Phases as a notion are perfectly compatible with a number of formalizations, whether these are based on phased numerations or constant lexical access, cyclic spellout or single spellout, low or high computational load, etc. Phase theory has been productive in areas as diverse as cyclic *wh*-movement (McCloskey 2000); QR and scrambling (Miyagawa 2003); mixed agreement in European Portugese (Costa & Pereira 2003); semitic root phenomena (Arad 2003); sentential stress patterns (Adger 2003b; Kahnemuyipour 2003); linearization and phonological domains (Wagner 2003); parsing and processing (Mulders 2003). I don't wish to argue against this degree of diverse productivity, and it will become obvious from the rest of this thesis that I am taking the existence of phases as pretty much a given, though the definition I adopt will be a non-standard one.

In chapter 7 I will come back to phase theory, and examine the extent to which it is plausible to replace the standard theory with the theory I will set up in in the following chapters. It will be demonstrated that I think the most important factors of the standard theory can be retained (e.g. derived cyclicity, vP–CP(–DP) parallelism), perhaps by different mechanisms; whereas the problematic aspects will be remedied in the alternative conception I give.

1.2   The structure of the thesis

Chapter 2 is a literature review, going over a number of the claims that exist in the literature relating to the claim that levels of functional structure identifiable with CP iterate in the clause, so that at least two CPs obtain in a basic clause: one, the standard one, above TP, and another above vP.

In chapter 3, I provide an analysis of quantification based on the general manifold CP idea. I draw a distinction between the clausal CPs and the same structure on top of nominals (i.e. DP structure), namely that the clausal CPs introduce semantically interpretable quantificational features into the representation, whereas nominal CPs introduce semantically uninterpretable quantificational features. Nominal CPs, then, are interpreted pretty much as if they were NPs, i.e. like indefinites.

I introduce a feature-based approach to existential closure over indefinites (Heim 1982; Diesing 1992), based on agreement between the uninterpretable quantifica-

tional features in the CP of the nominal phase and the interpretable equivalents in the clausal CPs. I then show that in fact this treatment extends to every DP/QP: indefinites are existentially or generically closed, universals are universally closed, etc. Second-order quantification can be dealt with under the assumption that second-order QPs are more complex, essentially partitive, structures.

A system for dealing with predicative dependencies, based on Adger & Ramchand (to appear), is also introduced, in order to deal with DP movement.

Whereas chapter 3 deals largely with the edge part of the CP phases, chapter 4 deals with the internal part, i.e. the domain. I examine argument and situation structure, proposing a theory where arguments act as binders of variables introduced by Root categories like V; I treat variables in terms of syntactic features (cf. Adger & Ramchand to appear), variable-binding in terms of valuation of those features. One of the variables the verb introduces, I take to be a situation variable (Davidson 1967; Kratzer 1995; etc.), and I argue that the standardly assumed existential closure over this variable is to be represented as quantification of it by the same existential operator in the verbal phase's CP that closes off indefinite nominals. I further argue that the tense-based phase introduces a similar variable, and that these two variables are temporally ordered by T to give us something like the Event time and Speech time respectively. Details of this are left till chapter 5, but a prediction with regard to the interpretive status of the verbal phase versus the tense phase as situational versus propositional is considered with respect to some interpretive ambiguities of negation and adverbs.

Chapter 5 goes in detail through an analysis of temporal relations based on the treatment of phases and clauses. Tense is argued to be a temporal predicate that orders pairs of situations in terms of the temporal relations between them, pretty much along the lines of Stowell (1996). I show how this kind of analysis falls straight out of the theory presented.

I then go on to an analysis of outer or viewpoint aspectual notions, i.e. perfect and progressive. Following Stowell (1996); Demirdache & Uribe-Etxebarria (2001), I argue that these should be treated analogously to tense, as dyadic temporal predicates ordering pairs of situations. Because of how the system is set up, it is necessary to treat perfect and progressive auxiliaries as introducing their own CPs, i.e. more phases, into the structure: so a clause with one aspectual auxiliary will have three phases instead of two; a clause with two aspectual auxiliaries will have four phases. This chapter offers some evidence based on negation for the re-

ality of these additional CPs, but delays some further, more decisive evidence, till the following chapter after some necessary extra assumptions regarding modality have been introduced.

I then move on to finiteness and infinitives, arguing that infinitives in this system can be profitably viewed as simply lacking the tense phase, so that (non-)finiteness can be straightforwardly equated to the presence versus absence of tense. I examine possible objections to this departure from more standard views based on propositionality facts, the claim of Stowell (1982) that at least some infinitives display some level of tense information, and apparent EPP effects in infinitives. Again, further evidence supporting this view, regarding modality, is delayed till the next chapter.

Chapter 6 deals with modality, arguing that modals can be treated as quantifiers over possible situations (Portner 1992, adapting the quantification over possible worlds analysis of Kratzer 1981, 1991). I instantiate this in terms of the quantificational operators of the CP levels quantifying over the situation variables introduced in their domain, with a relativization to possibility. I show that the well-discussed distinction between root and epistemic readings for modals is easily captured under this system in terms of scope: epistemic-interpreted modals are those scoping in the CP of the tense phase, and so over a proposition, and root-interpred modals are those scoping in the CP of the verbal phase, and so over a non-propositional situation.

I further show that modality can also scope in the CPs of the aspectual phases introduced in the previous chapter, providing strong evidence for the existence of those CPs. The evidence is based on the interaction of the modality with regard to the temporal information provided by the tense and aspect heads, following work by Condoravdi (2001); Stowell (2004). I show also that modality is available in infinitives, as it is predicted to be under this framework, and that the interpretation it receives in such non-finite contexts is also as predicted.

In chapter 7 I go back to the general concept of phases, examining to what extent the theory presented here fits in with the standard phase theory. I argue that we can replace the standard story with the one here with no real loss and substantial gain. The gain is of several types:

1. Precision: as noted in the introduction to this chapter, numerous elements of the standard theory are left vague. The approach presented here presents

a very exact definition of what a phase is, and so it is unable to leave these questions vague.

2. Coherence: phases in the general literature tend to be defined extensionally; any intensional definitions are largely incoherent (e.g. Chomsky's 1999 definition in terms of propositionality falls down under inspection — see chapter 4). Here, phases all fall into a single coherent definition based on domains of quantification.

3. Explanation: why do we have phases anyway? Chomsky's idea that they allow us to reduce the computational load doesn't really hold up, given that, say, we can add adjuncts, or embed raising verbs, not strong phases in the standard theory, basically indefinitely. Here, phases are required because it is necessary to have a means of closing off variables introduced by predicative categories before those categories can be selected as arguments by other predicates. For example, T needs to make reference to V's situation variable and its own to predicate any relation between them. To have reference made to them, they need to be referential, so they need to be closed off.

I also look more generally at the distinctions between how cyclicity and spellout (can) work in the standard system as compared to the system here.

Chapter 8 is a conclusion.

Chapter 2

CP Levels

(7) shows what we might reasonably refer to as the 'standard' structure for the simple clause within the generative framework; a structure that has been assumed with some stability for a good decade or so.

(7)

```
              CP
           ／      ＼
        Spec        C′
                ／      ＼
              C          TP
                     ／      ＼
                  Spec        T′
                          ／      ＼
                        T          NegP
                               ／      ＼
                            Spec        Neg′
                                    ／      ＼
                                  Neg        vP
                                         ／      ＼
                                      Spec        v′
                                              ／      ＼
                                            v          VP
                                                   ／      ＼
                                                Spec        V′
                                                        ／  ＼
                                                      V     …
```
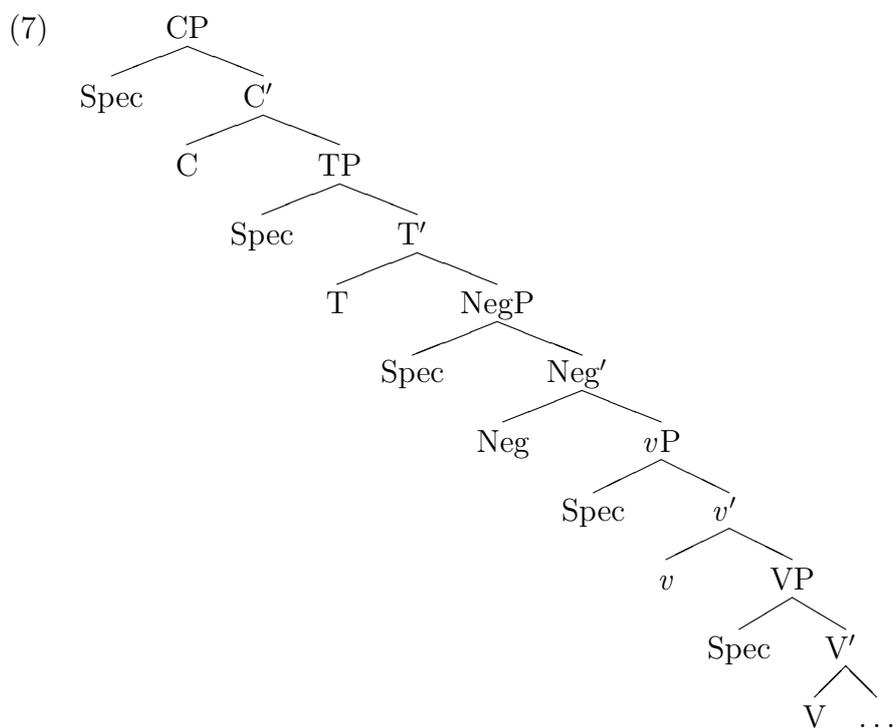
We have seen, of course, slightly different labelling conventions; the occasional expansion of particular nodes into more articulated forms (and subsequent reduction); addition/removal of specifier positions; possible ordering parameterizations; and the like — but essentially the skeleton of the clause has been taken to be

formed of a verbal (v/V) layer, topped by an inflectional layer, topped by a CP layer. However, there have been proposals floating around the literature since at least Starke (1993) that this structure should be revised along more radical lines than just relabelling, or exploding, say, one dual-role projection into two single-role projections. Some of these proposals are largely theoretically based, others more empirically, but the fundamental idea they all have in common is that some piece, or aspect, of the structure that sits on top of TP also sits further down, on top of vP. Since the structure that sits on top of TP in the standard clause is CP, many of the proposals identify the lower structure as a CP too, so that a simple clause has (at least) two CPs — the standard over TP, and another over vP, as in (8).

(8)

```
        CP
       /  \
      C    TP
          /  \
         T    CP
             /  \
            C    vP
                /  \
               v    VP
                    /\
                   ....
```
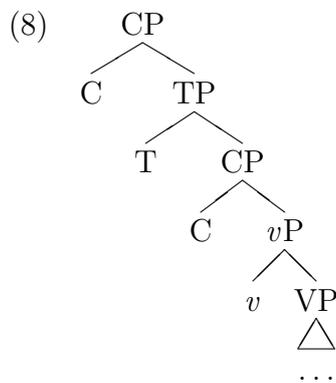
A number of the proposals also relate to quantificational information: they identify these iterated layers of structure with — to simplify their claims unforgivably — what are basically ordered series of quantificational syntactic heads. What we have, then, is a structure where quantificational information is syntactically interspersed throughout the clause, encoded through CP-level heads.[1]

This chapter will review some of the relevant proposals that have been made, with a view to extracting from them a coherent synthesis; subsequent chapters will show that the kind of structure under consideration can be used to account for a lot more than it might seem to.

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[1]NB: this is a generalization that anticipates later conclusions of this thesis, rather than reflecting directly the conclusions of all the work to be discussed.

2.1 IP-internal CPs

Belletti (2001, 2003) and Jayaseelan (2001) both argue for a Rizzi (1997)-style CP internal to the clause; as Belletti puts it, an 'internal CP'. Their arguments, much like the arguments Rizzi appeals to in his discussion of CP, are to do with topicalization and focalization facts cross-linguistically: Belletti deals mainly with Italian, Jayaseelan mainly with Malayalam and English.

2.1.1 IP-internal CPs in Romance: Belletti

Belletti's discussion is based around the phenomenon of 'Free (Subject) Inversion' (FI), common in Romance null-subject languages (NSLs). Free Subject Inversion structures are so-called because the subject may freely appear postverbally (9a–9b) as well as preverbally (in contrast with Stylistic Inversion as found in e.g. French, a non-null-subject Romance language, which as Belletti notes (p.3)[2] 're-quire[s] a "trigger" for inversion (wh, subjunctive...)').

(9) a. Ha parlato Gianni
         has spoken Gianni

         'Gianni spoke/has spoken'

     b. Il   giorno in cui   ha parlato Gianni
         the day   in which has spoken Gianni

         'The day when Gianni spoke/has spoken'

     (Belletti (2003): examples (2a,g))

Leaving aside the nature of the 'null' subject in NSLs (i.e. the argument whether we have *pro*, as commonly assumed, or just nothing, as in e.g. Alexiadou & Anagnostopoulou 1998), the question Belletti first addresses is whether the overt subject is high in the structure, as in Kayne & Pollock's (2001) remnant movement analysis of Stylistic Inversion in French (where the subject moves into the external periphery and then the remnant IP moves over it even higher), or low in the structure, with the verb having moved to some IP head, as in more traditional analyses.

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[2]Unless otherwise noted, references are to Belletti (2003).

Belletti notes that the postverbal Italian subject follows 'low' adverbs such as *completemente, bene*[3] (10 vs. 11). This is compatible with either a remnant movement analysis of FI or a more traditional analysis with the subject low. She also notes, however, that further material — specifically, a PP complement to the verb — may follow the subject (10a–10d). Although a remnant movement analysis could in principle deal with this, by moving both the subject and the PP to separate external peripheral positions, and then the remnant IP higher, there doesn't seem to be any semantic or prosodic reason for such a series of movements (in contrast to the French SI cases Kayne & Pollock discuss, where as noted a semantic trigger for movement is required).

(10)   a.   ?Capirà   completamente Maria
            will-understand completely   Maria
            'Maria will understand completely'

       b.   ?Spiegherà completamente Maria al diretorre
            will-explain completely   Maria to the-director
            'Maria will explain fully to the director'

       c.   ?Capirà   / spiegherà   bene Maria (al diretorre)
            will-understand / will-explain well Maria (to the-director)
            'Maria will understand/explain (to the director) well'

       d.   Capirà   / spiegherà   tutto   Maria (al diretorre)
            will-understand / will-explain everything Maria (to the-director)
            'Maria will understand/explain (to the director) everything'

(11)   a.   *Capirà   / spiegherà   Maria completamente (al
            will-understand / will-explain Maria completely   (to
            diretorre)
            the-director)

       b.   *Capirà   / spiegherà   Maria bene (al diretorre)
            will-understand / will-explain Maria well (to the-director)

       c.   *Capirà   / spiegherà   Maria tutto   (al diretorre)
            will-understand / will-explain Maria everything (to the-director)
       (Belletti 2003: examples (3–4))

Belletti also provides evidence for the low position of postverbal subjects in FI from NPI data.

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[3]See Cinque (1999) for the hierarchic ordering of adverbs in Italian (and in general).

(12)   a.   Non   ho   incontrato che   (i/dei)   linguisti
             not   have seen   'that' (the/some) linguists
             'I have not seen any linguists'

       b.   Non hanno parlato che   (i/dei)   linguisti
            not have   spoken 'that' (the/some) linguists
            'No linguists have spoken'

       c.   *Che (i/dei)   linguisti non hanno parlato
            'that' (the/some) linguists not have   spoken

(13)   a.   Non conosco alcun linguista
            not know   any   linguist
            'I do not know any linguist'

       b.   Non parlerà alcun linguista
            not speak   any   linguist
            'No linguist will speak'

       c.   *Alcun linguista non parlerà
            any   linguist   not speak

(14)   a.   Non ho   vista nessuno
            not have seen nobody
            'I have not seen anybody'

       b.   Non ha   parlato nessuno
            not has spoken nobody
            'Nobody has spoken'

       c.   *Nessuno non ha   parlo
            nobody   not has spoken

Each of the (a) examples in (12–13) has a direct object NPI that must be licensed under c-command by the negative element *non*. In the (b) examples, we find the same thing — a postverbal NPI, licensed, and therefore c-commanded, by *non*, but in this case the postverbal NPI is the subject. In the (c) examples, we have a preverbal subject NPI which *isn't* licensed/c-commanded by *non*. A remnant movement analysis like that considered above — that is, a high subject and an even higher IP remnant hosting *non* — would have to go to some trouble to establish the c-command relations that would capture these data, and moreover would have to ascribe an extraordinarily high position to the preverbal subject. On the other hand, a treatment where the postverbal subject remains low, a preverbal subject moves up to the edge of IP, and *non* doesn't go anywhere, has no trouble in accounting for the data.

Given the conclusion that the postverbal subject is in a low position, and the rather obvious assumption that the preverbal subject is in a somewhat higher position, Belletti notes an interesting contrast between the respective interpretations the subject receives in each of the two positions. She notes that a postverbal subject neutrally receives an interpretation where it has new information focus (15b), as opposed to the preverbal subject which can't felicitously receive such a reading (15c); the postverbal subject may also be interpreted as a topic, with suitable intonation and pragmatic conditions (16).

(15)   a. Chi è partito / Chi ha parlato / Che cosa è successo?

        'Who left / Who spoke / What happened?'

     b. E' partito / ha parlato Gianni
        has left   / has spoken Gianni

     c. #Gianni è   partito / ha parlato
        Gianni   has left   / has spoken

(16)   a. Che cosa ha poi fatto Gianni?

        'What has Gianni finally done?'

     b. Ha (poi)   parlato, Gianni
        has (finally) spoken, Gianni

    (Belletti 2003: examples (9–11))

Belletti follows Rizzi (1997) in viewing focused and topicalized elements as being associated with specific syntactic CP-level Focus and Topic projections. Rizzi, of course, analyses the standard left-peripheral CP in these terms; Belletti points out, though, that since the subject in her examples occupies a low structural position, we are led to postulate a clause internal 'periphery', with similar Focus/Topic projections contained in it.

Belletti further shows that the 'internal' and 'external' Focus projections can be empirically distinguished, on prosodic and semantic grounds. As is well known, the left peripheral Focus position identified by Rizzi is associated with a particular focal stress pattern. This is not the case with the internal focus position, which isn't associated with any particular stress marking. Moreover, the stress marking found on external focused elements corresponds not to the new information reading of postverbal subjects, but rather to a contrastive reading. So in (17–18), the left peripheral, stressed, focus elements are not felicitous in neutral answers to information questions — contrast (15).

(17)   a. Chi è partito / ha parlato?

'Who has left / spoken?'

b. #Gianni è partito / ha parlato

'Gianni has left/spoken'

(18)   a. Che cosa hai letto?

'What have you read?'

b. #Il libro ho letto (non il giornale)

'The book I have read (not the newspaper)'

What Belletti's data give us, then, is syntactic, semantic, and phonological evidence for a secondary CP layer internal to the clause, above the verb phrase.

2.1.2   IP-internal CPs in Malayalam and Germanic: Jayaseelan

Jayaseelan (2001) examines data from Malayalam and various Germanic languages, and arrives at essentially the same conclusion as Belletti: that there are Focus and Topic projections dominating vP.

Jayaseelan's initial observation is that in Malayalam *wh*- questions, there is a strict requirement that the *wh*- word be immediately left-adjacent to the verb (19–23).

(19)   a. ninn-e   aarə aTiccu?
       you-acc who beat-past
       'Who beat you?'

b. *aarə ninn-e aTiccu?

(20)   a. iwiTe aarə uNTə?
       here   who is
       'Who is here?'

b. *aarə iwiTe uNTə?

(21)   a. awan ewiTe pooyi?
       he   where went
       'Where did he go?'

b. *ewiTe awan pooyi?

(22) a. nii aa   pustakam aar-kkə koDuttu?
          you that book   who-dat gave
          'To whom did you give that book?'

      b. *nii aar-kkə aa pustakam koDuttu?

(23) a. nii entə aaNə tinn-atə?
          you what is   ate-nominalizer
          'What is it that you ate?'

      b. *entə nii aaNə tinn-atə?

      (Jayaseelan 2001: 40)

Malayalam is canonically SOV. It can be seen in the above examples, though, that this requirement for *wh-* word/verb adjacency requirement overrides the canonical word order, the *wh-* subject intervening between O and V.

Jayaseelan assumes a universal SVO base order; he takes the canonical SOV order displayed by Malayalam to be derived via movement of the subject and object to higher functional projections: the subject to [Spec, TP], the object to some position above *v*P but below TP. This being so, one possible analysis of the examples above would be to say that where we have a *wh-* subject it simply stays where it is. However, this can't be right, since it isn't just *wh-* subjects that have to be contiguous to the verb but any *wh-* word. Most tellingly, where a clause contains multiple *wh-* words, we find them stacked up together as in (24).

(24) ii   kaaryam aarə aar-ooDə eppooL paRaññu ennə eni-k'k'ə aRiy-illa
          this matter   who whom-to when   said   COMP I-DAT   know-neg
          'I don't know who told this matter to whom, when'

This looks very much like a case not of non-movement, but a case of multiple movement as we find for *wh-* words in e.g. Polish — the difference being that Malayalam has multiple *wh-* words not at the left periphery of the clause but more like the left periphery of the verbal complex.[4] Jayaseelan therefore makes the straightforward assumption that exactly the same syntactic process derives the Malayalam cases in (19–23), only rather than moving to the left peripheral FocP of Rizzi (1997) to check their [WH] features, the Malayalam *wh-* words are moving to a clause internal FocP.

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[4]Cf. Brody (1990) for similar facts in Hungarian.

One outcome of this analysis that Jayaseelan notes is that the successive cyclic movement of *wh*-phrases through the Specs of *v*P and CP (the phase edges of Chomsky 1999) becomes unified as a generalized edge–edge movement, clearly a theoretically desirable result.[5]

2.1.2.1 Extensions to English

Jayaseelan also notes a couple of constructions in English which can be usefully analysed using the proposed structure.

He suggests a structure for English clefts whereby the focalized element sits in the internal [Spec, FocP], as in (25).[6]

(25)   a. It was Mary that I saw

      b.

               TP

        it   T$'$

             was$_i$ FocP

                 Mary$_j$ Foc$'$

                         t$_i$ *v*P

                                t$_i$ CP

                                     that I saw t$_j$

      (Jayaseelan 2001: 63)

Jayaseelan also proposes that his internal FocP can be used to explain pseudo-gapping data, as in (26).

(26)   a. Mary hasn't dated Bill, but she has Harry

      b. Speaker A: has she dated Bill?
          Speaker B: *Yes, she has Bill

---

[5]Jayaseelan actually generalizes it to a Foc–Foc movement; I translate this here to a framework that includes phases and the notion of the edge.

[6]Jayaseelan does note that it isn't entirely clear where the cleft focus moves *from* in this story.

    c. Speaker A: I've never seen you on campus before
        Speaker B: Neither have I you

    d. Neither have I [_{FocP} you [_{VP} ~~seen~~ ~~\<you\>~~ ~~on campus before~~]]

Jayaseelan notes that the 'remnant' left by pseudogapping must bear contrastive focus. If contrastive focus isn't possible in the context, as in (26b), then pseudogapping isn't grammatical. He also notes that the remnant can come from the middle of the deleted VP, as in (26d). These two facts taken together naturally suggest a focus-related movement by the remnant out of VP, followed by VP deletion, or somesuch operation. Jayaseelan's internal FocP elegantly derives such an analysis.

He notes that in all of the cases he discusses, the more standard analyses of positing an outer Spec of vP, or an object agreement phrase, or any similar story, do not capture the data as well, in that in each case (*wh*-, clefts, pseudogapping), focus plays a prominent role, easily stated under the FocP analysis, not so easily under others.

2.2   Intra-clausal quantificational heads

Other authors make different kinds arguments for the type of intra-clausal recursion I posit in this thesis, based on quantificational facts. Though not all the authors tie their quantificational recursion into CP recursion, my claim will be that these two things should be tied together.

2.2.1   DPs, NPs, and scope: Sportiche, Hallman

Sportiche in a number of places (Sportiche 1995, 1999, 2002) suggests that CP layers are more prolific than usually supposed; certain of his claims are followed up by Hallman (2000).

2.2.2   CP recursion and quantification: Sportiche

2.2.2.1   CPs and small clauses

Sportiche (1995) looks at the behaviour of the French predicate clitic *le*. 'Predicate clitic', as Sportiche shows, may be something of a misnomer for this item as it is able to pronominalize a whole array of categories, not all of which are straightforwardly dealable with as predicates: PP, infinitival CP, NP, AP, adjectival participial, verbal participial (27).

(27)   Louis l'a   été, {en colère / à plaindre / professeur / fidèle   à ses
       Louis it-has been {in a-rage / to pity   / professor / faithful to his
       amis   / adoré de ses enfants / trahi   par ses amis}
       friends / adored by his children / betrayed by his friends}

Sportiche goes through the pronominalization of each of these categories by *le* and shows that, actually, we often have to allow that *le* is doing a lot more than it seems. For example, where we do clearly have a predicate, such as AP in (28), *le* must be standing not just for AP but also (at least) for degree modifiers such as *très* 'very' or *si* 'so', which he takes to be introduced by a Degree Phrase (DegP).

(28)   a. *Louis {l'est très / le devient très / le reste   très}, fidèle
          Louis {it-is very / it becomes very / it remains very}, faithful

        b. Louis {l'est / le devient / le reste},   très fidèle
          Louis {it-is / it becomes / it remains}, very faithful

Sportiche goes through a number of other examples showing a similar thing: often, *le* must be pronominalizing something bigger than it seems to be at first sight. His claim then is that if we want to reduce the behaviour of *le* as much as possible, as seems reasonable, we can treat *le* as a clitic for CPs. Obviously this will deal with the cases where it stands in for an infinitival CP, as in (28); it also deals with other cases where *le* clearly stands in for what is unambiguously a CP, as when it pronominalizes the complements of verbs expressing modal notions such as *devoir* 'must', *pouvoir* 'may/can', *se demander* 'wonder', which obligatorily take clausal complements (29).

(29)   Marie le doit / Marie le pourrait / Marie se le demande

       'Marie must it / Marie can it / Marie wonders it'

Cases where *le* seems to pronominalize something not a CP, Sportiche argues, are simply dealt with by assuming that there are parts of the CP we don't see,[7] though some of them may be detected as with DegP in (28).

This has several consequences for clause structure, the one to note here being that in addition to the standard CP, we end up needing a CP structure over all the categories *le* seems to pronominalize, which is essentially every predicative category. This then leads to a treatment of small clauses where they are these same predicate-based CP categories (cf. Kayne 1993; Starke 1995). One of the most straightforard outcomes of this is that it allows an analysis of non-like conjunctions such as (30): rather than treating this as conjunction of an AP and a PP, we can treat it as conjunction of two small clause CPs.

(30)   Louis est [[triste] et   [en colère]]
       Louis is   sad   and in a-rage

Most relevantly here, he furthermore notes that if we accept Stowell's (1996) version of the view of tense as a temporal predicate — Sportiche goes so far as to actually ascribe it the category V, positing P as a possible alternative — 'the structure of a simple clause changes from [(31a)] to [(31b)], with the first AGR corresponding to AGR_S, and the second to AGR_O' (Sportiche 1995: 311); this ties in with previous work of his (Sportiche 1994) where he suggested the same kind of structure based on the idea that it allows cyclic *wh*-movement to be treated as exclusively [Spec, CP]–[Spec, CP]–[Spec, CP], rather than [Spec, CP]–[Spec,*v*P]–[Spec, CP] (cf. Jayaseelan's similar observation in §2.1.2).

(31)   a. [$_{CP}$ C AGR_S T AGR_O [$_{VP}$ V ...
       b. [$_{CP}$ C [AGR [$_{VP}$ V [$_{CP}$ C [AGR [$_{VP}$ V ...

A very similar view to this of the role of tense in the clause will be pursued in detail in chapter 5.

Sportiche further posits that if this view is taken as far as it can be, we also have to decompose VP shells along similar lines — so that (these are not his terms) in an l-syntactic *v*P complex, not only V must project a full CP, but so must *v*, and if we want to decompose any further, any other V/*v*-level head. *v*, on this

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[7]As is in fact standardly assumed for root clauses in English, say.

view, will mean something like, say, CAUSE, and will be just as much a predicate as whatever V happens to be. Sportiche doesn't go into this in a great deal of detail, but exactly such a level of decomposition is argued for in Hallman (2000) (see §2.2.3), and in Jelinek & Carnie (2003); and pretty much implied in Brody & Szabolcsi (2003) (see §2.2.5).

2.2.2.2   Definiteness and quantification

In a couple of presentations based largely on reconstruction data (Sportiche 1999; Sportiche 2002), Sportiche (i) again proposes the decomposition of polyadic predicates into as many 'clauses' (CPs) as we can identify semantic predicates; and (ii) proposes that the arguments of these predicates are not, say, DPs (or atomic elements more generally), but other predicates. These two predicates are then related by a structurally higher quantificational element, like a definiteness or quantificational operator. That is, he assumes the (DP) structure standardly assumed to sit on top of NP actually sits on top of 'VP's (where V covers at least V and T, as above), acting as binders for variables within VP; both individual variables introduced in NP and situation/event variables introduced in VP. This buys straightforwardly a system of scope taking much like that presented in Beghelli & Stowell (1997) (see §2.2.4). Again, Sportiche doesn't go into great detail about this, but the next section runs through an instantiation (and extension) of it from Hallman (2000); and a similar idea will be followed through in the present thesis, in chapter 3.

2.2.3   Quantificational recursion: Hallman

A version of Sportiche's story from §2.2.2.2 is gone over in more detail by Hallman (2000). His explanation runs as follows:

2.2.3.1   DPs and existentials

Quite generally, there is a distinction between weak and strong DPs (in the sense of Milsark 1974), which is that weak DPs can reconstruct to (what is usually taken to be) their base, predicate internal, position, whereas strong DPs can't. Sportiche/Hallman show this using raising data (32–34).

(32)   a. No star was proven to be close to every planet

$no > prove > every^8$

$*prove > every > no$

b. A star was proven to be close to every planet

$a > prove > every$

$prove > every > a$

(33)   a. Most stars were proven to be close to every planet

$most > prove > every$

$*prove > every > most$

b. At least one star was proven to be close to every planet

$one > prove > every$

$prove > every > one$

(34)   a. Most numbers between 1–1000 were proven to divide by every prime number

$most > proven > every$

$*proven > every > most$

b. Some number was proven to divide by every prime number

$some > proven > every$

$proven > every > some$

(Hallman 2000: 103–104)

Sportiche/Hallman also show that the selectional requirements of the lower clause in raising constructions, which are generally taken to be satisfied locally, don't bear on the choice of determiner. That is, there is a clear dependency between the verb and the subject NP, but choice of determiner doesn't interfere with this dependency; so an ungrammatical NP will never be rescued by a particular D, a grammatical NP will never be disallowed because of a particular D (35).

(35)   Every/each/no/a/some child/cat/friend/*Buick/*proximity/*month was proven to be sleeping

(Hallman 2000: 106)

---

[8]> marks scope precedence: A> B = A scopes over B.

The conclusion drawn is that just because a verb and its subject have to be in a local relation at the point of Merge, this doesn't mean that subject has to be a DP, since the s-selectional requirements are requirements on NP alone. Further evidence for this conclusion comes from compounds (36).

(36)   a. song writer

      b. bear hunter

      c. novel writing

      d. girl chasing

    (Hallman 2000: 109)

We don't find determiners on the NPs in compounds like those in (36) (the constituency of *a song writer* is clearly [a [song writer]], not [[a song] writer]), so it looks here like NPs are overtly satisfying the verbal selection requirements by themselves. We might assume that there is a null determiner in these cases, but the interpretation we actually get is generic (i.e. a bear hunter is someone who generally hunts bears; novel writing is the general practice of writing novels; etc.), and the usual assumption is that generic interpretations come from an operation of closure over stray variables, which isn't the same thing. (We still might assume a null indefinite determiner, and that indefinite DPs introduce variables *à la* Heim 1982; Diesing 1992,[9] but the argument would still run, since the crucial distinction is really strong vs. weak, not missing vs. present.)

Sportiche also points out that the selectional requirements of V could be seen to force a configuration where they are satisfied only by NP, since s-selection seems to hold between V and NP, not V and DP, and if D were there, there would be an element intervening between V and NP when really their relation should be maximally local. This last point is far from immune to criticism, of course, but it is interesting as a possible theoretic justification for Sportiche's story nonetheless.

The idea that Sportiche/Hallman end up with, then, is that determiners aren't introduced with NP, but are rather introduced in the clause structure, dominating V-like categories (V, T, etc.) and relating the VP predicate to the NP predicate, giving a tripartite structure akin to that derived by Heim's (1982) quantifier construal rule. The different distribution of strong and weak determiners points to a different distribution of determiners in the two 'layers' of the clause. The

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[9]An idea more along these lines than along Sportiche's will be presented in chapter 3.

particular structure Hallman proposes is shown in (37), where D is the category of strong determiners, and δ the category of weak determiners. As a rough guide, Hallman says D $\ni$ GEN, *every, most, a, no, three, several*; and $\delta \ni \varnothing$ ('the intersective bare plural determiner': Hallman p. 111), *a, no, three, several.*

(37)

```
                DP
              /    \
            D        \
                   /     \
                 NP       IP
                / \      /    \
            firemen    I       δP
                       |      /    \
                       be   δ        \
                              /           \
                            NP             AP
                           /  \           /    \
                       firemen   NP        A
                                 /  \       |
                             firemen   available/intelligent
```
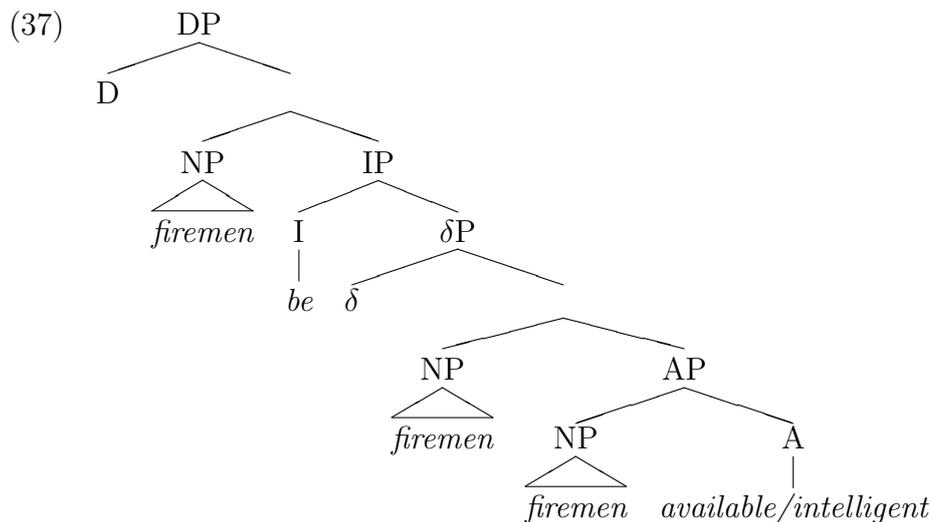
(Hallman 2000: 111)

NPs then move into the specifier positions of the unlabelled projections on the tree, below the determiner positions, and we get the right kind of structures for the semantics to interpret.[10] Existential *there*, Hallman claims, occupies the specifier position below D (the 'standard' subject position, basically). This blocks NPs from raising to this position, so we only get them raising as far as the lower subject position, below δ, and thus get only weak subjects in existential contexts.

2.2.3.2   I-level predicates

The outline above should make it fairly clear how Hallman's instantiation of Sportiche's story works in general. One thing it doesn't deal with as it stands is I-level predicates; more particularly, the lack of a low/weak reading for subjects in I-level contexts.

Hallman proposes that this lack is down to the lack of δ in these kinds of constructions: I-level predicates (in fact non-existential constructions generally) simply

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[10]Some of the details of Hallman's story are simplified here, but this puts across the picture well enough.

don't have δP.

Hallman relates this connection between ILPs and non existentiality to the connection between ILPs and non-eventivity noted by Kratzer (1995). He posits that the correlation we see here between non-eventivity and non-existentiality is not just casual but causal: existentiality is parasitic on eventivity. His story is based on Stowell (1991), where it is proposed that the existential closure operation of Heim (1982) isn't in fact a semantic operation, but rather the result of a syntactically (but not phonetically) realized existential quantifier sitting low in the clause. To be precise, Stowell assumes that eventive predicates project an event argument, as the highest argument in their theta grid, and that the existential operator responsible for $\exists$-closure is dependent on this theta position being projected. So where there is no event argument projected, there is no $\exists$-closure. Hallman formulates this as a selectional requirement: δ obligatorily selects an eventive predicate. Since ILPs aren't eventive, we will never find them in combination with δ, and thus we will never find a weakly quantified (=reconstructed) subject with an ILP.[11]

2.2.3.3   ILPs/SLPs predicate internal

Hallman assumes an l-syntactic decomposition of polyadic predicates into their constituent sub-predicates. He further shows that certain operator elements can intervene between these subpredicates, operating over the lower one but leaving the upper one unaffected (so, say, something might operate over VP but not vP). In particular, he looks at the prefix *un-*.

As an adjectival prefix, *un-* serves to negate a stative predicate: Hallman gives the following examples:

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[11]It should be noted here that these assumptions are Hallman's, not mine: the status of ILPs/SLPs will be further discussed in §5.5.2.2.

(38) a. unmanagable   =   [not[managable]]

b. unsatisfactory   =   [not[satisfactory]]

c. unappealing   =   [not[appealing]]

d. uncertain   =   [not[certain]]

e. unfortunate   =   [not[fortunate]]

f. unfamiliar   =   [not[familiar]]

g. unsound   =   [not[sound]]

h. unstable   =   [not[stable]]

(Hallman 2000: 35)

Hallman notes that when *un-* appears on a transitive verb, we get a different pattern: *John untied his laces*, going by the paradigm in (38), we would expect to mean *john didn't tie his laces*, but it doesn't. What it rather means is that he caused his laces to be in a state of not-tied-ness. If transitive verbs are to be decomposed into a structure something like [*v* [VP]], where *v* is causative and V stative, as Hallman argues they are, we can actually buy this fact very easily by allowing *un-* to operate over the result state VP, but under the causative *v*. Again, what we find is that *un-* is applying to a stative predicate and negating it, exactly as in (38), and this negated result state is then what is interpreted as being caused. The paradigm Hallman gives for transitive verbs is shown in (39).

(39) a. untie   =   [cause[not[tie]]]   ≠   [not[cause[tie]]]

b. unpack   =   [cause[not[pack]]]   ≠   [not[cause[pack]]]

c. unfold   =   [cause[not[fold]]]   ≠   [not[cause[fold]]]

d. unwind   =   [cause[not[wind]]]   ≠   [not[cause[wind]]]

e. uncover   =   [cause[not[cover]]]   ≠   [not[cause[cover]]]

f. unseal   =   [cause[not[seal]]]   ≠   [not[cause[seal]]]

g. unbend   =   [cause[not[bend]]]   ≠   [not[cause[bend]]]

h. unlock   =   [cause[not[lock]]]   ≠   [not[cause[lock]]]

(Hallman 2000: 36)

Hallman also shows that the same syntactic decomposition holds with unaccusative verbs: if we have *unfreeze*, this doesn't mean *not freeze*, but *become not frozen*. If we analyse unaccusative verbs as involving a stative VP dominated by an inchoative *v* with a meaning something like *become*, again the data fall out in a simple way (40).[12]

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[12]Although this isn't Hallman's main point, what we have here is negation, often taken to

(40)   a.   unfreeze =   [become[not[freeze]]] $\neq$ [not[become[freeze]]]

        b.   unlock   =   [become[not[lock]]] $\neq$ [not[become[lock]]]

        c.   uncoil   =   [become[not[coil]]] $\neq$ [not[become[coil]]]

        d.   undress   =   [become[not[dress]]] $\neq$ [not[become[dress]]]

        e.   unfurl   =   [become[not[furl]]] $\neq$ [not[become[furl]]]

        f.   unwrap   =   [become[not[wrap]]] $\neq$ [not[become[wrap]]]

        g.   unravel   =   [become[not[ravel]]] $\neq$ [not[become[ravel]]]

        h.   unroll   =   [become[not[roll]]] $\neq$ [not[become[roll]]]

        (Hallman 2000: 37–38)

Having established that this kind of lexical decomposition is a reasonable path to pursue, Hallman pushes Sportiche's story for determiners even further, and shows that vP internal effects can be detected. Specifically, he pursues the analysis of ILPs from the previous section, inside two classes of intensional verbs. These two classes he labels transitive subject-experiencer verbs (SEVs) (41a) and Montagovian intensional verbs (MIVs) (41b), so named for their discussion by Montague (1974). Both these classes are marked out as unusual by never inducing an existence presupposition for indefinite objects.

(41)   a.   John {fears/hates/loves/respects/loathes/admires ...} ghosts

        b.   John is {seeking/looking for/advertising for/hunting ...} ghosts

However, they do induce existence presuppositions when they have (Milsark) strong objects (42).

(42)   John fears {the ghost/every ghost/each ghost/both ghosts/most ghosts/all ghosts}

They are different in this respect from extensional verbs, where presuppositionality doesn't fail in affirmative expressions whether the object is weak or strong.

There is a distinction to be made between the two classes also, discussed by Carlson (1977), which is that weak objects of SEVs allow a strong reading (though still

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be a clausal/propositional operator, scoping over something that wouldn't by most people be considered a clause or proposition. There aren't any really strong claims to be made on the data above, but the idea that 'propositional' operators such as negation and modality actually operate over smaller proposition-like elements than usually assumed will be followed through in detail in chapter 6.

non-presuppositional) (43a), whereas weak objects of MIVs don't (43b). (43a) implies a fear of a couple of specific ghosts (though possibly non-existent ones), whereas (43b) doesn't require specificity: any old ghosts may do.

(43)   a. John fears a couple of ghosts

b. John is looking for a couple of ghosts

The point Hallman is pursuing here is that there is a kind of definiteness effect at work within these intensional predicates, bringing up a parallel between the objects of SEVs and the subjects of ILPs, and between the objects of MIVs and the subjects of SLPs: indefinite objects of SEVs must be interpreted as strong, as subjects of ILPs must, and indefinite objects of MIVs have an option of getting a weak or a strong reading, as do subjects of SLPs.

The important point is this: even when the indefinites in object position receive a strong reading, they still scope inside intensionality: they are not interpreted as presuppositional. Since intensionality is introduced by the predicate, this means that these objects must get their interpretation inside the predicate. This means they don't fit into the tree in (37), because all the determiners there are generated outside the predicate. Further evidence for this is provided by the fact that weak subjects of these kinds of predicates never get interpreted inside the scope of intensionality: the leprechaun in (44) is subject to presupposition; we have seen the ghosts aren't.

(44)   a. A leprechaun fears ghosts

b. A leprechaun is looking for ghosts

(Hallman 2000: 128)

Given Hallman's general story, then, the conclusion is that there is another position in the clause where weak determiners are generated, inside of the predicate. His treatment of the verb phrase predicate as being formed out of a stative V and a causative or inchoative (or, he later concludes for SEVs, experiencer) v allows a simple means of doing this: this determiner position can sit betwen v, the assumed introducer of intensionality, and V. Objects, generated inside VP, will then be inside the scope of this determiner position, and so inside the scope of the intensional predicate as a whole, whereas subjects will be outside the scope of this position and therefore outside the scope of the intensionality. This ties in with

what we saw at the beginning of this section with *un-*, where an operator element sits in between the two parts of the *v*P, affecting aspects of VP but not of *v*P. It provides support, then, both for Sportiche's claim that determiners are tied in to particular bits of the clause structure, and for his claim that polyadic predicates should be decomposed into a number of monadic predicates, with (somewhat) parallel structure sat on top of each one.

2.2.4   Features and scope: Beghelli & Stowell

Beghelli & Stowell (1997) develop a system of scope taking for quantified nominals based on feature checking, whereby a QP, rather than just undergoing classical May (1977)-style quantifier raising (QR), raises to a specific quantificational functional projection in the clause to check its quantificational feature. So, for example, a referential DP will have a referential feature, which according to Beghelli & Stowell will cause it to raise to the specifier of a projection RefP (which they locate at the very top of the clause) for Spec-head checking with Ref⁰.

The general clause structure they propose is shown in (45).

(45)

```
        RefP
       /    \
     Ref     CP
            /   \
           C    AgrSP
               /    \
            AgrS    DistP
                   /    \
                 Dist   ShareP
                       /    \
                    Share   NegP
                           /    \
                         Neg    AgrOP
                               /    \
                            AgrO    VP
                                    △
                                    ...
```
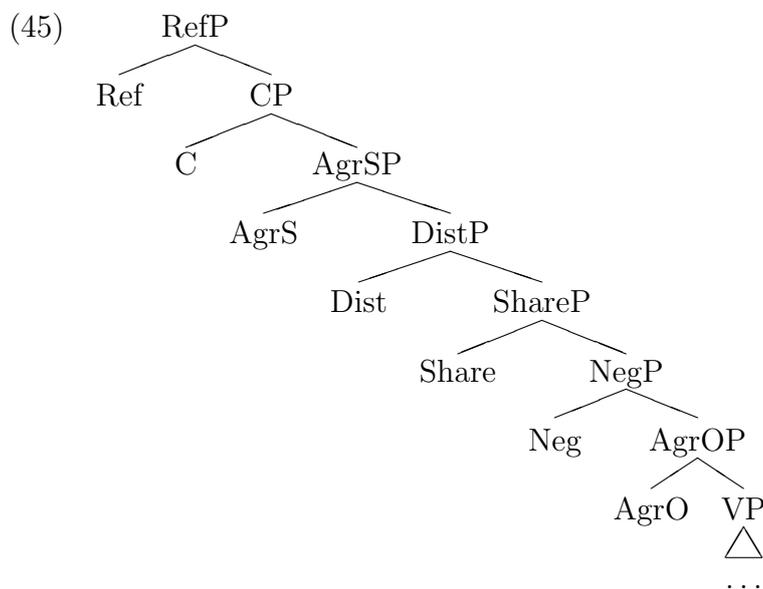
Drawing on the work of Szabolcsi (1994), Beghelli & Stowell distinguish five major types of QP and determine their relative scope possibilities. These are:

- **WhQPs**. These are interrogative *wh-* elements like *who, which Monkee*; Beghelli & Stowell make the standard assumption that they have a [+WH]

feature which is checked in [Spec, CP] via Spec-head agreement with an interrogative Q operator in C.

- **NQPs**. These are negatively quanitified elements like *no bananas*, *no-one*; Beghelli & Stowell assume that, similarly, they have a [+NEG] feature that is checked in [Spec, NegP] by a negative operator in Neg, as in Zanuttini (1991) and Moritz & Valois (1994).

- **DQPs**. These are what Beghelli & Stowell call distributive-universal QPs. They are headed by the universal quantifiers *every* and *each*, and they result in a distributive reading for whatever they quantify over: for example, *Each Byrd has long hair* distributes the property of having long hair over the members of the set of Byrds. Beghelli & Stowell's claim is that this reading comes about because DQPs have a feature [+DIST], which is checked by Spec-head agreement with a distributive operator in Dist.

- **CQPs**. These are counting QPs, which include monotone decreasing quantifiers like *few*, *fewer than three*, *at most three*, and cardinality expressions with modified numerals like *more than three*, *between three and eight*. These 'count individuals with a given property, have very local scope (... essentially *in situ*) and resist specific interpretations' (Beghelli & Stowell 1997: 74). By *in situ*, they mean CQPs take scope in their case position.

- **GQPs**. These are group-denoting QPs, headed by indefinites like *some*, *a*, by bare numerals, and by definites like *the*. They denote groups, as their name suggests, including plural individuals. They have various interpretations, which Beghelli & Stowell take to derive from the various scope positions available to them: they can be referentially independent, in which case they sit in [Spec, RefP]; if they are indefinite, they can either quantify over individuals whose existence is presupposed in the sense of Heim (1982), Diesing (1992), in which case they check a [+SHARE] feature in [Spec, ShareP], or be non-specific in which case they scope in their case positions.

There are various predictions this makes:

a. A WhQP should always take wide scope with repect to any other QP in their clause, other than GQPs when these are assigned scope in Spec of RefP.

b. A GQP should be scopally ambiguous with repect to a clausemate DQP, depending on whether the GQP moves to Spec of RefP or to Spec of ShareP.

c. A GQP object should be scopally higher than clausal negation, owing to the fact that it takes scope in Spec of ShareP or Spec of RefP — except in the case ... where an indefinite or bare-numeral GQP remains in its Case position (Spec of AgrO-P) and receives a counting interpretation ... A GQP subject should always take wide scope with respect to clausal negation and/or a clausemate NQP.

d. A CQP in object position should never be able to take inverse scope over a GQP or DQP occurring in subject position.

(Beghelli & Stowell 1997: 80)

Prediction (a) is of course standard: WhQPs are generally assumed to take wide scope over most QPs, whatever mechanism is assumed to assign scope to *wh*-elements. (b) is also standard.

(46)   a. Every/each student read two books

       b. Two students read every/each book

(Beghelli & Stowell 1997: 80)

In both (46a) and (46b) the QPs can be interpreted in either scopal order. Standard QR (May 1985) deals with this by saying that the QPs are both adjoined to the same projection, hence not subject to any government constraints requiring them to be interpreted in a particular order. Beghelli & Stowell's system, on the other hand, derives the ambiguity by saying that the DQP (*each/every...*) is invariably located in [Spec, DistP], whereas the GQP (*two...*) has a number of possible positions depending on its interpretation: [Spec, RefP], [Spec, AgrSP], [Spec, ShareP], and [Spec, AgrOP]. The first two positions are above Dist, the second two are below it, so the scope alternation is expected.

The predictions in (c) are illustrated with (47a–47d).

(47)   a. The students didn't read two/some books

       b. No student read two/some books

       c. Two/some students didn't read this book

    d. Two/some students read no books

    (Beghelli & Stowell 1997: 82)

(47a) and (47b) show that indefinite GQP objects can scope over negation; (47a) also shows that some GQPs can be interpreted below negation, which follows from the claim that they can take scope in their case position, as mentioned above. (47c) and (47d) show that indefinite GQP subjects must scope over negation. In Beghelli & Stowell's system, this is because there are three possible landing sites for such subjects — [Spec, RefP], [Spec, ShareP], and [Spec, AgrSP] — all of which are above Neg in the clause.

Prediction (d) is illustrated with (48a) and (48b).

  (48)   a. Some/one of the students visited more than two girls

          b. Every student visited more/fewer than three girls

          (Beghelli & Stowell 1997: 83)

In neither case can the CQP object take inverse scope over the subject (a GQP in (48a), a DQP in (48b)). Beghelli & Stowell's analysis derives this from the fact that object CQPs can scope no higher than [Spec, AgrOP], while subject GQPs can scope no lower than [Spec, ShareP] and subject DQPs can scope no lower than [Spec, DistP], both of which are above AgrO.

The number of additional functional projections required in this system with apparently not much to motivate them other than the scope facts discussed may seem questionable, but Beghelli & Stowell point out that all the projections they use can in fact be given independent motivation: RefP they identify with a standard topic position, and ShareP with a scrambling position discussed by Diesing (1992: 107–108) for narrow scope presuppositional QPs. They also cite evidence from Kinyalolo (1990) and Khalaily (1995) that DQPs must move overtly in, respectively, the Bantui language KiLega and Palestinian Arabic, to a position corresponding to their DistP. Additionally, Szabolcsi (2001) discusses a range of data from Hungarian, and shows that many QPs in Hungarian occur at PF in positions corresponding to those proposed by Beghelli & Stowell.

The most salient part of Beghelli & Stowell's story here is what operators sit where in their tree. Working bottom up, they assume that a negative operator sits in Neg; that an existential operator ∃ sits in Share (the same ∃ proposed in
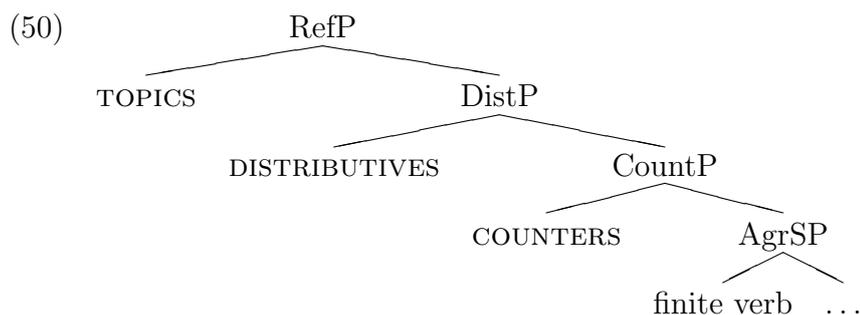
Stowell 1991, discussed in §2.2.3.2 above); that a universal/distributive operator $\forall$ sits in Dist; that a *wh-* operator sits in C; and that another $\exists$ sits in Ref. Negation and *wh-* are commonly tied in to the Focus projection of Rizzi (1997); this gives us a pattern of operators as in (49).

(49) $\exists > \text{wh}_{\text{FOC}} > \forall > \exists > \text{Neg}_{\text{FOC}}$

The evidence isn't overwhelming here, but again it looks rather like we have a repetition of similar operator structure on top of the clause and midway down. Further, Tim Stowell (p.c.) informs me that additional data not incorporated into Beghelli & Stowell (1997) points to a similar repetition down inside what is labelled VP in (45); which ties in with the claims of Hallman (2000) in §2.2.3.3, and with the extension of Beghelli & Stowell's system by Brody & Szabolcsi (2003), discussed in the next section.

2.2.5   Hungarian: Szabolcsi, Brody

Szabolcsi (1997) and Brody & Szabolcsi (2003) adopt a system of scope taking similar to that of Beghelli & Stowell's in the previous section, looking specifically at quantifier scope in Hungarian. They show that preverbal QP types in Hungarian line up in a very clear, string linear, order, easily demonstrable by the relative position of other elements such as adverbs around them; so that, for example, (adopting Beghelli & Stowell's labels, though as will be seen below not quite their structure) 'a preverbal quantifier in CountP (or focus) triggers the inversion of the verbal particle if the verb has one ... one in DistP does not; quantifiers in RefP can be followed by unfocused adverbs like *tegnap* 'yesterday', those in DistP cannot' (Brody & Szabolcsi 2003: 21). The general structure they propose for the ordering seen is as in (50).

(50)

```
            RefP
           /    \
      TOPICS     DistP
                /    \
     DISTRIBUTIVES    CountP
                     /     \
              COUNTERS      AgrSP
                           /    \
                    finite verb   ...
```

Topics include things like *vala-* 'some', *hat*, 'six', *a legtöbb* 'most'; distributives include *minden* 'every', *több, mint hat* 'more than six', *legalább hat* 'at least six'; counters include things like *kevés* 'few', HAT/SOC 'six/many' (focussed), *több, mint hat* 'more than six', *hatnál több* 'more than six'.

The linear order of these elements corresponds to their scope order; their grammatical function is immaterial. So (51) shows that preverbal *minden* 'every' will invariably outscope preverbal *kevés* 'few', whichever is subject/object.

(51)   a. Minden ember   kevés filmet   nézett meg
          every   man.NOM few   film.ACC viewed prt
          'every man viewed few films'

          Scope: *every*$_{\text{subj}}$ > *few*$_{\text{obj}}$

       b. Minden filmet   kevés ember   nézett meg
          every   film.ACC few   men.NOM viewed prt
          'few men viewed every film'

          Scope: *every*$_{\text{obj}}$ > *few*$_{\text{subj}}$

       c. *Kevés ember   minden filmet   megnézett / nézett meg
          few   men.NOM every   film.ACC prt.viewed / viewed prt

       d. *Kevés filmet   minden ember   megnézett / nézett meg
          few   film.ACC every   man.NOM prt.viewed / viewed prt

       (Brody & Szabolcsi 2003: 20, example 3)

However, this preverbal field is not the only place QPs are found in Hungarian: they are also licit postverbally, in which case neutrally,[13] the preverbal QP will take scope over the postverbal QP, whatever their quantificational status. So in (52), a preverbal *kevés* can outscope a postverbal *minden*.

(52)   Kevés filmet   látott minden ember
       few   film.ACC saw   every   man.NOM
       'Every man saw few films'

       Scope: *few* > *every*

In addition, where we have two QPs in the postverbal field they can occur in either surface order; and again whatever order this is will correspond with their

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[13]With neutral intonation, surface order equals scope order; with the postverbal quantifier stressed, inverse scope may be obtained. See Brody & Szabolcsi (2003) for a detailed analysis of how the inverse scope cases come about in terms of Brody's (2000a) Mirror Theory.

surface order. Given Brody & Szabolcsi's two main assumptions — one, the well known claim that Hungarian surface structure corresponds to its LF; and two, that quantifier scope is determined by feature checking with quantificational functional heads *à la* Beghelli & Stowell (1997) — the only way they can deal with these kind of data is to say that the Ref–Dist–Count series is iterated below the surface position of the verb (which they take to be AgrS). This buys (52); it doesn't though buy the cases where, say, a postverbal Count QP can outscope a postverbal Dist. They therefore have to say that the whole series reiterates more than once below AgrS: in fact the specific proposal that they make is that 'the operator series Ref–Dist–Count reiterates itself above all inflectional heads and possibly above the verbal heads' (Brody & Szabolcsi 2003: 22). In proposing such an iterative syntax, they follow Hallman (1997) (see also his story in §2.2.3 above), and obviously the idea bears affinities with various of the other ideas we have seen in this chapter already. The general structure is thus something like (53), with (52) exemplified concretely.

(53)

```
        C
       ╱╲
       Ref
      ╱╲
      Dist
     ╱  ╲
        Count
       ╱    ╲
  kevés filmet  AgrS
            ╱    ╲
         látott   Ref
               ╱    ╲
                   Dist
                  ╱    ╲
         minden ember  Count
                      ╱╲
                       T
                      ╱╲
                      Ref
                     ╱╲
                     Dist
                    ╱╲
                    Count
                   ╱╲
                    ...
```
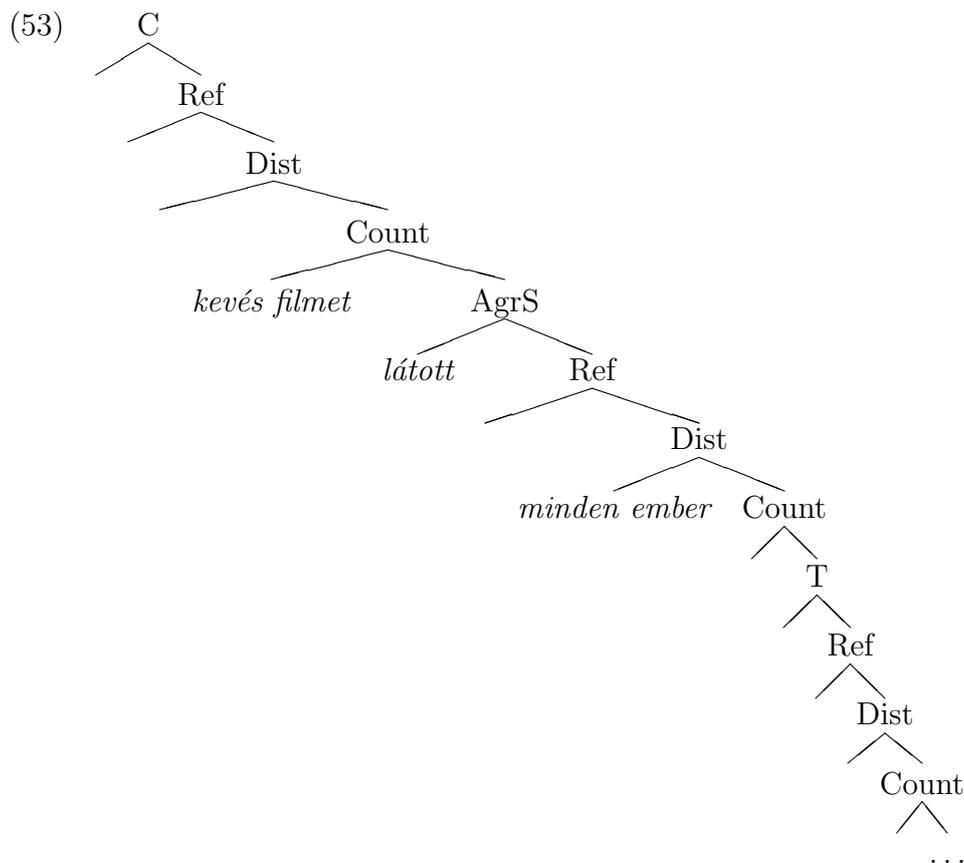
2.3   Summary

We have seen various arguments in this chapter for clause internal positions that closely parallel clause peripheral positions. Belletti (2001a,b) and Jayaseelan (2001) present empirically based arguments showing that at least a Focus projection[14] similar to that proposed by Rizzi (1997) for the standard clause peripheral CP is available clause internally, at the left edge of the verbal complex.

Sportiche (1995, 1999, 2002) argues along more theoretical lines, proposing that any predicative lexical item will be topped by a clause-like structure topped with a CP. His arguments come from empirical data bearing on the behaviour of the French predicate clitic *le*; and from the behaviour of determiners/NPs in cases of A-reconstruction.

This latter argument is pursued further by Hallman (2000), who proposes an instantiation of it that not only divides tensed clauses into two predicate based CP layers (one for V, one for T), but further divides polyadic verbal complexes into as many monadic predicates as can be divined, each with its own CP-like layer.[15]

Beghelli & Stowell (1997) propose a theory of quantifier scope whereby QPs obtain their scope by means of checking their quantificational features in specific quantification related functional projections in the clause. Their clause structure can be seen as containing two layers, one clause peripheral and one clause internal, with a certain parallelism betwen the two, very similar to the claims of Sportiche and Hallman regarding determiner/quantifer construal.

Szabolcsi (1997); Brody & Szabolcsi (2003) extend this parallelism, arguing based on data from Hungarian that these two layers are in fact not just similar, but the same *Ref > Dist > Count* quantificational field iterated twice in the clause. They further suggest, like Hallman, that in fact the iteration is greater than this, with the *Ref > Dist > Count* array sitting on top of perhaps every inflectional and every verbal head.

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[14]Although it wasn't discussed here, both authors in fact present evidence that Topic positions are available too.

[15]Actually, Hallman does not call them CPs but his general analysis follows on from Sportiche's and Sportiche does.
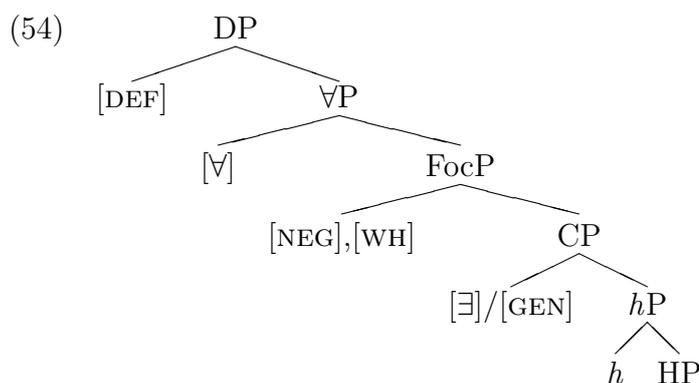
2.4   Extending the proposal

Most of the data discussed in work reviewed above bears on the interpretation of DPs (or QPs): their possible scope positions, their semantic interpretation, etc.; a notable exception is the suggestion of Sportiche (1999, 2002) that the quantificational elements found in the CP layers can also operate over VP-related elements: so that, for example, Number can operate over nouns (giving us plurals) or verbs (giving us iterative aspect).

This thesis will follow up on this latter idea, arguing that the quantificational operator elements in these CP layers are basically unselective, being able to bind any variables in their scope, whether these be individual variables ($\mathbf{x}$) situation/event variables ($\mathbf{s}$), or whatever; this blurs the distinction that is often made between D- and A-quantification (Partee et al. 1987), which is in itself a welcome result, since quantification is fundamentally quantification, whatever it quantifies over. This general treatment of quantification will be shown to explain numerous facts to do with temporal/aspectual interpretation, modality, individual vs. stage level predicates, and clause structure generally, as well as providing a new way of looking at the phase based theory of syntax proposed by Chomsky (1999).

Chapter 3

The Quantificational Nature of CP

Anticipating the following chapters, I suggest a synthesis of the proposals in the previous chapter, wherein a clause divides up into smaller 'sub-clauses' with the structure in (54).[1] I take this structure to be what is referred to in recent minimalist literature as the PHASE (Chomsky 1999; etc.): the basic building block of the derivation. So for the standard vP phase, H = V, $h = v$; for the standard CP phase, H = T, $h = t$ (see chapter 5 for t). A basic tensed clause, then, will consist of two such phases.

(54)

```
                    DP
            ╱              ╲
        [DEF]              ∀P
                      ╱          ╲
                    [∀]          FocP
                           ╱            ╲
                    [NEG],[WH]          CP
                                   ╱          ╲
                            [∃]/[GEN]         hP
                                           ╱      ╲
                                          h       HP
```

---

[1]For concreteness, I make the facile — to the extent that it may be inaccurate, but for my purposes here I don't think it matters too much — assumption that this structure maps to a standard Rizzi (1997)-type decomposed CP as follows: ∀P = ForceP; FocP = FocP; CP = FinP. I assume also that Rizzi's TopPs may be present in this structure. DP I would take to map to Bhatt & Yoon's (1991) Sub: a head which 'make[s] a clause available for (categorial) selection independently of its force' (Rizzi 1997: 328, note 6) (see chapter 7 for relevant discussion). To tie it in with what we have seen so far, we may also map this to a version of Beghelli & Stowell's structure: DP = RefP, ∀P = DistP, CP = ShareP.

I also take this structure to be the structure of the standard DP, where H = N and $h = n$ (for n, see e.g. Radford 2000; Marantz 2001; Adger 2003a).[2] There is a long tradition in the literature investigating structural parallelism between CP and DP: Szabolcsi (1983, 1989, 1994); Siloni (1990); Cardinaletti & Starke (1999); Radford (2000); Bernstein (2001); Ogawa (2001); Pesetsky & Torrego (2001); etc. I follow this tradition here, but, like Starke (2001), I treat a simple DP as equivalent to just a single instance of these sub-clausal units — i.e. a single phase — rather than to a full clause, built out of two phases.

Since I am proposing that all phases share a common, CP-topped structure, it doesn't really make sense to refer to the CP phase, as distinct from the vP or DP phase. Instead I shall hereonin refer to phases by the category of their lexical core — so the CP phase becomes the T phase, the vP phase becomes the V phase, and the (putative) DP phase becomes the N phase.

In addition to the data we saw in the last chapter, particularly in §2.2.4 from Beghelli & Stowell (1997), we will see further data backing up this structure later in the dissertation, most saliently that involving the scope interactions of modality and quantified subjects in chapter 6. The current chapter is more general though: I will be concentrating not on the precise structure, but rather on the treatment of QP scope I will adopt here. I will therefore take (54) as a given, for now. This runthrough will serve two purposes: first, to relate the various theories for scope-taking we saw in the previous chapter to what I am going to be doing with scope here, and second, to introduce some of the fundamental concepts I will be making use of later in the dissertation.

3.1   Quantification and scope

In §3.1.1, I go through two aspects of Heims's (1982) classic analysis of QP scope: one, her treatment of non-indefinite QPs, and two, her treatment of indefinites. In §3.1.2, I relate these to one another and show how the story for indefinites

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[2]Note that I am not suggesting that there are no other functional categories in the DP (nor, incidentally, the clause) than those shown here: there is ample evidence for several more below the quantificational level I am concentrating on, such as NumP, K/CaseP (Ritter 1991), DegP (Matushansky 2003b), etc.; these won't be my concern here, however. There are also plausibly others in and around the peripheral area I am concentrating on, such as a P level on the top of (54) — cf. §3.2 — which I will bring into the discussion if and when necessary.

can be extended to cover the other cases, given the quantificational CP layers proposed here.

3.1.1   Quantificational construal: Heim

Heim (1982) presents an influential tripartite system for quantifier scope taking wherein she posits a split in behaviour between, broadly, non-indefinite QPs[3] and indefinite QPs.[4]

Heim adopts a generally tripartite analysis of quantification (Lewis 1975): within a tripartite system, any proposition expressing quantification needs to represent three elements — the quantificational operator (Q), the restrictive scope (or restriction), and the nuclear scope (or just scope). The restriction and scope each denote a set, and the Q gives us the relationship between those sets. Schematically, the representation for quantification is as in (55a) (in the notation of Heim & Kratzer (1998)); a more concrete version is in (55b).

(55)   a. Q: f(x). g(x)

‘for Q x, if x has the property f, then x also has the property g’

b. $\forall$: dog(x). bark(x)

‘for every value of x, if x has the property *dog* (is a member of the set of dogs), then x also has the property *bark* (is a member of the set of barking things)’

Heim proposes that this tripartite semantic structure is reflected explicitly in the syntax; she derives the syntactic representation by a series of two operations. The first is NP-PREFIXING, which moves the whole QP up to adjoin at sentential level; basically equivalent to standard quantifier raising (QR) (May 1977, 1985). The second is QUANTIFIER CONSTRUAL, which moves the Q out of the moved QP, again to ajoin at sentential level; this is adopted more or less wholesale in Aoun & Li's (1993) reinterpretation of QR. This derives the structure in (56).[5]

---

[3]I use QP as a cover term, to include DPs.

[4]I will leave aside other quantificational elements, such as temporal adverbs and modals, for now: later I will analyse them as quantifiers over situation variables.

[5]This is a very much updated version of Heim's structure with regard to labelling, branching, etc.

(56)
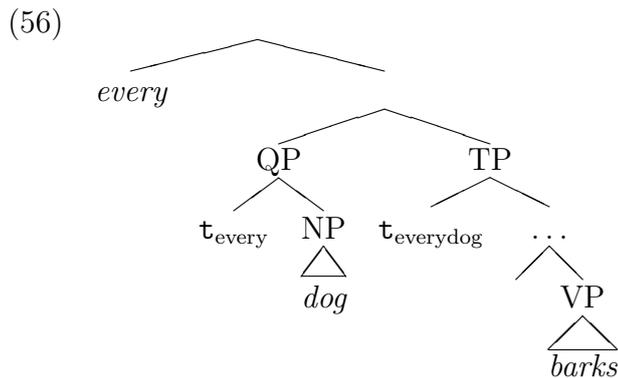
The NP then functions as the restriction of Q, the rest of the structure as its scope. Heim's rationale behind positing this pair of movements is that the way the sets denoted by the restriction and scope are represented in the syntax is by, in each case, the predication of the relevant property to a variable, as x in (55a) above. Heim proposes that the variables are introduced by indices; we don't need to worry about this here — I shall propose a different method for introducing variables in the syntax in §3.1.2.1. However we represent these variables syntactically, though, we have to have one in the restriction (NP) and one in the scope (everything else); and to do its job, Q needs to bind both of them. Under standard assumptions, binding occurs under c-command. This means Q needs to c-command both NP and the rest of the structure.

The second step, quantifier construal, then, is essential for Heim's story to come out right. What about the first step, NP-prefixing (QR)? It isn't so clear that this is necessary from the example (56), since we only have one Q here, and we could seemingly just move it up in one step, leaving the NP in its base position. If we have another Q element, though, it matters. In (57a), the epistemic modal *might* has been introduced. Leaving aside the details of how this is to be represented (see chapter 6), it is clear that the Q *each* outscopes the modal *might* here. Epistemic modals are widely assumed to take sentential scope, which I represent in (57b) by placing *might* in C. Q, then, must scope over C. Assuming the subject sits in [Spec, TP], If we just moved Q, then its NP restriction would stay in [Spec, TP]. The resulting structure is pretty much incoherent (58); at any rate, the best interpretation we can get for it isn't the one we want. This means that Q can't be split from its restriction by another scopal element.[6] To ensure

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[6]Cf. Pesetsky's (2000) INTERVENTION EFFECT:

(1)   A semantic restriction on a quantifier (including *wh*) may not be separated from that quantifier by a scope-bearing element.

that this doesn't happen, it seems simplest to suppose that quantifier construal is preceded by NP-prefixing, resulting in a maximally local relation between Q and its restriction.

(57)   a. Each dog might bark

= 'For each x such that x is a dog, it is a possible assumption that x barks/will bark'

b.
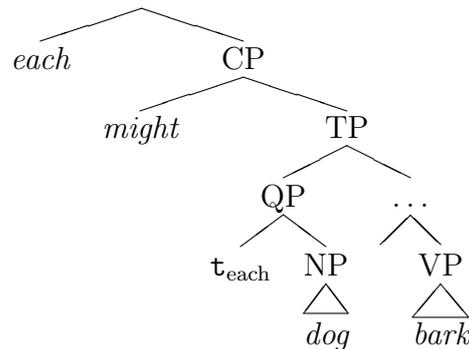
= #'For each x, it is a possible assumption that if x is a dog then x barks'

Heim's treatment of indefinites is very different: she treats them as introducing only a restricted variable into the representation, without inherent quantificational force. So while *every dog* above was taken to translate as $\forall$: dog(x), *a dog* wouldn't be taken to translate as $\exists$: dog(x), but rather just dog(x). This variable is then subject to closure (binding) by other quantificational elements in the structure: other Qs, quantificational adverbs, modals, and Heim's $\exists$-closure operator. I will go over this last option here.

Existential closure is a process whereby an existential operator $\exists$ is introduced at the mapping of an expression from the syntax to the semantics, serving to unselectively close off, i.e. bind, any variables that have been left unbound (free) by that stage. The reasons for positing this view of indefinites are well-known, and I won't go into them here: rather, I will run through a simple example to give the general idea.

There are a few different points where Heim says $\exists$ can be inserted. One of these is at the top of the nuclear scope of a quantificational expression, which is

(Pesetsky 2000: 67)

to say directly underneath the position where a (genuinely quantificational, i.e. non-indefinite) QP ajoins for NP-prefixing.

For an indefinite, then, we get a structure as in (58).

(58)   a. Every dog saw a cat

       b.
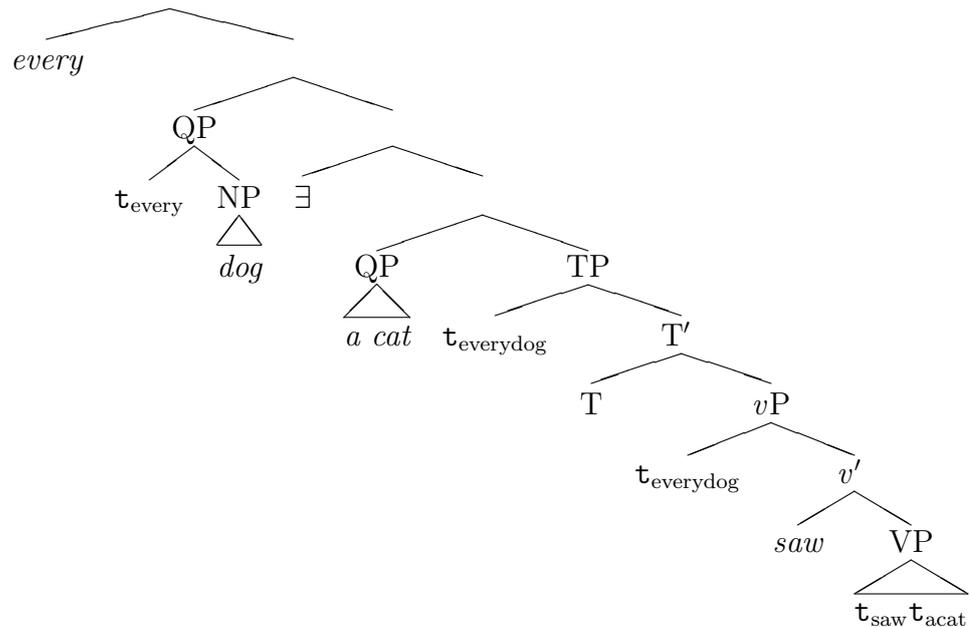
     = 'For every x such that x is a dog, there is a y such that y is a cat and x saw y'

Note that in this case, though the QP *a cat* has undergone NP-prefixing, *a* hasn't undergone quantifier construal, because for Heim it is essentially meaningless. However, because of the stipulations Heim makes about how her rules operate, exactly the same relationship obtains between ∃ and its restriction, and *every* and its restriction.

I find this last point suggestive, and I shall argue that in fact this similarity should be derived in some way. Specifically, I argue that all QPs should be treated something like Heim's indefinites, so that they introduce just a restricted variable into the representation, which is bound by a higher quantificational operator (cf. Sportiche and Hallman's analyses in the previous chapter; also Kratzer & Shimoyama 2002; Kratzer to appear b). The quantifier in QP itself serves to make this binding selective. The distinction between definites and indefinites that led Heim to treat them differently, I propose, is down to indefinites being ambiguous between existential and generic readings, both at the QP and the higher operator

level (see (54)), so that indefinites seem to be freer in their interpretation.

3.1.2   Extending Heim's analysis of indefinites

Heim's story for indefinites relies on the notion that there is an existential operator $\exists$ present (on some level) in the structure of the clause, which crucially is not lexically/structurally associated with the indefinite DPs it ends up being related to. Given the kind of structure for the clause I am proposing here, following the kind of research reviewed in chapter 2, where the clause contains several more such operators, it seems like a reasonable line to take that essentially the same kind of treatment that Heim gives to indefinites can extend to other QPs also. Basically, any QP introduces a restricted variable, which associates with one of the clausal quantification encoding CP level heads.

The way I suggest that this works is as follows. As noted at the beginning of this chapter, I take the sub-clausal CPs to equate to the phases of Chomsky (1999); etc. Each simple clause, I take to consist of two such phases:[7] one with V as its lexical core, the other with T. I also take DPs to instantiate this phase structure, where the lexical core is N. However, we need a distinction between the N phase and the V/T phases, if the N phases aren't really doing quantification. The most straightforward way of representing this difference in the framework I adopt is to say that quantificational heads in the N phase's CP contain uninterpretable quantificational features $[u\mathrm{Q}]$, whereas the V and T phases' CPs contain interpretable quantificational features $[i\mathrm{Q}]$ with which the $[u\mathrm{Q}]$s Agree. It is probably easiest to show how this works by running through a derivation. To give the general idea, I'll do this on a more basic level first; then I'll go through the details of the technicalities of how to get the required binding and Agree relations.

To obtain basically the same kind of tripartite structure for quantification as Heim ends up with, in (59) the QP *every man's conscience* moves to [Spec, TP] as subject (59b); it then moves to a [Spec, TopP] immediately below the clausal $\forall$P (59c)[8] (this step corresponds to Heim's NP-prefixing/QR); the clausal $[i\forall]$ Agrees with the QP's $[u\forall]$, which is deleted (59d), leaving $[i\forall]$ in a position c-

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[7]With the proviso that it may be possible to analyse the verbal layer as decomposing into further phases, one per sub-predicate/argument; cf. the discussion in §2.2.3.3.

[8]This claim is for expository purposes only; I shall revise it shortly.

commanding both its restriction (NP) and its nuclear scope (TP) (this last step corresponds to Heim's quantifier construal).[9]

(59)   a. Every man's conscience is vile and depraved

   b.

$\forall$P
├── [$i\forall$]
└── TopP
     ├── Spec
     └── TP
          ├── $\forall$P
          │   ├── $every_{[u\forall]}$
          │   └── NP
          │   └── *man's conscience*
          └── CP
               └── t *is vile and depraved*

   c.

$\forall$P
├── [$i\forall$]
└── TopP
     ├── $\forall$P
     │   ├── $every_{[u\forall]}$
     │   └── NP
     │   └── *man's conscience*
     └── TP
          ├── t
          └── CP
               └── t *is vile and depraved*

   d.

$\forall$P
├── [$i\forall$]
└── TopP
     ├── $\forall$P
     │   ├── $every_{[u\forall]}$
     │   └── NP
     │   └── *man's conscience*
     └── TP
          ├── t
          └── CP
               └── t *is vile and depraved*
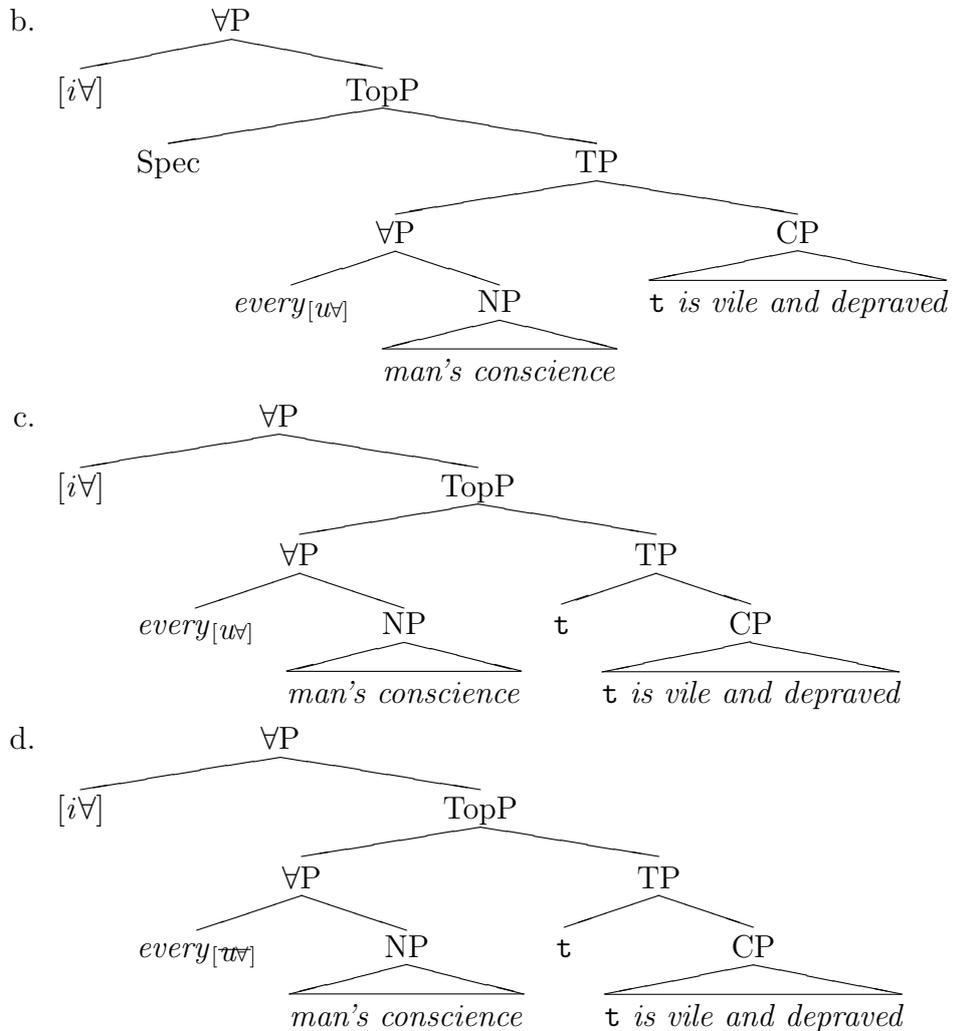
Note that as far as interpretation is concerned, this ties in well with the analyses of Sportiche and Hallman, set out in §2.2.1, whereby NPs were Merged as arguments, separately from their determiners/quantifiers. Here, I assume we do Merge DPs as arguments (since constituency tests are pretty clear on this) but with uninterpretable D/Q features (meaning they have essentially the semantics of NPs) which Agree with higher interpretable D/Q features Merged separately.

It should be clear enough what the general approach is that I'm advocating. But there are (at least) four things that need to be made explicit:

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[9]Note that I have missed off several of the CP level heads that I proposed in (54) in these trees. This is just for space purposes.

1. The above derivation posited a Topic phrase immediately below ∀P. This entails having a TopP immediately below all the other Q heads in a CP also, for the relevant QPs to move to. Do we want this?

2. I have said that operators bind variables, but I haven't represented these variables — are they in the syntax, and if so how are they represented?

3. We clearly have selective binding here — how does this work?

4. In (59), we had an interpretable feature apparently probing for an uninterpretable one. This is the opposite to what is currently canonically assumed in minimalist circles, where in order for a syntactic object to be 'active', i.e. able to take part in Agree relations etc., it needs to have uninterpretable or unvalued features. Does this matter?

As to the first question, it is certainly possible that we do have all these TopPs in the CP, making it look a lot like Rizzi's original split CP but a bit bigger. In fact, given that each TopP would be in a sense associated with a particular Q head, we would derive something like the more current notion that there are different 'kinds' of TopP, doing different kinds of topicalization: contrastive topic, etc. (Puskás 2000a,b). However, we get questions then of whether these are really all the same kind of thing (i.e. all TopPs), which may or may not be important; of how a particular QP 'knows' how to get to the right TopP (does it move straight there, or go stepwise via any lower TopPs — basically indirect feature driven movement — till it can be interpreted); of what triggers the movement; etc. I won't take a particularly firm stand on this; but for concreteness what I will actually propose is that the QP is never in a TopP, but in a Spec-head configuration with the relevant clausal Q head, and rephrase the c-command requirement for scope in terms of dominance.[10]

As to the second, I will represent variables syntactically, specifically as the [ID] features of Adger & Ramchand (to appear), claiming they are introduced by Root categories, basically as something like argument slots, as in Butler (2004). This leads into the third question: I will derive selective binding from the valuation of

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[10]An interesting alternative is suggested by Hallman (2004), who proposes that selection/checking obtains between a head and both its complement and the specifier of its complement: so a head licenses its complement's specifier. This would be another way of keeping manifold TopPs out of the story, while retaining standard c-command notions of scope domains.

the [ID] features by a QP's [Q] feature, its uninterpretability not being an issue for this.

As to the the fourth question, it's possible it doesn't matter, depending on what assumptions you make;[11] but I shall again make use of [ID] features here, claiming that the clausal Q heads have [uID] features which probe for the [iID] features on the QPs. [ID] features then are crucial in making binding relationships explicit.

3.1.2.1   [ID] features

As noted previously, I assume a general decomposition of the verbal complex into V and v layers, where V is a pretty underspecified lexical Root category: some kind of encyclopaedic entry giving us the big 'meaning' semantics — it tells us what property is to be predicated. It doesn't do much else though: in particular, it doesn't do predication, situation structure, and argument structure. I take these to be dealt with by the v-level heads (Harley 1995; Pylkkänen 2002b; Ramchand 2003; etc.). An intransitive verb will have one v head; a transitive will have two; etc. Each v head introduces a (sub-)situation[12] into the representation (stative, causative, inchoative, or whatever), which build up compositionally to give the verbal complex the denotation of a particular kind of macro-situation. So if the V Root gives us the property of, say, laughter, and v denotes a process, we get a verbal complex denoting an event whereby a process of laughter occurs, realized in English as the intransitive verb *laugh*. All v heads can (but don't necessarily) introduce an argument as specifier: the highest v introduces the external argument, lower heads introduce internal arguments.

I take it these arguments are introduced by EPP features. Although I treat V as an underspecified Root category, I take it that it does give us a certain amount of information that will be useful in building the v layer. In particular, I take it that it tells us enough that we know whether we are talking about a property or a relation; i.e. an intransitive or a transitive concept. The syntax will thus have enough information to structure the situation in the v layer. This is where

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[11]For example, Manzini & Roussou (2000) set up a very interesting minimalist system for 'A-movement' (in inverted commas because they don't invoke movement) where interpretable features do probe unproblematically.

[12]I use 'situation' as a cover term for events, states etc., similar to Bach's (1986) term 'eventuality'.

[ID] features come in, formalizing the conceptual knowledge so the syntax can read it: a property denoting V Root will have a single [ID] feature; a (binary) relation denoting V Root will have two [ID] features. [ID] features, as noted, are interpreted as variables: in this case, variables over the holders of the property or relation. In this sense they are like the argument slots in a selectional grid; we might see them as a featural reinterpretation of θ-roles (cf. Hornstein 1998; Manzini & Roussou 2000).

As variables, the [ID] features need to be bound; I follow Adger & Ramchand in treating binding as feature valuation. The means of binding I will initially consider here is binding by Adger & Ramchand's [Λ] feature, which maps to the semantics straightforwardly as a predicate abstraction operator λ. As in Butler (2004), [Λ] features are a reinterpretation of [EPP] features. I take [Λ] features to be introduced in the verbal complex by v heads. Each [Λ] that is introduced will bind one [ID] — so if we have a V Root with two [ID] features, and we Merge the internal v with a [Λ] feature, that [Λ] will bind one [ID], triggering λ-abstraction over the variable, and we will get a predicate. This will be satisfied by Merge of an argument as Spec. We then Merge the external v with another [Λ]; that [Λ] binds the second [ID], we have another predicate so we Merge another argument. We thus have a cycle within vP of building 'small' propositions[13] from each sub-situation and then abstracting over them.

I take [ID] features to be obligatory on the Root V, since they serve to structure the verbal complex, and we know that even when we don't see overt arguments, non-overt or implicit arguments are well motivated. However, I take [Λ] features to be optional. Where v has no [Λ], we don't get either a predicate or an overt argument.

The mechanism for Agree I assume is based largely on Chomsky (1999) and subsequent related works. This mechanism makes crucial use of the notion of the (in)activeness of syntactic elements based on their features. Elements that are active can (in fact have to) take part in syntactic processes, to become inactive. Inactive elements can't initiate syntactic processes. Elements can be active for either of two reasons: because they contain uninterpretable features, or because they contain unvalued features.[14] Uninterpretable features need to be checked and

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[13]'Propositions' in Chomsky's (2000), syntactic, sense of full argument structure, rather than the more usual, semantic, truth-evaluable sense; see §4.3.3 for discussion of this.

[14]It is a matter of contention whether these two notions really boil down to just one, specif-

deleted, unvalued features need to be valued, for the structure to be interpreted by the interfaces. Once these conditions are met — i.e. once an element has checked its uninterpretable features and/or valued its unvalued features — it is rendered inactive. If the conditions are not met, we get a crash since the structure will not be semantically/phonologically (depending on the features at stake) coherent.

Both checking and valuation take place via feature matching and the operation AGREE. An active element will search or PROBE its complement to find a GOAL with matching features. Matching features are features of the same type, but not necessarily with the same value — so a valued probe can match an unvalued goal. If it finds one they will Agree, which means their (relevant) features will be checked/valued. (This will not necessarily render them inactive since they may have other features that need checking still.)

What this means here is that v needs to be active in order for the Agree relation between $[\Lambda]$ and $[\text{ID}]$ to obtain. I follow Adger & Ramchand's general story in assuming a head with an interpretable $[\Lambda]$ feature like v also has an uninterpretable $[u\text{ID}:\Lambda]$. v is thus active. The $[\text{ID}]$s on V I take to be interpretable, but unvalued, notated as $[\text{ID}:]$. Adger & Ramchand propose $[\text{ID}]$ features can have either of two values:[15] they can be $[\text{ID}:\phi]$ or $[\text{ID}:\Lambda]$. The first of these is to be read as $[\text{ID}]$ with the value ϕ; that is, a variable with ϕ features. This is essentially a pronoun and I won't be concerned with it here. The second is $[\text{ID}]$ with the value Λ. This is a variable that is bound by a $[\Lambda]$ feature via Agree. The way this goes is as follows.

We start off with a Root V; concretely, as above, expressing the notion LAUGH. This being a one-place property, it has on it one unvalued $[\text{ID}:]$ feature.[16] V is thus active. It can't really probe at this point (or can do so only vacuously) so we go on.

We Merge a process-denoting v head. This v has on it an interpretable $[\Lambda]$ feature, and an uninterpretable $[u\text{ID}:\Lambda]$ feature (60a). It too, then, is active. It probes its domain for a feature matching its $[u\text{ID}:\Lambda]$, and it finds the unvalued $[\text{ID}:]$ on

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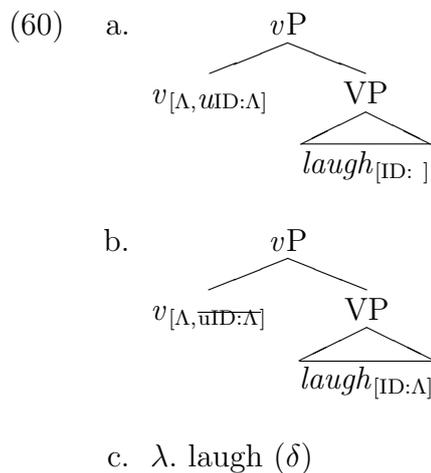
ically the former. If they do, it should be possible to reformulate the story here. I shall just keep the two notions as set out in the text.

[15]I extend this proposal here to allow additional binding possibilities; specifically binding by quantificational operators.

[16]This ignores the possibility of an 'event argument' (Davidson 1967; Kratzer 1995); I return to this in the next chapter.

V. The $[u\text{ID}{:}\Lambda]$ on v is checked (notated by striking it out), and the $[\text{ID}{:}\,]$ on V is valued $[\text{ID}{:}\Lambda]$. $[\text{ID}]$ features and they way they are valued correspond to some extent to traditional variables and indices: they make binding relations explicit in the syntax. We end up with (60b).

(60) a.

vP
$v_{[\Lambda,\,u\text{ID}{:}\Lambda]}$ VP
$laugh_{[\text{ID}{:}\,]}$

  b.

vP
$v_{[\Lambda,\,\overline{u\text{ID}{:}\Lambda}]}$ VP
$laugh_{[\text{ID}{:}\Lambda]}$

  c. $\lambda.$ laugh (δ)

(60b) maps to the semantics as (60c): a λ-abstract predicating the property of laughter over some value for V's $[\text{ID}]$ variable. I use δ here as a kind of meta-variable, or variable across variable types — so δ can end up ranging across individuals ($\mathtt{x}$), situations ($\mathtt{s}$), or anything else we might want to have variables for; I only restrict it here to variables over atomic entities.[17] The reason for generalizing λ-abstraction away from the usual $\lambda\ldots\mathtt{x}$ is that we can of course get arguments that seemingly don't denote individuals — such as gerunds, CP subjects, or locative PPs.

As a predicate, (60b) needs to be saturated, which is achieved by merging an argument of the appropriate type as a specifier for vP. v's situational properties in this case give it the denotation of a process, so the argument is interpreted as the undergoer or experiencer of that process. We get (61a), which is read by the semantics as (61b).

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[17]The eventual interpretation of the variable's range will likely be determined by the kind of property the head it appears on denotes.

(61)   a.

$$vP$$

Arthur v'

$v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$ VP
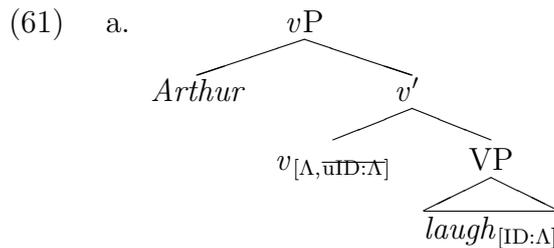
$laugh_{[\text{ID}:\Lambda]}$

b. $\lambda. [\text{laugh}(\delta)]$ (Arthur)

= Arthur laugh

I will go in more detail through my assumptions about argument structure in the next chapter; this is just to show in general how I am taking variables/[ID] features to be introduced and bound.

After (61a), we Merge the V phase's CP, which we will assume isn't doing anything relevant here, and then T. T has an [EPP] feature, i.e. a $[\Lambda]$, and parallel to v, a $[u\text{ID}:\Lambda]$. T can't introduce a new argument, however, which I take to be because it doesn't have the same kind of thematic properties that v heads have: if it did try to, there would be no way to interpret this argument in terms of the thematic properties of the clause. T therefore probes to find matching features. It finds the $[\text{ID}:\Lambda]$ on V, and they Agree;[18] T's $[u\text{ID}:\Lambda]$ is checked, and its $[\Lambda]$ is interpreted as a λ-abstractor over $[\text{ID}:\Lambda]$ on V. This being so, it can only Remerge (= Move, but I assume a non-movement, i.e. copy, analysis of multiple occurrences) the same argument as was Merged for v, since otherwise we would be trying to provide two values for one variable, which isn't coherent. After the T phases's CP is Merged, we get (62).

---

[18]I assume it doesn't see the $[u\text{ID}:\Lambda]$ on v, since this has already been checked; maybe this is wrong and it is visible till spell-out, and that's when checked uninterpretable features get deleted, as in Pesetsky & Torrego (2001). If so, then I assume Λ matches both v and V.

(62)

```
                        CP
                   ╱         ╲
                  C            TP
                        ╱           ╲
                    Arthur           T′
                              ╱           ╲
                        T[Λ,u̅ID:Λ]          CP
                                       ╱         ╲
                                      C            vP
                                            ╱           ╲
                                        Arthur           v′
                                                   ╱         ╲
                                            v[Λ,u̅ID:Λ]         VP
                                                                 ╲
                                                              laugh[ID:Λ]
```
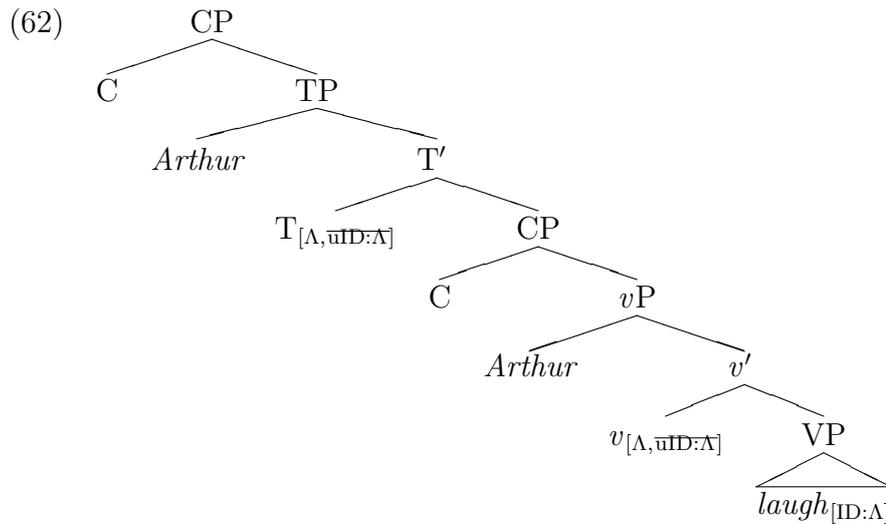
For introducing the variable in the N phase, I assume the following. Root N, say DOG, introduces an unvalued [ID:]. This is Merged with n. n doesn't have a [Λ] (= EPP) feature (*dog* doesn't take arguments,[19]) but I assume it does have a [uID:] feature to get the relationship between the denotation of n and the variable. [uID:] on n, then, probes for matching features, and finds the [ID:] on Root N. They match, and n's [uID:] is checked (63a); [ID:] on N isn't valued, though, since there is no value to be assigned. Then the CP heads are Merged. Say the QP we're wanting to build is *every dog*. In that case, ∀P will have a [∀] feature.[20] Given the approach I am advocating, though, it will be a [u∀] feature. It will also, parallel to the [Λ] features discussed above, have a [uID:∀] feature (63b). It will thus be active, so it will probe. It will find the matching [ID:] feature on N, and they will Agree;[21] [ID:] on N will be valued [ID:∀] (63c).

(63)   a.

```
                    nP
               ╱         ╲
          n[u̶I̶D̶:̶ ]         NP
                     ╱         ╲
                  dog[ID: ]
```

---
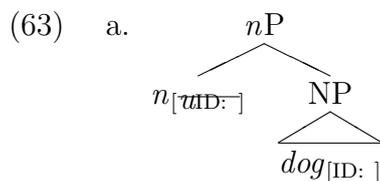
[19]I assume for derived nominals that do take arguments — *assignment*, *invasion*, etc. — that the arguments come from v structure below the Merge position of n.

[20]I assume the other quantificational CP heads just won't have any features on them.

[21]Possibly it also matches with the checked [uID:] on n; cf. fn. 18.

b.

$\forall P$
        $every_{[u\forall,\,u\text{ID}:\forall]}$ $n P$

$n_{[\overline{u\text{ID}:}\;]}$ NP

$dog_{[\text{ID}:\;]}$

c.

$\forall P$
    $every_{[u\forall,\,\overline{u\text{ID}:\forall}]}$ $n P$

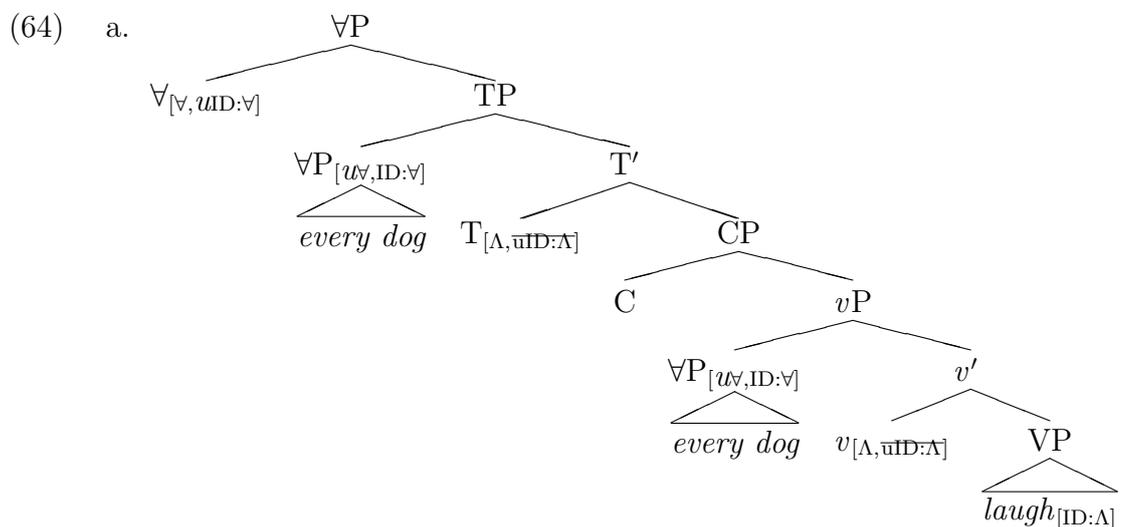$n_{[\overline{u\text{ID}:}\;]}$ NP

$dog_{[\text{ID}:\forall]}$
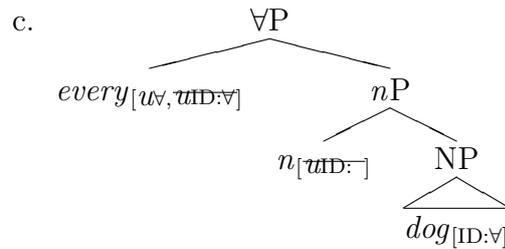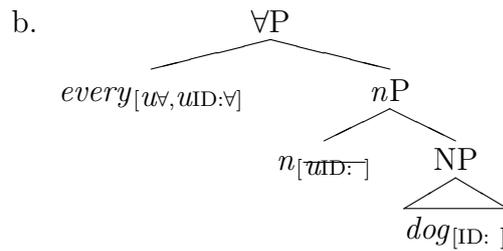
(63c), then, is the general structure I posit for N phases (= QPs): an nP with an associated variable ([ID]), and an uninterpretable quantificational [uQ] CP layer that gives the variable an appropriate value. These are then Merged into the clause, where they Agree with an interpretable clausal Q head that checks [uQ] and becomes interpreted as the binder of the variable.

So say we have the same vP as in (60b). Instead of merging in *Arthur*, we Merge in (63c) *every dog*. The V phase's CP is Merged, and again we'll assume it isn't doing anything important here. T is Merged, and its [Λ] does just what it did in (61b). Then the T phase's CP is Merged, and this is where the interpretable [$\forall$] feature is introduced, along with a [uID:$\forall$] (64a). $\forall$ is thus active, and probes for matching features. It finds them on the N phases. The T phase's [uID:$\forall$] is checked, and so are the N phases' [$u\forall$]s. The T phase's [$\forall$] binds the N phases' [ID:$\forall$]s, and we get (64b).

(64)   a.

$\forall P$

$\forall_{[\forall,\,u\text{ID}:\forall]}$ TP

$\forall P_{[u\forall,\,\text{ID}:\forall]}$ T'

every dog $T_{[\Lambda,\,\overline{u\text{ID}:\Lambda}]}$ CP

C vP

$\forall P_{[u\forall,\,\text{ID}:\forall]}$ v'

every dog $v_{[\Lambda,\,\overline{u\text{ID}:\Lambda}]}$ VP

$laugh_{[\text{ID}:\Lambda]}$

b.

$\forall$P
$\forall_{[\forall,\overline{u\text{ID}:\forall}]}$ TP

$\forall$P$_{[\overline{u\forall},\text{ID}:\forall]}$ T$'$
every dog T$_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$ CP

C vP

$\forall$P$_{[\overline{u\forall},\text{ID}:\forall]}$ v'
every dog $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$ VP

laugh$_{[\text{ID}:\Lambda]}$
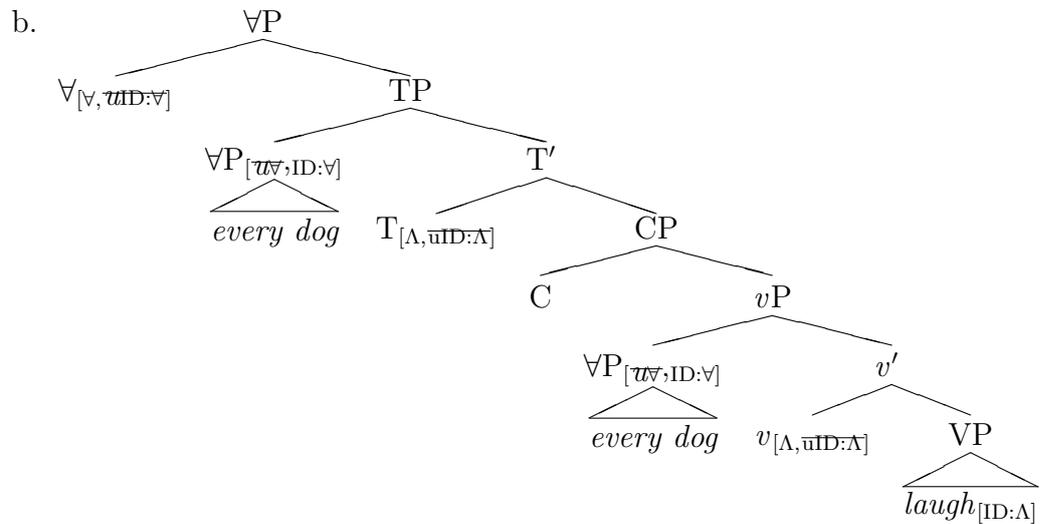
As it stands, this structure is fundamentally equivalent (but see the discussion after (66) below) to Heim's in (56), where the quantifier underwent quantifier construal out of the QP. Recall, though, that we needed to maintain a maximally local relationship between Q and its restriction (57a). This relationship is apparently there in (64), but there are no other scopal elements that could be intervening. If there were — which in the current system means if any of the quantificational heads under $\forall$ in the T phase's CP, such as [NEG], [WH], or [$\exists$/GEN], were in operation — this relationship would be interrupted as in (57b). Since it is quite possible that such elements would be operating,[22] we should make it certain that a maximally local relation is always maintained. I propose to do this via a Spec-head relationship, meaning the $\forall$ head must also have an EPP feature too. So rather than just (64b), we get (65).

Note that in this structure, though, the T phase's $\forall$ head doesn't c-command the N phase in its Spec. We could say that this doesn't matter as it could have already Agreed with it before it moved, but this requires putting an ordering on operations that isn't really otherwise motivated.

An alternative is to redefine the binding configuration in terms of dominance, under a bare phrase structure framework. In bare phrase structure terms, a head isn't distinguishable, featurally, from any of its projections: so each time a head projects, all its features project too. As far as the tree in (65) is concerned, this means that the feature specification [$\forall$,uID:$\forall$] of the T phase's $\forall$ head will also

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[22]In fact I take it that $\exists$ *will* be operating in the general case, to close off a situation variable introduced inside the T phase; see chapter 5.

(65)
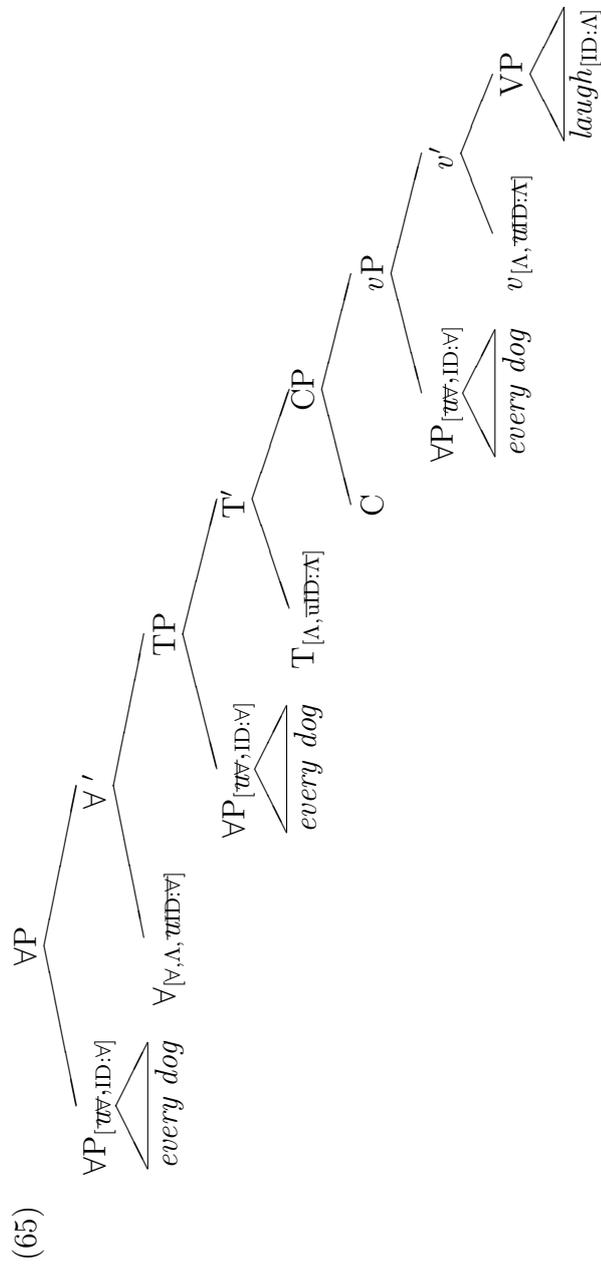
be the feature specification of $\forall$ ' and $\forall$P. The interpretable $[\forall]$ feature will thus dominate both its restriction, in Spec, and its nuclear scope, basically TP down[23] (see Fitzpatrick 2002 for a general critical comparison of c-command and dominance, where dominance seems to fare better).[24]

Hereonin, then, I assume that (65) is the relevant configuration for QP scope relations: an N phase (QP) is situated for interpretive purposes (i.e. at LF) in the Spec of an equivalent clausal quantificational C head. The features of that head, projected to its maximal level, dominate both the N phase restriction and the nuclear scope, which is basically TP in (65) but will in general be the XP sister of the quantificational head, whatever that is. A question arises here, though, regarding how to deal with multiple occurrences: (65) contains three occurrences of the N phase *every dog*; we know we only pronounce one of these, but do we also only interpret one of them?

Often in copy-style theories of movement, it is assumed that this is what we do. Given the way I have defined EPP features, though, this isn't really possible, since each occurrence is Merged to satisfy a predicate, giving a proposition, and the derivation carries on on that basis.[25] If any occurrence subsequently wasn't interpreted the resulting structure would cease be coherent, since we would basically just have lots of holes in the structure.[26] An alternative is to have some kind of rule that converts all the copies but one into a variable linked to the interpreted copy, as in Fox (2002), but this is pretty stipulative, and it would be nice to avoid it. Here, I will go with Sportiche (2002), and claim that we simply interpret everything interpretable. This means all the occurrences of *every dog* in (65) will be interpreted as *dog (δ)*. The semantics will build up as in (66).

---

[23]Note that this derives the relation of m-command (Chomsky 1986) for the cases under consideration.

[24] A treatment of projection on these lines could probably be pushed to an even more minimal level, something like Brody's (1997, 2000a,b) Mirror Theory, or Starke's (2001) *fseq* story, where the distinction between heads and specifiers is effectively neutralized. I won't pursue this here.

[25]For arguments that we need to invoke λ-abstraction to deal with XP movement generally, see Heim & Kratzer (1998); Nissenbaum (1998); Sauerland (1998). Taking their arguments to be basically correct, which I do, the reasoning here still holds even if we don't define EPP features themselves as λ-operators.

[26]Note that the usual problems with regard to interpreting different parts of the QP (operator and restriction) in different parts of the chain for partial reconstruction, in *wh-* constructions for example, don't apply in the theory set out here, because the split between operator and restriction is already built into the syntax.

(66)   a. Merge V and v

$= \lambda. \, [\text{laugh}(\delta)]$

b. Merge vP and *every dog*

$= \lambda. \, [\text{laugh}(\delta)] \, (\text{dog}(\delta_1))$

$= \text{laugh} \, (\text{dog}(\delta_1))$

c. Merge the V phase's CP; Merge T; Merge *every dog* again

$= \lambda. \, [[\text{laugh} \, (\text{dog}(\delta_1))] \, (\delta)] \, (\text{dog}(\delta_1))$

$= [\text{laugh} \, (\text{dog}(\delta_1))] \, (\text{dog}(\delta_1))$

d. Merge T phase's $\forall$; Merge *every dog* again

$= \forall.\lambda. \, [[[\text{laugh} \, (\text{dog}(\delta_1))] \, (\text{dog}(\delta_1))] \, (\delta)] \, (\text{dog}(\delta_1))$

$= \forall.[[[\text{laugh} \, (\text{dog}(\delta_1))] \, (\text{dog}(\delta_1))] \, (\text{dog}(\delta_1))]$

'for every δ_1 such that δ_1 is a value for dog, δ_1 is a value for dog and δ_1 is a value for a dog that laughs'

The syntax–semantics mapping looks idiosyncratic here: a λ-abstract, which basically wants an atomic argument, is being satisfied by *dog(δ)*, which looks like it should be a predicate. However, on the view espoused here it isn't actually a predicate *per se*, because the variable isn't bound by a $[\Lambda]$, and this is what I claim makes predicates. Rather, I treat *dog(δ)* as something more like an unspecified value for the property 'boy' — i.e. some (unspecified) member of the set of boys. See also Kratzer & Shimoyama (2002); Kratzer (2003, to appear b), where a similar view of quantification is espoused and the restricted variables introduced by the relevant QPs are treated within a Hamblin (1973)-type semantics as 'denot[ing] 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 b: 10). *dog(δ)*, then, can be treated as type e, and licitly satisfy a λ-abstract wanting an atomic argument.

We can end up with a pretty big structure, both syntactically and semantically, in this way; however, it is a perfectly interpretable structure,[27] and as Sportiche (2002) points out it either derives or represents, depending how you want to look at it, the conservativity of natural language determiners (Keenan & Faltz 1985) in the syntax. On this view, there's no way they couldn't be conservative.

---

[27]And it is well known that size doesn't matter.

Note that one distinction between the structure in (65) and its interpretation in (66), and that of Heim, as for example in (56), is that in Heim's structure, the quantifier *every* binds both the variable in NP and the variable in VP, whereas in (65), only the variable in NP is bound, whereas the variable in VP is valued by the Merge of *every dog* as an argument for the vP predicate. This might be taken to suggest that this approach is on the wrong track, since if the $\forall$ feature doesn't actually need to c-command/dominate the [ID] feature on V, maybe it would be possible to treat QPs in a generalized quantifier style (Barwise & Cooper 1981), instead of tripartite. I don't think this is the case, though, for two major reasons:

1. If each occurrence of the N phase (QP) in (65) were a generalized quantifier, we would again have difficulties about what to interpret with multiple occurrences: which one scopes, etc.

2. A crucial idea later in the thesis will be that the V and T phases' CP layers are the pieces of structure associated with the scope of quantificational elements other than QPs: standard $\exists$-/GEN-closure of situation variables, and modality, which I treat as quantification over possible situations. If this is different from how QPs get scope, we would need two ways of assigning scope in the syntax, which isn't really desirable.

2 should speak for itself. 1 should be gone into in a little more detail.

If we have multiple occurrences of a QP, as in (65), we get an acceptable interpretation if we interpret it as in (66) — this gives us one quantificational operator, and as many occurrences of the restriction of that operator as there happens to be in the structure. If we had as many operators as occurrences, though, which would be the case if we had generalized quantifiers, we would run into problems.[28] (65) would come out something like *every dog is such that every dog laughs*, when really it should be something more like *... such that that dog laughs*. Scopally ambiguous sentences would give us even more problems. For example the inverse reading of (67a) would come out at best as (67b), which is just wrong.

---

[28]I am not denying here that generalized quantifiers, in the sense of relational quantifiers (*most*, *many*, etc.), exist at all (see §3.2 for how I deal with these along the same lines as the treatment here, following the lines of Szabolcsi (1997)). Rather, I am suggesting that we are better dealing with the scope behaviour of QPs uniformly (tripartite) whether they are relational or not.

(67)   a. Someone loves everyone

        b. Everyone is such that someone loves everyone.

There are ways of getting round this. One way, adopted by Adger & Ramchand (to appear) for [Λ] features, is to say that where you have multiple occurrences of an operator, you only interpret one, motivated by the fact that if you interpret more you get the wrong outcome. This is basically a rescuing strategy or filter, then, to avoid situations like (67b). However, in this sense it isn't that dissimilar to Fox's (2002) trace conversion rule, which Adger & Ramchand explicitly criticize. This method of interpreting QPs seems to involve more work for the syntax than the method I am advocating, and as I don't even treat [Λ]s along these lines, I would be wary of adopting any extension of it to other operators.

Another, based on Tsoulas (2003), would to say that QPs are interpreted as generalized quantifiers, but that the V and T phases' CP heads are endowed with scope features, which Agree with the relevant QPs and assign scope to them. Although this is surely workable, it isn't clear to me exactly how it might be formalized; and though the notion of a scope feature seems intuitively plausible, it is difficult to pin down exactly what such a thing would be. And again, this approach seems to require more machinery than the approach I set out above.

Apart from multiple occurrences, there are also issues from control to look at.[29] In a control structure like (68a), we assume basically that PRO is interpreted as exactly equivalent to its controller, which in (68a) is *everyone*. If we interpret *everyone* as a generalized quantifier, then PRO will be interpreted as a generalized quantifier too, and we'll get a reading like (68b), i.e. *everyone expected everyone to leave*, which clearly is wrong. If we interpret *everyone* as I am claiming here, on the other hand, as a restricted variable like a Heim indefinite, then PRO will be interpreted this way too, and we'll get a reading like (68c), which is the reading we want to get.

(68)   a. Everyone expected to leave

        b. # Every person x expected every person x to leave

        c. Every person x expected person x to leave

---

[29]There are of course theories where control is analysed as involving multiple occurrences, such as Hornstein (1998, 1999). I happen to think these are wrong — see Brody (1999, 2001); Landau (2003); among others, for pretty damaging critiques — but even if they are right the discussion in the text isn't affected.

3.2   Second order quantification

The analysis presented so far deals reasonably with basic $\exists/\forall/\text{GEN}$ quantification, but QPs that aren't well dealt with in such straightforward first-order terms fall less obviously under the theory here.

I will deal with such QPs, following essentially Szabolcsi (1997); Beghelli & Stowell (1997), as introducing not individual-interpreted variables into the representation, but rather 'group'-interpreted variables, which is to say variables ranging over the minimal witness set of the QP. Departing a little from Szabolcsi's and Beghelli & Stowell's precise assumptions, I will refer to these as set variables.[30]

Again following Beghelli & Stowell, I will treat these set variables as subject to closure by the clausal quantificational heads. These means a QP like *two monkeys* will introduce a set variable ranging over minimal witness sets of two monkeys,[31] and this will be closed off by, say, $\exists$. *Two monkeys sat up a tree* then is interpreted as 'there exists a set of two monkeys such that they sat up a tree'.[32]

How does this work? Fundamentally, I claim that such QPs should be dealt with as complex DPs, one embedded in the other, leading to a kind of partitive reading, where the quantificational element is actually a nominal Root category which introduces a set that is part of the set denoted by the lower nominal element — so a QP like *two boys* will denote a set of two elements from within the general set of boys (69). *two of the boys* will denote a set of two elements from within a particular discourse relevant set of boys (70). *The two boys* will denote a particular discourse relevant set of two elements from within the general set of boys (71). Etc.

---

[30]Their analyses treat only indefinites and bare plurals as introducing straightforward individual variables; their DQPs (universals) as introducing set variables; their GQPs as introducing group variables, which they distinguish from set variables; and their CQPs as introducing no variables but being interpreted as standard generalized quantifiers. It may be that some such finer set of distinctions is necessary, but here the point I am making is served adequately by referring just to individual vs. set variables.

[31]Which being sets containing exactly two monkeys, and no non-monkeys.

[32]For space reasons, I factor out — perhaps wrongly — distributive vs. collective interpretations as straightforward interpretive ambiguities; cf. Beghelli & Stowell (1997); Beghelli (1997).

(69)

$$
\begin{array}{l}
\text{CP} \\
\quad \exists_{[u\exists,\,\cancel{u}\text{ID}:\exists]} \quad n\text{P} \\
\qquad\qquad n_{[\cancel{u}\text{ID}:-]} \quad \text{NP} \\
\qquad\qquad\qquad two_{[\text{ID}:\exists]} \quad \text{PP} \\
\qquad\qquad\qquad\qquad \text{P} \quad \text{CP} \\
\qquad\qquad\qquad\qquad\quad \text{GEN}_{[\text{GEN},\,\cancel{u}\text{ID:GEN}]} \quad n\text{P} \\
\qquad\qquad\qquad\qquad\qquad\qquad n_{[\cancel{u}\text{ID}:-]} \quad \text{NP} \\
\qquad\qquad\qquad\qquad\qquad\qquad\qquad boys_{[\text{ID:GEN}]}
\end{array}
$$

(70)

$$
\begin{array}{l}
\text{CP} \\
\quad \exists_{[u\exists,\,\cancel{u}\text{ID}:\exists]} \quad n\text{P} \\
\qquad\qquad n_{[\cancel{u}\text{ID}:-]} \quad \text{NP} \\
\qquad\qquad\qquad two_{[\text{ID}:\exists]} \quad \text{PP} \\
\qquad\qquad\qquad\qquad of \quad \text{DP} \\
\qquad\qquad\qquad\qquad\quad the_{[\text{DEF},\,\cancel{u}\text{ID:DEF}]} \quad n\text{P} \\
\qquad\qquad\qquad\qquad\qquad\qquad n_{[\cancel{u}\text{ID}:-]} \quad \text{NP} \\
\qquad\qquad\qquad\qquad\qquad\qquad\qquad boys_{[\text{ID:DEF}]}
\end{array}
$$
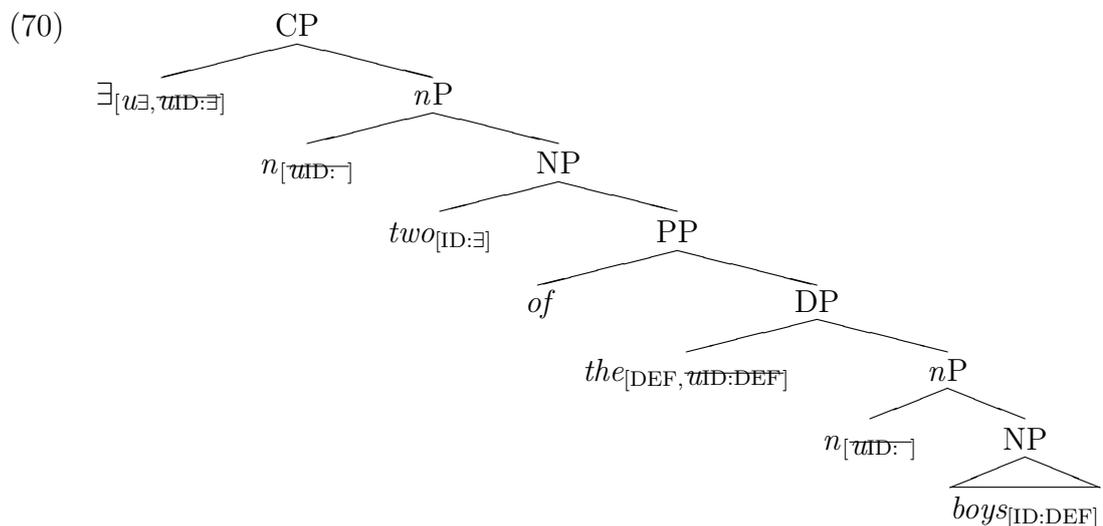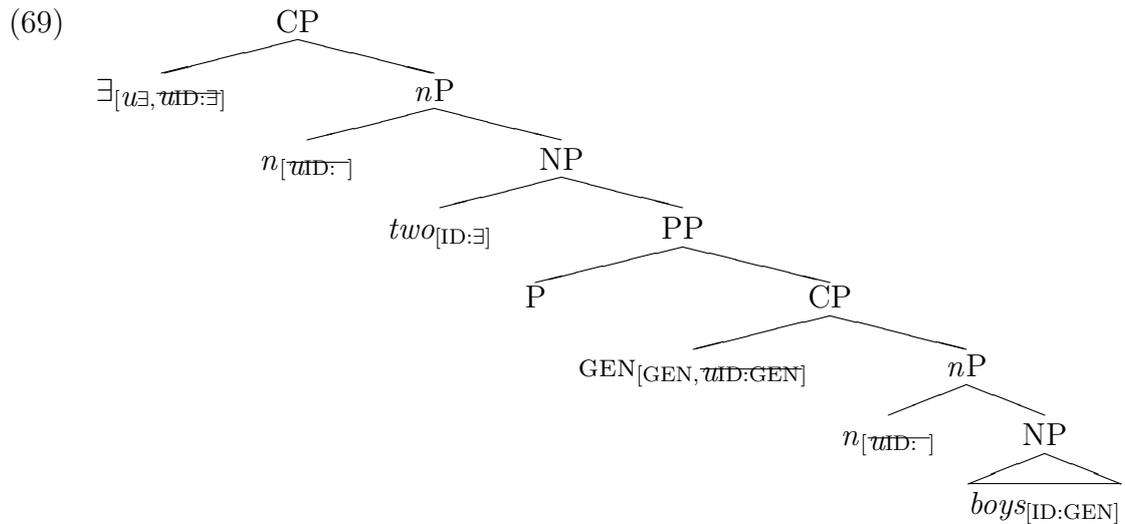
A couple of observations should be made about these structures.

First, I have assumed that the partitive element — which I have labelled P with no especial commitment as to whether it should be distinguished from other preposition-like elements such as locative preopositions, particles, etc. — is the topmost head of the CP structure of the embedded nominals. Treating at least some Ps as the topmost heads of DPs/CPs is by no means a new idea: see among others van Riemsdijk (1978); Emonds (1985); Grimshaw (2003); Starke (2003). This general approach towards partitive Ps seems justified by the existence of partitive determiners in French, Italian, Dutch, etc., where the partitive determiner can be considered to be either a realization of P rather than D, or D incorporated into P. See Chierchia (1998) for an analysis of Italian partitive determiners very similar to the structures given above.

Second, it seems necessary to say in these cases that the embedded nominal

(71)

```
            DP
          /    \
 the[uDEF, uID:DEF]  nP
                   /    \
              n[uID:]    NP
                       /    \
                two[ID:DEF]  PP
                            /   \
                           P     CP
                               /    \
                  GEN[GEN, uID:GEN]  nP
                                    /   \
                               n[uID:]   NP
                                        /  \
                                   boys[ID:GEN]
```
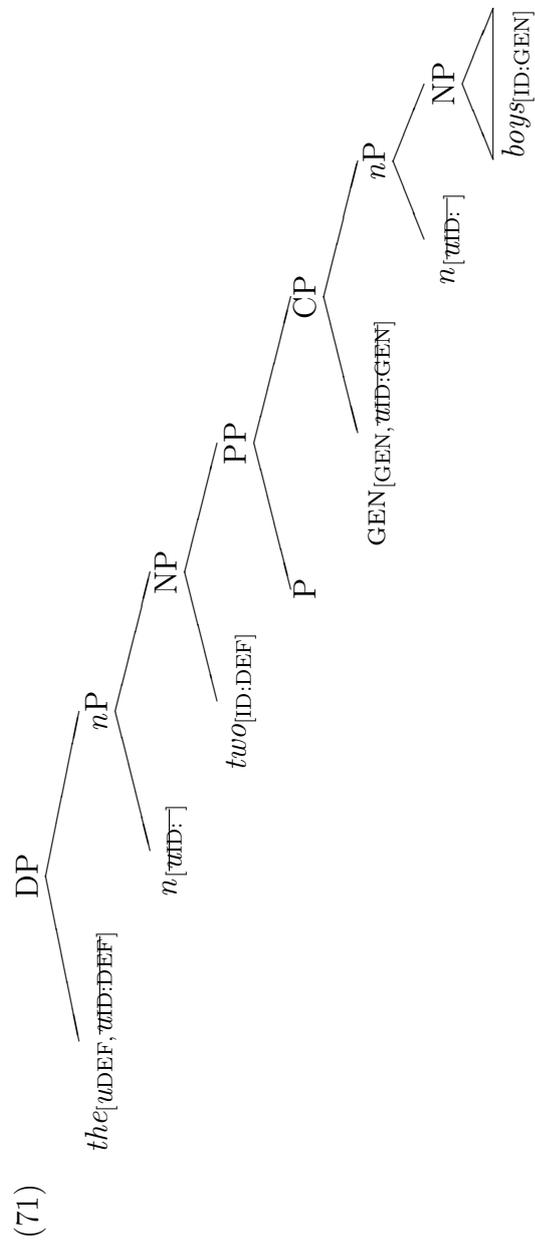
CP has to be closed off independently of the clausal closure heads, and only the embedding 'quantifying' CP is closed off in the usual way. This may just be down to semantics — it would very likely be incoherent if we tried to get the embedded CP out of the scope of its selector in these cases. More interestingly, it may be that the partitive nature of the embedded CP is what leads to this independent closure — cf. Zamparelli (2002), who treats the P as a 'partitive operator'. If non-partitive nominal CPs don't have this level,[33] then the independent closure won't ensue. I won't follow this up here.

In general, though, the treatment of second-order quantification in terms of a second layer of nominal structure seems workable, and I shall assume that it is essentially correct here.

3.3   Summary

The treatment of phase and clause structure posited here leads to a system of scope-taking for N phases (QPs) where they are uniformly treated analogous to Heim (1982) indefinites: any QP introduces a restricted variable, but no quantificational force of its own.[34] The variable is valued by Agreeing with the uninterpretable $[uQ]$ feature introduced by the QP's quantifier, thus making it subject to selective closure by an equivalent clausal quantificational head. The clausal CP heads, then, function as a generalized extension of Heim's $\exists$-closure operator.

This leads to a blurring of the distinction between indefinites and other N phases, and also points towards a blurring of the distinction between D- and A-quantification, both of which are welcome results from a minimalist/reductionist point of view.

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[33]Or it isn't active in their CPs.

[34]The embedded N phase in what I have treated as partitive constructions in §3.2 may be an exception to this, though they may not, given the suggestion of Zamparelli (2002) that they contain a 'partitive operator' which plausibly leads to this clause-independent closure.

Chapter 4

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Phase Domains

In phase theory generally, two distinct fields are taken to exist within the phase: the domain, and the edge. The domain is the lexical core of the phase, dealing with such matters as predication and argument structure, and the edge is the part above the core, functioning essentially as a space for movement and non-core interpretive aspects (force, focus, etc.). Though this split certainly seems to play a role once the general notion of phases is in place, in the orthodox version of the theory there is no real explanation for why this is so.

The system presented here has not just phonological and semantic parallels between the standard vP and CP phases, but also syntactic: phases have a CP-like left periphery. This not only buys us (equivalents of) the two standard phases straightforwardly, but also gives us an account for the distinction between the properties of the edge vs. the domain: the two layers deal with fundamentally different aspects of interpretation. The edge is the CP level, being that part of the phase that deals with quantification/closure over variables introduced within the phase; the domain of the phase is the level below C, dealing with situational and predicative information. This chapter will detail what I take the internal structure of phase domains to be. For more on the edge–domain split, see chapter 7.

4.1   Argument structure and situation structure

The assumptions about argument and situation structure adopted here will follow the lines (not always straight ones) leading (not always directly) from Marantz (1984) through, among others, Larson (1988); Hale & Keyser (1993); Harley (1995); Kratzer (1996); Borer (1998); Ramchand (2002, 2003); Pylkkänen (2002a); Marantz (2001); Svenonius (2001); and other work by most of these authors also.

The influential (1996) paper by Kratzer will be taken as a baseline for the analysis that will be presented, but it will be noted where other literature diverges from this, if relevant, and also where I diverge from it.

4.2   Argument severing: Kratzer (1996)

Kratzer's starting point is the observation by Marantz (1984) that various data suggest a verb's 'external argument' in the sense of Williams (1981) isn't in fact a true argument of the verb at all; rather, Marantz claims it is an argument of the whole VP predicate. His reasoning is based on the fact that particular types of internal argument correlate to particular interpretations of the verb, as in (72-74): something which isn't true of external arguments (75–76).

(72) a. throw a baseball

b. throw a party

c. throw a fit

(73) a. take a book from the shelf

b. take a nap

c. take an aspirin

(74) a. kill a cockroach

b. kill a conversation

c. kill an evening watching TV

(75) a. The policeman threw NP

b. Aardvarks throw NP

c. Throw NP!

(76)   a. Harry killed NP

b. Everyone is always killing NP

c. Cars kill NP

(Marantz 1984: 25–26)

Kratzer agrees that external arguments aren't true arguments of the verb, but she argues that Marantz's claim that they are arguments of the whole VP predicate is difficult to maintain satisfactorily in any explicit semantic way. She rather suggests that external arguments are arguments of a separate functional head immediately dominating VP, which she labels Voice. The external argument is Merged in the specifier of Voice, the internal argument (direct object) is Merged in the specifier of V, and any other arguments (indirect objects) are Merged as specifiers of further functional projections, taken by Kratzer to be below VP, and tied to the semantics of the verb via some kind of secondary predication (this is about as specific as Kratzer gets on this last point).

The way this works is as follows. Kratzer assumes that all verbs select something like a Davidsonian event argument (Davidson 1967). That *all* verbs take such an argument is important,[1] and as often noted it makes the label 'event' argument somewhat confusing, since the selection of an event argument by non-eventive verbs is rather counter-intuitive. However, Kratzer points out that her use of the term is not restricted only to events *per se*, but covers 'actions, states, events proper, and so on' (p.122), noting that she means something corresponding closely to the 'situation types' of Smith (1991). I therefore, following Higginbotham (1987), himself drawing from the terminology of Barwise & Perry (1983), use the term 'situation argument' rather than 'event argument'. The function of the situation argument is to specify what situation type is expressed by the verb. The situation argument is realized for Kratzer as an implicit variable $\mathbf{s}$, present in the semantic representation but not in the overt syntax. It is therefore sensitive to semantically orientated operations (binding, quantification, etc.) but not to syntactically orientated operations (raising, incorporation, etc.).

Kratzer's Voice head selects for V: more precisely, it selects for the situation type of V. As noted, V's situation argument specifies the situation type of V:

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[1]Kratzer is of course by no means the only person to make this particular assumption: cf. Higginbotham (1987); Barwise & Perry (1983); Chierchia (1995); etc.

action, state, event proper, etc. 'An action predicate like *wash the clothes*,[2] then expresses a partial function that is only defined for actions. A stative predicate like *own the clothes* expresses a partial function that is only defined for states' (p.122). Different Voice heads select for different situation types; generally, the Voice head has a denotation like (77), where $\mathtt{x}$ is a variable over individuals, $\mathtt{s}$ over situation types (which is restricted for the type of situation it can take in, depending on the particular Voice head; see immediately below), and 'thematic role' varies according to the selectional properties of the specific Voice head in question.

(77) $\lambda\mathtt{x}.\ \lambda\mathtt{s}.$ [thematic role $(\mathtt{x})$ $(\mathtt{s})$]

         (modified from Kratzer 1996: 122 ex. 22)

When Voice is Merged with VP, a compositional principle Kratzer labels 'Event Identification', called here 'Situation Identification', kicks in. Situation Identification is a type of conjunction operation that identifies the situation referred to by the Voice head and the situation referred to by the VP as the same:

> [Situation] Identification makes it possible to chain together various conditions for the event described by a sentence. It takes a function f and a function g (order irrelevant) as input and yields a function h as output. Input functions f and output functions h are of type $<e,<s,t>>$ [where e is the type of individuals, s is the type of situations, and t is the type of truth values]. Input functions [g] are of type $<s,t>$.'

> (Kratzer 1996: 122)

Situation identification is constrained in terms of semantics, in that it can only operate over two input functions that specify the same situation type: a Voice head specifying an action will never be able to select for (or will cause the derivation to crash if it does select for) a VP whose situation type is a state, for example.

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[2] Note that in Kratzer's system a VP predicate is a predicate only over situations, not over individuals as it would be if V did select its external argument itself; that is, VP is saturated in terms of its overt argument structure, but it still denotes a predicate because its situation argument is a variable which needs to be given a concrete value.

The upshot of this is that we end up with a strucure that can be represented as a λ-expression of the type in (78): that is, predicating over a situation and over an individual.

(78)   λx. λs. [... (x) (s)]

Since one of the selectional properties of the Voice head is that it selects for a thematic argument in its specifier, it now does so, the thematic role of this argument being determined by the situation type described by the expression as a whole (i.e. an action situation will require an agent subject; a stative situation will require what Kratzer (p. 123) calls the 'holder' of the state — an experiencer, for example — and so on). This argument undergoes functional application with the λ-expression denoted by Voice plus VP (i.e. 78), and we end up with a one-place predicate over situations again.

This conception of how the external argument is introduced into the derivation is reminiscent of the v analysis of Hale & Keyser (1993); Chomsky (1995), itself in many ways a refinement of the Larsonian shell analysis of VP (Larson 1988).[3] In this analysis, VP is dominated by a 'light' verb v which has some kind of agentive or causative semantics: the external argument is Merged in the specifier of v and is thus construed as the agent or causer of the event referred to by the verb. In both the Voice and the v analyses, it is assumed that V moves to the higher functional head for some kind of checking, thus deriving the right word order.[4] Hale & Keyser introduce the term 'l-syntax', for lexical syntax, which has been generally adopted to describe this syntactic decomposition of the traditional verb and VP into smaller lexical and functional units.

Subsequent work along l-syntax lines has concluded that in fact there are a number of such functional heads in VP, each with a particular contribution to make to the argument structure of the full VP complex. The basic intuition is that traditional θ-roles such as AGENT, PATIENT, THEME, GOAL, etc., derive directly from arguments being Merged as the specifiers of particular functional heads and being associated with the particular meaning of those heads; in a sense deriving some-

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[3]Though for Hale & Keyser, and others in the same tradition, external arguments are still represented in the lexical specification of the verb, something explicitly out in Marantz's and Kratzer's stories.

[4]For related ideas and discussion, cf. Johnson's (1991) μ head, Larson's (1988) higher V head, Bowers' (1993) Pred head, etc.

thing like the Universal Theta Alignment Hypothesis (UTAH) of Baker (1988)
via the semantics of functional heads; or more radically ridding us of θ-roles al-
together (Borer 1998). Argument structure, then, is on this view determined by
the structure of the various functional elements inside the VP complex.[5] The
question then is what determines this functional structure.

4.3   Situation structure

The answer is provided by the other major strand of l-syntactic decomposition of
VP, which is that the whole complex structure spells out situation structure syn-
tactically: the conclusion is thus that argument structure is directly determined
by situation structure (Borer 1998; Ramchand 2003; and many others). In this
kind of approach, the situation is built up compositionally from what have been
called 'sub-events' (Parsons 1990; Pustejovsky 1991; etc.), which following my
general practice I translate here as 'sub-situations'. Each sub-situation can be
taken to correspond to a particular 'little' head. A number of these approaches
diverge from Kratzer's in that they claim situational semantics is never part of the
meaning of the lexical verb V, but is introduced only by little heads (Harley 1995;
Pylkkänen 2002b); indeed many claim that there is no lexical verb V, but rather
simply a lexical Root of no syntactic category, which is in a sense 'assigned' its
eventual category by the particular little head (like v, or nominal and adjectival
equivalents n and a) that selects it or its extended structure (i.e. it plus any lit-
tle heads already associated with it) (e.g. Marantz 1997, 2001; Harley 1995 etc.;
Svenonius 2001; Ramchand 2003; etc.). For Ramchand, and her collaborators
(Butt & Ramchand 2001; Folli & Ramchand 2002), this lexical Root doesn't even
project but is rather directly inserted in the lowest (operational) functional head
in the verbal complex.

Of course all these approaches differ with regard to what exact little heads exist,
and to how exactly situation structure builds up. A full review of these differences
isn't really possible or necessary here; rather than go through them I shall simply
proceed on a particular set of assumptions deriving from various of the analyses.

First, I take it that lexical V is better thought of in terms of a lexical Root under

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[5]It is important to note here that not all little heads must necessarily introduce an argument:
what the head actually is, and what construction it appears in, may make a difference to this.

some verbalizing v head(s); to distinguish the verbal domain from other lexical domains, however, I shall continue to use the term V.

Second, following Harley (1995) etc., I take it that situational semantics is dealt with by little heads rather than Roots. Thus a number of (sub-)situation-denoting little heads are introduced in the l-syntax above the Root (assuming, *contra* Ramchand, that the Root does project, though this isn't crucial). This means that this structure must be more complex than just a single head like Kratzer's Voice, and I assume that this is so. Specifically, I assume that a number of v-level heads are introduced, one for each sub-situation that can be discerned for a particular verb; each of these v heads is associated with (and thus in a sense licensed by) an [ID] feature on the V Root. So an intransitive verb will introduce one [ID] feature and be associated with one v head; a transitive will introduce two. I assume also a further v head above the 'basic' argument heads, to deal with Kratzer's (1995) version of the event argument; see §4.3.1 for details.

For the most part, this fine-grained structure of the verbal complex won't matter, so for compatability with the literature generally I will use v as a cover term for the whole complex of little heads above the verbal Root, with the understanding that this is a shorthand.[6]

Third, I assume that since event structure determines argument structure, no argument is directly introduced by the Root (since only little heads have situational semantics, only little heads can introduce arguments).

There are other important aspects of little heads that haven't been mentioned so far. Two of the main ones are: (1) accusative case checking/assignment: argued to be taken care of by one or other little head; and (2) passivization: argued by Kratzer to be a function of the Voice head, her analysis based broadly on those of Jaeggli (1986), Roberts (1987), and Baker et al. (1989); Marantz (2001) posits that Voice takes care only of the active voice (given its generally agentive nature), and that there is a separate PASS head that takes care of passives. Again, these aren't directly relevant to the point being pursued here, so they won't be addressed now.

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[6]Following the discussion in chapter 2, particularly Sportiche's and Hallman's work, it may well be the case that we would wish to decompose this complex l-syntactic structure into a number of CPs, one dominating each monadic sub-predicate. I leave this for now.

4.3.1   Expletives

Kratzer (1995) differs from Kratzer (1996) in that in the 1995 paper, she assumes
that some verbs take an event argument, whereas some don't, and she buys
various facts from the consequent presence or absence of eventivity; whereas in
the 1996 paper, as mentioned above, she assumes that all verbs take such an
argument, but that it can denote various different kinds of situation, not just
events proper. The same facts can thus be bought based on whether the argument
is interpreted as eventive or not for a particular verb. A further distinction
is that in the 1995 paper, Kratzer introduces her event argument overtly, as
the outermost argument of the verbal complex, whereas in the 1996 paper she
introduces it 'covertly', as a variable associated with the verbal heads, as discussed
in §4.2.

I think Kratzer's two versions of the event argument/variable can be profitably
distinguished here: in her 1995 paper, Kratzer remains agnostic about whether
the event argument she claims is the outermost argument of the verbal complex
really denotes an event, or whether it denotes the spatiotemporal location for an
event.

I am assuming here that the lower v heads, which the equivalents of, in Kratzer's
1996 paper she treats as introducing situation variables, actually denote situa-
tions, with their arguments tied to them by [Λ] features binding [ID] variables on
V as in chapter 3. These v heads build up compositionally to provide a macro-
situation. This means they don't provide situation variables in themselves. The
situation variable, I assume, is provided just like other variables in the verbal
complex as an [ID] feature on V. Inasmuch as [ID] variables are like argument
slots, then we really have something much more like the situation argument of
Davidson (1967). If there is an [ID] feature for the situation argument on V, then
given the way I have set my system up, there has to be a v level head associated
with that [ID], and this is where Kratzer's 1995 story comes in. For Kratzer
(1995) the head associated with the situation argument is the outermost head
of the verbal complex, i.e. in my terms the outermost v. The semantics of the
outermost v is basically spatiotemporal location, as posited in Kratzer (1995). If
this v head has a [Λ], that will bind the situation argument [ID], and we would
Merge an argument relating to the situation; if it didn't have a [Λ], we'd have an
unbound variable, which would therefore be subject to ∃-closure.

So what if we do have a $[\Lambda]$ on the situation argument's v head? I suggest that in this case, what is Merged is an expletive, which I take to work as follows.

The Chomsky-style analysis of expletives (basically, expletives are semantically null elements that simply serve to check syntactic features and are deleted at LF) is widely assumed, despite the fact that expletives are known to be associated with particular semantic effects: definiteness effects are probably the most discussed; theticity effects less so. However, various people have convincingly propounded an alternative view of expletives that does take into account their semantic effects, where they relate not to the subject of vP (as this is normally understood), but rather to its situation interpretation. Higginbotham (1987) treats expletives as overt existential quantifiers over the Davidsonian event variable; Stowell (1991) treats them as overt existential situation argument QPs (like Kratzer's 1995 event argument), Merged as the outermost argument of vP and thus selected, because of locality, for raising by EPP on T; Ramchand (1996) treats them similarly, as the (raised) overt realization of Kratzer's (1995) event argument; Felser & Rupp (2001) extend Ramchand's treatment, relating it to Diesing's (1992) mapping hypothesis and proposing a treatment of Transitive Expletive Constructions that fits with their analysis. These can all be seen as different instantiations of basically the same idea, that expletives function as situation anchors.

The way I take this to work, which corresponds most closely of the approaches cited above to Stowell's, is this. Above, I have described a view of situation/argument structure based on a decomposition of the v-layer into as many v heads as can be discerned sub-situations, plus an extra one on the very top of vP that provides a spatio-temporal location for the macro-situation. Any argument associated with this highest v head will be interpreted as the 'holder' of that location, in the same way that an argument of a state-denoting v head is interpreted as the holder of that state; an argument of a process-denoting v head is interpreted as the undergoer of that process; etc. Given the compositional nature of the v-layer, the highest v head will be interpreted as the location for the whole complex below it, in the same way that a cause-denoting v on top of a state-denoting v will be interpreted not just as a cause generally but a cause of that state. Spatiotemporal v on top of this would give us the location of the cause of the state.

For non-expletive sentences, I assume standardly that the situation variable is subject to $\exists$-closure. I treat expletives, on the other hand, following Stowell

(1991), as overt existential QPs over the variable, just like any other argument of a v head. This results in the expletive being raised where there is one, because of locality, and the subject (standardly construed) being raised when there isn't.

In order for spatiotemporal v to be associated with the [ID:] on V, we want it to be active. Just as for v in the examples in chapter 3, then, we assume it has a [uID:] feature. Given that the situation argument is generally covert, we don't take Event^0 generally to have a [Λ] feature. So what happens?
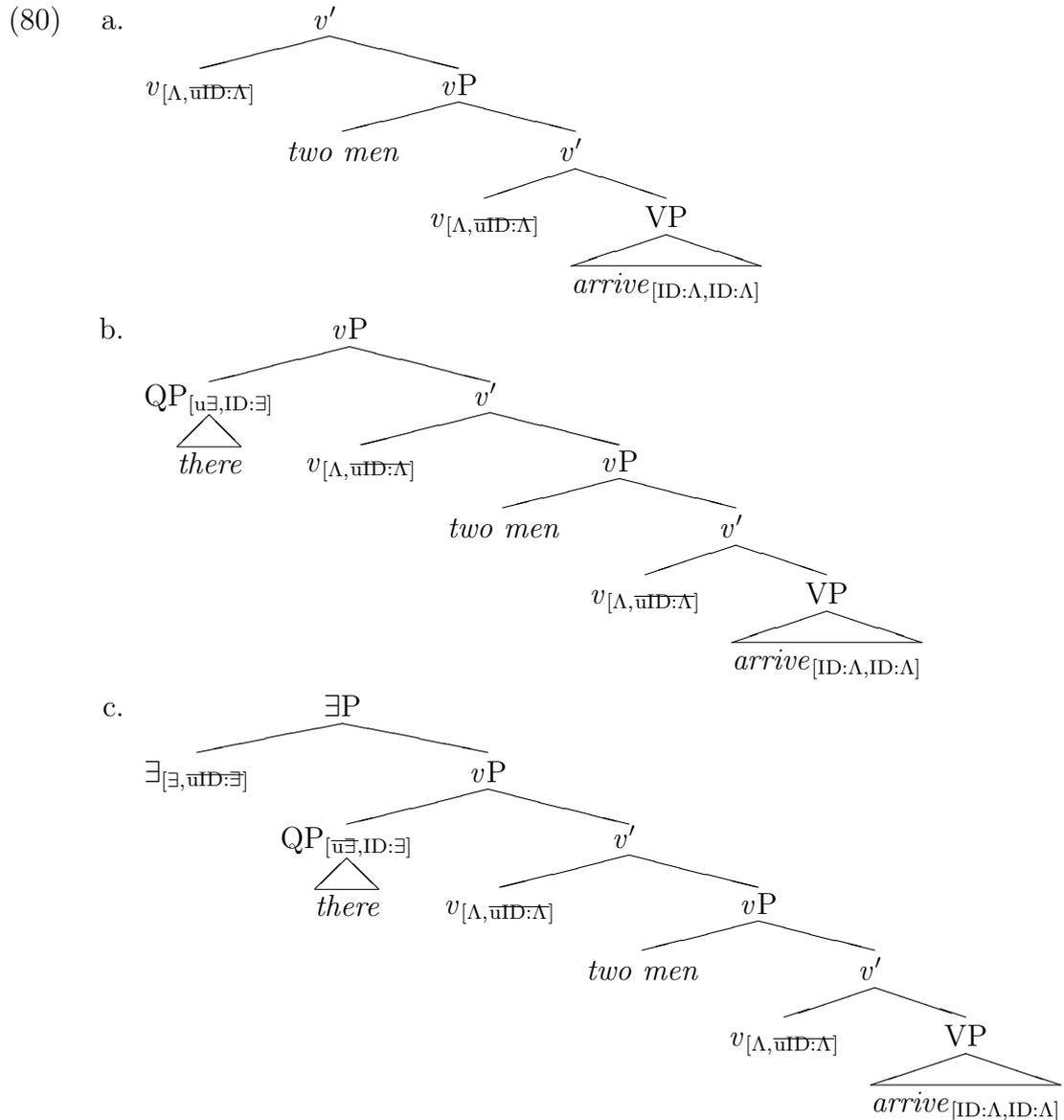
We have a structure like (79a). V has introduced two [ID:] features. One of these has been bound by [Λ] on v, creating a predicate that has been satisfied by Merge of *Arthur* as Spec. The other has matched with the [uID:] on spatiotemporal v, but has not been valued (bound). It is thus a variable over holders of the spatiotemporal location of the process of Arthur laughing. As a variable it is available for binding, and I make the standard assumption that it is bound by ∃-closure. I take ∃-closure to be represented by the ∃ head of the V phase's CP; this will be gone through in more detail in chapter 5. ∃ introduces an interpretable [∃] feature and an uninterpretable [uID:∃] feature, in line with what we saw for ∀ in (64) above. [uID:∃] probes for matching features. It finds the [uID:] on spatiotemporal v, matches it, and values it [uID:∃]; given the Agree relation that already obtains between spatiotemporal v's [uID:] and V's [ID:], V's [ID:] is also valued [ID:∃]. We have (79b).

(79)   a.

vP
- $v_{[\overline{u\text{ID}:}\ ]}$
- vP
  - *Arthur*
  - v'
    - $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
    - VP
      - $laugh_{[\text{ID}:\Lambda,\text{ID}:\ ]}$

b.

$\exists P$
- $\exists_{[\exists,\overline{u\text{ID}:\exists}]}$
- vP
  - $v_{[\overline{u\text{ID}:\exists}]}$
  - vP
    - *Arthur*
    - v'
      - $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
      - VP
        - $laugh_{[\text{ID}:\Lambda,\text{ID}:\exists]}$
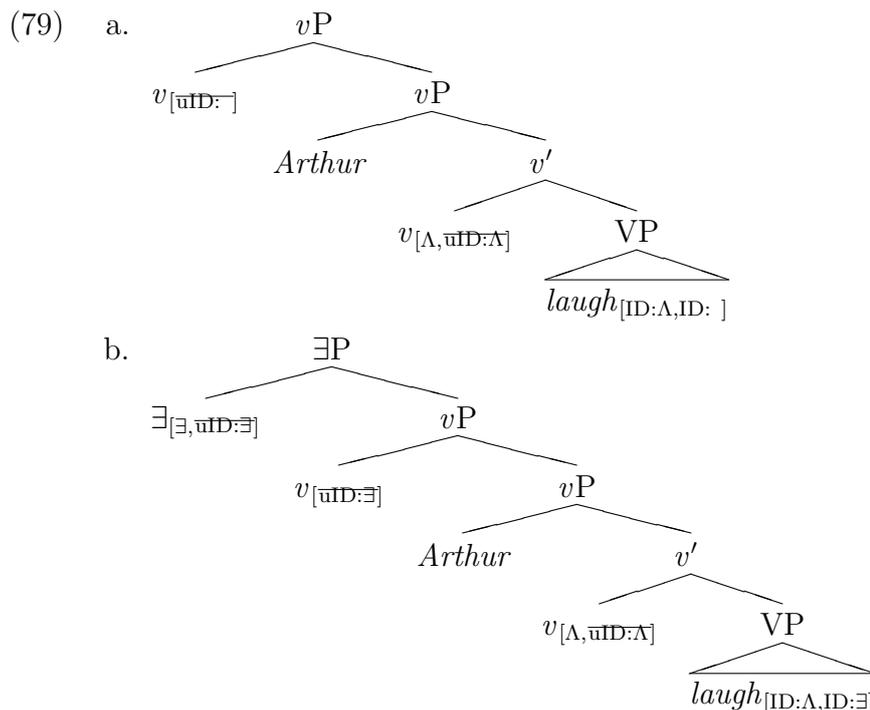
This is how I take ∃-closure to work in the cases seen so far. For expletive constructions, I follow Stowell in taking expletive *there* to be (in my terms) an existential QP over situations, realized as an overt argument of spatiotemporal *v*. The derivation is then as in (79b), following just the same lines as the derivation featuring QP *every dog* in (64), chapter 3.

(80) a.

v'
- $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
- vP
  - *two men*
  - v'
    - $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
    - VP
      - *arrive*$_{[\text{ID}:\Lambda,\text{ID}:\Lambda]}$

b.

vP
- QP$_{[u\exists,\text{ID}:\exists]}$
  - *there*
- v'
  - $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
  - vP
    - *two men*
    - v'
      - $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
      - VP
        - *arrive*$_{[\text{ID}:\Lambda,\text{ID}:\Lambda]}$

c.

∃P
- ∃$_{[\exists,\overline{u\text{ID}:\exists}]}$
- vP
  - QP$_{[\overline{u\exists},\text{ID}:\exists]}$
    - *there*
  - v'
    - $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
    - vP
      - *two men*
      - v'
        - $v_{[\Lambda,\overline{u\text{ID}:\Lambda}]}$
        - VP
          - *arrive*$_{[\text{ID}:\Lambda,\text{ID}:\Lambda]}$

(80c) means something like 'there exists at some spatiotemporal location an event of arrival by two men'. We Merge T, and the derivation continues basically as we have already seen: T probes and finds matching $[u\text{ID}:\Lambda]$ on spatiotemporal *v*. They Agree and a semantic dependency is obtained between $[\Lambda]$ on T and the $[\text{ID}:\Lambda]$ on V that is bound by spatiotemporal *v*'s $[\Lambda]$. We thus have to Remerge *there* as [Spec, TP] for the same reasons we had to Remerge *Arthur* in (62) and

every dog in (64), in chapter 3. As to deriving the right word order, I adopt the standard position that V *arrive* moves up to the highest v-level head, i.e. spatiotemporal v.[7]

Given the general 'aboutness' property associated with the subject (in subject prominent languages, at least), this means that sentences with and without expletives will be about different things: without an expletive, the sentence will be 'about' the (standardly construed) subject, and what it will tell us about the subject is its properties. This is what is known as a categorical or presentational sentence. With an expletive, the sentence will be 'about' the situation argument, and what it will tell us about is the properties of the situation. This is a thetic sentence. One of the things often noted in the literature on theticity is that expletive sentences do indeed receive thetic interpretations. If we adopt this view of expletives, then this fact falls straight out.
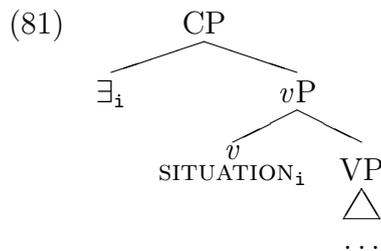
To sum up, then, we have the following:

- The 'verb' decomposes into a lexical Root and a series of little functional heads, this whole series to be shorthanded here as v.

- Each of these functional heads introduces a sub-situation, all of which build up compositionally to give the macro-situation denoted by the predicate; the Root itself doesn't contribute situational semantics.

- Each of these sub-situations is systematically related to a possible argument, Merged in its specifier. In this way argument structure is directly determined by situation structure.

- The macro-situation so derived is given a spatio-temporal location by the highest v-level head, giving us what basically corresponds to Kratzer's (1995) version of the classical Davidsonian event variable.

- Above the event and argument structure, there is a CP level containing

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[7]It has been pointed out to me by several people that I predict the possibility of transitive expletive constructions (TECs) universally on this view, and therefore have a problem when it comes to languages, like English, that don't have them. In fact, I don't really have any more of a problem than anyone else since any story for expletives needs to explain this distinction. It seems to me, also, that predicting TECs universally, and then having to explain the exceptions, is the right way to go, rather than treating TECs as the exceptions, since it gives us a more general theory.

quantificational information which serves to 'close off' the situation; this closed-off situation is the vP phase.

This gives us a (simplified) structure for the vP phase as in (81).

(81)

```
              CP
          ┌────┴─────┐
         ∃ᵢ          vP
                  ┌───┴────┐
                  v        VP
               SITUATIONᵢ  △
                           ⋯
```

The idea being defended here is that (81) in fact gives us the general structure for *all* phases: a predicative Root, dominated by little heads encoding situation structure, dominated by a CP. The basic clause structure, on this view, is represented in (82).

(82)

```
                  CP
          ┌────────┴─────────┐
         ∃ⱼ                  tP
                      ┌───────┴────────┐
                      t                TP
                   SITUATIONⱼ      ┌────┴────┐
                                   T        CP
                                        ┌────┴─────┐
                                       ∃ᵢ          vP
                                               ┌───┴────┐
                                               v        VP
                                            SITUATIONᵢ  △
                                                        ⋯
```
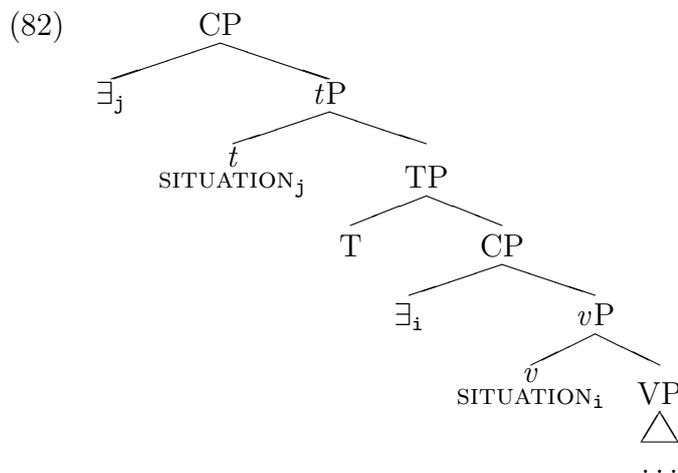
That is, we predict T will be dominated by a little head t which binds a situation-interpreted [ID:] variable introduced on T, closed off by the ∃ operator in the high CP. This basically gives us something like the speech time S. T is a temporal ordering predicate over situations as in Stowell (1996), ordering the situation associated with t (S) and the situation associated with v (E) and thus deriving a propositional reading; this will be gone into in detail in the next chapter. First, though, there are a few other interesting predictions that should be looked at.

4.3.2   t structure

The kind of semantic evidence that has been adduced for positing v, and sub-
sequently for decomposing it into a series of little heads, isn't so obvious for t
without looking in more detail about what t actually does, which will be done in
the next chapter. However, syntactic evidence may be more obvious.

There is a lot of evidence pointing to some functional projection corresponding to
t in the TP domain: a number of authors assume, or have assumed, that there is
some position Agr(S) encoding subject agreement that immediately dominates T
(Pollock 1989 and much subsequent literature); more recently and along different
lines, Cardinaletti (1997, 2000) has proposed that (at least) two distinct overt
subject positions obtain overtly on the edge of TP, perhaps pointing to a finer-
grained structure of t corresponding to the fine-grained structure that v stands
for here. Decomposition of this area of the clause has also been argued for by
Cardinaletti & Roberts (1991); Sportiche (1996); Cardinaletti & Starke (1999);
Manzini & Savoia (2002). One of the main reasons for such a decomposition
is to do with clitic placement: the general pattern seems to be that 1st/2nd
person clitics pattern together, separately from 3rd person clitics. A speculative,
but suggestive, explanation of why this might be is this: the situation that t is
associated with is (commonly) something like the speech situation. Any potential
sub-situations, then, are going to be related to the act of speaking. Person can
be related to the act of speaking in the following way: it has been suggested (e.g.
Harley & Ritter 2002) that 1st and 2nd person mean something like, informally,
'speaker' and 'hearer' — we can make this more formal by having a feature
[±SPEAKER] which relates to the specific speech situation: [+SPEAKER] then
means the person who is speaking in that speech situation, [–SPEAKER] means
the person who isn't speaking in that speech situation. 3rd person then means
essentially 'everyone else' — a kind of 'elsewhere form'. This being so, we have
two basic features relevant for construing person: [±SPEAKER], and whatever we
use to formalize the elsewhere form (it doesn't really matter here). It is of course
entirely possible for these two features to appear on different heads, and it may
be the case that this is why 1st and 2nd clitics pattern together, separately from
3rd clitics, in the analyses cited. These two putative heads would have different
statuses with regard to the speech situation as a whole: the head dealing with
the [±SPEAKER] notion relates to the the people who are directly involved in the
speech situation, the head dealing with 3rd clitics relates to people who aren't
directly involved. It thus may be possible to decompose the t level into smaller

pieces based on the structure of the situation it makes reference to, as with the v level, though as noted this is speculation.

Another justification for the existence of t will be provided in chapter 6, where the situation variable associated with it and that associated with v will be analysed as being what modals quantify over, in a Kratzer (1991)-style analysis of modals as quantifiers over possible worlds — this will be reinterpreted as quantification over situations (cf. Portner 1992).

4.3.3   'Kinds' of phase: situational vs. propositional

Extensionally, phases have been defined in the literature as vP and CP (and possibly also DP), which have been given the (somewhat fuzzy) characteristics of being

> semantically and phonologically coherent and independent. At SEM [the semantic level], vP and CP ... are propositional constructions: vP has full argument structure, CP is the minimal construction that includes Tense and event structure, and (at the matrix at least) force. At PHON [the phonological level], these categories are relatively isolable (in clefts, VP-movement, etc.).
>
> (Chomsky 2001: 22)

Intensionally, then, phases have been defined as propositional elements, albeit in somewhat different senses: CP in the basically standard sense of being evaluable for truth, etc.; vP in that it has full argument structure. Characterizing phases propositionally brings up certain issues that need addressing: the main one being the definition of propositionality at stake. Of course it is perfectly licit to define propositionality in terms of full argument structure for languages of formal logic; but for natural language, this isn't so. It is extensively argued in the semantic and philosophical literature that propositions should be defined for natural language with regard to truth: that is, a proposition is something that can be evaluated in terms of truth or falsity.[8] It is clear that this latter, more usual use of the term proposition doesn't apply to vP. We can never assign truth conditions to

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[8]There are various ways of defining propositions precisely within this basic notion: a proposition might be something that has well-defined truth conditions, something that has a concrete

vP, since information that we crucially need to be able to do so isn't there — in particular, temporal information. For example, as I write, (83a) is true, (83b) isn't; but we assume their argument structure, and their vP structure generally, is the same.

(83)   a. I was drinking coffee

       b. I am drinking coffee

There is no way we can evaluate the information we get from vP as true or false without also making reference to the time at which the event/situation it tells us about took place (event time $\mathtt{E}$); and more specifically, the relation between that time and the speech time $\mathtt{S}$.

It may, then, be the case that both vP and CP are propositional elements, but the definitions of propositionality used in each case are different enough that characterizing phases this way really isn't coherent.

I have already basically abandoned this definition of phases in favour of one based on quantification anyway, but we don't want to ignore propositionality altogether, since, as noted, the T phase really is truth-evaluable propositional, albeit in a derived way, and it differs from the V phase in this respect, which gives us some interesting empirical consequences.

Leaving aside the putative N phase for the time being, and concentrating on the T and V phases, we now have two 'kinds' of phase: that is, we have a distinction in the sort of information that each kind gives us. The sort of information the V phase gives us is to do with the situation under consideration; it perhaps also encodes some discourse-type properties to do with how we want the thematic relations among the participants in the event to be perceived, such as voice (Baker et al. 1989; Kratzer 1996). We can then think of the V phase as a situational phase, as opposed to the T phase, which is a propositional phase.[9] If this is a correct distinction to make (and even on the standard view of phases, or even

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truth value, etc. What precise definition is chosen isn't really relevant here: the point is that natural language propositions need to be defined with some reference to truth, and not simply to argument structure.

[9]Here, propositional should be understood in terms of the interpretation the phase receives — whether it is actually interpreted as a real-world proposition, with a well-defined truth value/set of truth conditions — rather than in terms of argument saturation.

clause structure, it is an obvious conclusion to arrive at — cf. Carlson 2000), we predict that we will find differences in terms of how operators on the two types of phase are interpreted: as operating over situation-related information, or over proposition-related information. Tellingly, we find that just such differences exist.

4.3.3.1   Negation

In Butler (2003a), the interpretive position of sentential negation is tied down to a Rizzi (1997)-type Foc position (see Starke 2001 for a similar assumption): the internal Foc position posited there and here is taken to be the neutral interpretive position for sentential negation, essentially equating to the NegP of Pollock 1989 etc., with the external Foc providing an alternative wide-scope position which is marked in various ways, the evidence there being adduced from the interactions of sentential negation and modality (see also §6.1.2.1).

The claims made in that paper are mainly scope-related, and they don't deal with the proposition/event distinction being discussed here. However, given that these two scope positions for negation obtain, under the assumptions presented here we predict that negation scoping in each of the positions will give rise to distinct readings: specifically, we expect a distinction between negation operating over temporally unanchored situations — i.e. scoping in the internal Foc — and negation operating over fully temporally specified propositions — i.e. scoping in the external Foc. Ramchand (to appear) demonstrates that this distinction shows up quite clearly in Bengali.

Ramchand looks at the morphological and semantic behaviour of two distinct markers for sentential negation in Bengali, *na* and *ni*. The core fact relevant here is the alternation illustrated in (84): *na* is not itself marked for tense, and it co-occurs with a tense-marked (84a) verb,[10] whereas *ni* occurs with a verbal form that is marked only for person (84b), and so apparently carries (past) tense information itself.[11]

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[10]According to Ramchand, *na* is also the form of negation found in non-finite clauses, though as this isn't very relevant for her purposes she gives no examples. Note this is distinct from what we see in (84b) in the text: there, we see a verb that isn't marked for tense, but the clause itself is nevertheless interpreted as past perfective, hence Ramchand's conclusion that the tense (and aspectual) information is carried by negation in these cases.

[11]I am simplifying Ramchand's discussion here a little, since a large part of it relates to

(84)   a. ami   am-Ta   khel-am   na
           I.NOM mango-CLASS eat.PAST-1ST NEG

           'I didn't eat the mango'

       b. ami   am-Ta   kha-i   ni
           I.NOM mango-CLASS eat-1ST NEG.PAST

           'I didn't eat the mango'

As may be discerned from the English glosses, (84a) and (84b) have approximately equivalent meanings. However, Ramchand makes the claim that they are semantically distinct: specifically, that *ni* expresses propositional negation, whereas *na* expresses situation negation.[12] She analyses this in terms of variable binding: *na* is negation binding the verb's situation variable, *ni* is negation binding a time variable (which she takes to be contributed by an Asp(ect) node above *v*P). Various pieces of evidence are offered to back up this position; two of the most straightforward demonstrate that the two constructions are not in free variation — they are felicitous in different discourse contexts, and they show an interpretive alternation with certain temporal adverbs.

One example of the discourse distinction is (85): the third proposition in the discourse contains *na*; an alternative with *ni* would be according to Ramchand 'very infelicitous'.

(85)   John deRi kore uThlo
       John late got up

       'John got up late'

       o   janlar baire   dekhlo
       he out.of the.window looked

       'He looked out of the window'

       o   briSTir kono lokkhon dekhlo   na
       he sign   of   rain   see.PAST.3RD NEG

       'He didn't see any sign of rain'

       tai o chhaTa chere rOna   holo
       so he without his   umbrella left

_____

aspectual factors that aren't directly relevant right here.

   [12]Ramchand uses the term event rather than situation; usual disclaimers apply.

'So he left without his umbrella'

According to pretty standard assumptions about this kind of discourse, the understanding of the set of propositions as ordered sequentially is down to how what Ramchand calls the time variable of each proposition (basically corresponding to the 'speech time' of Enç 1987 or the 'reference time' of Stowell 1996; see §5.1 for discussion) is anchored: rather than being closed off existentially, or anchored via context to the actual time of speech, the speech time variable of each proposition is anaphorically linked to the situation variable of the immediately preceeding proposition. According to Ramchand's analysis, *na* binds a situation variable, not a time variable. *Na* should have no effect on the sequential understanding of the discourse, then: it should simply negate a situation within the discourse. On the other hand, *ni* is analysed as binding a time variable itself. If this is so, then that time variable won't be available for the kind of anaphoric binding of time variables that contributes the sequential understanding of the discourse. This makes *ni* infelicitous in these contexts.

The distinction with regard to temporal adverbs is illustrated in (86).

(86)   a.   kalke   gaRi 'start' hoi   ni
            yesterday car   start   become.3RD NEG.PAST
            'Yesterday, the car didn't start (at all)'

       b.   kalke   gaRi 'start' hol-o   na
            yesterday car   start   become.PAST-3RD NEG
            'Yesterday, the car didn't start (but then the neighbour came and fixed it)'

Ramchand here argues that the adverbial specifies a 'universe of discourse' for the proposition's time variable, i.e. it gives it a concrete range — so that if that variable is then negatively quantified by *ni*, we are talking about it not being the case for the whole of yesterday that the car started. If the situation variable is negatively quantifed by *na*, on the other hand, then all we are saying is that there was some single situation during the time specifed by the adverbial, i.e. yesterday, at which the car didn't start — but this doesn't of course mean that there weren't other situations during that same time frame where it did start.

Similar facts apparently obtain in Dravidian languages, such as Malayalam. According to Thomas Chacko (personal communication), Malayalam also has two

negative morphemes, *illa* and *alla*: the former, like Bengali *na*, is analysable as negating situations, the latter, like Bengali *ni*, as negating propositions.

We can translate this elegantly into the terms of the current thesis, treating *na/illa* as negation scoping in the internal Foc position, ranging over the situation variable of the V phase, and *ni/alla* as scoping in the external Foc position, ranging over the situation variable of the T phase. An analysis similar to Ramchand's should then fall out pretty straightforwardly.

4.3.3.2   Adverbs and adverbials

It is well discussed (since at least Jackendoff 1972) that certain adverbs are ambiguous between a 'manner' (87a) and a 'sentential' (87b) reading,[13] which ambiguity tends to be reflected as in (87) by the syntactic positioning of the adverb.

(87)   a. Oddly, Arthur answered the telephone

= 'It was odd that Arthur answered the telephone'

b. Arthur answered the telephone oddly

= 'Arthur answered the telephone in an odd manner'

In (87a), Arthur may have answered the 'phone in a perfectly normal fashion, but that he answered the 'phone at all is deemed odd; in (87b), that he answered the 'phone may have been exactly what was expected, but the way in which he answered it was unusual (in some unspecified way — e.g. he may have bellowed into it with force, or picked it up with his feet, or given an unusual greeting).

In general, this ambiguity is argued to be scopal, so that sentential readings scope over the clause whereas manner readings scope over the VP, broadly speaking. Semantically, this equates to sentential readings functioning as propositional operators and manner readings functioning as situation (=event) modifers. This is a pretty straightforward way of dealing with the phenomenon, and also pretty standard — it is reflected in approaches ranging from Jackendoff's (1972) highly semantic treatment of adverbial placement, where adverbs can attach where it is semantically appropriate for them to do so, to Cinque's (1999) highly syntactic

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[13]The ambiguity also obtains more generally with adverbial clauses: see among others Sweetser (1984); Declerck & Reed (2001); Haegeman (2002).

treatment, where certain adverb-related projections occur more than once in an articulated clause structure. Exactly how the adverbs should be treated formally, though, is a matter of debate: certainly the manner readings can be treated as predicative over situations, but under most approaches the same isn't true of the sentential readings, since they seem to operate over propositions, and propositions and situations are commonly taken to be discrete atomic objects. Without some tinkering with the theory, this view pushes an analysis where the adverbs at issue are lexically ambiguous, either in the sense that two homophonous lexical entries obtain for each of them, one a predicate over situations, the other a predicate over propositions; or in the sense that the lexical entry for each has to specify that the adverb is ambiguous between these two uses.[14]

The approach to structure building and interpretation put forward here can avoid this kind of lexical ambiguity, however: under the analysis I have proposed, propositions in the truth evaluable sense (rather than the full argument structure sense) are not primitive but derived: specifically, they are situations that embed other situations and are temporally anchored with regard to them. So I have proposed that the V phase's situation argument is just interpreted existentially, whereas the T phase's situation argument is interpreted propositionally because of the temporal predication at that level.

In this case, we can treat the adverbs in question as uniformly predicates over situations. If they operate over the V phase situation, then they get the manner reading, since they will modify the situation argument of the verb — so in (87b), there was a situation of 'phone-answering, which situation was odd with regard to normal 'phone-answering situations — whereas if they operate over the T phase situation, they get the propositional reading — so in (87a), the situation of Arthur having answered the 'phone was odd with regard to more normal situations of something else having happened. (See chapter 6 for a similar treatment of the distinction between epistemic and root interpretations of modality.)

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[14]A possible third view is that the lexical entries are underspecifed, and able to operate over numerous possible elements. However, it seems to me that this kind of underspecification would have to be made very precise, so that it wasn't *too* underspecifed, otherwise it would very easily lead to overgeneration. But then this boils back down to a single lexical entry with ambiguity built in, as in the text. See Ernst (2000) for an approach more along these lines, where adverbs are specifed to take in any element from a particular set of elements Ernst refers to as 'Fact–Event Objects', based essentially around the two primitives 'event' and 'proposition'.

Chapter 5

Temporal Relations

This chapter will deal with how various temporal relations can be dealt with in the system being set out: specifically, tense, (outer/viewpoint) aspect, and infinitives. It will not be my intention here to set out a complete theory of these temporal relations, to critically contrast it in detail with competing theories, etc., since to do this would require (at least) three separate dissertations. Rather, I shall simply set out and justify a particular view of temporal relations, similar to certain views already familiar in the literature, the point being that this view falls out straightforwardly from the theory of phase and phrase structure espoused here.

5.1   The phase structure of tense

I will here take as a given the pretty common view that tense is predicative (Reichenbach 1947; Hornstein 1990; Stowell 1996; Giorgi & Pianesi 1997; etc.), describing a temporal relation between the time at which the situation described by the verb[1] holds (the Reichenbachian EVENT TIME), and one or both of the SPEECH TIME (the time at which the utterance takes place) and the REFERENCE TIME (which can be loosely defined as the 'point of view' from which the verbal situation is to be seen) (see cited references for various instantiations of this notion; the analysis presented below will most closely resemble Stowell's theory).

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[1]Or more precisely, in the approach outlined in the previous chapter, by the l-syntax structure dominating the verbal Root.

This being so, there needs to be included in the structure something which can fix the value of the situation variables associated with v and t, in order for the T predicate to be able to do anything with them (they can't be referred to by T as they stand, since they don't denote situations but are only variables over situations; variables require their value to be fixed by some quantificational/operator type element before they can be referred to by other elements; see §7.4). In §4.3.1 it was claimed that CP layers dominating t and v provided such quantificational elements. The following sections provide more justification for such a position.

5.1.1   Tense and C

It has often been noted that some aspect(s) of the CP domain seems to be relevant to temporal interpretation. Stowell (1982) claims that a 'tense operator' appears in C, fixing the tense interpretation as [±PAST]. Enç (1987) (like many others) convincingly refutes the existence of tense 'operators' *per se*, but (again like others) attributes to C the property that 'in matrix sentences [it] denotes the speech time' (p. 641). Tense too denotes a time for Enç, functioning as the temporal argument of the verb (so essentially denoting the event/situation time), as well as encoding the notions [PAST] and [PRESENT],[2] which serve to order the event time time denoted by T with respect to the speech time denoted by C (so PAST says something like 'I denote a time which is temporally ordered previous to some other time'; this obviously makes no sense by itself, ordering being a relational notion, which is why speech time is required as well). These times, as pronoun-like expressions, can be bound or what Enç refers to as 'anchored', either by context or by government, to receive particular values.

This isn't directly compatible with the direction taken here, but the idea that some aspect of C fixes the speech time and interacts with some aspect of T does fit in. If some element in CP fixes the value of the situation variable associated with t, (i.e. fixes the speech time), the system presented here leads us to expect a similar element in the internal CP that fixes the value of the situation variable associated with v. The obvious assumption is that this would then denote the situation time (more traditionally referred to as the event time). We assume this

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[2]Enç leaves aside discussion of future in this paper; elsewhere (Enç 1996), she convincingly argues like others that there is no tense [FUTURE], but rather that future shifting is a function of modal elements. Elsewhere (Enç 2000) she argues that even [PRESENT] doesn't exist, leaving [PAST] as the only true tense.

is so.

5.1.2   ZPs

Stowell (1996) develops a system of tense which makes crucial use of a time-related functional category ZP:

> I posit an additional functional category ZP (ZEIT-PHRASE) inter-vening between TP and [*v*P]; it is this ZP category that serves as the time-denoting internal argument of T, denoting the Event Time E. ZP is a referential category analogous to DP ... Loosely speaking, ZP bears a structural relationship to [*v*P] that is analogous to the relation that DP bears to NP.
>
> (Stowell 1996: 280)

That is, (some part of) ZP operates over the situation variable in *v*P, giving it a particular denotation, in the same way that DP operates over NP to give *it* a particular denotation. Stowell assumes this is done by an operator in [Spec, ZP] rather than by the head Z itself, which is not the usual treatment of the DP–NP relation, but the general notion is clear enough.

5.1.3   DPs as CPs

It has often been noted that CP and DP are parallel in a number of ways (Abney 1987; Szabolcsi 1989,1994; Siloni 1990; Cinque 1994; etc.). Cardinaletti & Starke (1999); Pesetsky & Torrego (2001, forthcoming) take this observation to its strongest conclusion and postulate that the parallels are in fact absolute, and that D and C are the same category; the same assumption was made in chapter 3.

5.1.4   ZP = DP = CP

The discussion in the above sections comes together very neatly as follows: Stowell (1996) claims *v*P is dominated by a category ZP which is DP-like in its function.

Cardinaletti & Starke (1999); Pesetsky & Torrego (2001, forthcoming) argue that DP is better viewed as CP: that is, nominal phrases are given referential status not by D but by some C element. Stowell's claim, under this view, then becomes that vP is dominated not by a DP-like element but rather a CP-like element. This corresponds very closely to a core proposal of this thesis, that vP is dominated by a CP. In §5.1.1, it was assumed, following the discussion of Enç (1987), that the internal CP hosted some operator-like element which fixed the value of the situation variable in vP, giving it referential status. Again, this corresponds very closely to the function of Stowell's ZP.
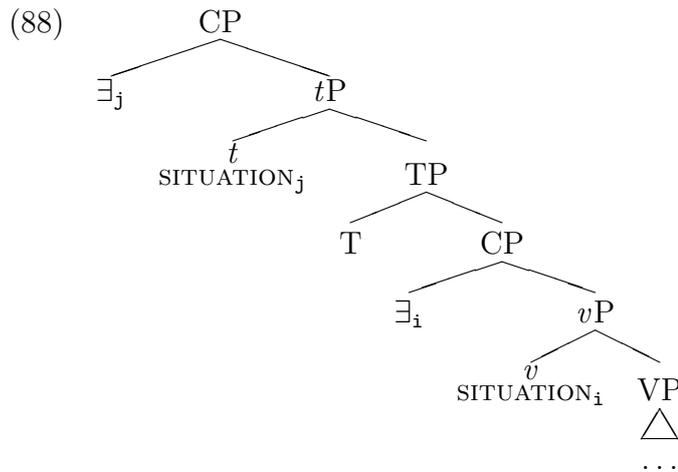
5.1.5   Tying it together

In short, the above discussion reduces to this: vP is immediately dominated by a CP, which contains some operator element closing off the situation variable associated with v and giving it referential status (§4.3.1 went over this in more detail); this operator–variable pair gives us something like the situation time (i.e. event time E). We assume the same thing happens with external CP and the situation variable associated with t, giving us something like the speech time S. The function of T is to specify some temporal ordering relationship between these two objects.

We end up, then, with a structure like that given in (82), repeated here as (88) (many details simplified).[3]

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[3]Actually, if the V phase is an argument of T, and the view of argument structure set out in chapter 4.1 is correct, the structure of the clause will be a little different from the structure shown in (88). Specifically, T would introduce two [ID:] variables, one for its internal argument (the V phase) and one for its own situation argument. The V phase would be Merged as a specifier of a t head, binding one [ID:], in parallel to the way DP arguments of V are introduced as specifiers of v heads; then a second t head would anchor the other [ID:] prior to it being closed off by the T phase's ∃ — cf. the treatment of closure in §4.3.1. This won't make a substantive difference here, but it would make the trees more difficult to read, so in general I keep to a more standard structure, using this more precise structure only where I consider it useful to do so.

(88)

```
                    CP
                  /    \
                ∃ⱼ      tP
                      /    \
                     t      TP
              SITUATIONⱼ   /   \
                         T      CP
                              /    \
                            ∃ᵢ      vP
                                  /    \
                                 v      VP
                           SITUATIONᵢ   △
                                        ...
```

As far as interpretation of (88) is concerned, we can, allowing for certain differences in structure, follow the ideas set out in Stowell (1996).

Stowell decribes tenses as 'dyadic predicates of temporal ordering ... they take two time denoting phrases as their arguments, and it is in these categories that the referential properties associated with tenses reside' (Stowell 1996: 279). In Stowell's system, these phrases are ZPs; here, they are the operator–variable constructions associated with CPs and t/v. The tense predicate 'temporally locate[s] the denotation of E in relation to the denotation of its external argument ... PAST means "after"; *will* means (at least) "before", and PRESENT ... means "simultaneous with" or "overlaps" ' (pp.280-1). This clearly carries over easily to the structure given here.[4]

There are a few terminological differences to clear up. A trivial one is that Stowell refers to event time, I refer to situation time: as discussed in §4.1, this is simply because the term 'event' is potentially misleading, given that it is used to refer to states, etc., as well as events proper. Less trivial is that the term speech time has been used in the above discussion, following from Enç's use of it; Stowell labels the corresponding notion in his system reference time. Neither of these terms quite corresponds to Reichenbach's original Speech time and Reference time; they do, however, correspond quite closely to each other.

Enç's use of speech time covers two things: one is the actual time of speech, and this is what we get in matrix clauses where C is 'anchored' contextually; the other is subordinate speech time, where C is anchored via government: that

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[4]Except that I follow Enç (1996) in taking futurity to be connected to modality rather than a tense predicate; I will also treat PAST and PRESENT as a binary valued [±PAST] predicate.

is, the 'speech time' is determined relative to higher c-commanding temporal information, so that a matrix past tense may shift the subordinate clause 'speech time' to some past time. Speech time in this case, then, is also a misleading term since it denotes the actual time of speech only in a restricted set of circumstances. More neutrally, Enç's speech time is simply the time that the situation time is ordered with respect to.

Stowell's use of reference time is essentially the same; indeed, the very fact that denoting the speech time isn't the basic function of this element is the reason he calls it something else:

> like the internal ... argument [of T], it denotes a time [and] has no fixed indexical denotation; it simply refers to a time relative to which the [situation time] is ordered. In a main clause, it so happens that the Reference Time ... denotes the Speech time S, but in subordinate clauses, it typically denotes the [situation time] of the immediately higher clause.
>
> (Stowell 1996: 280)

Stowell works this by control theory: as mentioned, Stowell's ZP (corresponding here to the operator–variable construction involving the operator in C and the implicit arguments of v/t) is analogous to DP. He assumes more particularly that the reference time ZP is analogous to PRO.[5] In a subordinate clause, this PRO-like element is controlled by the nearest c-commanding ZP; in a matrix clause, it isn't c-commanded by any other ZP, so it behaves like an indexical and denotes the speech time by default.[6] I shall assume Stowell's analysis is basically right, and also adopt his more neutral notation 'reference time' rather than Enç's 'speech time'.

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[5]The situation time, on the other hand, he takes to be analogous to a full DP. In his system, this is captured quite neatly by the fact that the internal (situation time) argument of T is ZP dominating vP — that is, it has internal structure of its own — whereas the external (reference time) argument of T is a 'bare' ZP without internal structure, in the same way that PRO might be taken to be 'bare' DP. In the system here this isn't so simply captured, since the structures of the reference time argument and the situation time argument are exactly the same kind of operator–variable construction, and T doesn't have an external ZP argument in the same way.

[6]The analogy with PRO is somewhat weaker here, since it would be odd to claim that PRO ever behaves like an indexical.

A third difference is that Stowell assumes that ZPs give situation variables the denotation of particular times. What I have said so far leads us to expect that the CPs assumed here will give situation variables the denotation of particular situations, which aren't the same as times, or else spatiotemporal locations of situations, which may or may not be equivalent to times. This reflects a lack of clarity on this subject in the literature generally, but I don't think it matters too much: even if we take just the particular situation route, we probably end up with more or less the same result, since if the function of T is to order two situations temporally relative to one another, then this in itself will have the effect of making the situations (seem to) correspond to times. That is, they will end up apparently denoting times just as a function of the temporal predicate meaning of T. If we take the particular spatiotemporal location route, then this denotation is provided not by anything in CP but rather by the denotation of the t head that is associated with the [ID:] variable. The operator elements in CP can themselves remain underspecified, then, without stipulating that they give any particular temporal value to variables they operate over. This is theoretically preferable on grounds of parsimony; and empirically better also, since allowing the same operators to bind different kinds of variables buys us a host of seemingly unrelated effects with little machinery, as we have seen.

As the bones of an approach to tense, then, this is promising. But it has various specific details that need to be dealt with. In particular, it involves only two times, as opposed to (neo)-Reichenbachian approaches which often make use of three (speech time S, reference time R, and event time E) (though note that both Enç's and Stowell's approaches also involved only two times). The reason a third time gets invoked is to deal with 'complex tenses' such as the English perfect, which can't be dealt with straightforwardly by appealing to just two time points. Therefore it needs to be shown that this system is theoretically capable of dealing with the kind of empirical data that led to the Reichenbachian three-time system.

5.2   Aspect

5.2.1   Complex tenses

Reichenbach's (1947) original proposal to treat tenses as relating three times rather than just two was motivated by the fact that certain constructions can't

be dealt with using just two. In particular, the use of three times allows perfective constructions to be represented. (89a–89c) illustrate the English past, present, and future perfect respectively, and it is clear that the differences in interpretation among these three sentences can't be captured with reference to just two times.

(89)   a. Susan had gone to sleep

        b. Susan has gone to sleep

        c. Susan will have gone to sleep

In the past perfect (89a), the situation time is taken to be ordered before the reference time which is before the speech time: that is, the situation is being evaluated as past from a time which is itself in the past with regard to the speech time. In the present perfect (89b), the speech time and reference time are taken to be simultaneous, with the situation time preceding them. Note that this isn't the same as simple past, where the situation time precedes just the (Stowellian) reference time: there is a distinct difference between (89b) and (90), and it is captured well by the use of three time points.

(90)   Susan went to sleep

In the future perfect (89c), the speech time and situation time are taken to be unordered with respect to one another, with the reference time following both (cf. Comrie 1985; Hornstein 1990). Again, this isn't the same as simply having the reference time follow the situation time, since this would be simple past, clearly not the meaning of (89c). Three time points once more seem necessary to capture the distinction.

In a system where T relates only two time points, this problem obviously has to be dealt with along different lines. Here, I follow Vikner (1985); Zagona (1990); Stowell (1996); Demirdache & Uribe-Etxebarria (2001) in taking the obvious way out and treating perfect aspect as essentially equivalent to a [+PAST] tense predicate. This means perfect tenses generally express a relation between two separate tenses: *have* will order the situation time relative to its reference time argument; the denotation of this reference time argument will be fixed as in the usual case of a subordinate clause, by being controlled by the closest c-commanding time. The implicit situation argument of (the head hosting) *have*, Stowell claims, is a result situation, so that

> the higher tense [T] provides its usual temporal argument structure, locating the time of the result state (the time of 'having') in relation to a Reference Time (i.e. the Speech Time, in a main clause). The past participle provides an additional PAST tense, which orders the thematic verb's [Situation] Time in relation to that of the result state.

> (Stowell 1996: 285)

What we have, then, is a Reichenbachean 3-time system only when we need it, which is a good thing.

5.2.2   Aspectual phases

I am taking the perfect/progressive aspect to be fundamentally equivalent to tense, in that they introduce a new situation into the representation, and place this situation temporally relative to the situation denoted by their complement. Perfect aspect correlates with past tense, ordering the situation it introduces as after the situation denoted by its complement; progressive aspect correlates with present tense, placing the situation it introduces temporally within the situation denoted by its complement. In general this makes perfect sense and is more than reasonable; see for example Stowell (1996) (and the discussion thereof above); Demirdache & Uribe-Etxebarria (2001); Butler (2003b) for extensive justification of what the system can get us.

For this story to run in the present framework, though, it is necessary also to postulate the same general structure for perfect/progressive elements as for the V and T phases: namely, a core predicative category (PERF/PROG), topped by l-syntactic situation stucture (*perf*/*prog*), which is in turn surmounted by a quantificational CP layer — that is, a sentence like (91a) must have a structure like (91b) (words omitted).

(91)   a.   Arthur has been teething

b.
```
              CP
           ╱     ╲
          C       tP
               ╱     ╲
              t       TP
                   ╱     ╲
                  T       CP
                       ╱     ╲
                      C      perfP
                          ╱      ╲
                        perf     PERFP
                              ╱      ╲
                           PERF      CP
                                  ╱     ╲
                                 C      progP
                                     ╱      ╲
                                   prog     PROGP
                                         ╱      ╲
                                       PROG      CP
                                             ╱    ╲
                                            C      vP
                                                ╱   ╲
                                               v    VP
                                                    △
                                                    ...
```
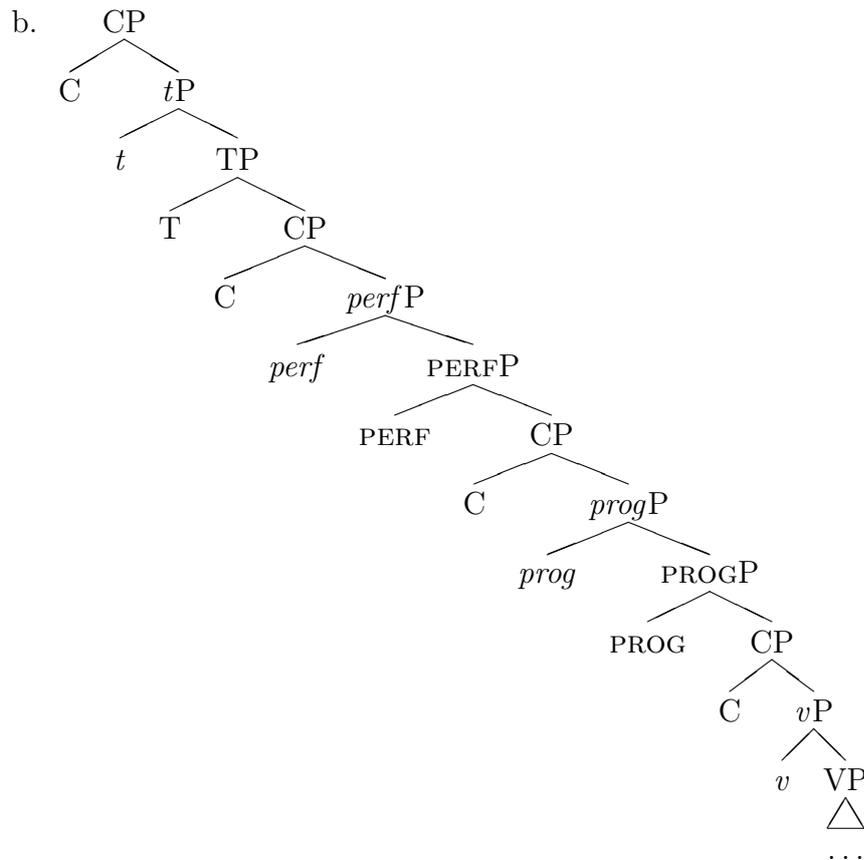
Some questions arise at this point:

1. What empirical (syntactic or semantic[7]) evidence do we have for these 'extra' CP levels?

2. Do we take these CPs to be phases or not?

3. How are we to deal with XP movement — particularly subject movement — through these aspectual CPs?

§5.3 will deal with question 1, showing that negation, argued in chapter 6 to be related to CP levels, provides syntactic evidence for the existence of these CPs. Further arguments based on interpretive facts to do with modal/aspectual interaction will be delayed until chapter 6, after a theory of modal interpretation has been introduced, but they will show that apart from negation, the quantificational layers must also be active in the aspectual CPs.

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[7]Or, indeed, phonological, though this won't be my concern here.

As to whether these CPs should be considered phases or not, this depends on how exactly we end up defining phases; discussion of this will be reserved till chapter 7. For now, I will simply assume that the aspectual layers introduce phases on a par with the T and V phases.

Question 3 will be considered in §5.4, where it will be suggested that subjects must move only via the phase edges, not via the specifiers of PROG/*prog* and PERF/*perf*, the more traditional 'A-type' movement that might be expected. We thus get movement from something like an A-position, to something like an A′ -position, back to something like an A-position, which would of course standardly be dismissed as IMPROPER MOVEMENT (Chomsky 1981); I argue that in fact this movement is not improper, and that in those cases where it seemingly is, what actually leads to impropriety is not the general format of the movement, but rather independent factors in each case.

5.3   Negation

In Butler (2003a) it was argued that CP layers host a negative operator, sited for concreteness in a Rizzian Foc position. Thus, the standard NegP is replaced by the internal FocP; the external FocP provides a wide-scope position for negation (cf. §4.3.3.1). Given that there are two further CPs represented in 91b, we make a straightforward prediction that negation should be able to appear in (the Foc projection of) these CPs also. This prediction is borne out (92).[8]

(92)   [$_{CP}$ [$_{TP}$ Arthur might [$_{CP}$ (not) [$_{perf P}$ have [$_{CP}$ (not) [$_{prog P}$ been [$_{CP}$ (not) [$_{v P}$ teething]]]]]]]]]

There are various points raised here: one is that we don't see negation overtly in the highest CP; this seems to be a fact of English though — negation does appear here in many languages (Italian, for example), and it is certainly an available scope position for negation in English. Another is that we don't tend to see all these negations surfacing simultaneously, though there is nothing in the theory to force this; in fact this seems most likely to be a processing problem, since with appropriate context and stress patterns we can make the example reasonably

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[8]*Might* is added in (92) as a neutral probe for the syntactic position of T.

felicitous. A third is that the two highest positions for negation, i.e. those either side of T, seem to give sentential negation whereas the lower positions seem to give constituent negation. In fact this falls out of the story in Butler (2003a), where subject movement played a crucial role. Canonically, we take the subject to sit in [Spec, TP] (or [Spec, tP] here, it doesn't really matter), having moved up from its base position in [Spec, vP]. In order to interpret the subject in this position, though, we have to create a λ-abstract immediately below it (see Heim & Kratzer 1998 for why, and a way of doing this; see Butler 2004 — a more fleshed out version of §3.1.2.1 — for a variation on Heim & Kratzer's story whereby the λ-abstract is the semantic reflex of the EPP feature on T (or t)). The λ-abstract so created then constitutes the main sentential predicate. Once the topmost CP is sorted, we then have a real proposition. If negation appears in the topmost CP, we of course have propositional, i.e. sentential, negation. If it appears in the next CP down, it will basically negate the predicate; this negative predicate will then apply to the subject, and this will also give you sentential negation; see Horn (1989) for detailed discussion, especially chapters 2 and 7. If, however, the negation is below this, it will be too low down inside the predicate to give you either propositional or predicate negation in the relevant sense,[9] so you get only the constituent negation reading in these cases.

This, then, seems to be good syntactic evidence that we have at least a Foc projection on top of the proposed PERF and PROG phases.
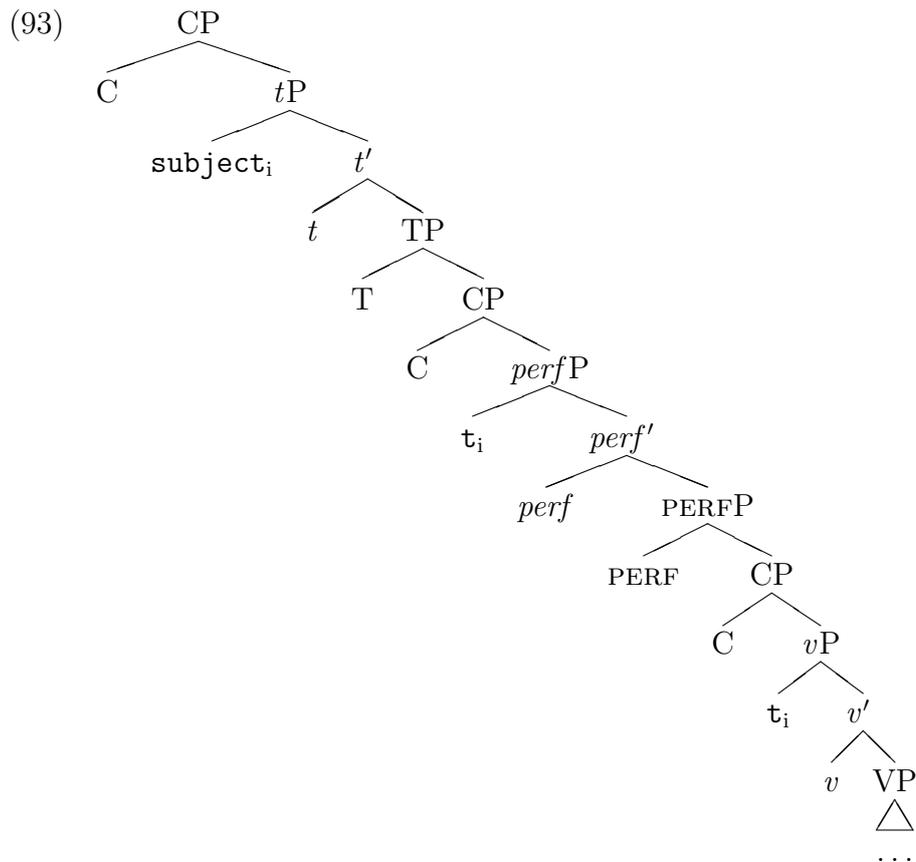
5.4   Subject movement

On the assumption that subject movement takes place from [Spec, vP] to [Spec, tP], there are basically two possibilities for how this movement occurs in the present system when there is an intervening aspectual phase, as in (91b).[10]

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[9]In fact, it could be said that *every* instance of negation in (92) is proposition or predicate negatation, since every phase is some kind of proposition, formed out of a core predicate; however, sentential negation only results when we negate either the main sentential predicate, with T in it, or the main proposition, i.e. the T phase.
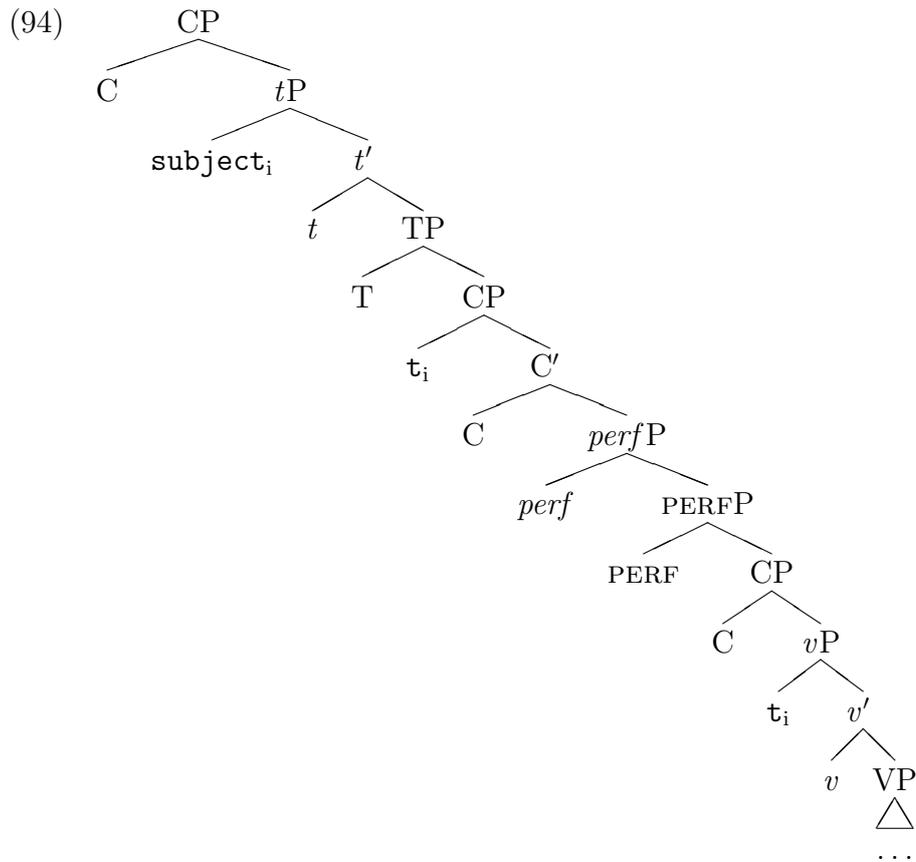
[10]We can rule out the possibility that the subject simply moves directly, skipping the intervening phase, since this goes against the whole notion of what phases do. We may want to make a distinction between strong and weak phases, as in Chomsky's work, but not unless it buys us anything, and from the limited discussion in the literature it doesn't seem that it really does.

1. The subject could move via the specifier(s) of the intervening aspectual heads(s) *perf* or *prog* — i.e. a standard A-movement(-like[11]) story (93).

2. The subject could move via the aspectual phase edges — i.e. more like A′ -movement — and then to [Spec, *t*P] — giving rise to an 'improper' mixed A-/A′ -movement story (94).

(93)

```
           CP
          /  \
         C    tP
             /  \
      subjectᵢ   t′
               /  \
              t    TP
                  /  \
                 T    CP
                     /  \
                    C   perfP
                       /   \
                      tᵢ   perf′
                          /   \
                       perf   PERFP
                             /   \
                          PERF    CP
                                 /  \
                                C    vP
                                    /  \
                                   tᵢ   v′
                                       /  \
                                      v   VP
                                          △
                                          ...
```

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[11]A-movement-'like' because there is no clear definition of the A-/A′ -distinction in current minimalism. This means that the discussion in this section will be pretty speculative in places, simply because it is difficult to talk more concretely without a more concrete frame of reference.

(94)

```
                    CP
                  /    \
                 C      tP
                      /    \
               subjectᵢ     t′
                           /  \
                          t    TP
                             /    \
                            T      CP
                                 /    \
                               tᵢ      C′
                                     /    \
                                    C    perfP
                                        /    \
                                    perf     PERFP
                                            /     \
                                        PERF       CP
                                                 /    \
                                                C      vP
                                                     /    \
                                                   tᵢ      v′
                                                          /  \
                                                         v    VP
                                                              △
                                                              …
```

Theoretically, in the system proposed here (94) is the preferred option, for the following reasons.

The subject is taken to be moving to [Spec, tP] for reasons of feature checking. There are various proposals around as to exactly what feature is relevant: nominative case on DP, ϕ features on T, specificity, etc. So far I have treated it simply as [EPP](=[Λ])-checking, reaching down to find the nearest argument and Remerging it. In fact this is a simplification: so locative inversion structures, for example, are problematic for such a story if we take the locative element to be in the canonical subject position; also I am assuming here that the V phase is an argument of T, Merged lower than the Remerge position of the subject. If [Λ] on t simply looked for the next argument down, this is the one it would find, and so this is what would move.[12] In fact it seems likeliest that [Λ] features are always in a sense 'parasitic' on other feature (set)s[13], in the way that the [Λ] on $\forall$ in (65)

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[12]See Sifaki (2003) for an analysis of null-subject constructions in Greek that is not incompatible with the idea that actually this may sometimes be what happens.

[13]Similar to the claim of Pesetsky & Torrego (2001: 4) that EPP should be seen as a 'subfeature of a feature'.

in chapter 3 was parasitic on the other features of $\forall$. To go into this idea in any detail would lead me too far afield, but there is some discussion of it, and how it can buy us general minimality effects, below; see also Butler (2004).

Whatever feature we choose to believe is ultimately responsible for the relation between t and the subject, we have no reason to also believe that *perf* or *prog* are in the same kind of relation to the subject as t, so there isn't any reason for the subject to undergo any kind of agreement with or movement to *perf* or *prog* in the first place. If there isn't any reason for it to go there, the minimal assumption should be that it doesn't. There could potentially be an indirect reason for it to go there, which is simply that it's on its way somewhere else and that's the only place available for it to stop off — a standard cyclic A-movement story — but given the CP layers proposed here that isn't true.

The CP layers here act as the edge of the phase, providing a kind of escape hatch. Anything in the domain of a phase that doesn't want to be there (i.e. something with unchecked features) has to get out of the phase before it is sent to spellout or the derivation will crash (indirect feature-driven movement). The subject in (93–94) starts out in [Spec, vP]; it can't stay there because of its featural requirements so it has to get out before the V phase is sent to spellout. However, it can't just move to any old position, which is essentially what is happening in (93) — the only way it can get out is via the CP escape hatch.[14] It may be the case that it even escapes the V phase via its own CP, though this isn't a crucial point here; it could equally well stay there till the PERF phase's CP is (at least partially) built, if the standard assumption is that the domain of a phase is sent to spellout when the next phase up is built is right. However, it *has* to escape via the PERF phase's CP, since this is the only way it is eventually able to get to [Spec, tP] without stopping off in some other, essentially arbitrary, place. This means we have to end up with a mixed A-/A$'$ -story.

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[14]More precisely, I assume that a definite subject would escape via [Spec, DEFP], a universal would escape via [Spec, $\forall$P], etc. In this sense the movement would be less indirectly feature driven: it would plausibly be driven by the Agree relation between two sets of, say, [uDEF] features, one on the N phase subject, one on the movement position, plus a [Λ] on the movement position. This is discussed in more detail in §7.1

5.4.1   Improper movement

The mixed A-/A′ -movement movement proposed here would ususally be dismissed as improper movement (Chomsky 1981), something we might wish to avoid.[15]

Framework internally there is no problem with this, as discussed above; in fact it is the only way for the movement to proceed.[16] However, it would be premature to dismiss the objection without discussion, since if a ban on improper movement really obtains (assuming we have a well-defined notion of what improper movement is, and it holds in the cases under consideration) then there is a conflict here. So what about the classical cases of improper movement? I argue that they in fact fall into a few different classes, each of which should be ruled out for independent reasons, and not because of any overarching impropriety in the general format of the movement.

The classic cases of improper movement are illustrated in (95a–95e).[17]

(95)   a. *John tried [$_{CP}$ t [$_{TP}$ t to win]]

        b. *John seemed [$_{CP}$ t [$_{TP}$ Bill would see t]]

        c. *John is possible [$_{CP}$ t [$_{TP}$ Bill will see t]]

        d. *It seems [$_{CP}$ t [$_{TP}$ t to rain]]

        e. *It seems [$_{CP}$ t that [$_{TP}$ John expected [$_{CP}$ t [$_{TP}$ t to rain]]

            (Chomsky 1981: 199)

As to (95a) and (95d), Chomsky (1995: 326) notes, uncontroversially, that in such cases the subject of the infinitive should in fact be PRO, so the structures are out before we even start trying to move.

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[15] 'Improper movement may cause permanent paralysis or death' — www.vnh.org/FirstAid ForSoldiers/Fm211_4.html

[16] And even if the A/A′ distinction is meaningful, it is not necessarily the case that the (aspectual) CPs at issue should be considered A′ -positions.

[17] Here, Chomsky's S̄ is represented as CP, S as TP. This isn't quite accurate since in at least some cases S̄ is assumed to be formed by adjunction to S. So in the examples (95), probably where we have an infinitive Chomsky (in his 1981 clothes) would assume that there was no CP, and that the relevant trace was in an adjoined position, in itself a very odd claim, and probably sufficient to ignore these as problems (see Chomsky 1995: 326).

As to (95b–95c), the pre-movement structure for the embedded CP is presumably a topicalization structure, as shown in (96a–96b).

(96) a. [$_{CP}$ John [$_{TP}$ Bill would see t]]

      b. [$_{CP}$ John [$_{TP}$ Bill will see t]]

The raising element then selects this CP as object; it also needs a subject. Raising predicates are generally taken not to select their own subjects, but to raise a lower element, hence their name. We assume, then, that the raising predicate probes for something to raise — the most obvious way to formulate this is probably to say it has [EPP](=[Λ]) features, but no suitable thematic properties, on a par with t. The lack of thematic properties means no new argument can be Merged (since it would have no role to play in the thematic structure of the clause), so the [EPP] feature raises the first suitable element it can find. The word *suitable* is crucial here, and it is at this point that it gets more complex.

Before I go through the examples in (95b–95c), I will go through a simpler case, illustrated in (97a), with an embedded CP pre-movement structure indicated in (97b).

(97) a. *Bill seemed [$_{CP}$ (t) [$_{TP}$ t would see John]]

      b. [$_{CP}$ [$_{TP}$ Bill would see John]]

This case is slightly simpler than (95b–95c) in that it doesn't involve a topicalization structure in the embedded clause. So all we have is a raising predicate, looking down for the first suitable thing it can find — standardly taken to be the first DP, which is the subject *Bill* of the embedded clause. But whether we take it to transit through the embedded [Spec, CP] or not (the parentheses in the structure indicating optionality), this movement is out: (97a) is ungrammatical. Why should that be?

Under the general minimalist system I adopt, movement, and indeed all syntactic dependencies, are taken to be motivated by features. More specifically, I am taking them to be motivated in terms of (un)interpretability and value of features, and thus in terms of the (IN)ACTIVE status of the element containing the features (Chomsky 1999). If an element has uninterpretable/unvalued features, then these features need to be checked/valued: this requirement is what makes something

active. Once features are checked, through matching with (broadly[18]) identical interpretable features, they are deleted. Once an active element's uninterpretable features ($[u$F$]$s) have been checked, then this element is no longer active, and thus no longer able to take part in syntactic operations. For the kinds of examples I am looking at here, this means that the element that gets raised by a raising predicate has to be active: if it was inactive, the raising predicate essentially wouldn't be able to see it, and so it couldn't do anything with it.

If we look at the embedded CP for (97a) in (97b), we simply have no reason to suppose that the subject *Bill* is active. All its features should have been checked/valued within the embedded clause (note that (97b) is a perfectly good root clause). This being so, the raising predicate in the next clause up won't be able to raise it out of the embedded clause.[19] So what we have here is a prohibition on a particular movement out of a particular embedded clause which makes no reference to the idea of improper movement.

(95b–95c) look like improper movement could be more relevant, since in this case what we are trying to move has been topicalized in the embedded clause; i.e. it is in an A$'$ -position and we are trying to move it to an A-position. However, exactly the same considerations apply as to (97a).

I will concentrate on (95b), since the structures are essentially equivalent for my purposes here. In (95b), the derivation for the embedded clause (96a) would need to include Topic features. On the assumption that information structure related features are introduced at the point of Merge, as seems reasonable, then the object *John* will presumably have an uninterpretable $[u$TOP$]$ feature on it. To tie in with the general system of movement to CP-level heads set out in chapter 3, this $[u$TOP$]$ will need to match with an interpretable $[$TOP$]$ on a Topic head in CP — and for this to be the case, I take it that Top will also host an uninterpretable TOP-valued $[$ID$]$ feature: $[u$ID:TOP$]$, to probe. So both Top and the object will be active. Top (more precisely, the $[u$ID:TOP$]$ feature) will probe for matching features, which it will find on the object. Top will also need an $[$EPP$]$ feature, to raise the object to [Spec, TopP]. All this having happened, the object will no

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[18]i.e. I am not going into the question of quite what 'identity' means for matching; see Chomsky (1999) for discussion.

[19]This suggests a possible generalization of Chomsky's (1973) Tensed Sentence Condition, or later versions thereof such as the Nominative Island Condition (Chomsky 1980), under the same kind of motivation.

longer be active: all its 'usual' (case, ϕ, etc.) features will have been checked in the normal way, and its [uTOP] feature will have been checked also. Neither will the subject be active, since it too will have all its features checked, just as in the discussion of (97a) above. Again, note that the embedded clause, (96a), is a perfectly valid main clause: we have exactly no reason to suppose anything in it is active. Again then, there is no way a dependency could be formed between the topicalized object and the raising predicate to allow raising. And again, improper movement just doesn't come into it.

The same kind of story deals with (95e): here, the matrix subject *it* is supposed to have moved from the exceptionally case-marked subject position of the most deeply embedded clause. But once more, there is no reason to suppose *it* is active in this case: the object of the raising predicate is a perfect main clause, and we don't have cause to suppose it contains [uF]s; either on *it*, or on the first level embedded subject *John*. There is thus no means for a dependency to be created, and so no raising of either of these elements is possible.

There is another classic case related to improper movement, more complicated, to do with *wh-*/raising interactions. The relevant construction is exemplified in (98a); it involves *wh*-movement out of a raising predicate.

(98)   a. [$_{CP}$ Who [$_C$ does] [$_{t}$P it seem [$_{CP}$ t$_{who}$ [$_{t}$P John saw t$_{who}$]]]]

      b. *[$_{CP}$ Who [$_c$ does] [$_{t}$P t$_{who}$ seem [$_{CP}$ t$_{who}$ [$_{t}$P John saw t$_{who}$]]]]

The question that is asked is why the expletive is necessary. In the embedded CP, we have an active element *who*, which needs to get up into the matrix clause. This being so, the question is why it doesn't go via [Spec, tP], checking the [EPP] feature, and then move on to CP, as in (98b). The standard argument is that it doesn't do this because this is improper movement, from an A$'$ - to an A-position, and if improper movement is barred, it can't. The only option is therefore to satisfy matrix t's [EPP] feature with an expletive, and then go on to move the *wh-* word to matrix CP. Obviously this doesn't hold water here since it rests on the notion that expletives aren't arguments, whereas I claim that in fact they represent the situation argument of the verb, following Higginbotham (1987); Stowell (1991); Ramchand (1996); Felser & Rupp (2001). This being so, we can argue that in (98a) the expletive is in fact the closest argument, and so gets raised to [Spec, tP] exactly like any other argument would. Again, improper movement isn't an issue.
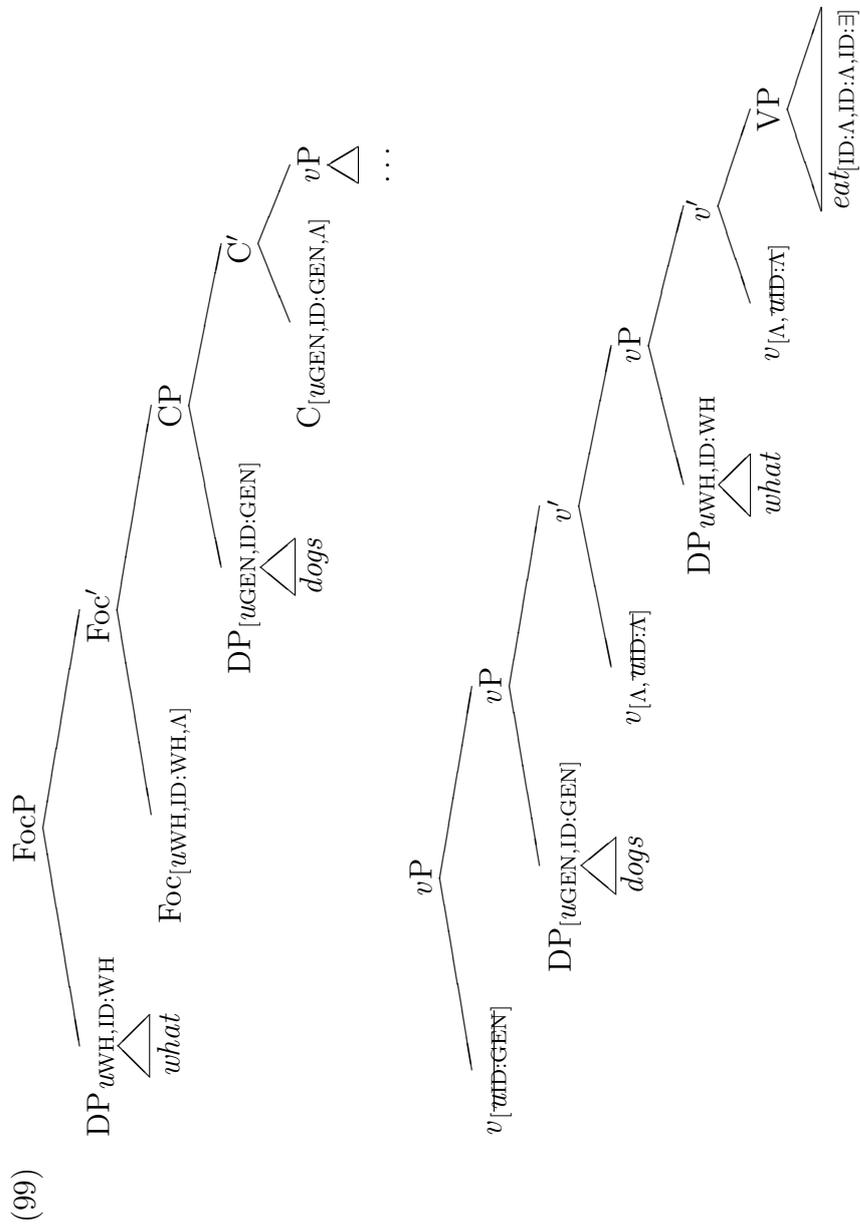
This argument holds if we assume the structure given in (98a). It weakens, however, under just about any phase-based analysis of movement: specifically under any analysis that assumes *wh*-movement moves through the edge of the *v*P phase. We can see why this is by considering a clause with a non *wh*-subject and a *wh*-object. The subject will be Merged in [Spec, *v*P], as normal. The object will move to the edge of *v*P. This being so, the *wh*-object will always be closer than the subject to *t*. This is regardless of whether the edge of the *v*P phase is considered to be an outer *v*P Spec, as the standard assumption, or a CP level, as here. These differ from the topicalization cases discussed above, because we do take a moving *wh*-phrase to be active, having (at least) unchecked [WH] features. An example is in (99), the V phase structure of *what do dogs eat?* In this kind of situation, we have to say that the *wh*-phrase is invisible to the [Λ] feature on *t*.

Why would the *wh*-phrase be invisible to *t* if it is active? As noted in the previous section, I am tacitly taking [Λ] to be 'parasitic' on other features in the general case[20] — so in (99) for example, [Λ] on Foc is parasitic on the [WH] feature set of Foc; as noted, this is equivalent to Pesetsky & Torrgo's (2001) claim that EPP is a subfeature of a feature, or the more general minimalist claim that Move is Agree+EPP. Then the [Λ] on *t* is also parasitic on some other feature; for space reasons I haven't examined what this feature might be, but for concreteness I will say for now that it is case. This means that what the feature set of *t* is actually looking for is an active case feature, and it will Remerge as its Spec the first element it finds with such a feature. The case feature of the *wh*-phrase will, by assumption, have been checked/valued by the point in the derivation represented in (99), so the *wh*-phrase won't be accessible to *t*. Although the *wh*-phrase is active, then, it isn't active in a way that *t* cares about. This essentially derives relativized minimality. *Dogs*, on the other hand, will have an active case feature and will be accessible to *t*; therefore *dogs* will move to [Spec, *t*P].

What about a situation where we have a subject *wh*-phrase? Then the relevant structure would be (100), the V phase of *what eats dogs?* In this case, the case relations will just be the exact opposite: the *wh*-subject will be case-active, the non *wh*-object won't. Then the *wh*-subject will be Remerged as [Spec, *t*P], and it will move there from the edge of the V phase. Few people would want to dismiss

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[20]This is clearly plausible for cases of movement; for first Merge cases, we could say that [Λ] is parasitic just on [ID] features. Since I have suggested in §3.1.2.1 that [ID] features can be seen as something like θ features (cf. Hornstein 1998; Manzini & Roussou 2000), this amounts to saying something like that first Merge is parasitic on θ-roles, which makes sense.

(99)

FocP
├─ DP$_{u\text{WH,ID:WH}}$ — *what*
└─ Foc′
   ├─ Foc$_{[u\text{WH,ID:WH},\Lambda]}$
   └─ CP
      ├─ DP$_{[u\text{GEN,ID:GEN}]}$ — *dogs*
      └─ C′
         ├─ C$_{[u\text{GEN,ID:GEN},\Lambda]}$
         └─ vP …
            └─ vP
               ├─ vP
               │ ├─ v′$_{[u\text{ID:GEN}]}$
               │ └─ DP$_{[u\text{GEN,ID:GEN}]}$ — *dogs*
               │   └─ v$_{[\Lambda,\,u\text{ID:}\Lambda]}$
               └─ v′
                  └─ vP
                     ├─ DP$_{u\text{WH,ID:WH}}$ — *what*
                     └─ v′
                        ├─ v$_{[\Lambda,\,u\text{ID:}\Lambda]}$
                        └─ VP
                           └─ *eat*$_{[\text{ID:}\Lambda,\text{ID:}\Lambda,\text{ID:}\Xi]}$

this as improper movement, since we do actually want to say that *what* moves to [Spec, *t*P], and then to [Spec, FocP].

Note that it isn't crucial here that the structures are exactly as in (99–100), with a CP level over *v*P: even under the standard assumption that the edge of the *v*P phase is *v* and its Spec(s), we want to bar a *wh*-object from moving to [Spec, *t*P], but allow a *wh*-subject to go there. The only way to ensure this seems to be to say that the features of *t* just don't care about [WH] features, only case (or whatever). Improper movement doesn't come into it.

There is one more objection that could be levelled at the story here as compared to the orthodox story for phases that ought to be addressed. The orthodox story has it that a phase doesn't spell out till the next higher phase defining head is Merged: so *v*P doesn't spell out till C is Merged; and then what spells out is *v*P's DOMAIN, i.e. everything below *v*. If things only move to the edge of a phase to escape spellout, then actually a *wh*-object won't have move to the edge in a case like (99); it could potentially move direct to [Spec, CP] and there wouldn't be an issue here. However, there are at least two problems with this. The first is that this condition appears suspiciously like it is motivated solely to rule out the movement in question, so that we don't get problems with improper movement. If improper movement isn't a problem anyway, we don't have to make this assumption: phases can spell out at a more intuitively plausible point (see §7.3).

The second problem is that as soon as we consider long-distance *wh*-movement, it is no longer possible theoretically to claim that *v*P's domain spells out even at this point: in a case like (101), there are six instances of $what_{[u\text{WH,ID:WH}]}$ active in the structure before the interpretable [WH] feature in the highest Foc is finally Merged.

(101)   [$_{CP}$ what$_{[\mathit{u}\text{WH},\text{ID:WH}]}$ Foc$_{[\text{WH},\text{\sout{\mathit{u}ID:WH}},\Lambda]}$ do you [$_{CP}$ t think [$_{CP}$ t that Susan [$_{CP}$ t told Arthur [$_{CP}$ t that he should [$_{CP}$ t do t]]]]]] ?

where t $= what_{[u\text{WH,ID:WH}]}$

Under standard minimalist assumptions, nothing can spell out if it has unchecked uninterpretable features, since these will be incoherent at one or other of the interfaces. This means that in fact none of the lower phases, i.e. those containing the *wh*-phrase with uninterpretable features, should be able to spell out until the

(100)
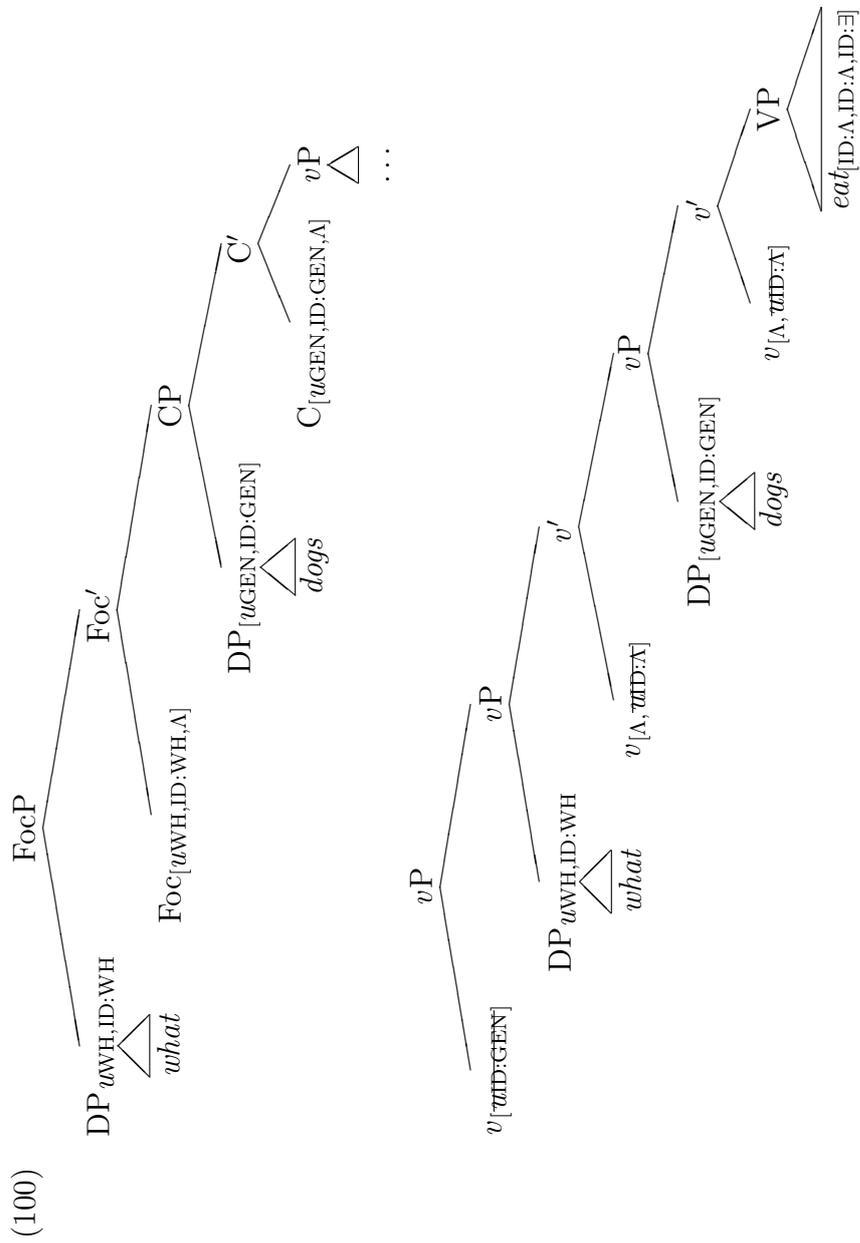
interpretable [WH] is Merged in the CP of the highest phase.

In the orthodox theory, we have two options for dealing with this. One is to say that in fact we can spell out things with uninterpretable features, but the circumstances under which this was allowable would have to be specified very precisely, and it isn't clear what they would be; in any case, this seems stipulative. The other option is to accept that in these cases spell out of phases is indeed relativized to convergence, rather than being strictly cyclic (see Atkinson 2000; Svenonius 2000; Felser 2004 for related discussion; also chapter 7). This then raises questions of how well the orthodox theory motivates cyclic movement after all: if a phase isn't spelling out, no 'escape hatch' is needed, and there isn't any strong reason why long *wh*-movement shouldn't take place in one step. We don't want to say this though, since there is considerable empirical evidence for cyclic *wh*-movement.

This is not to say the orthodox theory couldn't be rescued; but in the theory here, nothing even needs rescuing: we still expect cyclicity, since we still have an essentially cyclic set of heads relevant to *wh*-movement (FocPs) interspersed through the clause, and these sets of heads correspond to the edges of the orthodox phases, so we don't lose that notion.

5.4.1.1 Summary

In the theory here, then, it seems necessary to allow something that looks like what is standardly considered improper movement (94), i.e. movement from something like an A′ -position to something like an A-position. This could be deemed problematic, but I have argued it isn't, on three counts:

1. The A/A′ distinction isn't even well-defined in current theory.

2. Even if it was defined, it isn't clear that the kinds of CPs I am talking about here (those of aspectual phases) would count as A′ -positions.

3. The ban on improper movement is a stipulation anyway, and even in standard cases (i.e. not those aspectual cases relevant here) the effects attributed to it can be derived independently (§5.4.1).

5.5   Infinitives

Infinitives are generally considered not to carry tense information, at least in the same way / to the same extent that finite clauses do. Given this, it is tempting to treat them in the present analysis as simply lacking the T phase. This would obviously explain the lack of tense information, as well as the lack of nominative case in infinitives, assuming a tight connection between nominative and T. Of course, this isn't really the standard view of infinitives, which are often taken to have T, but non-finite T (whatever that means), and maybe to lack CP (so some infinitives are just bare TPs). Despite this, I will argue here that in fact, infinitives do lack the T phase, and moreover that they don't lack a CP, but are topped by the same CP structure that every phase has in my analysis.

I examine possible objections to this proposal from (a) propositionality facts (§5.5.2); (b) the claims of Stowell (1982) that (some) infinitives carry non-specified tense information (again, whatever that means) (§5.5.3); and (c) apparent EPP effects attributed to non-finite T in infinitives (§5.5.4). In chapter 6, I provide further evidence for my arguments based on the interactions of (non-)finiteness, aspect, and modality.
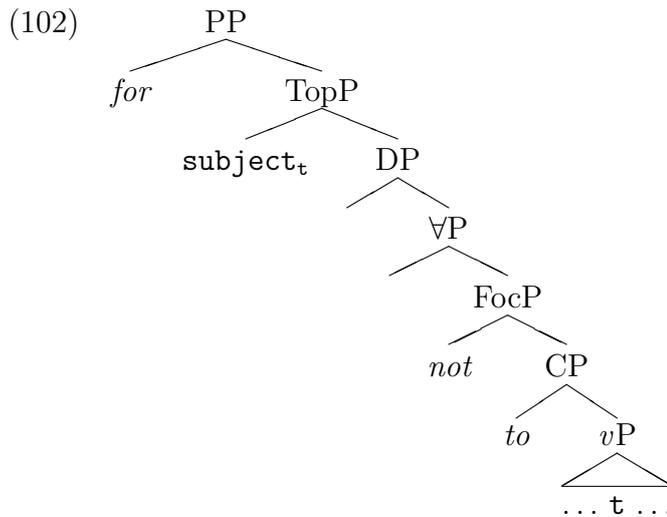
The conclusion I come to is that although it is difficult to prove that infinitives lack T, if they do have it it doesn't actually do anything, so we can at the very least politely ignore it.

5.5.1   The structure of infinitives

As a preliminary to the discussion below, I repeat the structure I assume for phases in general, and their CP layers in particular, here as (102), along with the various elements that we find in infinitives (*for*, *to*, etc.) inserted in the approximate positions I assume they occupy.[21]

---

[21](102) illustrates a simple V infinitive; for infinitives with aspectual elements, see §5.5.2.

(102)

```
                PP
              /    \
           for      TopP
                   /    \
             subject_t   DP
                        /  \
                       ∀P
                      /  \
                   FocP
                  /    \
               not      CP
                       /  \
                     to    vP
                          /\
                         /  \
                       ... t ...
```

A few relevant points regarding this structure: as to the P status of the complementizer *for*, it isn't crucial, and I use this notation mainly to parallel my treatment of partitive Ps in DPs in §3.2. However, there is of course a preposition *for* with properties in common with complementizer *for*: e.g. the ability to assign case, etc.

As to the low position of *to*, a number of authors (e.g. Travis 1994, 1999; Wurmbrand 2001) have suggested infinitival *to* occupies just such a low position. Travis claims it is head of an 'event phrase' peripheral to VP; Harley (1995) and Boeckx (2000) basically identify this with *v*P; here we can identify the relevant position with part of the infinitive's CP, and specifically with that lowest part argued by Rizzi (1997) to deal with finiteness (recall I equate CP with Rizzi's FinP). The status of *to* as a C head (rather than an I/Aux/v/V head, all of which have probably been more frequently proposed) has been claimed recently by Roberts & Roussou (2003: chapter 3) (they argue, in fact, that it is the lowest C-level head, as here), as well as by Rosenbaum (1967) and Lencho (1992).

As to the subject, the TopP indicated may in fact be DP, if DP equates to Beghelli & Stowell's (1997) RefP, and their claim that RefP equates to a standard topic position is correct. This isn't crucial, and I will just assume TopP here.

5.5.2   Propositions and situations

The structure given in (102) for infinitives leads to an immediate prediction given the discussion so far, particularly the discussion of situational vs. propositional

phases in §4.3.3. There, it was seen that, in general, the V phase gives us essentially situation related information (that is, relating to the Davidson-esque situation variable introduced in the V phase), whereas the T phase, by relating two situations temporally, gives us truth evaluable propositional information.

The prediction is that infinitives formed on just the V phase will not have propositional readings. The V phase restriction is important. Obviously, given the claim that infinitives lack T I won't be looking at infinitives formed on the T phase. Infinitives formed on aspectual phases (which I will refer to as aspectual infinitives) are perfectly licit, though (103). We actually predict that aspectual infinitives *will* get propositional readings, since aspect is treated exactly like tense in the relevant way: it is a temporal predicate describing a relation between two situations. This is enough for truth conditions, so a propositional reading should ensue.

(103)   a. ... to have gone

      b. ... to be going

      c. ... to have been going

It turns out that the prediction regarding aspectual infinitives is correct: they always receive a propositional reading. The prediction regarding V infintives doesn't turn out always to hold though: they too can receive a propositional reading, in a restricted set of circumstances — specifically, when their predicate gets a generic (habitual or I(ndividual)-level) reading. An explanation for this restriction will be given in §5.5.2.2.

5.5.2.1   Tests for situational/propositional status

1. **Complement of physical peception verb**
   It has often been noted that physical perception verbs' infinitive complements must denote situations and not propositions, as situations can, but propositions can't, be perceived physically (Higginbotham 1983; Vlach 1983; Parsons 1990). For example, in (104a), what is perceived is the event of leaving, not the proposition that 'John left/John is leaving', or whatever. While this is true for the bare V infinitive (104a), it isn't for (104b): here, it clearly is the case that a proposition is being perceived — a reasonable paraphrase is 'I saw that John had left' — but importantly, the perception

here must be mental rather than physical (that is, it means something more like *understand*), and of course propositions are pretty basic candidates for objects of mental perception.

(104)   a. I saw John leave

      b. I saw John to have left

      c. *I saw John to leave

Oddly, given what has been said so far, the *to* V infinitive (104c) is bad on the relevant physical perception reading, though it is slightly better on the mental perception one at least for some speakers if the leaving is interpreted as somehow generic (habitual/I-level) ('I saw John to be a person who left'[22]). Discussion of this effect is in §5.5.2.2.

2. **Complement of propositional attitude verb**
Propositional attitude verbs such as *believe*, *think*, *suppose*, etc., describe the speaker's attitude to the proposition they embed. They are therefore by definition unable to take non-propositional complements. If V infinitives denote only situations, whereas aspectual infinitives denote propositions, then we predict that V infinitives will not, but aspectual infinitives will, be licit complements to propositional attitude verbs. (105a) shows that, indeed, bare V infinitives are bad as complements to propositional attitude verbs; (105b) shows that aspectual infinitives are good.

(105)   a. *I believe John leave

      b. I believe John to have left

      c. I believe John to leave

Again, we get a slightly odd effect in (105c), with a *to* V infinitive, in that a propositional meaning is licit but again only if we construe the leaving as generic/habitual.

3. ... *which is true*
Pesetsky (1991) uses the continuation '... which is true/false' as a test for propositionality (crediting James Higginbotham, personal communication, for the idea). That is, if we have an infinitive that gets read as a proposition,

---

[22]I have deliberately used an implausible candidate for this kind of reading — leaving being typically not a generic property of a person — to show that the effect is quite strong; it is stronger in (105c) and (106c) than it is in this example.

then we can overtly predicate truth (or falsity) of it in this way, whereas if we have a non-propositional infinitive then of course we can't. As we see in (106a), we can't do this with a bare V infinitive; we see in (106b) that we can do it with an aspectual infinitive.

(106)   a. *I assumed John leave, which was true

         b. I assumed John to have left, which was true

         c. I assumed John to leave, which was true

We again see the effect in (106c) that a *to* V infinitive is fine with a propositional reading if it is interpreted as generic/habitual.

The tests above, then, provide a certain amount of backup for the predictions the theory makes: aspectual infinitives are uniformly interpreted as propositional, V infinitives are interpreted as non-propositional, except for the very specific exception where they get a reading that can be generally described as generic/habitual/I-level. To explain this obviously requires a story for how such readings come about, for which see §5.5.2.2. First, though, it should also be noted that in all the cases above, the propositional reading for a non-aspectual infinitive correlated with being a *to* infinitive, whereas the non-propositional reading correlated with being a bare infinitive.

This is interesting in two ways. First, it has been claimed by Pullum (1982) that English infinitival *to* is in fact an auxiliary verb in the same sense that *have* and *be* are, which might place *to* infinitives on a par with aspectual infinitives. However, Pullum claims that *to* is a 'non-finite' auxiliary, and as with the notion of non-finite T, it is very far from clear what this means. Second, whether we were to accept Pullum's claim or not, we might suppose that the correlation is causal rather than casual: that is, either the generic reading is dependent on *to* or vice versa. While this may be true, one way or the other, for the V infinitives we have seen, it isn't true for all non-finite complements: for example, adjectival or nominal complements (107a–107b).

(107)   a. I believed him stupid, which was true

         b. I believed him a genius, which was true

It might be objected that the complements in (107) are not infinitives but small clauses; in the system here the only difference is that what are usually called small

clauses are nominal/adjectival whereas what are usually called infinitives are ver-bal.[23] I will assume that presence/absence of *to* doesn't entail presence/absence of structure, but rather a difference in meaning of the head in which it does/doesn't appear (§5.5.2.2; and fn. 23).

5.5.2.2   GEN/$\exists$; I-level/S-level

As seen above, a propositional reading for non-aspectual infinitives is possible only when that infinitive receives a reading that can be loosely described as generic. In this section, I show that this reading derives from generic rather than existential closure over the V phase. This derives the distinction between habitual and punctual interpretations for predicates formed out of basically eventive verbs, and the distinction between I-level and S-level predicates (cf. Chierchia 1995).
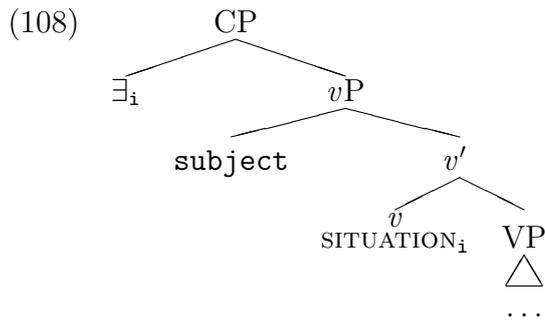
The story runs as follows: the general structure for phases, the relevant parts of which are repeated (for the V phase) below as (108), is a Root category telling us what property is to be predicated (V), topped by an l-syntactic layer of little heads (*v*) which deal with situation interpretation, topped by a quantification encoding CP.

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[23]That only verbal non-finite elements require *to* might be the strongest evidence that in-finitives do have a TP, on the grounds that only verbal elements require TP at all. In fact, it can be argued that really the distinction is down to the (im)possibility of eventive readings in certain cases. Verbal infinitives essentially seem to use *to* as a means of distinguishing eventive from non-eventive readings for their predicates. Adjectives don't have eventive readings, so they don't need this; some nominals are eventive, and it is interesting that for these, we don't get a propositional reading (1; cf. 107b).
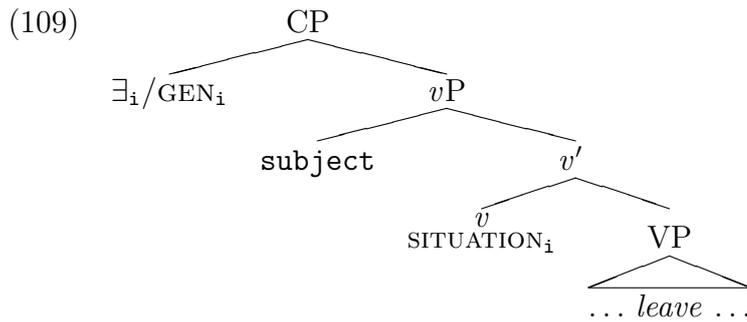
(1)   *I believed it an explosion, which was true

I argue in §5.5.2.2 that the eventive vs. non-eventive distinction derives from situation quantifi-cation in the CP layer of the V phase, and specifically in the lowest quantificational head, which I also take to be the head hosting *to*. There is then a good reason to have such a marker in V infinitives, since verbal predicates can often shift between eventive and non-eventive readings, whereas there is no reason to have it in nominal or adjectival non-finite complements, since these don't fluctuate.

(108)

```
                    CP
              ╱           ╲
          ∃ᵢ                vP
                       ╱         ╲
                  subject          v′
                              ╱        ╲
                             v          VP
                          SITUATIONᵢ    ╱╲
                                        ‥
```

The CP includes at its lowest level an existential quantifier ∃ corresponding essentially to the ∃-closure operator of Diesing (1992). It operates over the macro-situation variable **s** associated with the v complex, giving it reference so that it can act as an argument of higher predicative heads such as T. It may also operate over the subject if the subject is an indefinite interpreted in its base position, thus deriving an existential interpretation for it. Indefinites moving higher than this position may escape ∃-closure, giving rise to the well known definiteness/specificity effect. This much is standard, bar the claim that the ∃ is syntactically real.

In most stories for closure, though, ∃ is not the only available closure operator: there is also a second one, GEN, which results in a generic reading for whatever variables it closes. Accounts differ as to how these two operators interact: Diesing, for example, assumes that ∃ operates over vP, GEN over the whole clause. Other authors (e.g. Chierchia 1995) assume like Diesing that ∃ is connected to vP, but treat GEN as freer, being able to insert anywhere above vP. In most current DRT stories, ∃ and GEN are pretty much just alternative ways of closing off variables. I exploit this here by treating ∃ and GEN as different values for the same head — C in (108) (corresponding to Rizzi's Fin). Basically, vP can be closed off either existentially, or generically, but not both. This has a number of good ramifications.

First, and most saliently here, it allows the situation variable associated with v to be interpreted as generic rather than existential. So a V phase like (109) will have two possible readings, depending on which value the head takes.

(109)

```
                        CP
        ∃ᵢ/GENᵢ                  vP
                     subject            v′
                                 v           VP
                              SITUATIONᵢ
                                         … leave …
```

The ∃ reading is the reading we have seen so far: 'there exists a situation of leaving (by `subject`)'. This, clearly, gives rise to a punctual eventive reading for the predicate (a single, basically instantaneous, event of leaving). The generic reading will mean something more like: 'in general, there are situations of leaving (by `subject`)' — i.e. the subject has the property of leaving quite generally, across situations. This is the reading we saw in the (c) examples of (104–106) above: the verbal predicate is construed as holding quite generally. Even without temporal predication, this is enough to derive the truth conditions we need for a truth evaluable proposition. So we now have an explanation of why some non-aspectual infinitives get a propositional interpretation anyway. And as noted in fn. 23, we also have a possible reason for the correlation of this propositional reading with infinitival *to*: *to* appears when the C head of the V infinitive is GEN valued;[24] it doesn't appear when it is ∃ valued.[25]

---

[24]Even in cases where this isn't immediately obvious, it is a plausible analysis: the infinitive in (1) isn't obviously interpreted as a proposition (unsurprisingly, since it is basically a question), but there is future shifting, which given the analysis I develop in §6.3 means there must be some kind of covert modality in there.

(1)   I asked John to leave

Bhatt (1998) argues that covert modality is always interpreted as universal (in fact, that covert quantificational operators in general are always universal — e.g. arbitrary PRO; the covert quantification in donkey sentences; covert modality in *wh* infinitives (see example 114 in the text) (default ∃-closure is an obvious counterexample to this proposal)). That these are all universal is actually quite a strong claim: generic is probably a better characterization. For example, PRO_ARB in *it is fun* PRO *to play baseball* doesn't really mean it is fun for everyone, but just for people generally; the covert quantification in *if a farmer owns a donkey, he beats it* doesn't necessarily mean that *every* farmer will do this, but just farmers generally; the covert modal in *wh* infinitives is better glossed as the weaker *should* than the strong *must*. This being so, the the presence of *to* in (1) may again be marking the GEN value for C.

[25] *To* also appears in the aspectual phases, where it isn't the case that the phase it closes must be generically construed: it might then be the case that *to* isn't in fact a marker of genericity, but rather of Tense-less propositionality (in a broad sense, to allow questions in — see fn. 24).

This is also the way Chierchia (1995) characterizes I-level predicates: generic quantification over the situation variable in vP. However, Chierchia assumes that GEN is a discrete quantificational operator inserted above vP, and more particularly above the domain of existential quantification over vP. He thus has to do some tricky things with the base subject position in order to ensure that the subject in ILPs can't reconstruct to its base position and get interpreted as existential, as noted by Diesing (1992) among many others.[26] On the version of the story presented here, we don't have to do anything to ensure the subject doesn't get an existential reading: it is quite free to reconstruct to its base position in ILPs, but if it does so it will get a generic reading anyway, since C is valued GEN, not ∃. This also avoids the problems of Diesing's, and Kratzer's (1995), treatment of this phenomenon, where they attribute strange (thematic/control) properties to TP in ILPs. Here, nothing special needs to be said about anything above the single head C.

5.5.3   The 'tense' of infinitives: Stowell (1982)

Stowell (1982) shows that certain infinitives seem to carry some limited temporal information: specifically, that they get what he describes as a 'possible future' interpretation. His crucial examples are shown in (110–111).

(110)   a. Jenny remembered [PRO to bring the wine]

         b. Jenny remembered [PRO bringing the wine]

(111)   a. Jim tried [PRO to lock the door]

         b. Jim tried [PRO locking the door]

Stowell notes that

> In each case, the tense of the infinitival complement is understood
> as being unrealized with respect to the tense of the matrix; thus in

---

It isn't clear how this would be formalized, but it is clear that some predicates are sensitive to propositionality, for s-selection purposes, and given that all that ever really gets c-selected is a CP in my story, this kind of information needs to be visible one way or another at the CP level.

[26]Essentially, Chierchia stipulates that a special kind of trace appears in the base subject position, one which doesn't appear anywhere else.

> [(110a)], Jenny has not yet brought the wine at the point at which
> she remembers to do so, while in [(111a)], Jim does not succeed in
> locking the door when he tries to do so. In contrast, the understood
> tense of the gerund is completely malleable to the semantics of the
> governing verb.
>
> (Stowell 1982: 563)

The conclusion is that infinitives do in fact have tense information encoded, but
that this tense information is unspecified — i.e. they are neither [+PAST] nor
[–PAST], but unspecified for [PAST]. Whether we actually notate this as [PAST]
or simply the lack of the [±PAST] feature at all doesn't matter, what it ends
up saying is that infinitives are neither past nor present. They thus denote a
situation which is unrealized with regard to the matrix situation. This, according
to Stowell, gives them the interpretation of a possible future.

The intuitions are pretty clearly on Stowell's side here: his description above is
quite accurate.[27] However, there are certain theoretical problems with the view
that this shows that infinitives somehow have tense information encoded in them,
in however unspecified a way.[28]

A first problem is again the notion of a tense whose possible values are [±PAST]
being realized with neither of these values. If the tense is genuinely not doing any
of the things that it is able to do, then we gain precisely nothing by proposing that
it is there. Note that Stowell does not propose anywhere in Stowell (1982) that
[FUTURE] is a possible value for tense — the (possible) future reading is supposed
to stem solely from the lack of a +/− specification for the [PAST] feature. I contend
that in fact exactly the same outcome follows if we just don't have tense at all.
The second problem is an extension of this, which is that it has been convincingly
argued by, among others, Enç (1996) that future shifting is not a property of
tense itself — i.e. [FUTURE] is not a value of T — but rather a property of modal
elements. (And of course, possibility in general is a defining characteristic of

---

[27]NB: I obviously disagree with the notion that the 'tense' of the infinitive — or for that
matter the gerund — is understood as anything, since I claim that infinitives lack any tense;
however, I agree with Stowell if 'tense' is understood in a fairly loose way.

[28]It should be noted that, as far as Stowell is concerned, these views on infinitives no longer
necessarily hold, given his radically different treatment of tense in Stowell (1996). However, they
are still widely cited by others as demonstrating that infinitives (can) encode tense information,
so they are worth discussing here anyway.

modal elements.) I go through this in more detail in §6.3, but it is worth noting what the finite paraphrases of some of Stowell's examples would be (112), and in fact what some of the ones he gives in his paper are (113–114).[29]

(112)   a. Jenny remembered [that she should bring the wine]

        b. Jenny remembered [that/when she brought the wine]

(113)   a. The table on which to put your coat is in the next room

        b. The table on which you should put your coat is in the next room

(114)   a. I don't remember who to visit

        b. I don't remember who we should visit

(112) shows that where we had a 'possible future' infinitive (110a), we get a modal in the finite paraphrase (112a), whereas when we had a not 'possible future' gerund (110b), we get no modality (112b). (113–114), taken from Stowell (1982), show that this is quite general — any time we get an infinitive with this 'possible future' interpretation and can paraphrase it with a finite clause, that finite clause contains a modal.

5.5.4 Non-finite EPP

One last piece of evidence often cited in support of the idea that infinitives contain T is that EPP-like effects obtain in some cases where we have a series of embeddings of raising infinitives. So in (115), if we take seriously floating quantifiers as diagnostics for intermediate subject positions (however we might want to formalize this), it seems that the subject has raised cyclically through the embedded infinitives to the matrix clause; in particular, it seems that it has raised both through the embedded [Spec, vP] (appearing between *to* and the verb) and through a higher position above *to* in every case.

(115)   The children (both) happen (both) to (both) seem (both) to (both) be
        (both) likely (both) to (both) appear (both) to (both) be (both) sick

---

[29] It isn't possible to paraphrase (111) in this way since *try* obligatorily takes a non-finite complement.

Standardly, this higher position is taken to be [Spec, TP] of the non-finite clause, with *to* marking T. I am arguing that non-finite clauses don't have T, though, so I have to explain this elsewise. I do have a subject position above *to* in infinitives, however, assumed in (102) to be [Spec, TopP];[30] and [EPP] features have for some time now been considered not solely T-related, but more generally available to motivate (at least) XP movement. As far as I can see, this is all that is necessary to deal with these EPP-effects; the data is no less compatible with the theory here than with a more standard treatment.

[30]Cf. the treatment of movement through aspectual phases in §5.4, where intermediate steps of movement were also taken to be through intermediate CP levels.

Chapter 6

Modality

As with the previous chapter, it will not be my intention here to give a complete and detailed account of all facets of modality in natural language, a comparison with existing accounts, etc.; since again this is a topic worthy of a dissertation in its own right. Instead, I will show how the account developed so far derives a theory of modality that is theoretically and empirically well-justified, and provides further back-up to the general view I have developed of how syntactic structures are derived and interpreted. First I run through how the system, based on Butler (2003a), works generally in simple tensed clauses, and then I extend the data to show how it interacts with aspect and infinitives.

6.1 Modals and phases

In Butler (2003a) I proposed a treatment of modal auxiliaries based on Kratzer's (1977, 1981, 1991) notion that they are uniformly propositional operators, quantifying over possible worlds (PWs) and relating these to the proposition under consideration. However, I there relativized the notion 'proposition' along lines discussed in Chomsky (1999): i.e. I took a proposition to be any predicate with fully saturated argument structure. The problems with this defintion as far as natural language is concerned have been discussed in §4.3.3, but for the purposes of quantification, it is legitimate to operate over elements that are propositional in this non truth conditional sense (quite clearly, since we can operate over individual variables, situation variables, etc., which aren't propositional in any sense).

This led to the prediction that modals should be able to scope not just sententially, over the temporal predicate T, but also lower in the clause, over vP. This prediction is borne out, as will be demonstrated below, and I concluded that in fact the scope positions for modals are rigidly fixed in the CP peripheries of phases, as the tree in (54). Specifically, I tied necessity modals (e.g. *must*) in to the ∀ position, since they are taken to quantify universally over PWs, and possibility modals (e.g. *can*, *might*) in to the ∃ position, since we take them to quantify existentially over PWs. I will go through evidence for this in more detail in §6.1.2 below.

Portner (1992) reanalyses Kratzer's world semantics for modals as a situation semantics for modals, so that rather than quantifying over possible worlds, modals quantify over possible situations.[1] I shall rope in Portner's view here, and claim that in fact modals function extremely like we have seen tense and aspect to function so far. Tense and aspect are associated with existential quantification over the situation variables provided by [ID] features: I will take modals to operate over the same variables, but with two additional factors: one, they can quantify universally as well as existentially; two, they relativize these situation variables with regard to possibility, so that rather than having ∃s, as we have seen for tense and aspect, we have (informally[2]) ∃ps (or ∀ps) (where ps = possible situation).

6.1.1   Epistemic–root scope distinctions

There is a common claim in the literature on modality that a scope distinction obtains between epistemic and root readings of modals, namely that epistemics scope high, at the top of the clause, whereas roots scope low, somewhere just above VP; see for example McDowell (1987); Picallo (1990); Brennan (1997); Roussou (1998); Cinque (1999); among others. Given the commonness of the claim, I don't give a wide review of all the work cited — I will rather briefly review two pretty straightforward pieces of evidence given by Brennan: the interactions of modality with symmetric predicates (§6.1.1.1), and with predicate adverbials (§6.1.1.2). Then in §6.1.1.3, I will run through how the derivation of modality works formally in the system here.

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[1] This refers to Kratzerian situations — cf. Kratzer (1989) — rather than Barwise & Perry (1983).

[2] More formally: ∃/∀s: s ∈ {possible situations}.

6.1.1.1   Symmetric predicates

Brennan (1997) shows epistemic and root modals have different semantic behaviour in particular contexts. She explains this by characterizing epistemics as 'S(entence)-modals' — i.e. propositional operators — and roots as 'VP-modals' — i.e. predicate operators. This is really a stipulation in her system, but one that is empirically motivated. One piece of evidence she gives is the behaviour of modals in clauses with symmetric predicates.

> Symmetric relations are ones for which the following inference pattern is valid: R(x,y) → R(y,x). Two classes of predicates denoting such relations are predicates with commitative *with* such as *shake hands with*, *walk with*, *play basketball with*, and equivalence relations such as *get the same score as*, *be as tall as*, and *be in the same room as*.
>
> (Brennan 1997: 190)

Brennan points out that in clauses with symmetric predicates containing modals, the inference R(x,y) → R(y,x) only remains valid under epistemic readings for those modals. (116a–116c) show this.

(116)   a. Arthur looks like Susan → Susan looks like Arthur

        b. Arthur must/might look like Susan → Susan must/might look like Arthur (epistemic)

        c. Arthur must/can look like Susan ↛ Susan must/can look like Arthur (root)

It is clear that in (116a), the entailment from R(x,y) (where R = look like, x = Arthur, and y = Susan) to R(y,x) goes through straightforwardly. Similarly in (116b), if it is a necessary/possible assumption that Arthur looks like Susan it must then be a necessary/possible assumption that Susan looks like Arthur too. In (116c), however, if Arthur is required to look like Susan (in order that he may be passed off as her illicitly, say) there is clearly no requirement that Susan also look like Arthur. Or if Arthur is able to look like Susan on account of his excellent impersonation skills, this in no way entails that Susan is as capable of impersonating Arthur.

If epistemic modality scopes over propositions (that is, predicates already combined with their subjects) and root modals scope over only predicates (without their subjects), then exactly the pattern Brennan notes is predicted: a propositional operator shouldn't be able to affect the way a predicate and an argument combine, since they are already combined before it even gets a look in, whereas a predicate operator will by definition change the nature of a predicate: the subject will no longer combine with the original predicate but rather the modalized one, and there is no reason to suppose properties such as symmetricality should carry from one to the other.

> A sentential [propositional] operator would not affect the logical properties of the VP [predicate]. Since modals with [root] properties do affect such logical properties, we have to conclude that they are actually concatenated in the semantics with the VP, not with the sentence.

(Brennan 1997: 192)

6.1.1.2   Predicate adverbials

A second point Brennan notes is the behaviour of predicate adverbials in modal contexts.

(117)   a. Anna may take out books on semester loan (root/epistemic)

    b. Anna must teach introductory courses (root/epistemic)

(118)   a. in virtue of being a graduate student, Anna may take out books on semester loan (root only)

    b. in virtue of being a graduate student, Anna must teach introductory courses (root only)

The presence of the predicate adverbial *in virtue of being a graduate student* in (118a) and (118b) only allows a root reading for the modals, and what is modified by these adverbials is clearly not just the main verb predicate VP, but the modalized predicate [modal+VP]: 'Anna's freedom to take out books on semester loan is a result of her being a graduate student, [rather than] taking out books on semester loan in virtue of being a graduate student is something she is allowed to do' (Brennan 1997: 199). If the adverbial is trying to modify a

modalized predicate but epistemic modals operate over propositions, the lack of
an epistemic reading for (118a) and (118b) is predicted. Again, this shows that
root interpreted modals operate over the sentential predicate, but under (some)
predicate adjuncts, whereas epistemically interpreted modals operate higher.

6.1.1.3   Deriving modality

Whereas Brennan's theory essentially stipulates the distinction between epistemic
and root modality, in the story here it is a derived property, stemming from
the scope positions associated with the modality in each case: epistemics scope
over the whole sentential proposition, i.e. the T phase, so they are interpreted
as propositional operators, whereas roots scope over only the V phase, so they
aren't interpreted as propositional operators. Additionally, because the subject
typically moves out of the V phase to its canonical position [Spec, TP], *past* the
scope position associated with root modality, root interpreted modals typically
will end up intervening between the subject, in [Spec, TP], and its properties as
expressed in the V phase.

Obviously this latter claim is muddied here by the fact that (1) I am not assuming
subjects really scope in [Spec, TP], but rather somewhere in the CP level, and
(2) I am assuming a multiple occurrence story for 'movement'. But nevertheless,
something of the kind still holds: if the subject gets to [Spec, TP] at all, it can only
go up from there, hence still remains higher than root modality; and low copies of
the subject don't count for computing scope relations since they just introduce a
restricted variable (chapter 3), and it isn't till the subject gets associated with a
higher quantificational operator that we can say anything about its scope. Given
these facts, Brennan's data fall out of the theory here as it has been presented.[3]

This section presents a precise formalization of how the effects discussed are to
be obtained, by running through syntactic and semantic derivations of sentences

---

[3]One thing that is probably worth noting here, though it will receive more discussion
throughout this chapter, is that the above discussion means that the property of root modality
that it often mediates the relation between a subject and a predicate is an entirely derived one:
it comes about just because of subject movement. The actual definition of root modality that
I am taking here is just the negative definition root = non-epistemic. Unfortunately, because
subject movement does often contribute this interpretation to root modality, the waters are
often muddied when we try to look at more complex cases and decide what kind of modality
we are dealing with (§6.2).

containing epistemic and root interpreted modals.

Epistemic interpretations are probably the most straightforward to illustrate. (120–124) gives a derivation for an epistemic reading of (119).

(119) The joint must rip

(120)

$$\text{CP}$$

Tree diagram:
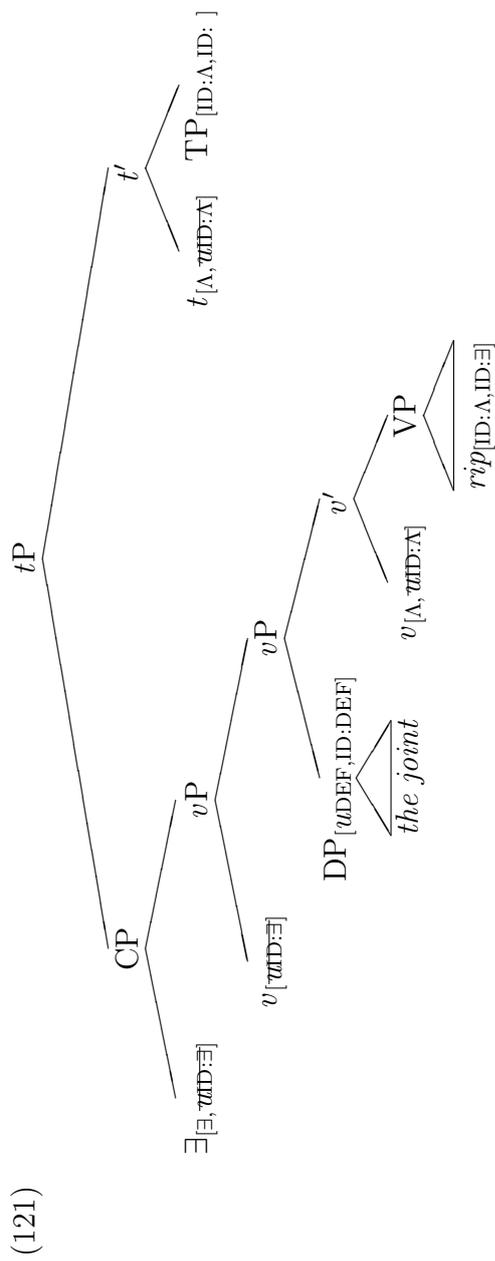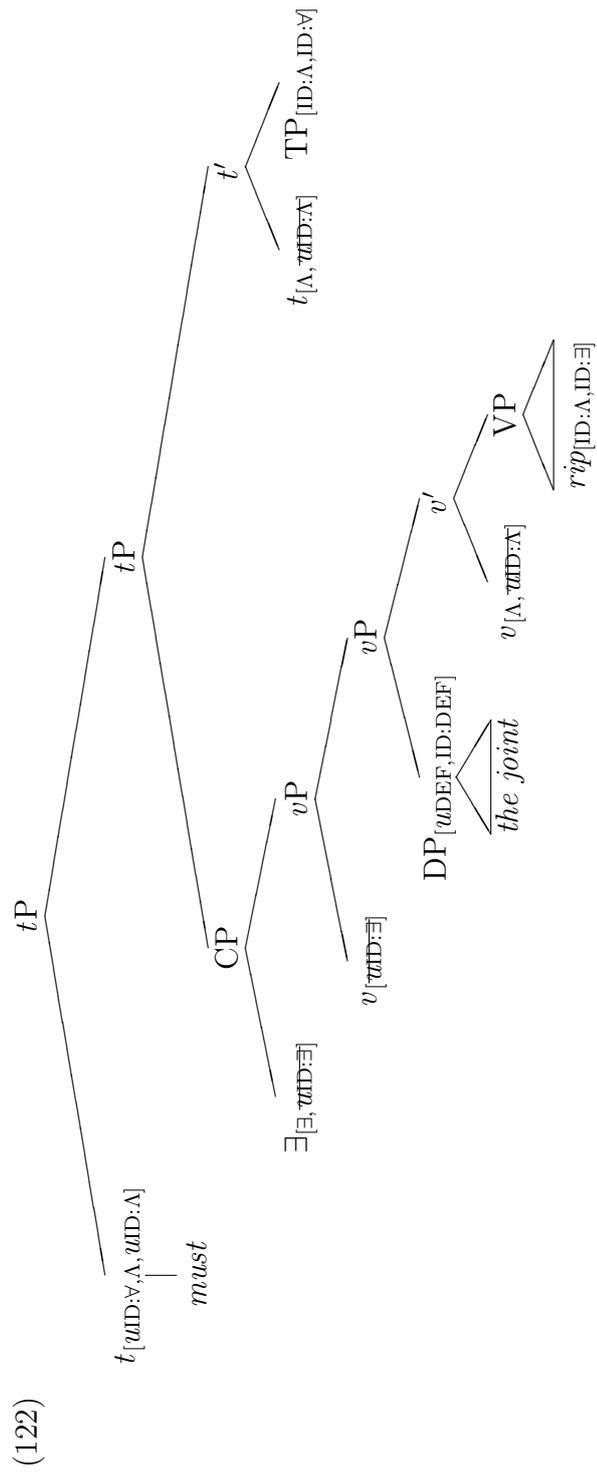- CP dominating $\exists_{[\exists, u\text{ID}:\exists]}$ and vP
- vP dominating $v_{[u\text{ID}:\exists]}$ and vP
- inner vP dominating $\text{DP}_{[u\text{DEF}, \text{ID}:\text{DEF}]}$ and v'
- DP dominating *the joint*
- v' dominating $v_{[\Lambda, u\text{ID}:\Lambda]}$ and VP
- VP dominating $rip_{[\text{ID}:\Lambda, \text{ID}:\exists]}$
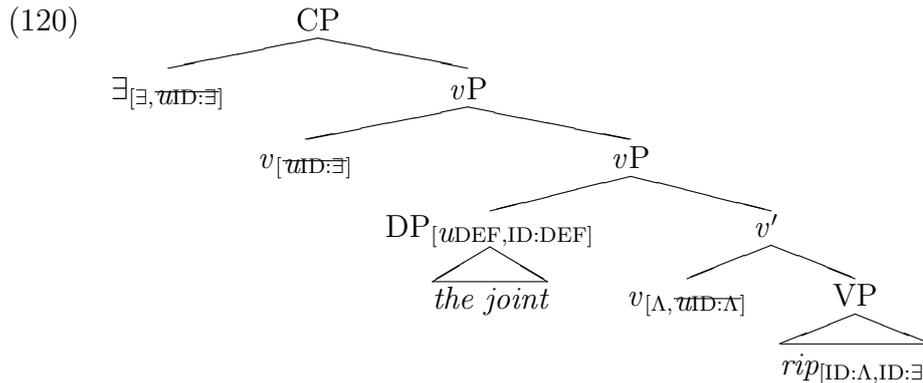
In (120) the V phase is built up as we have seen: V *rip* introduces two unvalued [ID:] features, one relating to its 'subject' argument, the other to its situation argument. One v is introduced, with interpretable [Λ] and uninterpretable [uID:Λ]. This Agrees with one of V's [ID:]s, valuing it as [ID:Λ], and introduces the subject, *the joint*, with the features [uDEF,ID:DEF]. A second v is introduced, associated with the situation argument, valued [uID:∃]. This Agrees with V's second [ID:], valuing it [ID:∃]. The CP layer is introduced, with an interpretable [∃] and uninterpretable [uID:∃]. Agree takes place between this and the outer v and the situation argument is existentially closed. Semantically, this is a predication of the property of ripping to some unspecified member of the set of joints. There isn't anything here that we haven't seen already.

In (121) T(P) introduces two [ID:]s, again one associated with its overt argument, i.e. the V phase, and one with its situation argument. A t valued [Λ,uID:Λ] selects TP, and values one of its [ID:]s as [ID:Λ], exactly as with overt arguments inside the V phase.

In (122) the inner tP is selected by an outer t hosting *must* and valued [uID:∀]. This Agrees with the second of T's [ID:]s, valuing it [ID:∀], ensuring that it will be selectively bound by a universal feature at some point; i.e. 'universal' closure equivalent to the ∃-closure in the V phase. This t is also, I assume, the one responsible for the EPP property of the T phase, i.e. the subject dislocation property, so it also has the features [Λ,uID:Λ]. Since there are no [ID:] features

(121)

tP

CP

$\exists_{[\exists,\,u\text{ID}:\exists]}$

vP

$v_{[u\text{ID}:\exists]}$

vP

DP$_{[u\text{DEF},\,\text{ID}:\text{DEF}]}$

the joint

v'

$v_{[\Lambda,\,u\text{ID}:\Lambda]}$

VP$_{[\text{ID}:\Lambda,\,\text{ID}:\exists]}$

rip/dir

t'

$t_{[\Lambda,\,u\text{ID}:\Lambda]}$

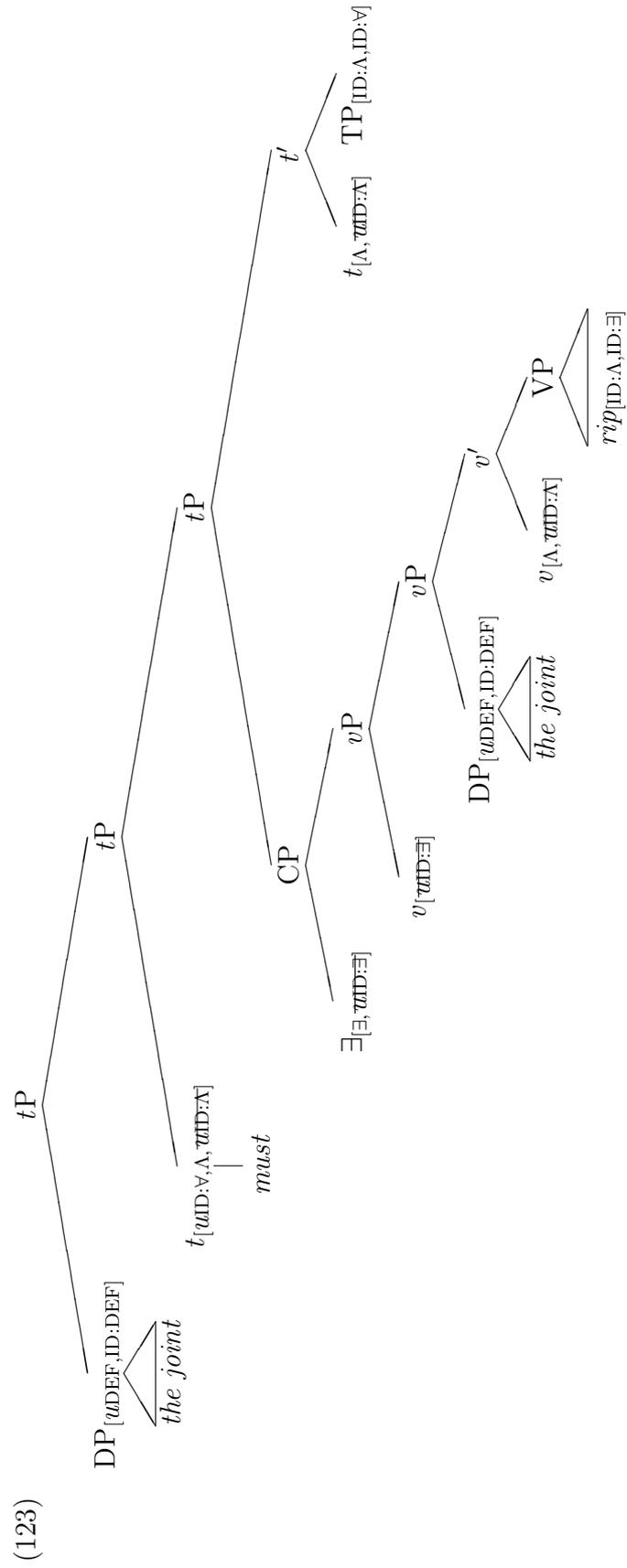TP$_{[\text{ID}:\Lambda,\,\text{ID}:\ ]}$

(122)

left for this feature set to bind, they Agree with the next set down, as discussed in §3.1.2.1, and so Remerge the subject from the V phase.
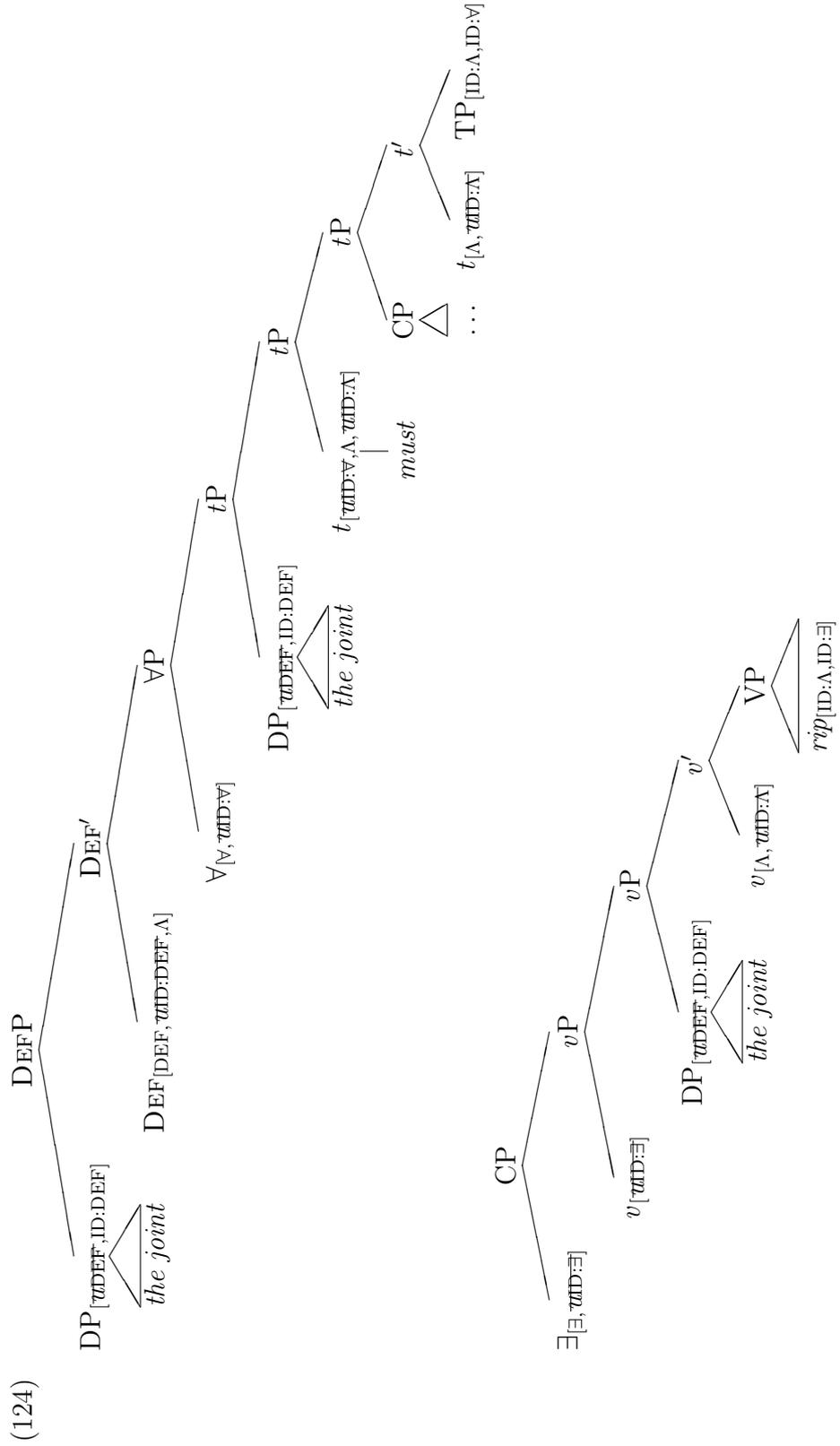
In (123) the subject is Remerged, and the $[u\Lambda]$ feature of the higher t is deleted. At this point, we have two dependencies to be resolved: one, the [ID:∀] feature on T, which though it has Agreed with the $[u$ID:∀$]$ on the outer t, still has not been bound by an interpretable [∀] feature as it needs to be; two, the [ID:DEF] feature on the DP(s), which also has Agreed with $[u$ID:DEF$]$, but remains unbound by an interpretable [DEF].
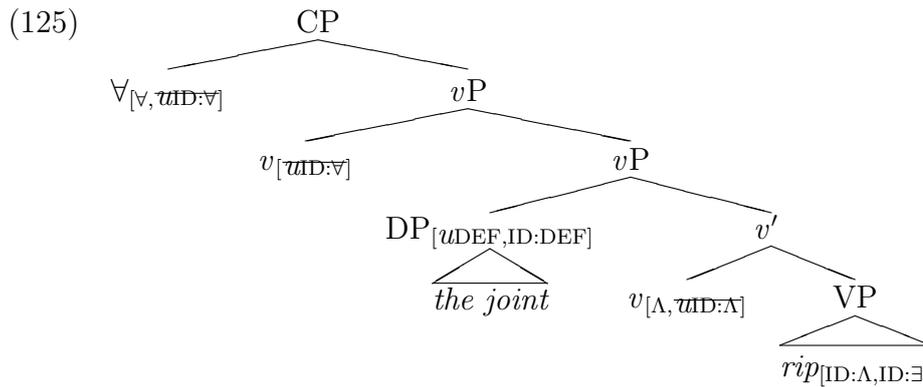
In (124) the T phase's CP introduces both an interpretable [∀], which serves to bind the [ID:∀] feature on T via the Agree relation between the two and the $[u$ID:∀$]$ on the outer t; and an interpretable [DEF] feature which binds the [ID:DEF] features on the DPs via the $[u$ID:DEF$]$s in the DP edges. Additionally, DEF hosts a $[\Lambda]$ which causes DP to Remerge in Spec–head configuration to achieve the required locality between the operator and its restriction, as discussed in (65), chapter 3.

The end product, then, is something that says 'the unique(δ).joint(δ) is such that at every possible reference situation, there exists an event situation such that rip(joint(δ))', which is pretty much what we want: any number of reference situations where there is at least one event situation where the joint rips. The only thing really missing from this derivation is the relativization to possibility provided by the modals. As far as this goes, it seems most straightforward to assume that this is some kind of implicit domain restriction provided pragmatically, as for any other quantificational expression — see Papafragou (1998), who suggests a formalization in terms of Relevance theory (Sperber & Wilson 1995). Her story basically boils down to the idea that a modal relation only takes into account those possible situations that are relevant. Exactly how this is achieved in relevance theoretic terms isn't hugely important here, since it is clear that the modal force provided by *must*, say, can be evaluated with regard to any number of relevant combinations of possible situations, and the level of information required can't be provided either by the narrow syntax or by a direct mapping of that syntax to a semantic representation: some pragmatic process has to be assumed. What that process is, and how it might be represented theoretically, is somewhat tangential right here, but see §6.1.1.4 below for discussion.

A derivation for a root reading of the same sentence is provided in (125–126).

(123)

$$
\begin{array}{l}
\textit{t}P \\
\quad \text{DP}_{[u\text{DEF},\text{ID:DEF}]} \ \textit{the joint} \\
\quad \textit{t}P \\
\qquad \textit{t}_{[u\text{Infl},\text{V:A},u\text{M:}\square]}\ \textit{must} \\
\qquad \textit{t}P \\
\qquad\quad \text{CP} \\
\qquad\qquad \text{E}'\ \text{E}_{[u\text{M:}\square]} \\
\qquad\qquad \textit{v}P \\
\qquad\qquad\quad \textit{v}_{[u\text{M:}\square]} \\
\qquad\qquad\quad \textit{v}P \\
\qquad\qquad\qquad \text{DP}_{[u\text{DEF},\text{ID:DEF}]}\ \textit{the joint} \\
\qquad\qquad\qquad \textit{v}' \\
\qquad\qquad\qquad\quad \textit{v}_{[\text{A},u\text{M:A}]} \\
\qquad\qquad\qquad\quad \text{VP}_{[\text{ID:V:DI},\text{V:E}]}\ \textit{trip} \\
\qquad\quad \textit{t}' \\
\qquad\qquad \textit{t}_{[\text{V},u\text{M:}\square]} \\
\qquad\qquad \text{TP}_{[\text{ID:V:DI},\text{V:A}]}
\end{array}
$$

(124)

DefP
- DP[uDEF,ID:DEF] *the joint*
- Def′
  - Def[DEF,uID:DEF,V]
  - tP
    - t[A:un,V]
    - tP
      - DP[uDEF,ID:DEF] *the joint*
      - tP
        - t[uID:A,V,un] *must*
        - tP
          - CP △ …
          - t′
            - t[V,un]
            - TP[uID,V:A]
              - vP
                - CP E[E:un]
                - vP
                  - v[E:un]
                  - vP
                    - DP[uDEF,ID:DEF] *the joint*
                    - v′
                      - v[un,V]
                      - VP *rip…* [uID,V:A,ID:E]

(125)

$$
\begin{array}{c}
\text{CP} \\
\forall_{[\forall,u\text{ID}:\forall]} \qquad v\text{P} \\
v_{[u\text{ID}:\forall]} \qquad v\text{P} \\
\text{DP}_{[u\text{DEF},\text{ID}:\text{DEF}]} \qquad v' \\
\textit{the joint} \qquad v_{[\Lambda,u\text{ID}:\Lambda]} \qquad \text{VP} \\
\textit{rip}_{[\text{ID}:\Lambda,\text{ID}:\exists]}
\end{array}
$$

In (125) the V phase builds up differently. The inner v still introduces the subject in the same way, but the outer v introduces not a $[u\text{ID}:\exists]$ but rather a $[u\text{ID}:\forall]$, since here we have root necessity, i.e. universal quantification over the temporally unanchored V phase situation variable. So rather than the C layer introducing $[\exists,u\text{ID}:\exists]$, it introduces $[\forall,u\text{ID}:\forall]$. Agree takes place and buys us the modality, with the same reservation as above about the relativization to possibility.[4]
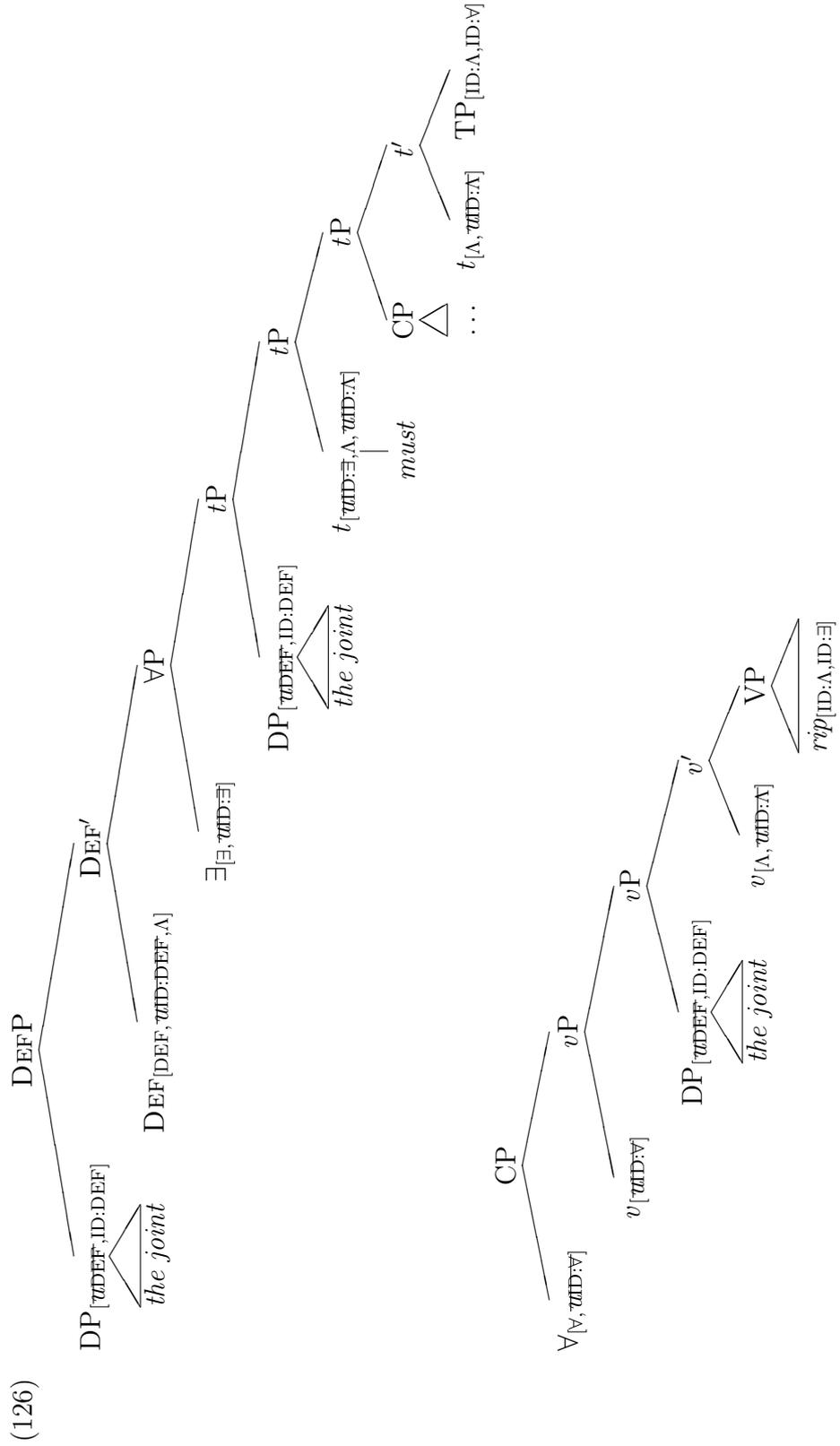
In (126) the derivation proceeds straightforwardly: T introduces two [ID:]s, one for its internal argument — the V phase — and one for its own situation argument; an internal t introduces $[\Lambda,u\text{ID}:\Lambda]$ and Merges the V phase as Spec; an external t values T's second[ID:] as [ID:∃], and is responsible for subject Remerge, as usual; and the C heads do their normal stuff, closing off the sitautaion argument (existentially this time) and Remerging the subject in [Spec, DEFP].

This time we end up with a reading which says that 'at the current reference situation, the unique(δ).joint(δ) is such that in every possible event situation, rip(joint(δ))', which again is what we want: one reference situation, specifically the current one, where every relevant event situation is one of the joint ripping.

6.1.1.4   Other types of modality

As far as straightforward epistemic and root readings for modals go, then, I argue that the ambiguity is a scopal one: informally, epistemic modals scope higher

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[4]It may seem odd here that the modality is all dealt with down inside the V phase, seemingly divorced from the modal marker which is Merged in a different phase altogether. In fact I will show in §6.3 that in non-finite clauses we can find exactly the same thing without any modal marker at all.

(126)

DefP
├─ DP[*u*Def,ID:Def] — *the joint*
└─ Def′
   ├─ Def[Def,*u*ID:Def,λ]
   └─ λP
      ├─ ∃[∃,*u*ID:∃]
      └─ tP
         ├─ DP[*u*Def,ID:Def] — *the joint*
         └─ tP
            ├─ t[*u*ID:∃,λ,*u*ID:λ] — *must*
            └─ tP
               ├─ CP … △
               └─ t′
                  ├─ t[λ,*u*ID:λ]
                  └─ TP[ID,V:ID,λ]
                     ├─ CP
                     └─ vP
                        ├─ A[A:*u*m]
                        └─ A′
                           ├─ v[v:*u*m]
                           └─ vP
                              ├─ DP[*u*Def,ID:Def] — *the joint*
                              └─ v′
                                 ├─ v[λ,*u*ID:λ]
                                 └─ VP
                                    └─ *rip…*[V:ID,V:ID,∃]

than tense,[5] and roots are just non-epistemic (i.e. they scope lower than tense). In the literature on modality, though, the distinctions between types of modality don't always stop at just root and epistemic: we also see numerous references to alethic and volitional modality, and less commonly to many supposed (sub)types such as deontic vs. ability root readings, dynamic modality (which according to Palmer (1990: 83) expresses 'neutral possibility, simply to indicate that an event is possible'), circumstantial modality (which Kratzer 1991 treats as to do not with any kind of speaker or agent viewpoint, but just with contingent facts of the world, which seems similar to Palmer's dynamic) metaphysical modality (which Condoravdi (2001) treats as something like either alethic or dynamic modality), etc. etc.

My contention here is that modality makes a unitary contribution to the syntax and the semantics wherever it appears: so that we don't have root modals and epistemic modals (and whatever other kinds of modals we might see fit to countenance) in the lexicon, but rather epistemic and root (and whatever else) readings of modality in context. This is not in fact an unusual position, but it does mean that any interpretive distinctions have to be dealt with elsewise than lexically. It seems to be the case that some such distinctions derive from different syntactic positions available for modality in the clause structure, but if there is really anything in the claims that more than two kinds of modality exist, then this can't be enough. Even if we propose more possible positions for modals to be interpreted (as I will show is sometimes the case in §6.2), I have given severely underspecified definitions for the straightforward root and epistemic readings, based solely on where they are in the clause in relation to temporal predication (and in fact §6.2 is essentially an extension of the same claim) — so basically anything operating over any temporal predicate should be epistemic, anything else should be non-epistemic, which is the only definition I have given of root.

If this is right, it forces the conclusion that there are in fact only two (derived) basic types of modality, epistemic and root, and anything else is really just one of these types with some independent sub-property that leads to a slightly different interpretation. In Butler (2003a) I followed Kratzer (1991) in taking this property

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[5]Informal, because I don't claim that tense itself has scope properties, and so it doesn't make formal sense to talk about other elements scoping around it; but the interpretive distinction between epistemic and root readings does come about because of the position of the modality in the clause relative to the temporal predication, i.e. whether or not it operates over a temporally anchored situation.

to be a feature of the semantics–pragmatics interface, deriving from the implicit domain restriction of the modal quantification.

Kratzer (1991) shows that to buy the gradability of modality – that is, the fact that cross-linguistically, we don't tend to see, say, one possibility modal and one necessity modal, but a set of each with differing strengths – the restriction of a modal needs to be evaluated along two distinct parameters: a MODAL BASE and an ORDERING SOURCE. The modal base is a set of possible situations,[6] specifically those possible situations that could be relevant for evaluating the modality. In one of Kratzer's examples, a man by the name of Girgl has been murdered, and as far as the police inspector is concerned any of the statements in (127) are reasonable (as are many others, of course).

(127) a. Michl must be the murderer

b. Michl is probably the murderer

c. There is a good possibility that Michl is the murderer

d. Michl might be the murderer

e. There is a slight possibility that Michl is the murderer

f. There is a slight possibility that Michl is not the murderer

g. Michl is more likely to be the murderer than Jakl

(Kratzer 1991: 643)

Since the police inspector lacks certain pieces of knowledge about the murder, all he has is 'evidence [which] is compatible with a set of worlds which, for all he knows, could be the real world. These worlds are the *epistemically accessible* worlds' (Kratzer 1991: 643), or epistemically accessible situations in my terminology, and they form the modal base.

However, in interpreting the modality it isn't really desirable to give equal consideration to every one of these possible situations; in the example under consideration, say, it is usual that if someone is murdered there is a motive. So a situation where some irrelevant stranger came in on a plane from overseas, murdered Girgl, and left, isn't a situation the police inspector is likely to consider as important in

---

[6]Kratzer talks about worlds in her paper, but as she notes elsewhere (Kratzer 1989) worlds can be seen as just maximally large situations, and as noted in the introduction to this chapter I am following Portner (1992) in reinterpreting Kratzer's world semantics for modals as a situation semantics.

sifting his evidence as a situation where Girgl has been killed in a robbery, or by a disgruntled ex-employee. If the police inspector couldn't give more weight to certain situations than to others, then it doesn't actually seem possible to make a modal statement like (127a), since then it simply wouldn't be the case that Michl *must* be the murderer at all: a random stranger would be just as likely.

This second part of the modal restriction is the ordering source: it puts an ordering on the set of possible situations under consideration (i.e. the modal base) such that those corresponding to some 'ideal' situation that can be seen as stereotypical or more in the normal course of events are considered 'more accessible' than those that don't correspond to the stereotype. It is the ordering source that allows modality to be graded into different degrees of necessity/possibility. Whether the police inspector uses (127a) or (127d) depends not on the modal base, since in either case this will contain situations where Michl did kill Girgl and situations where he didn't: rather, it depends on how the ordering source has ordered the situations in the modal base. If it has ordered them such that the situations where Michl is the murderer are more accessible, then (127a) is licit; if it has ordered them such that those situations are less accessible, then (127a) isn't licit, but the weaker (127d) is.

As well as the syntactic ambiguity I am suggesting, then, any modal utterance is also going to have two other parameters along which its meaning can vary: the modal base and the ordering source. This being so, we do actually expect that we might see different 'types' of modality without positing any lexical distinction, or any more syntactic distinction than proposed here.[7]

According to Kratzer, the modal base of a volitional modal is circumstantial (which she contrasts with epistemic modal bases, and which I take to be essentially what I am referring to here as root): that is, it picks out worlds 'in view of the relevant facts' (Kratzer 1991: 648). The relevant facts will determined, as usual, by the context of utterance. The ordering source is BOULETIC: 'in view of what I want' (p. 648): that is, the ideal situation it establishes is one that corresponds to the relevant desires of the person to whom the volition relates.

---

[7]And because the modal base and ordering source are both supplied contextually in every case, it is no surprise that we see so many different kinds of modality proposed, since the contextual distinctions at stake could be infinitely variable; nor that so many of them seem to be almost the same — e.g. Palmer's dynamic and Kratzer's circumstantial — since the distinctions could also be very small.

Kratzer gives an example involving a proposition something like that in (128), where this is intended to be understood as something like 'I don't want to go to the pub' (or 'I want not to go to the pub').

(128)   I won't go to the pub

The modal base here consists of situations picked out on the basis of whether certain relevant facts hold in those situations: for example, it is the evening, the speaker has a stressful deadline for the next day, the speaker has been invited to the pub by friends; and so on. The ordering source will establish the ideal situation in view of the speaker's wishes: basically, a situation where they don't go to the pub. The situations in the modal base will then be evaluated with regard to how closely they match up to this ideal. The meaning will thus be something like: 'in every relevant possible situation that matches closely enough to my wishes, I don't go to the pub'. This, then, is how volitional modality is derived in this system. As noted, the lexical semantics needs nothing additional said about it, and nor do we need to posit any syntactic ambiguity to distinguish straightforward epistemics and roots from volitional modals.

Alethic modality can be seen as 'pure' logical modality: that is, it doesn't relativize the modal to any particularly relevant context, but rather considers it objectively: its necessity forms, for example, quantifying over literally *every* possible world. Kratzer claims that the reason alethic modality[8] isn't relativized is simply because alethic modals have an empty ordering source: an alethic modal is

> one where the modal base is epistemic (a function which assigns to every possible world a set of propositions which constitute a body of knowledge in that world), while the ordering source is the *empty conversational background* (the function which assigns the empty set to every possible world)
>
> (Kratzer 1991: 645)

This explains the common observation in work on modality that order matters in modal discourse: the sequence in (129a) makes perfectly good sense, whereas the one in (129b) doesn't.

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[8]Kratzer uses the term 'pure epistemic' modality.

(129)   a. It might be raining outside [...] it isn't raining outside

       b. It isn't raining outside [...] # it might be raining outside

    (Groenendijk et al. 1996: 195)

We can't assert the possibility that it is raining using *might* if we know that, in the actual world, it isn't: that is, *might* is epistemically interpreted, relativising possibility to a particular piece of knowledge. The same isn't true of *could*.

(130)   a. It could be raining outside [...] it isn't raining outside

       b. It isn't raining outside [...] it could be raining outside

Even when we know it isn't raining outside, it is perfectly felicitous to assert the possibility of rain with *could* interpreted as alethic. The modal bases of (129b) and (130b) will be the same epistemic one: Kratzer's 'function which assigns to every possible world a set of propositions which constitute a body of knowledge in that world'. However, their ordering sources won't. (129b) will have a non-empty ordering source which constitutes something like the actual world: a world where it is known not to be raining outside. This means all the other accessible situations where it is not raining outside will be placed closer to the ideal than those where it is. These rain situations will then be less accessible than the rainless ones, hence (129b) is bad. (130b), on the other hand, will have an empty ordering source: it will impose no ordering on the accessible situations, and this means every situation will be considered equally. Situations where it is raining will be just as relevant for evaluation as situations where it isn't, so (130b) is fine.

The distinction between deontic and dynamic interpretations of root modals can be dealt with along the same lines. The modal base, as usual, will be selected with regard to certain relevant facts, depending on what the modal is intended to express in the particular context of utterance; but the ordering source will be different. For deontic interpretations, it will establish an ideal world where the salient obligation has been met, permission has been given, or whatever. The worlds in the modal base will then be ordered and evaluated with regard to this ideal, as we have seen.

Dynamic interpretations, on the other hand, can be derived like alethics: with an empty ordering source. A sentence like (131) doesn't, neutrally, express the deontic sense of *can*, where the modality would be relativized to situations where

permission can be given or withheld, etc., but rather the dynamic sense: it simply says that Arthur has the ability to jump. Whether he has or hasn't permission to do so in some particular situation is of no consequence.

(131)   Arthur can jump

It is clear, then, that numerous subtypes of modal interpretation can be derived under the system set out here, without specifiying any lexical distinctions, or any more syntactic distinctions than whether the modal scopes over or under a temporal predicate. It is interesting to contrast this with the analysis of modality in Cinque (1999), where it is claimed that any kind of modality is both lexically and syntactically distinct from any other kind. This will be discussed more in §6.2.2.

6.1.2   Necessity–possibility scope distinctions

The facts in §6.1.1, as well as others in the works cited, give credence to the idea that root and epistemic readings of modals differ scopally — specifically that root readings scope low, somewhere on top of VP, and epistemic readings scope high, on top of the whole clause.

The idea I am defending here is that the scope positions involved are the quantificational CP heads that define the edge of phases: the V phase has a quantificational CP layer, which is where the root reading comes from; and the T phase has one, which is where the epistemic reading comes from. Given that the V phase is basically interpreted as denoting a temporally unanchored situation, then we can define root modality as modality operating over such situations; the T phase, on the other hand, denotes a temporally anchored situation, i.e. a proposition, so we can define epistemic modality, standardly, as modality operating over propositions.[9] As discussed above, the fact that root modality also seems to have the property that it mediates the relationship between predicates and their subjects I take to be a derived property, falling out from the interpretation given by subject movement, as set out in §3.1.2.1 — there I took the movement of the subject to [Spec, TP] to derive a (sentential) predicate at t' (cf. Heim & Kratzer 1998),

---

[9]We saw in §5.5.2 that the V phase alone can also denote a proposition in certain circumstances; §6.3 discusses how this relates to the treatment of modality here.

so that anything below that level will be interpreted as part of that sentential predicate. Since I am taking the root scope position for modality to be the top of the V phase, this will in a simple clause be just below t' , and so the modality will end up intervening between the subject and its properties as expressed inside the verbal complex.[10] This interaction between subject movement and modal scope can, then, derive the facts from Brennan (1997) discussed above. However, given the precise structure of CP that I am proposing, it predicts more detailed interactions than this also.

6.1.2.1   Modal/quantificational scope relations

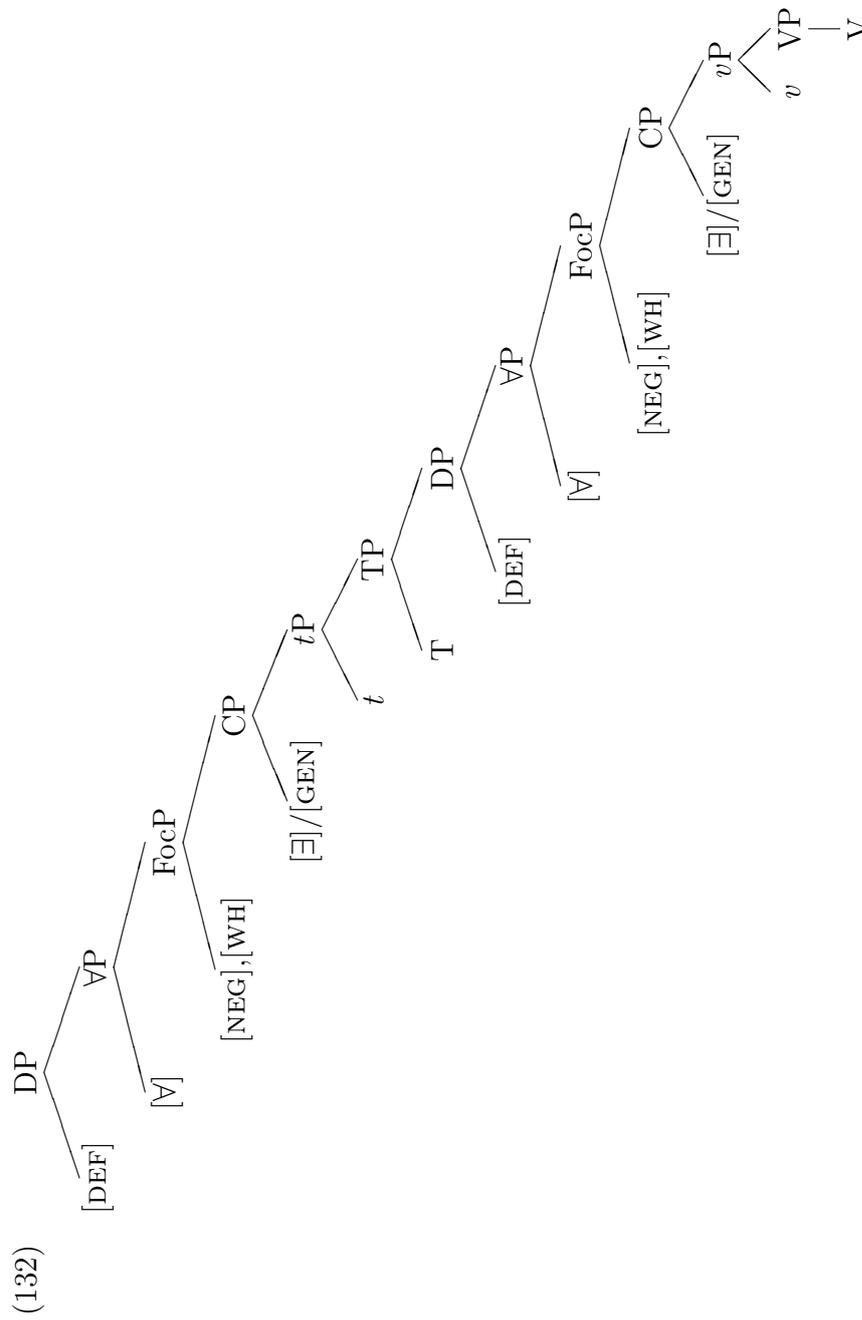Recall the basic overall clause structure being proposed here (132).

As we have seen, the clausal CP level quantificational elements contribute to scope relations in two ways: one, they close off situation variables introduced inside their own phases, leading most relevantly here to modal scope, and two, they close off individual variables inside N phases, leading to QP scope. Given that the sequence of quantificational elements occurs twice in even a simple clause, most scope relations between modals and QPs should be available. However, there are some specific predictions that we make given the precise ordering and interpretation of the elements in question: essentially, some quantificational heads are always going to appear over other quantificational heads just because of the way the clause is structured. This is going to restrict particular scope possibilities in certain ways.

1. It seems to be the case that [WH] features are only semantically active in the T phase's CP, not the V phase's. This being so, we should never see an existential scoping over a *wh*-phrase, which seems to be true (133). Any indefinite apparently doing so must be something like a 'referential indefinite' in approximately the sense of Fodor & Sag (1982), occupying [Spec, DP], and essentially scope neutral.

   (133)   Who has a problem?

            = For which person x, ∃ problem y, x has y

---

[10]See the discussion of the interpretations of propositional, predicate, and constituent negation in §5.3 for related ideas.

(132)

DP
  [DEF]
  AP
    [A]
    FocP
      [NEG],[WH]
      CP
        ∃/[GEN]
        tP
          t
          TP
            T
            DP
              [DEF]
              AP
                [A]
                FocP
                  [NEG],[WH]
                  CP
                    ∃/[GEN]
                    vP
                      v
                      VP
                        V

$\neq \exists$ problem y, for which person x, x has y

2. Similarly, we should never see negation scoping over [WH] (134).

    (134)   Who didn't see the fireworks?

            = For which person x, it is not the case that x saw the fireworks

            $\neq$ It is not the case that for which person x, x saw the fireworks

3. Though the clausal quantificational heads do formally the same thing in terms of deriving modality, the interpretation of the modality is different in the two cases — the T phase's quantificational heads derive epistemic modality, the V phase's derive root modality. This means we should never see an existential indefinite scoping over an epistemic interpreted necessity modal (135). Again, if we apparently do, the indefinite must be a 'referential indefinite'.

    (135)   Someone must know the answer

            = In every possible world, $\exists$ person x, x knows the answer

            $\neq \exists$ person x, in every possible world x knows the answer

4. Similarly, we should never see a root interpreted modal scoping over a wh-element (136).

    (136)   a. Who must tidy their room?

                = For which person x, x is required to tidy x's room

                $\neq$ It is required that for which person x, x tidy x's room

            b. Who can tidy their room?

                = For which person x, x is able to tidy x's room

                $\neq$ It is possible for which person x, x tidy x's room

5. All modals should be able to scope over negation except root interpreted possibility modals (137).

    (137)   a. Susan mustn't be in bed (epistemic)

                = must > not

            b. Arthur mightn't be in bed (epistemic)

                = might > not

    c. Emma mustn't stay up late (root)

       = must > not

    d. Emma can't stay up late (root)

       = not > can

       ≠ can > not

6. We should never see an epistemic possibility interpreted modal scoping over a universal QP (138). This one is less clear than the cases above — (138) does in fact have two readings, (138a) and (138b), which seem to correspond to distributive and collective readings, respectively: so (138a) says something like the murder could have been committed any single individual, (138b) says it could have been done by a whole group of individuals (cf. Christie 1934). It is clear for the distributive reading (138a) that the universal has to outscope the modality. The collective reading is murkier with regard to scope facts; I will assume as in fn. 32, chapter 3 (as there, perhaps wrongly) that this is an interpretive rather than a scopal ambiguity, and that the distributive case, being the clear cut case, is the one to look at.

    (138)   Everyone might be the murderer

        a. = 'for every person x, there exists a possible situation where person x is the murderer'

        b. = 'it is possible that the whole group of people committed the murder collectively'

7. We should never see a negative QP scoping over an epistemic interpreted necessity modal (139). The only interpretation for (139) on this view should be 'it must be the case that there doesn't exist a person x such that x is in bed'. In fact an alternative reading does seem marginally possible, 'there doesn't exist any person x such that it must be the case that x is in bed' but only under particular conditions, namely when the modal is heavily, and echoically, focus-stressed: 'no-one *must* be in bed'. The interactions of focus/focal stress and scope are notoriously complex (see Herburger 2000 for recent discussion), and I will have to leave this aside; at least the neutral scope, though, is the expected one.

    (139)   No-one must be in bed

Given the very precise nature of the predictions made, the fact that they are backed up by the data, in a mostly straightforward way, provides good support for the idea that scope relations are a reflection of the clause structure being proposed.
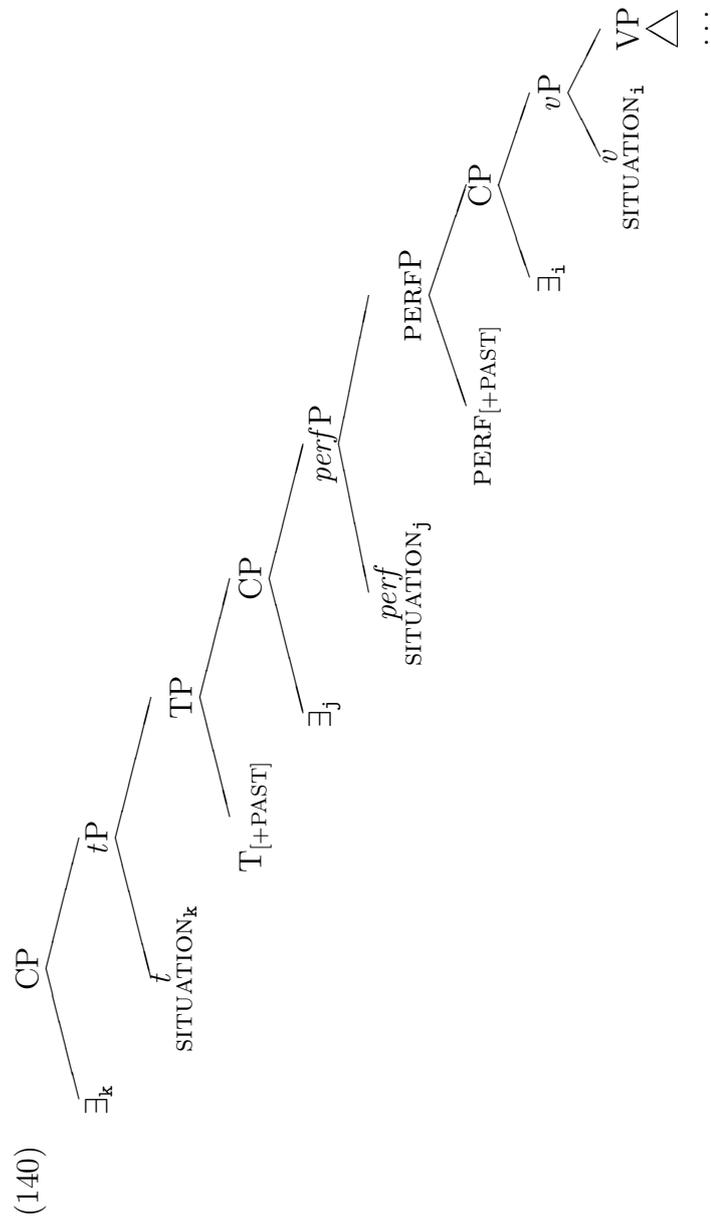
6.2   Modal/aspectual scope relations

In simple tensed clauses, then, a two-way meaning ambiguity is derived from two potential scope positions for modality: one in the T phase's CP, which gives us epistemic readings, i.e. propositional attitude readings, and one in the V phase's CP, which gives us non-epistemic, i.e. root, readings.

In more complex clauses that also contain aspectual auxiliaries, though, I predict a third possible scope position for modality. Recall (§5.2.2) that I am treating prefective/progressive aspectual auxiliaries structurally on a par with V and T in that they introduces phases (=CPs), and interpretively on a par with the T phase in that they are based around a temporal predicative core, with perfective *have* equivalent to [+PAST] T and progressive *be* equivalent to [–PAST] T. So structurally, a clause with a perfect auxiliary will look like (140); T is [+PAST] partly for concreteness, partly to aid the discussion to follow.

In (140) we still have the T phase scope position for modality, which we still expect to be epistemic, since the tensed clause as a whole we still expect to give us a proposition; and we still have the V phase scope position, which we still expect to be root, since we still just have a temporally unanchored situation at this point. But we also have a third intermediate position, in the CP of the PERF phase. There are certain questions we need to ask at this point:

- If this intermediate position really is an available position for modal scope, what do we expect the interpretation of modality here to be?

- Do we in fact see modality scoping here?

- Does it get the interpretation we predict?

As far as the interpretation question goes, we have a pretty straightforward answer, but with an interfering factor. The straightforward part is that we expect

(140)

```
            CP
           /  \
         Ǝₖ    tP
              /  \
             t    TP
       SITUATIONₖ  /  \
               T[+PAST] CP
                       /  \
                     Ǝⱼ   perfP
                          /   \
                        perf  PERFP
                   SITUATIONⱼ  /   \
                          PERF[+PAST] CP
                                     /  \
                                   Ǝᵢ    vP
                                        /  \
                                       v    VP
                                  SITUATIONᵢ  △
                                              ⋮
```

an epistemic reading for modality here. The reason is to do with how I have de-
rived the epistemic reading in general: I have said that it falls out of a case where
we have two situations temporally anchored with regard to one another, because
in such a case we have a truth evaluable proposition, and epistemic modality is
what we get when modality operates over such a proposition. The element giving
us this temporal relation in the examples so far has been T. However, that it
has been T isn't the crucial thing; what is crucial is the fact that it introduces
a situation variable, and predicates a temporal relation between that situation
and the situation denoted by its complement (the V phase). Since I have made
exactly the same claim about PERF, then PERF on top of the V phase should also
derive a truth evaluable proposition, exactly as T does. This being so, modality
operating in the CP of the PERF phase should get an epistemic reading just as
modality operating in the CP of the T phase does.

The interfering factor is subject movement: as noted above, one thing associated
with root readings is that they often apparently mediate the relation between the
subject and its properties; as noted I am treating this as deriving from movement
of the subject past the root modality scope position, rather than as an intrinsic
property of root modality.

In the case of (140), we are predicting an epistemic reading for modality, but un-
like with the epistemic reading the T phase gives us, it is underneath the canonical
position of the subject. This means we may still get the derived 'mediation' re-
lation with modals scoping in this position, and since this is typically considered
a root property, this may make it difficult to say exactly what interpretation we
are seeing in any particular case. What we might really expect to see is this
modality receiving in some cases a pretty straightforward epistemic reading, and
in some cases a reading with seemingly mixed properties. However, so long as
we can show that whatever reading we get has epistemic characteristics, I believe
the prediction will be backed up.

As to whether we do actually see modality scoping in this position, the examples
are going to have to be very carefully constructed to be able to say yes or no,
since distinguishing between two epistemic readings obviously isn't as easy to
do as distinguishing epistemic and root. However, a means for constructing the
relevant types of examples has been discussed recently by Condoravdi (2001) and
Stowell (2004).

The cases Condoravdi and Stowell discuss are past perfect clauses, as (140),

containing a modal. Though the details of the theories they present differ from
the details here, their central observation carries over, which is that modals in a
scope position higher than [+PAST] T and modals in a scope position lower than
[+PAST] T will differ with regard to when the relevant modal evaluation is taken
to obtain relative to the T phase's reference situation. So if we have two scope
positions for epistemic modality, as I am claiming here, one above and one below
[+PAST] T, we should be able to distinguish them by looking at the time the
epistemic evaluation is related to: specifically, we expect the reading associated
with the T phase's CP to be related to basically the speech time S, and the
reading associated with the PERF phase's CP to be related to some past reference
time R. I shall label these present and past epistemic readings, respectively.

A clear-cut case of the three-way ambiguity I am predicting is (141), where the
modality does indeed have three clear discrete readings: a present epistemic
(141a), a past epistemic (141b), and a root (141c). If I utter the sentence with
the continuation (141a), I am reporting on some aspect of my epistemic state at
S. If I utter the sentence with the continuation (141b), I am reporting on some
aspect of my epistemic state at some past reference time R; a crucial observation
here is that my epistemic state could have changed in the period between R and
S, and in fact this could be overtly acknowledged in the discourse with a further
continuation like *... in fact I subsequently found out that he had(n't)*. This kind
of continuation is impossible with (141a), as we would expect. If I use the contin-
uation (141c), I am simply saying that at E, the potential book-buying situation,
Professor Branestawm had the ability to buy the book.

(141)   Professor Branestawm could have bought the book...

    a. ... as far as I know now (present epistemic)

    b. ... as far as I knew then (past epistemic)

    c. ... he was in the shop and he had the money on him (root)

This kind of example shows, then, that an intermediate scope position for modal-
ity is available just where the theory presented here predicts there should be one,
and it can get the interpretation the theory predicts it should get.

As noted above, we also expect the possibility that examples like this may also get
a reading that has the characteristic commonly associated with troot modality,
where the modality mediates the subject–predicate relationship. We can see a
case like this in (142).

The present epistemic (142a) and root (142d) readings remain in (142) much as in (141), and a reading corresponding to (141b) is also available.

(142)   Professor Branestawm should have bought the book...

    a. ... as far as I know (present epistemic)

    b. ... as far as I knew (past epistemic)

    c. ... by last Tuesday (past epistemic/root?)

    d. ... when he was in the shop and he had the money on him (root)

There also seems to be an alternative reading for the intermediate case here, though (142c), and it seems to have root properties. Specifically, the reading in question is one where at some past time, explicitly *last Tuesday*, there was some kind of necessity that Professor Branestawm have bought the book. The necessity clearly doesn't relate to S here, since it is past; and it clearly doesn't relate to E either, since it is not saying that it was necessary for him to buy the book at any particular possible book-buying situation, as (142d) is. Rather, it is saying that there is some past time at which Professor Branestawm should *already* have bought the book; or to put it another way, the proposition *Professor Branestawm has bought the book* is required to be true at some past time. This time has to be R, i.e. this must be the scope position for modality associated with the PERF phase's CP.

This is again one of the interpretations I predicted above for modality scoping in this position: it has epistemic properties, in that it operates over a pair of temporally anchored situations, i.e. a proposition. However, it also seems like the responsibility for making this be the case is Professor Branestawm's. It could be argued that this is pragmatically derived, but I would argue that it is at also least in part derived from the relative interpretive positions of the modality and the subject: the modality, as in 'normal' root cases, intervenes between the subject and its properties.[11]

We have good evidence from the interactions of modality and aspect, then, that there is a quantificational CP layer on top of PERF parallell to the CP layers on

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[11] An interesting thing that it may be possible to do here is to formalize the distinction between what are known in the literature as *ought to be* and *ought to do* modals, along the lines of: *ought to be* modals are those where the modal scopes over aspect, but under the subject, thus having both root and epistemic properties; *ought to do* modals are those where the modal scopes under aspect, thus having only root properties. I won't pursue this in detail here.

top of the V and T phases. It is harder to use the same kind of reasoning to show that the CP of the PROG phase exists, since the temporal relationship at stake (present rather than past) doesn't allow the distinction to be so clearly made; but it seems entirely reasonable to infer that if the PERF CP exists, then so too does the PROG CP. If these CPs are there, then this in turn provides good evidence that the general view of structure and structure building that I am pursuing is on the right track.

6.2.1   But are they phases?

Just because the CPs over PERF and PROG exist, do we really have to claim they are phases?

The answer to this is yes, under the assumption that a phase is a domain of quantificational closure. If modal and temporal information is related to quantification at the CP level, in the sense that they both quantificationally close off situation variables associated with the l-syntax in their phase domain, then the demonstration in the previous section that modal scope and aspect interact in the way that they do shows that the quantificational elements in the PERF CP must be semantically as active as the elements in the V and T phases' CPs. If they weren't active, given the general theory of scope assumed here, it wouldn't be possible for anything to scope in the PERF CP at all. Since modality can scope here, these elements must be active, and therefore these CPs must count as phases on the definition I am adopting.

6.2.2   An interesting aside: deriving Cinque (1999)

Cinque (1999) has influentially proposed a hierarchy of functional heads which he takes to hold universally, i.e. across all clauses and clause types, cross-linguistically. His structure contains numerous heads, many more than motivated here, but interestingly, if we generalize the function of each head, what we see is a series of layers of the clause, which once VP is out of the way break up into alternating modal and aspectual/temporal layers (143).

(143)   **Modal layer**

[*frankly* Mood$_{\text{speech act}}$ [*fortunately* Mood$_{\text{evaluative}}$ [*allegedly* Mood$_{\text{evidential}}$
[*probably* Mod$_{\text{epistemic}}$

Temporal layer

[*once* T$_{\text{past}}$ [*then* T$_{\text{future}}$

Modal layer

[*perhaps* Mood$_{\text{irrealis}}$ [*necessarily* Mod$_{\text{necessity}}$ [*possibly* Mod$_{\text{possibility}}$

Aspectual layer

[*usually* Asp$_{\text{habitual}}$ [*again* Asp$_{\text{repetitive(I)}}$ [*often* Asp$_{\text{frequentative(I)}}$

Modal layer

[*intentionally* Mod$_{\text{volitional}}$

Temporal/Aspectual layer

[*quickly* Asp$_{\text{celerative(I)}}$ [*already* T$_{\text{anterior}}$ [*no longer* Asp$_{\text{terminative}}$
[*still* Asp$_{\text{continuative}}$ [*always* Asp$_{\text{perfect(?)}}$ [*just* Asp$_{\text{retrospective}}$
[*soon* Asp$_{\text{proximative}}$ [*briefly* Asp$_{\text{durative}}$
[*characteristically(?)* Asp$_{\text{generic/progressive}}$ [*almost* Asp$_{\text{prospective}}$
[*completely* Asp$_{\text{SgCompletive(I)}}$ [*tutto* (Italian) Asp$_{\text{PlCompletive}}$

Modal layer

[*well* Voice

Aspectual layer

[*fast/early* Asp$_{\text{celerative(II)}}$ [*again* Asp$_{\text{repetitive(II)}}$ [*often* Asp$_{\text{freqentative(II)}}$
[*completely* Asp$_{\text{SgCompletive(II)}}$

(Cinque 1999: 106)

This is also exactly what we find in the theory set out here: on top of the verbal layer of the clause, we get a quantificational layer which derives modality; then we (optionally) get an aspectual layer, then a quantificational layer deriving modality, then maybe another aspectual layer or a temporal layer, then a CP layer deriving modality ... and so on. I am not of course claiming that I have derived the exact hierarchy in (143). In fact, despite the huge amount of cross-linguistic data that led to the postulation of this hierarchy, it is still just an early pass at what it should look like, as Cinque freely acknowledges. However, the general observation that it splits the clause into these layers is clearly sound, and would likely survive any modifications of the detail, which is the important point here.

An additional thing to note is that Cinque's hierarchy contains numerous potential elements in each level, whereas the system here predicts fairly small layers: probably only one modal per modal layer, in any concrete instantiation, since there will only be one situation variable for modality to operate over per phase. This might seem problematic if Cinque's data were to prove it incorrect, but in fact this is unlikely to happen. While Cinque maintains that every one of these functional projections is there in every clause, this is entirely a theoretical claim: he has no data instantiating anything like this number of projections all in one go, at least as far as heads go. It may be that data could be constructed with a large number of adverbs (though still almost certainly not so large a number as Cinque claims heads exist), but this would only provide justification for Cinque's position if you accepted his other theoretical claim, that these heads host the specific adverbs seen in (143) in unique specifier positions. This is far from proven, and if we take the more standard view that adverbs are adjoined, the presence of lots of adverbs can tell us nothing about functional structure.

It may then seem puzzling that Cinque suggests that there are distinct modal projections in each modal layer, but in fact if they aren't all active simultaneously, this is not inconsistent with what I have said here: in general, there will be at least either a necessity or a possibility reading for modality in any layer, just because the structure of the CP layers includes a universal and an existential quantifier. There will also be further distinctions derivable pragmatically from different modal bases and ordering sources, as discussed in §6.1.1.4. Similar considerations extend to aspect, since again we have different quantificational elements in an aspectual phase's CP that could in quantifying over the aspectual element's situation variable plausibly lead to different readings for the aspect.

In general, though, the theory here is further supported by the fact that it derives a version of Cinque's story from deeper interpretive principles.

6.3   Modal/infinitive interactions

We already saw in §5.5.3 that some infinitives get a 'possible future' reading, where the situation they relate to is interpreted as unrealized with regard to the temporal/situational information in the matrix. It has been argued that this means they must encode some tense information.

I argue here that this isn't the case — while future shifting may reasonably be regarded as temporal in some intuitive sense, it is fairly commonly accepted in the literature that it is not a function of tense, but rather of modality (Enç 1996); and possibility is of course one of the core aspects of modality. A phenomenon defined along both these parameters suggests strongly that what these infinitives encode is not tense-related information, but rather modal information.

Further back up for this idea comes from the fact that wherever it is possible to paraphrase one of these infinitives with a finite clause, that clause necessarily contains a modal.[12] Some relevant examples from §5.5.3 are repeated below as (144–146).

(144)   a. Jenny remembered [to bring the wine]

        b. Jenny remembered [that she should bring the wine]

(145)   a. The table [on which to put your coat] is in the next room

        b. The table [on which you should put your coat] is in the next room

(146)   a. John convinced his friends [to leave]

        b. John convinced his friends [that they should leave]

The immediate question to ask in light of such examples is what the source of this modality is in the non-finite cases, since plainly there is no overt modal element either in the matrix or the embedded clause. There is a straightforward answer to this here: it is the same as the source we have seen so far for modality generally. That is, it is quantification over the situation variable of the infinitive by some C level head, plus the relativization to possibility of this variable.

A way of testing whether this general approach holds water is to look at how the modality is interpreted. The epistemic/root distinction, I have treated above as arising from the status of what the modality is operating over: an epistemic interpretation comes about when the modality operates over a proposition, defined in terms of two temporally anchored situations, whereas a root interpretation comes about when the modality operates over a non-propositional, i.e. temporally unanchored, situation. I showed in §5.5.2 that aspectual infinitives were interpreted

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[12]Or at least, a modal notion — for example, (144b) could just as easily be *Jenny remembered that she needed to bring the wine*; the notion of necessity, here, though, is a modal one, and it is the obligatoriness of modality in the paraphrase, rather than of a modal auxiliary verb itself, that is crucial.

as temporally specified propositions; generically construed *to* infinitives were interpreted as propositions holding true generically; and bare V infinitives were interpreted just as existential situations. I also showed in §6.2 that modals are able to scope in any of the clausal CPs, saliently here the CP of the V phase and the CP of the aspectual phases. This gives us another pair of predictions: in infinitives that get the 'concealed modal' interpretation, we expect that:

1. In a bare V infinitive, the covert modality will be necessarily interpreted as root, since we have no temporal or generic specification to derive the epistemic interpretation.

2. In an aspectual or a generically construed infinitive, there will be available an epistemic interpretation, since the aspectual information or genericity will be enough to give us a truth-evaulable proposition.

These predictions turn out to be correct. Bhatt (1998) goes in detail through the interpretation of the modality in the English *have to/ought to* construction and its cross-linguistic analogues, which he refers to as the obligational construction. He shows that such constructions are available in many languages, characterized as the possessive marker (*have* or its equivalent in English, German, Galician, European and Brazilian Portugese, etc.; *be* or its equivalent in Hindi, Gujerati, Punjabi, etc.) plus a non-finite verbal form (commonly an infinitive or gerund).

(147)   a. John has a book

      b. John has to read a book

(148)   a. Der Hans hat ein Buch   GERMAN
        the Hans has a book
        'Hans has a book'

      b. Der Hans hat rechtzeitig in Wien anzukommen
        the Hans has in-time   in Vienna to-arrive
        'Hans has to arrive in Vienna in time'

(149)   a. Xoán ten un libro de Bello   GALICIAN
        Xoán has a book by Bello
        'Juan has a book by Bello'

      b. Xoán ten que   comer esta mazá
        Xoán has that/to eat.inf this apple
        'Juan has to eat this apple'

(150)   a.   John-ko   sirdad   hai   HINDI
                 John-DAT headache be.PRES
                 'John has a headache'

         b.   John-ko   seb   khaa-naa   hai
                 John-DAT apple eat-GERUND be.PRES
                 'John has to eat the apple'

(151)   a.   Ram-ne   tav   che   GUJERATI
                 Ram-DAT fever be.PRES
                 'Ram has a fever'

         b.   mahne mārā bhāine   thodā paysā āpvānā   che
                 I.DAT my   brother.DAT a-little money give.GERUND.DAT be.PRES
                 'I have to give some money to my brother'

         (Bhatt 1998: 22–23)

It appears strongly that the obligational construction consists of is the usual pos-
sessive verb (Spanish and European Portugese distinguish possessive '*have*' from
other forms; they use the possessive form in the obligational construction) taking
a non-finite complement. A plausible paraphrase is then '`subject` possesses an
obligation to `verb`'.[13] That is, the possessive element seems to be contributing
just its usual semantics in these constructions. It is obvious that non-finite ele-
ments do not necessarily involve any element of obligation in their semantics,
since there are innumerable non-finite elements that simply refer to situations
or propositions existentially, with no component of modality. Bhatt therefore
proposes that the modality in the obligational construction is contributed by a
covert modal. Bhatt proposes a structure for such constructions, but given the
differences between the framework his proposal is in and the framework I am
adopting, I won't take up that structure here; instead I propose my own struc-
ture which will be fairly predictable: *have* takes an infinitive complement; the
situation variable of this infinitive is operated over by [∀] in the ∀ head of its CP,
with modality making its usual contribution of relativizing the relation in terms
of possibility (153). Since the infinitive contains no T phase, the modal can't
show up overtly in T, as in finite clauses, but its semantics are easily detectable.

What we saw in examples (147–151) above were bare V phases, and in each case
the modality was interpreted as root, hence Bhatt's 'obligational' label. This is

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[13]Bhatt decomposes possessive *have* into existential *be* with an incorporated lower head X,
following Freeze (1992); Kayne (1993). The paraphrase is then something like 'there is an
obligation [`subject` to `verb`] (Bhatt 1998: 24). It isn't necessary to go into this here.

(152)   Professor Branestawm had to buy the book

(153)

CP

∃$_k$

tP

Professor Branestawm$_m$

t'

t
SITUATION$_k$

TP

T

CP

∃$_j$

vP

t_m

v'

v
SITUATION$_j$

VP

V
had

CP

∀$_i$

to

vP

PRO

v'

v
SITUATION$_i$

VP

buy the book
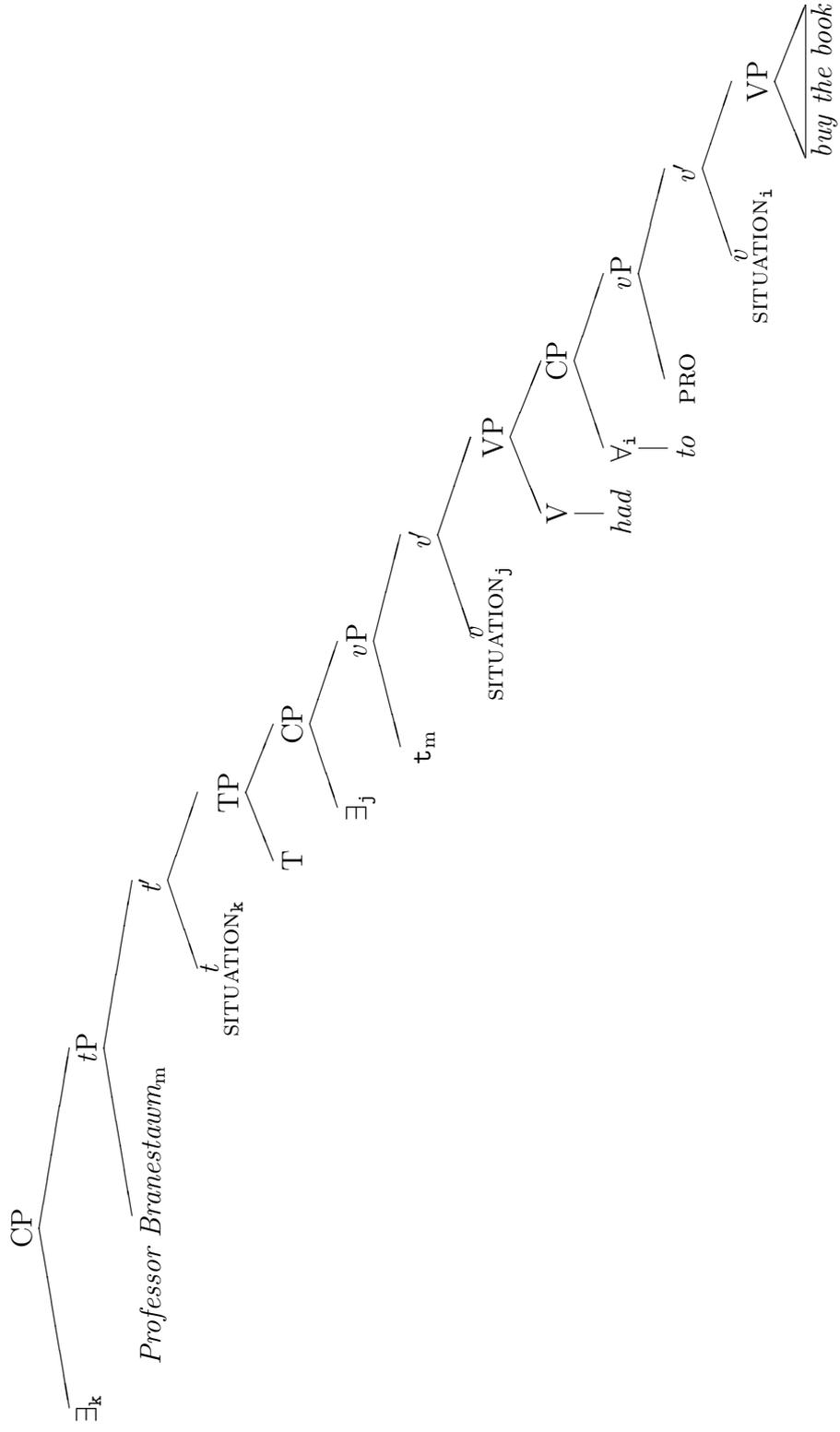
consistent with prediction 1 in the discussion above. I also made prediction 2, which was that when we didn't just have a bare V phase, but either a generically construed *to* infinitive or an aspectual infinitive, we would see an epistemic reading, consistent with what was demonstrated with regard to modal and aspectual interaction in §6.2. We do indeed see this (154a), cf. (142b). We also see the interfering 'mediating' reading where at the explicit R *last Tuesday*, there was some kind of necessity that Professor Branestawm already have bought the book, i.e. to have bought it at the embedded E (154b; cf. 142c). This reading was previously argued to derive from subject movement past the modality; here I am actually assuming a PRO subject in the lower clause, so this is control rather than raising. However, given that the interpretation of PRO will be controlled by the matrix subject, and specifically given the interpretation of a restricted variable (see (68) in §3.1.2.1), the effect will be essentially the same as if this were movement.

(154)   Professor Branestawm had to have bought the book

    a. ... as far as I knew (past epistemic)

    b. ... by last Tuesday (past epistemic/root?)

As to generically construed *to* infinitives, (155) shows us that the prediction holds true there too: an epistemic reading (155a) parallel to (142b) and (154a) is readily available. We also see a reading where there is some necessity for a generic proposition to hold (155b), which again stems from the intervention of the modality between the subject and its properties; again under the assumption that the difference between movement and control makes no substantive difference to this effect.

(155)   Professor Branestawm had to read books

    a. ... it was the only explanation for his tremendous erudition (past epistemic)

    b. ... by royal edict (generic epistemic/root?)

It seems very clear, then, that non-finite clauses can encode modality despite the lack of any overt element contributing to such meaning, and moreover that under the analysis of modality and more generally clause structure presented here, particular kinds of non-finite clauses encode exactly the kinds of modality that they are predicted to.

The existence and behaviour of this covert modality in infinitives, then, backs up:

1. The idea that modality is responsible for the 'possible future' properties of Stowell's (1982) 'tensed' infinitives. We predict that modality should be available in these cases, and both futurity and possibility are uncontoversially modal notions; this being so, we don't need to make any appeal to tense to deal with these properties.

2. The idea that infinitives are phases with a (quantificational) CP layer at the top, *contra* at least some standard analyses.

3. The analysis of the root/epistemic distinction presented here that modals operate at the edge of phases, receiving a root reading when they operate over a non-propositional element, and an epistemic reading when they operate over a propositional element (which can come about either by temporal predication involving either tense or aspect, or by generic closure over V's situation variable).

4. The proposal that perfect/progressive aspects make the same temporal semantic contribution that T does.

5. The treatment of habitual/ILP readings of infinitives as deriving from GEN closure over the infinitive.

Chapter 7

Phases

Up to now, I have just been creating a theory of structure building and interpretation which attributes particular properties to elements corresponding more or less to those elements classed in recent minimalist literature as phases — and also to other elements with apparently the same properties, not usually taken to be phases (the 'aspectual phases' of chapters 5 and 6). This chapter looks at whether the elements I have been talking about and the elements more ususally regarded as phases should in fact be considered equivalent. I argue that they should, i.e. that the fundamental intuition behind the idea that phases exist is a good one, and the general characterization of them as CP and vP, as well as the properties attributed to them, is basically sound, but that the actual implementation of the intuition should be broadly as set out here.

The careful wording of the previous sentence should indicate that there are points at which the standard implemetation for phase theory and the one I am defending differ. This is indeed so; some of these differences will be clear already — such as the claim that all phases are topped by a CP; phases should be defined in terms of quantification, not propositionality (or whatever); I assume more phases than most people do — others are more subtle, and these are what I examine in this chapter.

7.1   Differences and reconciling them

Some of the differences between the view of phases adopted here and the standard view have been pointed up already earlier in the thesis.

One difference, as noted in §1.1.1, relates to expletives; as discussed there, expletives simply aren't an issue here as far as phases are concerned — they function in all relevant respects like any other arguments.

A second difference that falls out of this is the numeration/subarray: the standard theory defines phases (partly) in terms of the numeration/subarray, and this allows the facts about expletives from the previous section to be derived. However, as discussed above, if we reject the assumptions about expletives adopted by Chomsky and adopt those justified in §4.3.1, we don't actually need anything special to capture those facts. As far as I can tell, there is no other real evidence for the notion of a numeration:[1] it should then be possible to get rid of it. This would mean lexical access would be constant throughout the derivation. There doesn't seem to be any problem with this on general grounds, and it avoids the problem of lookahead vs. randomness mentioned in fn. 3 of chapter 1. I suspect this is the right route to take, but it is obviously difficult to determine, and I don't really think it matters substantively here; I leave it at that.

As far as dealing with the kinds of effects the numeration was intended to account for, there are alternative ways of going about it. For example, one thing the numeration was intended to do was ensure the derivation happened in small chunks — so that, say, we finished with the vP phase before we started on anything else. The same effect can be derived without a numeration, though, essentially by assuming that you keep building a phase till you've finished with it. There are a few ways of understanding this notion. One is that v has a certain number of features that need to be satisfied, and that once we have started working with v, we can't leave it till all those features have been satisfied. Once we do leave it and go on to the next head, we can't leave that one till all *its* features have been satisfied; and so on. This is in fact a pretty common assumption, tacitly or explicitly; it is basically Chomsky's (2000: 132) condition on lexical access (156), labelled the Locus Principle by Collins (2002).

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[1] I leave aside unlikely metaphorical arguments for the numeration (Chomsky 2000: 100), on the grounds that they are largely meaningless and entirely opposite points could be made simply by choosing different metaphors.

(156)   Properties of the probe/selector α must be satisfied before new elements
of the lexical subarray are accessed to drive further operations.

(Chomsky 2000: 132)

Rephrasing the locus principle in terms of lexical access rather than lexical sub-
array access would buy just the same effects in a way that is compatible with
the suggestion here that we don't have subarrays.

There are potential issues with such an account regarding my treatment of DPs
here, given that I assume they are built and left at a point where they still have
uninterpretable, i.e. unsatisfied, quantificational features; though in fact the same
problem arises more generally with case features on DPs.[2] An alternative might
be to define phases in terms of extended projections in the sense of Grimshaw
(2003), so that each lexical Root sprouts a CP extended projection in any case,
without worrying about selection. We could obtain pretty much the same result
with this, and leaving DPs with uninterpretable features needn't be a problem.

A third difference between standard phase theory and the theory I set up here
relates to cyclic movement through phase edges, and it has been mentioned briefly
in various places throughout the thesis. In the standard phase story, there is no
obvious way to deal with the intermediate steps of successive cyclic movement.
A couple of non-obvious ways of dealing with it have been suggested. Probably
the most commonly adopted is indirect feature driven movement (IFM), whereby
intermediate target positions are assigned 'extra' features that they wouldn't
otherwise need, in order to force/allow (depending how you look at it) more local
movement — so a non-interrogative C can be assigned a [WH] feature (of some
kind) just in order to allow a local step of *wh*-movement through its Spec.[3]

I assume a very different motivation for successive cyclicity, which is that there
are dedicated positions in the phase edge to which particular kinds of arguments
are related — so actually a phase edge is always going to have something like

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[2]Collins (2002: 47) asserts that Chomsky (2000) makes the dubious claim 'that the function
of Case is to make a goal active (not to act as a probe)', though I have been unable to locate
this claim in Chomsky (2000).

[3]Again, this raises questions of lookahead that seem similar to those raised by the notion
of the numeration — when exactly in the derivation is the 'extra' feature assigned? (this
also relates to inclusiveness; see discussion in the text below) — how does whatever does the
assigning know to do it? — etc.

the IFM story's optional [wh] feature, whether it is an interrogative-interpreted phase or not. Whether something moves to the relevant position is going to be dictated by a more general fact, which is whether the relevant head has an [EPP] feature to drive movement to its Spec. To a degree, this is essentially equivalent to Chomsky's more recent suggestion regarding IFM: rather than assigning a [wh] feature to an intermediate C, an [EPP] feature may be assigned. This allows [Spec, CP] to act as a general 'escape hatch' for movement of anything that needs to move, *wh-* or otherwise. The notion of [EPP] features here is something very general like 'here is a position to which you can dislocate' (Chomsky 2002: 115).

Obviously having a specific [wh]-related projection vs. a general escape hatch is different, and at some levels this relates to the issue of (im)proper movement raised in §5.4.1. Phase theory requires that as well as moving through interme-diate [Spec, CP]s, successive cyclic movement also goes through intermedieate [Spec, vP]s, i.e. phase edges generally. As well as C, then, v can also be as-signed an [EPP] feature to derive cyclicity. Optional [EPP] features are assigned according to something along the lines of the principle in (157).

(157)   The head H of phase Ph may be assigned an [EPP] feature

      (Chomsky 2000: 109)

According to Chomsky, '[(157)] yields A- or $\overline{\text{A}}$-movement depending whether the phase head has ϕ- or P-features [="Periphery"-features: topic, focus, etc.]. It might have both' (Chomsky 2000: 110). (158) is a case where a phase head has both types of feature.

(158)   [Spec, C$_2$] ... [Spec, v_2] ... [Spec, C$_1$] ... [Spec, v_1] ... XP

      (Chomsky 2000: 110)

If XP in (158) is a *wh-*object, say, it will pass through all four Specs, by means of IFM-related inserted [EPP] features. '[Spec, v_2] is an $\overline{\text{A}}$-position, by virtue of the P-feature associated with the extra Spec introduced by [(157)]; v_2 also had ϕ-features involved in object Case/agreement but these would have been deleted phase-internally before [(157)] assigns the extra Spec' (Chomsky 2000: 110). As a footnote to this, Chomsky points out that: 'On some assumptions, though not here, IFM passing through [Spec, v_2] is improper movement' (Chomsky 2000: fn.

53). One reason this isn't improper movement[4] may be that Chomsky is assuming that [Spec, v_2] doesn't count as an A-position by the time XP passes through it, because the ϕ features associated with it are deleted before its extra, P-related, i.e. A$'$ -related, [EPP] feature is assigned. While this assumption does mean we don't have improper movement here, it requires a particular timing on the application of (157) that is problematic for Chomsky. If the A-related features of v_2 are satisfied *before* v_2 is assigned an [EPP] feature by (157), as Chomsky explicitly states they must be in the quotation above, this is a strong violation of his inclusiveness condition (159).

(159)   The *Inclusiveness Condition*: No new features are introduced by C_{HL}.[5]

       (Chomsky 2000: 113)

To obey the inclusiveness condition, the optional extra [EPP] feature on v_2 would have to be added either at the point of selection or Merge of v_2, depending on where you take the inclusiveness condition to start applying. There is no way it could licitly be added after the A-related features of v_2 have been satisfied, since at that point C_{HL} is in full swing. Obviously this is problematic for Chomsky's account: either the inclusiveness condition needs to be modified/abandoned, or the timing of (157) needs to be modified so that it doesn't violate inclusiveness. Either of these things is likely to cause problems elsewhere in Chomsky's story; thankfully it isn't my job to fix or even identify these problems here, but in particular whatever we say seems likely to (further) blur the distinction between A- and A$'$ -positions, providing even less support for the notion of improper movement standardly defined.

Interestingly, Chomsky does seem to blur this distinction more elsewhere, when he says:

> IFM subdivides into types depending on the attracting head H *in the final stage* [my italics]: (a) A-movement when H has ϕ-features... or (b) $\overline{\text{A}}$-movement when H has *P-features* of the peripheral system (force, topic, focus, etc.)

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[4]For the purposes of this discussion, I am taking improper movement to be real, with Chomsky.

[5]C_{HL} = the computational procedure for human language: i.e. syntax, for all relevant purposes here.

(Chomsky 2000: 108).

Under this assumption, only the final step of IFM is relevant to deciding what type of movement it is. So the movement in (158) would count as A′ -movement, because it lands up in [Spec, CP]; the status of intermediate Specs is irrelevant. This is interesting because if we look at something like (160), which is basically equivalent to (158) but with XP a *wh*-subject rather than an object, it is very unclear for at least the intermediate [Spec, v] positions whether we would want to say they ought to be A or A′ , as discussed in §5.4.1, regarding (100).

(160)   [$_{CP}$ Who [$_{TP}$ t seems t to t be likely t to t happen t to t leave?[6]

It is therefore unclear whether we would want to say this was improper movement, standardly defined, either. However, if we define A- vs. A′ -movement solely in terms of where the moving element lands, without reference to any intermediate steps, as Chomsky suggests in the above quotation, there is absolutely no way of defining improper movement. It can't even be determined locally whether a step (intermediate or final) of movement is improper, if only the landing site and not the base position is relevant. If this is true, then it isn't surprising that the supposed cases of improper movement discussed in §5.4.1 can be dealt with elsewise.

It should also be noted that in Chomsky's system, there is no reason why IFM should behave the way it does — that is, there isn't any explanatory motivation for cyclicity. Certainly, it seems reasonable to tie cyclicity (perhaps locality more generally) in to the notion of the phase once we have that notion, but tying it in to the phase edge isn't so obvious. The reason Chomsky does this seems essentially to be that that's how cyclicity looks like it works, and so there must be a reason for it.[7] He therefore stipulates (161), the PHASE-IMPENETRABILITY CONDITION (PIC).

(161)   In phase α with head H, the domain [=basically c-command domain] of H
        is not accessible to operations outside α, only H and its edge [=specifer(s)]
        are accessible to such operations.

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[6]That we do have cyclic raising here, whether we want to say these are 'strong' phases in cases of raising or not, is shown by examples such as (115) in §5.5.4.

[7]Obviously this is a perfectly legitimate piece of reasoning, but it isn't a theoretical account.

(Chomsky 2000: 108)

To be sure, the PIC ensures that successive cyclic movement[8] has to be to/through the phase edge, since anything remaining in the domain of a phase is frozen in place once the derivation leaves that phase and starts building up the next phase of the derivation: as noted, though, it is a stipulation that doesn't follow theoretically from anything else in the system.

The question that should be asked is why edges are so important for cyclicity (since it does seem to be the case that they are). In standard phase theory, there isn't really any explanation for this beyond the PIC, which isn't an explanation at all. In the theory here, on the other hand, we have a more satisfactory explanation for cyclicity that does tie it in naturally to phase edges.

Phase edges here, I have defined as the quantificational CP layer that deals with variables introduced in the phase domain. If a *wh*-phrase needs to get out of the domain of a phase, it seems natural to move it through a position relating to *wh*-phrases — a specifier of Foc, in the structure I have given here. As far as this goes, it isn't really much different from Chomsky's claim, inasmuch as the edge of a phase is, for him, also a position relating to *wh*-phrases, though not in the same sense. However, we can tighten this up here — in standard IFM stories, this is just a last ditch movement to escape some (usually unspecifed) danger that would come about if the *wh*-phrase remained in the phase domain. Presumably the derivation would crash, but again if this is the motivation it seems to involve lookahead. In the system here, it could rather be seen as an attempt (albeit a vain one) to check a feature: we have a structural position relating to the interpretation of *wh*-phrases, and we have a *wh*-phrase, and it is plausible that the syntax is mechanical or 'dumb' enough in that situation to bring the two elements into the requisite configuration for *wh*-interpretation even if the Foc position isn't actually able to contribute to that interpretation in that specific instance, i.e. if this is an intermediate rather than a final step of *wh*-movement. We still have to say there is a basically optional [EPP] feature and a [*u*WH] on Foc, as in the standard IFM account, but we do have some degree of explanation for why those features should be there at all.

The system would deal with other cyclic raisings in essentially the same way, dragging the relevant elements up to the phase edge in order to try (maybe fail)

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[8]And all other dependency forming operations.

to interpret them, thus providing a similar motivation for 'QR'-like operations in general.[9]

7.2   Diagnosing phases

The standard definition of phases in the literature is intensionally that they are 'propositional' elements; extensionally that they are CP and vP, and maybe DP. On the face of it, the definition I give here, based on quantification, is very different. Propositions aren't obviously related to quantification, and neither are CP or vP.

Actually the relation among all these things can be elucidated though. Propositions, for example, need to be fully saturated to be evaluable, so if they contain variables then those variables need to be closed off fist, which is what I take the quantificational heads at the edge of phases to be doing. CP is standardly associated with *wh*-elements and focalization, both of which are strongly tied in to quantification. vP is commonly associated with ∃-closure, also obviously quantificational.

More tellingly, there are three major diagnostics for phases that have been put forward, investigated in detail by Legate (2003).

1. Phase edges are possible QR targets.

2. Phase edges are possible reconstruction sites.

3. Parasitic gaps are licensed by a *wh*-trace at the V phase edge (Nissenbaum 1998).

Legate details how each of these three properties ties in to the notion of the phase and the phase edge. It isn't particularly relevant to go through this here: what is relevant is that each of these diagnostics is fundamentally quantification-based. Plainly QR relates to quantification, whether we take QR to be motivated by type-mismatch (see Heim & Kratzer 1998 for a recent formalization of this

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[9]'The picture might be extended to incorporate QR, if alongside the ϕ- and P-features that drive movement, there are also QU-features, attracting quantifiers that pied-pipe an appropriate phrase' (Chomsky 2000: 109).

approach compatible with the general minimalist framework I assume), the desire to derive an alternative interpretation for a sentence than that derived without QR (cf. Reinhart 1995; Fox 2000), or, as here, feature-checking requirements (cf. Beghelli & Stowell 1997). It is natural in the system I am proposing that phase edges would provide the targets for 'QR', since the quantificational heads in phase edges deal with the quantificational interpretation of N phases, i.e. QPs.

Reconstruction is, informally, essentially 'backwards' QR, in the sense that quantified nominal elements receive a semantic interpretation elsewhere in the structure than where they superficially appear, only it is lower in the structure rather than higher. Again, phase edges are the expected sites for reconstruction in my system, since phase edges provide the interpretive sites for QPs.

On the assumption that parasitic gaps are interpreted via some covert *wh*-(like-)operator (Nissenbaum 1998), then again, we have a quantificational diagnostic. *Wh*-elements clearly relate strongly to quantification; they have been assimilated variously at least to existential quantifiers (Kartunnen 1977; Reinhart 1997), ambiguous existential/universal quantifiers (Hintikka 1976), definites (Jacobson 1995), and indefinites (Ginzburg & Sag 2000). Whichever of this array of options is correct,[10] that *wh*-phrases are quantificational in some way or another is a widespread assumption. Again, we expect this diagnostic to pick out phase edges in the system here, since [WH] features, like other quantification-related features, are instantiated in the phase edge.

Even leaving aside the considerations discussed in this thesis, then, defining phases in terms of quantificational domains makes sense given the standard phase diagnostics. One issue here is that I have claimed that more phases exist than are normally countenanced; in particular I have assumed 'aspectual' phases in §5.2. The status of aspectual elements in phase theory hasn't been investigated elsewhere, as far as I know: if mentioned at all, they are either taken to be a part of the V phase or a part of the T phase, without comment. More usually, they just aren't considered.

Where they are considered, it could be claimed that it is surprising that the phase diagnostics don't pick them out as phases. I don't think this is so. First, if you aren't testing for something like this, you aren't likely to come upon it; this is particularly so with the phase diagnostics, since they are such that the test cases

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[10]Of course, none of them may be 'correct' entirely.

have to be very carefully contrived indeed.[11] It isn't surprising, then, that aspectual phases haven't been proposed before if no-one has been looking for them. Additionally, the phase diagnostics outlined above relate entirely to quantification over nominal arguments. If quantification over nominals works as I have claimed here, then this is a somewhat indirect way of identifying phases, since in this system nominal elements are actually quantifed over by other elements, the clausal quantificational heads. The most these diagnostics generally give is is that a QP is interpreted somewhere in the clause relative to something else in the clause: its PF position and/or the position of other scope-bearing elements. Given this indirectness and relativity, it may not actually be clear exactly where in the clause a QP scopes: only that it isn't as high or as low as we might expect based on its surface position. If we only assume a V phase and a T phase, then higher scope or lower scope can be equated to one or other of these two phases; if we assume more phases in some cases however, the scope may not actually relate to either of these two phases, but it is quite possible we won't be able to tell that since all we are looking at is high/low scope relative to other elements, precisely how much higher or lower usually being immaterial.

To give a concrete instance, Legate uses Fox's (2000) adaptation of Lebeaux's (1988) diagnostic for *wh*-reconstruction. Lebaux's test relates to *wh*-cyclicity through CP; Fox's adaptation extends this to cover cyclicity through *v*P. In (162), the bracketed *wh*-phrase contains the pronoun *he*, bound by *every student*, and the R-expression *Mary*, which by Condition C has to be outside the c-command domain of the pronoun *her* (162a)/*she* (162b). The underlines indicate potential reconstruction sites. The lowest site (the base position of the *wh*-phrase) is in each case okay as far as the binding of the pronoun is concerned, but out as far as Condition C is concerned. In (162b), the higher site (by assumption, at the edge of *v*P) is also ruled out by Condition C, since it is c-commanded by *she*. In (162a), though, the higher site (at the edge of *v*P) is okay: it is c-commanded by *every student*, so the pronoun inside the *wh*-phrase can be bound, and it doesn't c-command *Mary*, so Condition C isn't an issue.

(162)   a. [Which of the papers that he$_i$ gave Mary$_j$] did every student$_i$ __√__ ask
              her$_j$ to read __*__ carefully?

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[11]They are, as has been said about something else, 'an excellent illustration for just how much sweat it takes to construct such arguments. They do not come your way on a leisurely walk' (Kratzer to appear a: chapter 1).

b. *[Which of the papers that he$_i$ gave Mary$_j$] did she$_j$ ___*___ ask every
student$_i$ to revise ___*___?
(Fox 2000)

The basic requirement on potential reconstruction sites in these kinds of cases,
then, is that 'the *wh*-phrase must reconstruct to a position below *every student*
and above *her/she*' (Legate 2003: 507). The most straightforward assumption in
the case of (162a) is that the relevant site that satisfies this requirement is at the
edge of *v*P. However, if we look at (163), it isn't so straightforward.

(163)   [Which of the papers that he$_i$ gave Mary$_j$] has every student$_i$ __√__ t$_{has}$
__√__ been __√__ asking her$_j$ to read ___*___ carefully?

Assuming that the *wh*-phrase moves cyclically through the aspectual projections
as well as through the edge of *v*P gives us additional potential reconstruction sites
that satisfy the binding requirements on the elements in the *wh*-phrase. There
isn't any obvious way to distinguish the three possible sites that we have in (163),
since all the test tells us about is the reconstruction position of the *wh*-phrase
relative to the QP and the pronoun. As discussed above, all we can say from this
is that the *wh*-phrase may reconstruct somewhere between these two elements,
but we *can't* tell exactly where, and if we increase the space between the elements,
we get less and less able to be precise.

The phase diagnostics, then, though functional, are pretty woolly. But if we
generalize them, so that quantificational effects more widely construed identify
phases, we get a diagnostic that is not only better defined, but applicable in
a wider range of cases. Here, I have taken quantificational effects to include
modal scope, since I analyse modality as quantification over possible situations,
as instantiated by the situation variables introduced inside phase domains. Modal
scope can therefore be used as a phase diagnostic. In chapter 6, particularly
§§6.2,6.3, I showed that modality is able to scope at the edge of the aspectual
phases, as well as at the edge of the V and T phases. This means there must
be quantification going on at these levels, which in the theory here means these
aspectual phases must have quantificational CPs at the edge, i.e. they must be
phases on a par with the V and T phases.

7.3   Spellout

As noted in §1.1, phases seem to serve two distinct roles, which I labelled syntactic phases and semantic phases. The former relate to the building up of the clause cyclically in chunks, the latter relate to interpretation, also taken to be cyclic. The notion of the phase I have set up in this thesis is more similar to syntactic phases: it relates largely to the syntactic derivation of the clause, although I do take this to be subject to interpretive requirements. Adopting standard assumptions, the straightforward extension of this would be that the same notion of the phase is relevant for the semantic phase, that spellout is cyclic, and that what is sent to spellout is the phase domain. However, as noted by Atkinson (2000) these assumptions are problematic even for the orthodox theory, and because of some explicit assumptions I have made about the interpretation of 'moved' XPs in §3.1.2.1, I can't follow them here.

The specific assumptions at stake are that (a) we have a 'copy' theory of movement whereby lexical items are Remerged rather than moved, (b) QPs host uninterpretable quantificational features $[u\text{F}]$, (c) every occurrence of an element in a chain (i.e. every trace of movement) is interpreted. The problem was discussed in chapter 5 with regard to example (101), repeated here in more detail as (164).

(164) $[_{\text{CP}}$ what$_{[u\text{WH},\text{ID:WH}]}$ Foc$_{[\text{WH},u\text{ID:WH},\Lambda]}$ do you $[_{\text{CP}}$ t Foc$_{[u\text{WH},u\text{ID:WH},\Lambda]}$ think $[_{\text{CP}}$ t Foc$_{[u\text{WH},u\text{ID:WH},\Lambda]}$ that Susan $[_{\text{CP}}$ t Foc$_{[u\text{WH},u\text{ID:WH},\Lambda]}$ told Arthur $[_{\text{CP}}$ t Foc$_{[u\text{WH},u\text{ID:WH},\Lambda]}$ that he should $[_{\text{CP}}$ t Foc$_{[u\text{WH},u\text{ID:WH},\Lambda]}$ do t $]]]]]]$?
where t $= \mathit{what}_{[u\text{WH},\text{ID:WH}]}$

(164) as it stands is a perfectly good structure, submissible to spellout unproblematically. However, at no earlier point in the derivation of (164) would this have been true: every CP hosts both a Foc head with $[u\text{WH},u\text{ID:WH}]$ and a copy of *what* with $[u\text{WH}]$. On a strict phasal cyclic spellout account, these $[u\text{F}]$s are going to give us problems: the lower phases can't be spelled out if they contain $[u\text{F}]$s, but they can't not be spelled out if spellout is so strictly phase cyclic. The derivation should thus be impossible under standard assumptions: 'an uninterpretable feature in the domain determines at the phase level that the derivation will crash' (Chomsky 2000: 108). Clearly this isn't so here, though.

One option to rescue this might be to say that the $[u\text{F}]$s on the intermediate *what*s and Focs not only Agree but also delete one another's features. However,

we would then have to have some way of ensuring that *what* could continue raising, when really if its [uF]s have deleted it ought to be inactive. Though it might be possible to set up the system this way — say with something like the 'feature lifespan' idea of Pesetsky & Torrego (2001) — it isn't very attractive and it would likely have ramifications elsewhere that would have to be carefully explored.

Another alternative might be that the features are actually interpretable, not uninterpretable. This is something like the line that Adger & Ramchand (to appear) take for *wh*-dependencies, with the further claim that in such a chain of [WH] features, only one is chosen to be interpreted at the interface otherwise we would have semantic incoherence. However, this seems essentially to be a filter on representations, something that doesn't really fit with the minimalist program, and it also seems that it is a filter on *final* representations, since the choice of which [WH] to interpret has to make reference to the whole array of possibilities, i.e. the whole structure. It is hard to see how this could be instantiated alongside any idea of cyclic spellout.

A third option is to say that spellout isn't, in fact, strictly cyclic. This is a plausible enough claim, and it has been suggested elsewhere. Felser (2004) comes to essentially this conclusion based on discussion of long distance *wh*-movement examples like (164) similar to the discussion above. She concludes that while phases make good candidates for spellout, we can't say that they actually do spellout in every case. Rather, we have to wait at least until we have a convergent phase, i.e. one with no [uF]s, before we can send anything to spellout. She labels the notion 'Selective spellout'.[12] She further posits that this idea only has to apply to spellout at the syntax–semantics interface; spellout at the syntax–phonology interface will be subject to different convergence requirements, suggesting that we have something corresponding to LF and PF spellout, something like a phase-relativized Y-model.

Svenonius (2000) also makes the claim that spellout shouldn't be construed of as strictly phase cyclic; his argument runs as follows. The usual assumption is that a phase (or its domain) is sent to spellout when the next phase head is Merged — so in standard terminology, the vP phase is sent to spellout when C

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[12]Cf. Chomsky (2001: fn. 51): 'the wh-feature of a trace is not valued until a higher phase ... That suggests that there might be a more abstract notion of phase, based on the concept of valuation of features rather than just size of the category'.

is Merged. Svenonius points out that this 'is not the minimal assumption. It would be more parsimonious to determine *locally* whether a phase should go to spellout, rather than having to wait until the next phase is built'. He therefore postulates a condition of IMPATIENCE (165).

(165)   Evaluate X as soon as possible

'As soon as possible', he takes to mean broadly when all X's [uF]s have been deleted, and it is otherwise a good candidate for spellout.[13] He labels the notion impatience because (in his system) it could allow, say, a vP phase to spellout as soon as it is built, rather than waiting till the next phase is built as in the standard theory. However, this label is a little misleading, since as should be clear it also will act to delay spellout *until* it is possible, which may not in fact be sooner than we would expect on the standard model; actually it might be substantially later, as in (164).

7.3.1   Single spellout

Both Felser and Svenonius make the assumption that though spellout may not be strictly phase cyclic, it is still more-or-less cyclic. Essentially, the claim that though not every phase will necessarily give us one cycle of spellout, every phase that is convergent will.

This isn't the only conclusion to be drawn from the arguments though. Though I won't go through this in great detail, if we can't have strictly phase-cyclic spellout, another possibility is that we don't have cyclic spellout at all, but rather single spellout as in earlier models; e.g. Chomsky (1995), or perhaps more saliently here Groat & O'Neil (1996). The Chomsky (1995) treatment is basically a traditional Y-model, where spellout sends the structure to PF at some point during the derivation, then the derivation proceeds to LF, where presumably, under current assumptions about interfaces, we would say that something like 'LF spellout' occurs. Groat & O'Neil's model is interesting in that it only has this latter LF spellout level: i.e. derivations universally proceed all the way to LF, then spellout;

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[13]So X might have all [uF]s deleted, but contain a stray affix, and so still not be a good PF candidate for spellout, or it might be an unsaturated function, and so not be a good LF candidate for spellout. Exactly how to formalize these so that whatever deals with spellout knows whether or not something is an acceptable candidate isn't clear.

PF is determined by which copy in a chain is pronounced, but the chains are the same chains submitted to the single LF-level spellout.

This account seems straightforwardly compatible with the data Felser discusses; it may seem less so with Svenonius's arguments, since recall that he argues for 'impatient' spellout on the grounds that 'It would be more parsimonious to determine *locally* whether a phase should go to spellout'. This is so, but only if we assume spellout is cyclic. If we don't have this assumption already, then it seems parsimonious also to have a single spellout model like Groat & O'Neil's, so that we don't have to determine anything during the computation. The reason Svenonius assumes cyclic spellout to be a good thing is to do with lessening computational load, as in §1.1.2: 'Juan Uriagereka (personal communication) has suggested that the demands of processing make plausible a constraint requiring that Spell-Out be accessed as frequently as possible, in order to reduce the amount of material in working memory' (Svenonius 2000). While this constraint may be plausible, it isn't obviously clear that it holds, as discussed by Matushansky (2003a) and in §1.1.2.

Additionally, it isn't necessarily the case that sending convergent phases to spellout would lessen the computational load in any case. Svenonius's claim is basically that a phase can go to spellout when all its [*u*F]s have been deleted. However, if all [*u*F]s have been deleted, then they shouldn't be visible to the syntax anyway. As far as syntactic computation is concerned, then, it isn't obvious that it should matter whether the deleted features stayed in the 'workspace' or got sent off to spellout: if the syntax can't see them, it isn't going to make its job any more difficult having them there than not.

7.4   Why have phases anyway?

I believe it is clear that syntax does involve the notion of phases, though not quite as standardly defined, and that they play a big role both in derivation and interpretation. There remains a deeper question, which is why syntax should work this way? This question becomes even more salient if we reject the idea that the purpose of phases is to reduce computational load, since this is often taken, if tacitly, to provide an explanation. Clearly here, phases allow us to deal with quantificational facts, but this alone could conceivably be dealt with in one swoop at the end of a derivation, rather than by chunking derivations into

smaller quantification related constituents. The question then becomes: why the chunking?

I have defined phases here syntactically as CPs, and the answer to this question that I suggest is related to the general meaning of C: complementizer. Originally, a complementizer was considered to be an additional layer on top of the standard clause that allowed clauses to act as complements, which they otherwise couldn't. I am dealing not specifically with complements but more generally with arguments here, but essentially this is the line I suggest should be followed: the CP layer on top of phases deals with closure of variables inside the phase domain, allowing phases to be (in principle[14]) referential. If they are (in principle) referential, then the predicative elements at the core of higher phases can make reference to them, i.e. select them as arguments. Phases, then, allow predicates to have arguments.

I discussed the phase structure of tense in these terms in chapter 5: we need the CP over vP to close off the situation variable introduced by v. If we didn't do this, vP would justbe a predicate over situations, and since predicates aren't referential there is no way the temporal predicate T could refer to it. The situation variable introduced in t needs closing off for the same reason. If we didn't have something like these CP levels, there would be no way for the syntax to do anything except build up a lot of predicates. This is not very useful. Putting a CP on top allows us to close off these predicates so the syntax can continue.

This kind of idea has been explored before, on similar grounds, notably by Koopman & Sportiche (1991); Collins (2002). Koopman & Sportiche state the condition: 'No category takes as a complement a syntactic category corresponding to a non-saturated predicate' (Koopman & Sportiche 1991: 215), which we can modify here as: 'no predicate takes as an argument a syntactic category corresponding to a non-saturated predicate'.

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[14]In principle because I have assumed that, say, N phases are not in fact interpreted referentially in themsleves, but are given a reference by Agreeing extra-phasal elements. However, as far as the syntax is concerned, this is plausible enough: the (un)interpretability of quantificational features in CP layers is more of an issue at the interface, where we actually get to the point of interpretation, than in narrow syntax.

Chapter 8

Outro

This dissertation has defended two major interlinked claims: one, that the clause structure includes several layers of hierarchically ordered quantificational CP heads; two, that these layers define chunks of the clause corresponding to what are termed in current minimalist literature phases. Phases, then, are to be generally represented as schematized in (166), where H is some property denoting Root category, h its associated little head(s) as little v over V, and C a layer of quantification encoding heads.

(166)

```
        CP
       /\
      C   hP
         /\
        h   HP
            /\
           ...
```
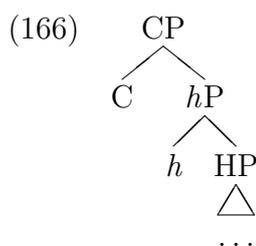
One immediate outcome of the linking of these two assumptions in this way is that the basic parallelisms between the two standardly assumed phases, vP and CP, are tightened up greatly, to the extent that they are in fact taken to be the same syntactic category, CP. In itself, this is a desirable result. Taking into account the well discussed parallelism between CPs and DPs, we can further reduce our system by defining DPs, too, in these terms, so that the standard vP phase becomes a V Root topped off by a CP, the standard CP phase becomes a T Root topped off by a CP, and DP becomes an N Root topped off by a CP, and thus automatically also a phase, as has been frequently suggested (e.g. Chomsky 1999).

A second important factor of phase theory that the version defended here derives straightforwardly is the splitting of phases into two distinct fields, labelled in the literature the edge and the domain. The domain is the core, lower part of a phase, dealing with selction, predication, etc. The edge is the higher, peripheral part, which in standard theory functions pretty much as a space for (cyclic) movement. In orthodox terms, the edge of the CP phase is C and its Spec(s). the edge of the vP phase is v and its Spec(s); again, the story presented here tightens this up considerably, so that the edge of any phase is just the CP layer. I argue that the CP edge and the lower parts of phases do fundamentally different things: the lower, domain, parts of phases I take to deal with situation and argument structure and predication. The CP edge deals with quantificational closure over elements introduced in the domain.

In the standard theory, the distinction between the edge and the domain is ill-defined and stipulative, but nonetheless it seems to be real; in the theory presented here the distinction is derived directly from the syntactic and semantic contributions each part makes, and thus precisely defined in terms of those contributions. Furthermore, the effects attributed to domains and edges in the orthodox framework remain in the system set out here: the domain deals with basically the same kind of phenomena, the edge functions as a space for (cyclic) movement because it provides specific positions for elements to move to in order to (attempt to) check features. Cyclicity effects commonly attributed to the Phase Impenetrability Condition of Chomsky (2000) are then derived without the need for this fundamentally stipulative condition. Defining phases in terms of quantification related edges also explains why the major diagnostics for (movement through) phase edges relate to quantificational factors.

Evidence for the general claim that phases exist and are best defined in these quantificational terms has been presented from several different areas: chapter 3 looked at the interpretation of N phases, i.e. DPs/QPs, and showed that an extension of Heim's (1982) well known ∃-closure mechanism, whereby every QP introduces a restricted variable subject to closure by intra-clausal quantificational operators, is elegantly derivable from the system presented.

Chapter 4 introduced some assumptions about the structure and interpretation of phase domains, which allowed a system for representing temporal information, broadly similar in its intuitions to that of Stowell (1996), to be set up in chapter 5. This system too provided evidence that phase edges, as conceived of here,

provide quantificational information relating to variables in the phase domain. A prediction was made with regard to aspectual elements, which was that they too should introduce quantificational CP layers, i.e. additional phases.

Evidence that this prediction is correct was adduced in chapter 6, where it was demonstrated that the scopal and interpretive behaviour of modality, defined in terms of quantification over possible situations (Portner 1992; cf. Kratzer 1977 *et seq*) also backs up the basic idea being defended. Modals were shown to have two scope positions in a simple clause: one corresponding to the CP edge of the V phase, the other to the CP edge of the T phase. Moreover, these two scope positions were shown to correspond to, respectively, root and epistemic interpretations for the modality, and it was demonstrated that this fact falls straight out of the assumptions made. This in place, it was then shown that the system as set up provides some very precise predictions concerning the interactions of modal and aspectual elements: specifically that if aspectual elements introduce additional phases, they then also introduce additional scope positions for modality at their phase edges. Data adapted from work of Condoravdi (2001) and Stowell (2004) showed that this is indeed the case, and that the modality in these circumstances receives exactly the interpretation we would expect it to, providing very strong support for the claims made.

The system being by this point coherently set up, chapter 7 went back to the general notion of phases and compared standard phase theory with the reinterpretation I provide. It was shown that the system presented here captures the major facets of the standard theory, but in a less stipulative and more elegant, and precise, way.

Chapter 8 was a conclusion.

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And I for my part will never lend myself to such a perversion (of the truth), until such a time as I am compelled or find it convenient to do so.

— Samuel Beckett — *Molloy* —

It is hardly necessary to observe that all of this is highly unlikely. There is substantial empirical evidence supporting the opposite conclusion at every point.

— Noam Chomsky — *Minimalist Inquiries* —