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THE (DIS)ORGANIZATION OF THE GRAMMAR: 25 YEARS

There is no doubt that the 25 years since the launching of *Linguistics and Philosophy* have witnessed an explosion in our understanding of linguistic semantics. There is, however, one area in which we have arguably made little progress – indeed I wish to suggest here that we have perhaps gone backwards. And this concerns the fundamental question of the overall organization and architecture of the grammar – in particular, how the systems of syntax and semantics work (or don't work) together. My purpose in this piece is not to provide detailed empirical arguments for or against any particular conception of this (although I will not try to hide what I believe – or at least hope – is correct). Rather, my purpose is to make the point that acceptance of a complex view *does* need to be argued for if a simpler view is available.

Over the last twenty five years, the field has moved from a state where the majority of researchers in semantics worked in a paradigm which embraced a relatively simple overall organization of the grammar, to a state where many practitioners now adopt a far more complex view. Of course there is nothing wrong with such a shift *if* it is motivated by some new discovery. But it seems to me that this shift was not precipated by any kind of discovery: the change in fashion seems to have happened largely without discussion. Connected with this shift has been a trend away from writing explicit 'fragments'. Thankfully, the standards in semantics still require explicit formalization of the semantic side of things, but exactly how one arrives at the structures which are assigned an interpretation is often left inexplicit. This practice provides a ready way to obscure complexities which arise from the increasingly popular 'modern' conception of the organization of the grammar.

What do I mean by the 'modern' conception of the grammar, and is it really fair to say that it embraces new complexities? I will spell this out more explicitly below, but here I will make a few points informally. My first point has to do with the notion of Logical Form; it has almost become axiomatic in much recent work that there is a distinct level of logical form

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which inputs the rules supplying a model-theoretic interpretation – as opposed to having the model-theoretic interpretation being directly supplied as syntactic expressions are 'built'. One can find paper after paper in the major semantics journals and conferences during the last ten or fifteen years which simply assume without argument that the input to the model-theoretic interpretation is the level of LF, and papers which even seem to take pride in LF as one of the great 'discoveries' of modern semantics. But why should we be proud of 'discovering' that we need *more* apparatus than we had once thought?

The objection to LF is not that it necessitates an additional 'level' – for a level is nothing more than the by-product of the rule system, and so it is the nature of the rule system which is (or should be) of primary interest. But indeed this view does entail a more complex rule system; the claim that there is a level of LF (distinct from surface structures) necessitates an additional set of rules mapping between surface structures and LFs. Moreover, even if there is good evidence for a level of LF distinct from surface structures, there remain various ways to conceive of the organization of the grammar, and what I am calling the 'modern' solution is surely one of the more complex ways that has ever been dreamed up. One complexity here has to do with the 'direction' of the mapping – do the rules map from surface structures to LFs or (as in Generative Semantics) vice-versa? It might seem to make little difference - but I will argue below that indeed it does make a difference. As will be detailed later, the Generative-Semantics-style solution is *a priori* simpler in that it allows the compositional syntactic rules (i.e., phrase structure rules or their equivalents) to be stated in tandem with the compositional semantic rules – and this in turn simplifies the statement of the latter. Aside from this issue, many of the researchers working within the 'modern' paradigm assume not just one additional set of rules (the mapping from surface structures to LFs) but of course also transformational rules mapping deep (or, if you prefer, 'D') structures to surface (or, if you prefer, 'S')-structures. There has even been considerable research embracing a model where things start out in one place, move, and then get put back by 'reconstruction' for the purpose of the semantic interpretation. Could anyone look at such a model seriously and not suspect that something is being missed?[1]

[1] I realize that these ideas have been to a certain extent recast in the Minimalist Program. I will, however, not deal the overall view of the organization of the grammar in the Minimalist Program since this has not (yet) had any major effect on the semantics literature. (Nor is it clear that the revisions proposed in this program will in any way represent a simplification with respect to the issues to be addressed below.) The one piece of Minimalist Program machinery which is relevant to some later remarks concerns the recasting of reconstruction as copy movement, and so I will turn to this briefly in fn. 7.

1. Some Competing Theories and Their Treatment of Quantifier Scope Ambiguities

To make these remarks more concrete, it will be useful to consider four competing theories of the syntax-semantics interaction, illustrating each with a discussion of quantifier scopes ambiguity, as in (1):

(1) Some man read every book.

A. *Strong Direct Compositionality*

anaphora; and agreement would be hard to avoid

Under this view, there is a set of syntactic rules which prove the well-formedness of the set of sentences (or other expressions) in the language. Put informally, these rules 'build' linguistic expressions, generally 'building' (i.e., specifying the well-formedness of) larger expressions in terms of the well-formedness of smaller subexpressions. Assume here that each such rule is a context-free phrase structure rule (or, highly generalized rule schema). Note that if this is the only form of syntactic rule in the grammar then the grammar need keep no 'track' of structure: the rules 'building' complex expressions merely concatenate strings. (Hence a tree is just a convenient representation of how the grammar worked to prove a string well-formed; it is not something that the grammar can nor ever would need to 'see'.) Coupled with each syntactic rule is a semantic rule specifying how the meaning of the larger expression is derived from the meaning of the smaller expressions.

Notice that Strong Direct Compositionality is not necessarily committed to the view that the grammar consists of a list of phrase structure rules, each of which is idiosyncratically associated with a semantic rule. Such a view has often come under fire for containing 'construction specific' semantic rules. But the issue of construction-specific rules is independent of the other issues that I am concerned with here, and nothing in the strong direct compositional view requires construction-specific rules. One can maintain that the actual semantic operations are predictable from each syntactic rule. In fact, one can go much further and assume (as in most versions of Categorial Grammar) that the syntax itself consists of just a few very general rule schemata (each of which is associated with a general semantic rule schema). A related issue concerns the use of type-shift rules: many researchers within the tradition of Strong Direct compositionality have also advocated the existence of unary 'type-shifting' rules which map single linguistic expressions into new ones, and in so doing change the meaning and/or category of an expression. There is disagreement in the literature as to just how many such operations there are and how generally they should be stated; again, though, this question is logically independent of the question of the overall organization of the grammar. (Note too that any theory allowing for silent operators is not very different from one allowing type-shift rules: a type-shift operation can always be recast as a silent lexical item which applies to the expression with which it combines.)

There are some very appealing properties of Strong Direct Compositionality. One is the fact that in building strings, the syntax need keep no track of structure, since all combinatory operations simply concatenate strings, and all unary rules have no effect on the internal structure. We can think of each linguistic expression as a triple of ⟨phonology; syntactic category; meaning⟩, where the rules take one or more such triples as input and give back a triple as output.

How are quantifier scope ambiguities handled under strong direct compositionality? As with most things, there is more than one proposal in the literature. The most influential proposal during the late 70's and 80's was probably that of Cooper (1975) (the 'Cooper Storage' proposal), which was designed to keep the syntax simple but relied on an enriched view of the meaning of an expression (allowing meaning to be a tuple of model-theoretic objects). Despite the historical prominence of this proposal, I will say nothing more about it here as its view of the semantics makes it more difficult for the purposes of cross-theoretical comparison. But there are other proposals for quantifier scopes within strong direct compositionality; in general these involve type-shift rules. One well-known proposal is developed in Hendriks (1993) it is a generalization of ideas in Partee and Rooth (1983). Here, a transitive verb like *read* is listed in the lexicon with a meaning of type $\langle e, \langle e, t\rangle\rangle$, but there is a generalized type-shift rule allowing any e-argument position to lift to an $\langle\langle e, t\rangle, t\rangle$ argument position. If the subject position lifts first and then the object position lifts, the result is the wide-scope reading on the object. Further generalizations of this can be used to allow for wide scope of embedded quantifiers; other proposals for wide scope embedded material makes use of a combination of type-shift rules and function composition. (For another kind of proposal, see Barker, 2001.)

B. Weak(er) Direct Compositionality

The above picture has often been enriched (and hence, weakened) by the adoption of two related revisions: (a) the combinatory syntactic rules are not all equivalent to context-free phrase structure rules but may perform some other operations, and (b) the syntactic rules do not build only completely unstructured strings but may build objects with more 'structural' nano syntax

information.[2] One can imagine various other versions – including one in which certain transformation-like operations can be performed in the 'building' of syntactic structures. One might, for example, imagine that the output of each syntactic operation is a tree rather than a string (see, e.g., Partee, 1976); hence linguistic expressions are now richer, and can be thought of as triples of the form ⟨phonological representation; syntactic structure – i.e., a full tree; meaning⟩. It seems to me that something more or less like this picture was assumed in a lot of the Montague-grammar-style work done in the mid and late 1970's.

Montague's 'Quantifying-In' rule – and various conceivable variants of it – is compatible with this basic view of the organization of the grammar. Since my concern is not with Montague's particular proposal but with this general picture of the architecture of the grammar, I will recast the Quantifying-In rule(s) so as to provide a maximal basis for comparison with other theories. Thus assume (contra Montague, 1974) that a transitive verb like *read* has a lexical meaning of type $\langle e, \langle e, t\rangle\rangle$. Assume further the usual theory of variables, and assume (along the lines of Montague's treatment) that we build syntactic representations with indexed pronouns, each of which corresponds to the samely-indexed variable in the semantics. We can have expressions like he_1 *reads* he_2 whose meaning – relative to some assignment function g – will be $[[reads]]^g\ ([[x_2]]^g)([[x_1]]^g)$. In addition, though not strictly necessary, we will let the syntax keep track of the indices on the unbound pronouns . To this end, assume that every node label is enriched with an IND feature, whose value is a set of indices, and that – unless a rule specifies otherwise – the IND value on the category which is the output of a combinatory rule is the union of the IND values of expression on the input. Thus the category of the expression given above is

[2] The mildest weakening of A is to be found, perhaps, in those proposals that add only Wrap operations in addition to concatenation operations (for the original Wrap proposal, see Bach, 1979, 1980). Here the combinatory syntactic operations allow two strings not only to concatenate, but also for one to be infixed into another. As such, the input to the combinatory operations has to be not just unstructured strings, for these strings need to contain at least enough additional information so as to define the infixation point. This has been formalized in a variety of ways; I will not pursue this here, although it is worth noting that Montague's Quantifying-In rule can be recast as an infixation operation, and so Weak Direct Compositional systems with infixation operations are one way to account for quantifier scopes.

S [IND: $\{i, j\}$]. We can thus accomplish Quantifying-In by the following two rules, the first of which is a type-shift rule:

(2) Let α be an expression of the form $\langle[\alpha]$; S [IND: X where $i \varepsilon X$]; $[[\alpha]]\rangle$. Then there is an expression β of the form $\langle[\alpha]$; Λ [IND: $X - i$]; $[[\beta]]^g$ is that function which assigns to each individual a in D, $[[\alpha]]^{g[a/x(i)]}\rangle$ (this of course is just the semantics of λ-abstraction).

(3) Let α be an expression of the form $\langle[x, he_i\, y]$; Λ, $[[\alpha]]\rangle$ and β be an expression of the form $\langle[\beta]$; DP; $[[\beta]]\rangle$. Then there is an expression γ of the form: $\langle[x$ DP $y]$; S; $[[\gamma]]^g = [[\beta']]^g([[\alpha]]^g)\rangle$.

One could also build weak crossover into this picture: Montague's rule itself required that if there is more than one pronoun with the same index, the substitution could apply only to the leftmost one. Should one want to take the more usual modern view that the appropriate restriction is stated in terms of c-command rather than linear order (cf., Reinhart, 1983) the rule can be restricted so that he_i in the above SD must c-command all other occurrences of the same indexed pronoun (such a restriction, of course, commits to the view that the input to these rules are as rich as trees).

C. Deep Compositionality

By this I mean something like the model proposed in Generative Semantics (see, e.g., Bach, 1968; McCawley, 1970; Lakoff, 1971) supplemented with apparatus to supply a model-theoretic interpretation to the Logical Forms. Thus, Generative Semantics assumed that deep structure was the same as Logical Form – which means that a series of phrase structure rules and/or rule schemata serve to define a well-formed structure. This was supposed to 'represent' the semantics, and in fact much work within Generative Semantics didn't worry about supplying an actual interpretation to these structures. (For that matter, this is equally true of some of the initial work within the 'modern' surface structure to LF view; see, e.g., Chomsky (1976).) But it is easy enough to embed the general idea into a more sophisticated theory of semantics with a model-theoretic component simply by having the 'building' and interpretation of the Logical Forms be as in the Strong Direct compositional approach: each local tree is specified as well-formed by the syntactic rules and – in tandem – is provided a model-theoretic interpretation by the semantic part of the rules. A key difference between this and Strong Direct Compositionality is that this view contains an additional set of transformational rules which map the Logical Forms to surface structures. A concomitant difference is that the rules 'building' syntactic structures must keep track of whatever structure is used as the input to the transformational rules; presumably then these rules are building trees rather than strings. Again, though the base rules can be seen as mappings from triples to triples.

The treatment of quantifier scope within this general view is well-known. First, unlike the Quantifying-In rule above, we have an actual level of representation at which quantified NPs are in the tree, but are in a raised position rather than being in their ultimate surface positions. The difference between their deep and surface positions is handled by a quantifier lowering rule. If we take the lexical meaning of a transitive verb like *read* to be of type $\langle e, \langle e.t\rangle\rangle$, then the appearance of a quantified NP in object position will always be the result of Quantifier Lowering. Scope ambiguities are handled in the obvious way: since each local tree is interpreted as it is 'built' by the phrase structure rules, the obvious formulation of the semantic rules will assign different scopes according to the initial height of the quantified NPs.

To elaborate, suppose the rules build deep structure expressions such he_1 read he_2 as in the Weak Direct Compositional approach shown above. Assume further that this is assigned the meaning and category as above. Further, assume the following two phrase-structure rule/semantic rule pairs; these mirror the rules in (2)–(3):

(4) Λ [IND: $X - i$] $\rightarrow$ S [IND: X, where $i \varepsilon X$]; $[[\Lambda]]^g$ assigns to each individual a in D $[[S]]^{g[a/x(i)]}$

(5) $S \Rightarrow \text{DP } \Lambda$; $[[S]]^g = [[\text{DP}]]^g([[\Lambda]]^g)$

Finally, this is supplemented by one transformation, as follows:

(6) $[_S \text{ DP } [_\Lambda A\ he_i B]] \Rightarrow [_S A \text{ DP } B]$.

(Again, one can build in Weak Crossover by restricting the rule so that the occurrence he_i which is analyzed as meeting the SD of the rule is leftmost and/or highest occurrence of he_i (see, e.g., Jacobson, 1977).)

D. Surface to LF

Which brings us to the increasingly popular 'modern' view. This is the view that there is a level of LF which receives a model-theoretic interpretation and which is derived from surface structures by some set of rules. There are actually two possible versions of this. One is that the surface structures are given directly by the compositional syntactic rules (i.e., the phrase structure rules or their equivalents) and these are then mapped into LFs. The second, and more standard view, is that the surface structures themselves are the end-product of a mapping from underlying structures. In terms of the treatment of quantifier scopes this makes little difference; but it does make a difference when we turn to *wh*-questions. The actual proposals cast within D generally do presuppose the existence of transformational operations in the syntax – this is because many of the arguments for D rely on similarities between the 'overt' transformational operations (mapping from deep to surface structures) and 'covert' operations (mapping from surface to LF).

The treatment here of scope ambiguities is also well-known; it is essentially the same as that given above under C, except that the direction is reversed. We first start out with a series of rules which ultimately define a well-formed surface structure at which the quantified material is *in situ*. Then there are rules mapping this into a structure like the Generative Semantics deep structure, and then the compositional semantic rules will work from the bottom up to interpret this.

Thus assume again that the lexical meaning of *read* is of type $\langle e, \langle e, t\rangle\rangle$, and assume a set of phrase structure rules which allow DPs like *every book* to appear in characteristic 'argument' positions. (No syntactic transformational operations of relevance apply in this case.) We thus build a structure like *some man read every book* in the syntax, but initially with no interpretation. The interpretive part can be accomplished by a combination of one transformation-like rule – Quantifier Raising (May, 1977) which is essentially the inverse of the Quantifier Lowering rule in (6)) – and two rules interpreting the relevant structures. The relevant rules are given in (7) – (9). (Incidentally, in the formulation that I give here in (7), QR is not the exact inverse of (6) – this is simply because I formulated this to be more or less in keeping with the treatment given in Heim and Kratzer (1998). But one could formulate the rules to be exact inverses.)

(7) $[_{S}\ A\ \ DP_{i}\ B] \Rightarrow [_{S}\ DP\ [_{\Lambda i}[_{S}\ A\ \ t_{i}\ B]]]$

The lowest S will be interpreted by the kind of interpretation rules that anyone needs; note that t_i here is interpreted as x_i. The additional rules of interpretation are pretty much just the inverse of what we have already seen:

(8) $[[\ \ [_{\Lambda i}\ \ S]\ \ ]]^{g}$ is that function which assigns to each individual a in D the value $[[S]]^{g[a/x(i)]}$

(9) $[[\ \ [_{S}DP\ \ \Lambda]\ \ ]]^{g} = [[DP]]^{g}\ ([[\Lambda]])^{g}$

2. Why Each View Is Weaker Than the One Before It

All other things being equal, it seems obvious that each position in this list represents a more complex view of the organization of the grammar than does the position above it. But I want to focus on the fact that there is a major cut between A–C on the one hand and D on the other. A, B, and C all have in common the fact that the syntax 'builds' in conjunction with the model-theoretic interpretation, and this is discarded in D. This one move is problematic: new complications arise the minute one moves away from the 'running in tandem' of the syntax and semantics.

The first objection to the divorcing of the syntax and the semantics might be purely subjective (although I don't really think it is). This is that there is a clear elegance to a system in which the grammar builds (i.e., proves as well-formed) syntactic objects in parallel with assigning them an interpretation, an elegance which is lost if the grammar contains two entirely separate systems, one of which (the syntax) must 'run' first because the other (the semantics) works on its output. But elegance aside, there are two other questionable aspects to divorcing the two combinatory systems. The first is that under the conception in D there is no explanation as to *why* these systems work on such similar objects, and the second (related) problem is that D requires a duplication of information not required in A–C.

Regardless of the question of whether there are transformations in addition to phrase-structure rules (or rule schemata), just about all theories agree on something like phrase-structure operations, each of which specifies the well-formedness (at some level) of a *local tree*.[3] Thus at the end of the day, all theories contain rules which can be seen as proofs of the well-formedness of some kind (or level) of structure, where these 'proofs' work bottom up in that they specify the well-formedness of larger expressions (at some level of representation) in terms of the well-formedness of smaller ones. Moreover, semantic theories generally agree that the semantics also works on *local trees* to give the meaning of the mother in terms of the meaning of the daughters. And, like the syntax, it also must work 'bottom up' – supplying the meaning of larger expressions from the meanings of the expressions which compose them. Given this, it would seem to be surprising to find that the two systems don't work in tandem. If

[3] As noted above, under strong Direct Compositionality, trees are not actually anything the grammar ever needs or gets to see, so under this view a tree is really a metaphor. Under Weak Compositionality this is also not quite the appropriate terminology. If the syntax allows operations like Wrap, then a 'tree' may also be not the right formal object to represent what the syntax does. However, I think that this terminology will do no harm here.

the semantics is divorced from the syntactic combinatory rules, then why *should* it be the case that it too works on local trees? Why *not* find rules taking large chunks of trees and providing an interpretation for these?[4] The fact that the syntax and the semantics work on similar objects is a complete mystery under the view that they are divorced from each other.

Moreover, there is a clear cost to the move of divorcing the syntactic combinatory rules from the semantic rules. The point is easiest to illustrate by a comparison of theory C to theory D, since these are otherwise most alike. Both theories contain an additional set of rules effecting a mapping between surface structure and LF; they disagree on the direction of the mapping. Crucially, in theory D, the semantic combinatory rules cannot be stated in tandem with the phrase structure rules (or their equivalents), and this means that the syntactic side of things must be stated twice: once as output of the syntax, and once as input to the semantics. As illustration, take a case where there happens to be no rule mapping between surface structure and LF (the two are thus the same). So consider the syntactic composition and semantic interpretation of a very simple case like *John walks*. Here is a 'fragment' in C to do this, and a 'fragment' in D:

(10) C. S $\rightarrow$ NP VP; $[[S]]^g = [[VP]]^g([[NP]]^g)$
D. syntactic rule: S $\rightarrow$ NP VP
semantic rule: $[[\ [_S \text{ NP VP}]\ ]]^g = [[VP]]^g([[NP]]^g)$

Reference to the local tree $[_S$ NP VP] is required twice in D but only once in C. The same point can be made by a comparison of the C-fragment given in (4)–(6) to the D-fragment given in (7)–(9). In the D-fragment, the two rules (8) and (9) repeat large parts of the output of the transformational rule (7), which creates the appropriate structure to serve as their input.

I can almost hear the voice of the skeptical reader here: doesn't this objection disappear once one moves away from 'construction-specific' statement of the semantic rules? Actually, it doesn't: restating the semantic rules as more general schemata certainly ameliorates the situation, but it does not eliminate the problem altogether. Regardless of how general one makes these, one still needs semantic combinatory statements which provide interpretations for classes of local trees. Hence the semantic rules

[4] I am oversimplifying, in that I am not sure we have any actual evidence that the semantics *does* necessarily work on local trees – this is pretty much just what everyone assumes (and it is, of course, an assumption which is forced under the view that the syntax builds in tandem with the semantics interpreting). Maybe a fairer way to state the situation, then, would be to say that no evidence has been found to suggest that the semantics *requires* bigger objects as its input, but if the syntax and the semantics were really divorced then there'd be no reason not to find such cases.

still need to refer to a set of local trees – even if in highly generalized forms – both in the input to the semantics and as output to the syntax. The fewer the rules, the less duplication there will be, but there will still be some, and this remains suspicious if there is an alternative theory which avoids this. And, in fact, a theory which states the rules together can avoid this.[5]

The only way to show that D avoids unwanted duplication would be to show that the syntactic rules and the semantic rules *should* actually be stated in terms of very different kinds of objects. This, for example, would be the case if the semantic rules interpreted chunks of non-local trees. Or, this would be the case if the semantic rules looked only at linear strings and not at syntactic structures. But, as mentioned above, no theory seems to maintain this and no one (to my knowledge) has found evidence that we need rules of this type. The claim that the rules operate on different objects could also be substantiated if one could show that the semantic rules took as their input a much larger or more general set of local trees than the syntactic rules give as output. If one could really make such a case, then divorcing the output of the syntax from the input to the semantics would be exactly the right move, but to my knowledge, there have been no serious arguments to this effect (I return to this in Section 4).

[5] To be fair, the statement of syntactic and semantic rules in classical Montague grammar also contained a certain amount of duplication. While these were assumed to be paired, they were usually stated within a notational format where a schematized version of the output of the syntactic rule was given as the input to the semantic rule. But this was really a notational oddity: the general program of having the syntactic and the semantic combinatorics linked does not *require* this notational awkwardness, while this duplication is essential in a theory like D.

But there is one important caveat here: a theory with direct compositionality and/or with deep compositionality will find itself in the same 'pickle' if it contains a number of specific syntactic rules and general semantic rules stated separately. Such, for example, was the case in some versions of early 'type-driven' GPSG, where the syntax contained various phrase structure rule schemata combined with a general principle as follows: for any rule $A \rightarrow B\ C$, if the semantic type of B is a function from C-type things to A-type things then the associated semantic rule will be functional application. Such a theory also contains duplication. (Note though that even this kind of theory is not vulnerable to the other criticism of D, which was it is an accident that the output of the syntax is the same general type of objects as is the input to the semantics.) But while there are versions of A–C which suffer from the problem of duplicating information, there are also versions (e.g., some versions of Categorial Grammar) where the syntactic schemata can be stated in forms as general as the semantic combinatory schemata, avoiding this.

3. Arguments for D: The Conventional Wisdom

Why, then, has D become increasingly popular? 25 years ago, most researchers within formal semantics were committed to exploring the feasibility of either Strong or Weak Direct Compositionality – and it was taken for granted that direct model-theoretic interpretation (i.e., no 'LF') combined with having the syntax build while the semantics interprets in tandem was an attractive view of things. Given that the model in D is more complex than other known alternatives, it should have been adopted only as a position of last resort.

To be fair, there are many arguments which have been given either implicitly or explicitly against A – Strong Direct Compositionality. I think that many of these arguments can be, and in fact have been answered. My own hope is that A (weakened perhaps with the adoption of Wrap operations) will turn out to be correct. There is a large body of work – especially within Categorial Grammar and/or the Type Logical tradition – trying to show that strong (or slightly weaker) direct compositionality can be maintained. I myself have argued in various papers that much of the apparent evidence for D vs. (a slightly weakened) A hinges on some mistaken notions about how binding works (see, e.g., Jacobson, 1999, 2000). But my purpose here is not to survey those arguments. My focus here is on the fact that the explicit or implicit arguments against A have been construed as having much further reaching implications than they actually do: they have been construed as evidence for the model in D. The intermediate positions are rarely discussed anymore.

It is worth considering a couple of the more standard arguments that one finds in the literature for D (though a full survey remains well beyond the bounds of this paper). Since many of these hinge on parallels between *wh*-movement constructions and quantifier scope, we will illustrate with the case of *wh*-questions. The common wisdom assumes that *wh*-phrases in questions have something in common with quantified NPs; we will take as our point of departure the analysis in Karttunen (1977) whereby these are in fact generalized quantifiers (with the meaning of existentially quantified NPs). Notice that this analysis divorced the question semantics from the quantificational aspect of *wh*-phrases. In English, *wh*-phrases occur only in *wh*-constructions and do not occur as normal indefinite NPs; this was accounted for in Karttunen's analysis in the syntax. A *wh*-phrase could be introduced only into expressions of a particular syntactic category (Karttunen's P_Q); these contain an abstract question operator and are interpreted as sets of propositions. (This abstract operator could easily be traded in for a type-shift/category-changing rule.)

Karttunen's paper – which appeared in the inaugural issue of *Linguistics and Philosophy* – was cast within the program of Weak Direct compositionality (more specifically, within the classical Montague program). As such, it contained no actual *wh*-fronting rule. Rather, a *wh*-phrase could be directly appended to the front of a sentence, triggering the deletion of one occurrence of an indexed pronoun. This was, not coincidentally, similar to Montague's Quantifying-In rule, which performs a substitution onto an indexed pronoun. Moreover, the *wh*-phrase combines semantically with the rest of the sentence in a way similar to what happens in Quantifying-In. The semantic rule is slightly adjusted in light of the fact that the generalized quantifier combines with a set of propositions rather than a proposition, but otherwise the two are similar. The important point is that in Karttunen's analysis there is no actual movement in the syntax (a point to which I return in Section 5), and the meaning of the full *wh*-question is built as the syntactic structure is built.

Consider now the view of *wh*-questions taken in standard Generative Semantics (supplemented with a model-theoretic component). Here things are potentially more complex, but it is worth spelling this out as a basis for theory comparison. Basically, this theory assumed that there is a syntactic rule of *wh*-movement – hence at some level of representation, *wh*-phrases are in normal argument positions and so must front. If this is put together with the idea that quantified NPs are in a 'raised' position at LF (Deep Structure) and are put into their 'argument' positions only by Quantifier Lowering, we would have the peculiar result that these are first lowered, and then raised again. As unpleasant as this is, I cannot resist the temptation of noting that some versions of D also move and move back, as in the common view that *wh*-phrases move in the syntax and part or all of them are later 'reconstructed' back into their original position for the purposes of certain binding constraints.[6]

Turning to D, the usual view is that the *wh*-material starts out in normal argument position, and moves (in the syntax) to a position where it is interpreted in LF (modulo the remarks above about 'reconstruction'). (Other views have been proposed; again I am just taking a rather simple conception which I believe is fair for the purposes of the present discussion.) Thus the arguments one commonly sees for the basic model in D revolve around

[6] Note, incidentally, that the basic architecture of C (LF + transformations) could avoid the lowering-and-raising strategy by essentially copying Karttunen's analysis. Thus let the phrase structure rules build LFs with *wh*-words in their final 'raised' positions, and have a rule deleting a pronoun which they bind. I leave it to the interested reader to spell out the details; it is a straightforward translation of Karttunen's analysis.

similarities between QR and *wh*-movement – both of which, on this view, raise material.

One of the most sacred bits of lore is the claim that wide scope quantification – like *wh*-movement – is subject to island constraints. It's not clear to me that common bit of wisdom is really true (since many of the island effects actually follow from independent constraints on wide scope quantification), but for the sake of argument, suppose that the conventional wisdom is correct. Does this necessitate acceptance of D over B or C? Certainly not. Generative semanticists, in fact, very loudly proclaimed this very observation as an argument for a movement rule of Quantifier Lowering. Their battlecry in the late 60's and early 70's was that since quantifier scopes obeyed the same constraints as movement, it must be a syntactic movement rule. For a clear statement of just this (appearing in an accessible and 'mainstream' journal), see Postal (1974), especially p. 383.

This way of putting it of course assumes the classical Generative Semantics view according to which *wh*-constructions do indeed involve movement. But can the constraints on *wh*-constructions and on quantifier scopes also be collapsed in Weak Direct Compositionality? The answer is, as has been known for years, of course; relevant discussion of just this point is given in Rodman (1976) (Rodman's discussion focuses on relative clauses rather than questions, but the extension to questions is obvious). Let the relevant constraint(s) be stated in terms of the possible positions of the indexed pronoun which is affected by a rule (thus an indexed pronoun is deleted when a *wh*-word is appended to the front and material is substituted in for it in Quantifying In.) Or, one could make the constraint sensitive to the path between the node which undergoes λ-abstraction (the constituent whose meaning ultimately occurs as argument of the generalized quantifier) and the pronoun which undergoes the rule.

To make an argument for the approach in D as opposed to these alternatives, one would need to show two things. The first would be that island effects are most simply accounted for in terms of constraints on *movement*. (This is still compatible with the Generative Semantics type solution, but would rule out the non-movement accounts of *wh*-constructions such as that taken in Karttunen.) There was considerable discussion of just this question in the syntactic literature in the mid and late 1970's, but it was inconclusive. See, for example, Bresnan and Grimshaw (1978), who show that even a Subjacency-inspired approach can be recast so as to constrain deletion as well as movement. The second thing one would need to show is that the constraint will apply in the right ways only in raising and not in lowering situations (or only if both wide scope quantification and *wh*-movement have to involve movement in the same direction). Such a demonstration would mean that quantified NPs (like *wh*-NPs) must move up the tree. This in turn means that scopes must be given by QR, and that in turn entails a model like D (and not C). But I know of no demonstration of this.

A similar argument commonly made for D centers on the observation that both quantified NP-pronoun binding relationships and *wh*-phrase-pronoun binding relationships show Weak Crossover effects:

(11) *His$_i$ mother loves every man$_i$.

(12) *Who$_i$ does his$_i$ mother love?

The apparatus of QR + *wh*-movement + traces has provided one set of tools for collapsing these. But these by no means are the only tools which can do this, and in fact the literature contains explicit proposals phrased in terms of B and C. One such proposal was developed in Jacobson (1972, 1977) within a Generative Semantics framework. I will not repeat the exact proposal here, but will give its obvious translation into Weak Direct compositionality, where it becomes nothing more than the obvious generalization of Montague's constraint on the Quantifying-In rule.

In Karttunen's analysis of *wh*-questions, one indexed pronoun is targeted as the pronoun to be deleted when a *wh*-phrase is appended to the front of the sentence. In Quantifying-In, one pronoun is targeted as the pronoun to be substituted. My proposal essentially said that for any rule in which there is more than one indexed pronoun meeting the structural description of the rule, only the leftmost one can actually be analyzed as meeting this. And this of course can easily be reformulated using c-command instead of left to right order. (In Jacobson, 1999 I show that generalizing these two kinds of cases is also possible under slightly weakened version of Strong Direct Compositionality supplemented with Wrap operations.)

The upshot, then, is that phenomena like these give no evidence for the increasingly popular 'modern' view in D over the simpler earlier alternatives. It has become quite popular to think of the similarities between *wh*-questions and quantifier scopes as being attributable to the fact that both involve raising material – but we see above that their similarities can equally well be stated in other terms. It has also become popular to talk of the difference between *wh*-movement constructions (in English) and quantifier scopes (or the difference between *wh*-movement in English and in Chinese – see Huang, 1982) as reducing to a difference between 'overt' and 'covert' movement. But – aside from the fact that this makes for a pleasant rhyme – it doesn't seem to be any more illuminating than saying that the difference is between appending to the front and substituting in (as in the classical Montague treatments) or between moving up and moving down (as in classical Generative Semantics).

We should also be leery of a large class of arguments which are designed to show the need for the theory in D which at best address only the need for a level of LF distinct from surface structure. A typical example goes like this. A reflexive pronoun must be (locally) c-commanded by a co-indexed NP (Principle A). But this cannot be stated at surface structure, because of sentences like the following:

(13) Which picture of himself$_i$ do you think that John$_i$ likes?

We therefore conclude that there is some level at which this constraint holds and at which the *wh*-material (*which picture of himself*) is in the object position of *likes*. The next step in the reasoning assumes that the relevant level must be LF, and so this leads to the conclusion that there is 'reconstruction' at which the fronted material is put back into its original position.

It would take an entirely separate paper to properly take on the reconstruction industry, but there are some obvious gaping holes in the type of reasoning given above. In the first place, I think that cases like (13) are a red herring; Kuno (1987), Zribi-Hertz (1989), Pollard and Sag (1992), Reinhart and Reuland (1993) and others have amply demonstrated that c-command is irrelevant for complements of 'picture nouns'. But if we really do believe that a reflexive must be c-commanded by some co-indexed NP, why assume that a post-surface structure level is the appropriate one? Principle A is, after all, stated as a purely syntactic constraint. Since D assumes a level of representation (deep structure) at which the *wh*-material is in the position after *likes*, wouldn't the obvious conclusion be that the relevant principle holds for that level? Why posit an abstract level like deep structure and then not use it to account for those properties which appear to hold at that level and not at surface structure? Notice that the leap to LF as the relevant level seems to be driven by the intuition that – despite the fact that Principle A is stated in purely syntactic terms – it ultimately constrains interpretation and so should be stated at a level 'closer' to meaning. But if we were to take this intuition seriously, then isn't the rational conclusion that the 'pre-*wh*-movement level' – which proponents of the model in D presumably think is motivated on independent syntactic grounds – is exactly the appropriate level at which to do the semantics? In other words, it seems to me that if one were to take cases like this seriously, one would be at least as tempted to embrace the view of the organization of the grammar taken in C as the view taken in D.[7]

4. Arguments for D: The Textbook Wisdom

That the model in D is nowadays often assumed without comment (and without apparent discomfort) becomes unsurprising once one looks at the treatment of these issues in the current semantics textbooks. An entire generation of semanticists has been and is being brought up on the textbooks of the 90's , and so it is worth looking at what these texts teach about these issues. To this end, I consulted two recent and highly influential semantics textbooks which treat the syntax/semantics interaction in some depth: Chierchia and McConnell-Ginet (1990), and Heim and Kratzer (1998).

Before continuing, let me clarify two points. First, I have no intention of 'trashing' either of these books – they are both wonderful in their treatment of semantic issues. Second, it is obviously beyond the scope of any book – and certainly a textbook – to deal with every possible alternative and to provide detailed arguments against it. I realize that many of the decisions of what and what not to cover are made for expository convenience. Thus my intent here is not to give a critical review of these texts: I am simply suggesting that the treatment of these issues in these texts helps explain why so much of the modern semantics literature treats it as a *fait accompli* that the model in D is correct.

To the extent that these texts argue for D or suggest that there could be alternatives, the general strategy is to give arguments against A (Strong Direct Compositionality). The only explicit discussion (though it is very cursory) I could find relevant to the choice of D vs. B or C was contained in Heim and Kratzer (1998), who try to provide a concrete argument *against* the view that the syntax builds in tandem with the semantics interpreting (p. 47). Heim and Kratzer address the fact that a consequence of

[7] In other words, the conclusion that something moves in the syntax and then moves back 'at LF' is surely not a 'result' that anyone could be happy with. To be fair, the reconstruction view is slightly improved in recent Minimalist literature; the idea here is that something moves and then moves back, but that rather all movement leaves a copy of at least the lexical material so that that can be used later for the purposes of, e.g., Principle A.

An improvement, perhaps, but still suspicious. After all, this requires one to endow surface structure with properties which are not visible (silent copies), when at the same time the theory is presumably committed to the belief that there is independent evidence for the pre-movement representation. Why posit extra apparatus (silent copies) when one has at one's disposal the abstract level with the properties one needs? Put differently, why posit abstract levels and then not use them?

the Montague-style conception is that the semantics interprets *only* what the syntax 'builds'. This, they claim, is problematic; they argue that the statements which serve as input to the semantic rules *should* indeed be distinct from those which serve as outputs of the syntactic rules. As discussed above, this is exactly the form of argument which *could* support D if one could really give a convincing demonstration of this point. Heim and Kratzer's particular argument is that there are structures which seem syntactically ill-formed but are nonetheless understandable (no example is given). By stating the semantic rules generally in such a way that they are not hooked in to particular syntactic rules, we can account for this. If, on the other hand, the input to the semantic rules is always the output of the syntactic rules, then there is no way to provide an interpretation for an ill-formed sentence.

At best, though, this seems to me to be an argument for very generalized rules. Under the theory in D – the theory that Heim and Kratzer are advocating – consider what it would mean to interpret an ill-formed syntactic structure. The input to the semantic interpretation rules is still trees – and so the interpretation has to be produced by *some* syntactic system. Thus in order to come up with a meaning for something which is actually ill-formed, we must be able to construct a tree for this. This in turn means that we can imagine a way that the syntax 'slipped up' and assigned our ill-formed sentence a tree. One could imagine that this happened, for example, by the addition of new syntactic combinatory rules (as long as these rules combine things whose semantic types can combine). Or, in a theory in which the syntactic rules are very general and are largely 'projected from the lexicon', an ill-formed syntactic structure could be built if a particular lexical item were assigned to the wrong category. Note that an interpretation would be possible only in the case that the misanalyzed lexical item is still given a semantic type such the general semantic principles allow it to combine semantically with its sister(s). But once we grant this power of imagination, the syntax-builds-while-semantics-interprets view actually does equally well. The point is clearest to illustrate in a theory like Categorial Grammar, where the syntactic combinatory rules are also stated in very general form. Here too one could simply imagine the ill-formed structure resulting from a misanalysis in which some lexical item were assigned to the wrong category. The syntax/semantics match in Categorial Grammar is such that if the misanalyzed lexical item could combine with some sister in the syntax, so could it in the semantics – and so we will have a syntactically ill-formed but interpretable structure. It is difficult to illustrate more thoroughly without actual examples, but once one considers what it actually means to interpret a syntactically ill-formed structure it is clear that there is absolutely no advantage to divorcing the two combinatory systems (as in the model in D).

The rest of the arguments on this issue that I could find really address A vs. D. For example, Chierchia and McConnell-Ginet introduce the question of quantifier scopes, present us with a choice between Cooper Storage and the positing of a level of representation at which quantified NPs are raised, and then go on to assume QR as a way to 'produce' this level. Solutions compatible with B or C are never mentioned, and they do not point out that the adoption of QR immediately eliminates the possibility of stating the syntactic and the semantic rules together. Nor is it that any but the most brilliant of students would notice, for the earlier fragments are presented in such a way that here too the two systems are already stated separately with no obvious close correspondence between the two.[8]

Heim and Kratzer also discuss the choice of A vs. D, and in fact devote an entire chapter to this (Chapter 7). This discussion is refreshingly honest: there are numerous footnotes pointing to alternative conceptions of things under Strong Direct Compositionality, and the authors are quick to note that the viability of A is still an open question. They also – quite reasonably – point out that the arguments they discuss are merely meant to illustrate how one could go about finding evidence for one theory over the other. But the point of relevance here is that the book pits Strong Direct Compositionality against D (surface-to-logical form), without considering any of the intermediate alternatives. Thus Chapter 7 points out that the existence of quantified NPs in object position (as in *John read every book*) is an apparent problem for the view that a transitive verb like *read* has a lexical meaning of type $\langle e, \langle e, t \rangle\rangle$. The authors then offer us two alternatives to solve this: repairing the type-mismatch by QR, or repairing the type-mismatch by type-shifting (which is, of course, one of the possibilities that has been proposed under Strong Direct Compositionality). Heim and Kratzer go on to supply three arguments for QR over type-shifting. One of the arguments is the familiar argument centering on scope ambiguity, one centers on Antecedent Contained Deletion, and one centers on the ability of the 'QR' solution to collapse quantification with variable-binding.

But each of these is equally compatible with the view of quantifier scopes taken in B and C, yet neither alternative is mentioned. Space precludes elaborating on these here, but suffice it to say that all three of Heim

[8] Since I am comparing modern views to classical Montague view, it is only fair to compare this to the presentation in Dowty, Wall and Peters (1981). As noted in fn. 5, the usual Montague notation also repeats information in the syntactic and the semantic rules, and Dowty, Wall and Peters do this too. But the fact that the two sets of rules can be stated together is made very clear by their careful use of numbering on the rules where one sees that each syntactic rule has a corresponding interpretation rule.

and Kratzer's points argue *only* for a 'level' of representation at which quantifiers are not in their surface position and where instead indexed pronouns, traces, and/or bound variables occupy the surface argument positions. The C-type (Generative Semantics) solution also of course contains such a level (Deep Structure/LF), and the Montague Quantifying-In solution does too – it is an intermediate representation 'built' before the Quantifying-In rule applies.

5. Evidence for Surface Interpretation Can Come Back to Haunt

I think it is safe to say that the historical basis for the surface-structure-to-LF view derives from the fact that this evolved in the syntax world from the 'Interpretive Semantics' position (e.g., Chomsky, 1970; Jackendoff, 1972). These authors rejected the Generative Semantics claim that *deep structure* was the only level of representation relevant to the semantics (they also made no use of the notion of Logical Form) and argued instead that at least some aspects of interpretation needed to be read off surface structure. Once Logical Form was adopted in, e.g., Chomsky (1976), the prior commitment to surface interpretation persisted, and led to the (generally unquestioned) assumption that Logical Form was derived from the surface structure. But herein lies the irony: some of the most convincing evidence against Generative Semantics is suddenly unaccounted for once one divorces surface structure from the level at which interpretation takes place.

A lovely illustration of this is an example drawn again from the domain of *wh*-questions. In particular, consider the seminal observation of Baker (1968) regarding the interpretation of multiple *wh*-questions. Baker points out that (14) is two but not three ways ambiguous:

(14) Which agent was assigned to find out which woman memorized which book?

Take (14) in the world of *Fahrenheit 451* (in which the government is burning all the books, so a band of people decide to keep them alive by each one memorizing a book). The most natural reading of (14) is that it is a question about agents: an appropriate answer would be Agent 007 was. A second possibility is that (14) is a question about pairing of agents and books; here an appropriate answer would be that Agent 007 was assigned to find out the memorizer of *Crime and Punishment*, Agent 008 was assigned to find out the memorizer of *The Brothers Karamazov*, etc. But it lacks a reading in which agents are paired with women.[9] The cleverness of this example derives from the fact that this cannot be attributed to a purely semantic violation. We can assume that *find out* (on the reading relevant here) has a meaning which requires a question as argument. But *in situ wh*-words can associate with higher questions (i.e., they can take wide scope); note that *which book* can associate either with the matrix question or with the embedded question. Hence if the mapping between the surface syntax and the semantic interpretation were not regulated in some way, there would be no *a priori* reason why (14) could not have the interpretation where *which book* associates with the embedded question, while *which agent* and *which woman* associate with the matrix question. The moral of Baker's example is that the surface is trying to tell us something: a 'fronted' *wh*-word, such as *which woman* in (14), has the property that its surface position marks its semantic scope.

This generalization follows naturally from the architecture of Direct Compositionality (at least in its Weak form), but does not automatically follow in theories (like C or D) in which the surface syntax is divorced from the level of interpretation. To demonstrate, consider first the problem that this case causes for the model in C (under the usual version which contains a *wh*-fronting rule in the syntax). For the sake of discussion, we will continue to assume that *wh*-phrases are like quantified NPs and hence are at LF/Deep Structure in a raised position which indicates their scope, and are then lowered by Quantifier Lowering into their pre-fronting positions. *Wh*-Movement then applies (obligatorily), making sure that one *wh*-word does move to the front of any S (or, CP) which is somehow marked as being a question. The problem for this view is that there is no connection between the LF/Deep Structure position of the fronted *wh*-word and its final surface position, since a fronted *wh*-word moves down and then moves back up. Nothing would stop a derivation in which *which agent* and *which woman* are scoped at LF over the matrix S and *which book* is scoped only over the embedded S. *Which woman* and *which book* both lower into argument positions in the embedded S, and *which woman* then fronts. The bottom line is that the connection between LF and surface structure is just not sufficiently regulated as to provide an account of Baker's observation;

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[9] There are two complications which I will gloss over here. First, it has often been noticed that a singular *wh*-phrase like *which agent* has a uniqueness presupposition when in a normal *wh*-question, but this goes away under the pair readings; under the pairing of agents and books, for example, there is no expectation that there is only one such pair. Second, there is debate in the literature as to just what if any uniqueness requirements there are for the pairings; thus Higginbotham and May (1981), for example, claim that there is a presupposition that the pairing is a bijection, while others have disagreed with this.

there is enough going on in the syntax to allow the two levels to be quite different.

But Baker's observation falls out in a very natural way from the architecture of a (at least Weak) Direct Compositional theory, combined with independently needed mechanics. Before continuing, note that there are two additional generalizations about *wh*-questions which any theory will need to capture. The first is that in at least an embedded *wh*-question, one *wh*-word *must* appear at the front; we cannot get questions like:

(15) *John wonders Mary memorized which book.

(As is well-known, matrix *wh*-questions escape this requirement in both Echo and 'Quizmaster' questions. Arguably these are different sorts of creatures, but in any case the fact that matrix *wh*-questions might not be subject to this requirement will not impact on the remarks here.) The second fact is that as long as there is one fronted *wh*-word, there can be any number of *in situ wh*-words:

(16) John wonders who put what where.

My claim is that the most natural implementation of any direct compositional analysis which can account for these two facts will derive Baker's observation as a consequence. The basic idea is quite simple: a *wh*-question (whether single or multiple) will – in the syntax – require that there be some *wh*-word at the front. But since the semantics is being built in tandem with the syntax, the semantic contribution of that *wh*-word will be made at the point that the syntactic *wh*-question constituent is 'built'.

To illustrate more concretely, consider the treatment of this in the Weak Direct Compositional analysis of Karttunen (1977). (I will suppress many of the details since I wish merely to make a point about the overall architecture or the theory.) Karttunen's analysis accounted for the fact that a *wh*-question must contain a fronted *wh*-word by having the syntax first build an expression whose category is P_Q, that is a 'proto-question', whose semantic type is a set of propositions. To do this, Karttunen used a silent operator ? but, as mentioned earlier, this could as well be done by a type-shift rule. A P_Q however, is not of the right syntactic category to be the complement of a question-embedding verb like *find out* which subcategorizes for a P_{WH}. An expression of this syntactic category is formed only when a *wh*-word is appended to the front. The associated semantics is similar to the semantics of Quantifying-In except that it must be adjusted in such a way that the generalized quantifier combines with a set of propositions rather than a proposition (the interested reader can consult

Karttunen's paper (p. 19) for the full details). The central insight, though, is that the semantic scope is of course linked to the point in the semantic composition at which the *wh*-word is introduced in the syntax, and so it follows that it has scope over just the sentence which it appears at the front of. Once we have an expression of category P_{WH}, we can Quantify-In another *wh*-word by a substitution rule to give back an expression of category P_{WH}. Again the syntax of this is just like Montague's Quantifying-In rule, and the semantics is the same as in the rule appending *wh*-words to the front of proto-questions. Because the Quantifying-in rule applies to a P_{WH} to give a P_{WH}, there can be any number of *in situ wh*-words. Baker's observation now follows as a consequence. The *in situ wh*-word could be quantified directly after the formation of either the embedded P_{Wh} or the matrix P_{WH}. The key is the following: the fronted *wh*-word necessarily associates with the embedded question, and this follows from the independent requirement that a syntactic expression of category P_{WH} requires a *wh*-word at the front. Of course this last requirement in and of itself does not follow from the architecture of the theory – nor should it, in view of languages like Chinese which have all *wh*-words *in situ* (Huang, 1982). But given that English does in general have a requirement to have a *wh*-word at the front, Baker's observation follows.[10]

I certainly do not mean to imply that Karttunen's analysis should be the last word on questions: it has problems both in its empirical coverage and in its rather clumsy statement of the semantic rules. Nonetheless, the fact that the basic architecture of (at least Weak) Direct Compositionality derives Baker's observation so effortlessly means that we should be loathe to abandon this architecture – and that refinements/revisions of this type of analysis should be careful to keep what is good about it.

I have claimed above that Baker's observation follows in a natural way from the architecture of Weak Direct Compositionality (which allows for some Quantifying-In), but what about under Strong Direct Composition-

[10] A very different approach to multiple *wh*-questions is proposed in, e.g., Higginbotham and May (1981), whereby the two *wh*-words merge together at LF (and in the interpretation) so as to form a question over a pair. This requires a rather complex mapping between the syntax and LF (or, the model-theoretic interpretation) in any theory. But if something like this turns out to be correct, then the remarks above will need modification. As far as I can tell, something like the Karttunen approach – modified in the appropriate way, will still account for Baker's observation: the simplest revision would be to let the two *wh*-NPs be introduced in the syntax at one point, where one is appended at the front and the other is substituted in. It still follows that the one appended at the front takes its scope at that point. What does not follow, however, is the fact that multiple and single *wh*-questions both require a fronted *wh*-word. This is because the rule(s) constructing multiple *wh*-questions are not parasitic off the rules constructing single *wh*-questions, and so it does not follow from anything that they should have this property in common.

ality? I am hopeful that the same observations would carry over there: essentially a *wh in situ* will be treated by whatever means one treats wide scope quantification in general. Moreover, it will have to pass up the information that it is ultimately looking for an expression of syntactic category P_{WH} (to continue with Karttunen's terminology), and the syntax and semantics will have to be linked such a way that its semantic scope will be just over this constituent. Again, though, an expression of the appropriate syntactic category must – on independent grounds – contain a *wh*-NP at the front. Moreover, in a run-of-the-mill (single) *wh*-question the rules forming expressions of this category will require the *wh*-NP which occurs at the front to have its semantic scope when this expression is formed. Baker's observation would then follow in the same basic way. Since the syntax and the semantics are being formed in tandem, a 'fronted' *wh* in a multiple *wh*-question will make exactly the same semantic contribution that it does in a single *wh*-question. This is, of course, somewhat promissory as it remains to embed this in a concrete proposal.

But now consider Baker's observation under the model in D. While I am sure that one could formulate an analysis under this model which accounts for the observation, it certainly does not immediately follow from the basic architecture. If one allows 'covert movement' of *wh*-phrases, then there is no *a priori* reason to block 'covert' movement of a *wh*-phrase which happens to have moved in the syntax.[11] But if that is allowed, the problem that we saw for the model in C simply rears its ugly head again, in a slightly new guise. Nothing would prevent an LF for (14) whereby *which agent* and *which woman* both raise to be associated with the highest question, while *which book* raises to be associated with the embedded question. As noted earlier, this cannot be blocked on semantic grounds (or, as a well-formedness condition on LF), since each question does have one or more associated *wh*-words at LF. Once again, though, the connection between the surface syntax and LF is not sufficiently regulated to make sure that a *wh*-word which is in a fronted position in the surface syntax has its semantic scope there.

Of course it could well be that an adequate account of the full range of facts concerning multiple *wh*-questions will simply be stuck with this problem and will need an additional stipulation. There is certainly no reason to take Baker's observation as the absolute guide for grammar construction – if many other facts lead to a simple analysis under a model like C or D then

[11] My inspiration for this section comes originally from class discussion by Irene Heim in a seminar in Spring, 2001; I would like to thank her for raising issues which led me to think about the relevance of Baker's observation to the overall question of the architecture of the grammar.

perhaps Direct Compositionality will just have to be abandoned. But this has not been demonstrated, and I would like to conclude by restating two morals of this example. The first is that if we do abandon Direct Compositionality (i.e., true 'surface interpretation'), then there is no obvious reason to leap to the more complex theory in D over the alternative in C. Baker's observation is, I believe, one of the most compelling pieces of evidence against C, but once one abandons the view of *real* surface interpretation then the move to D is seemingly quite irrational. The more important point is that the ease with which (at least Weak) Direct Compositionality handles this case – combined with the fact that its overall architecture is so much simpler than either C or D – should lead us to abandon the Direct Compositionality position only as a last resort.

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