MINIMAL REPLIES TO
DEKKER, HAJICOVÁ & SGALL, BERMAN, AND DE SWART

Kai von Fintel (MIT)

There is no way I can do justice to the quality and quantity of interesting suggestions and criticisms made by the commentators. What I should do instead is completely rewrite my paper, or better yet, start from the beginning and write a new paper. Perhaps, that's what I will do some time soon. For the moment, here are just a few thoughts.

DEKKER: SITUATION SEMANTICS

My conception of the structure of situations is a standard mereological one. Situations are parts of worlds. The usual axioms of classic mereology apply.[1] What I called mereological summation is also called mereological fusion. With classic mereology, I assume two properties of fusion: (i) the fusion of a and b contains nothing beyond what is already in a and b, (ii) for any a and b, there always is a fusion. The first property is a definitional property of fusion.[2] David Lewis writes in *Parts of Classes* (1991: 73):

(1)   Something is a *fusion* of some things iff it has all of them as parts and has no part that is distinct from each of them.

Or we could define fusion by using the notion of overlap, see Simons (1987: 14):

(2)   The mereological *fusion* of two individuals x and y, x⊕y, is that individual which something overlaps iff it overlaps at least one of x and y.

The other central tenet of classic mereology is that any two individuals possess a fusion. Simons writes: "Since individuals may be disjoint, spatiotemporally widely separated, and of quite different kinds, this assumption is very implausible." Lewis calls this prima facie implausible tenet of mereology Unrestricted Composition and gives the following longish defense (1991: 79-81):

> I say that whenever there are some things, they have a fusion. *Whenever!* It doesn't matter how many or disparate or scattered or unrelated they are. It doesn't matter whether they are all and only the satisfiers of some description. It doesn't matter whether there is any set, or even any class, of them. […] There is still a fusion. So I am committed to all manner of unheard-of things:

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[1]The classic references are Lesniewski (1983 [1927-1931]) and Goodman & Leonard (1940). For a recent survey, see Simons (1987).

[2]Within the wider realm of lattice theory, we can see fusion as a loaded version of the join operation. If one doesn't like mereology, there are weaker theories available.

trout-turkeys, fusions of individuals and classes, all the world's styrofoam, and many, many more. We are not accustomed to speak or think about such things. How is it done? Do we really have to?

It is done with the greatest of ease. It is no problem to describe an unheard-of fusion. It is nothing over and above its parts, so to describe it you need only describe the parts. Describe the character of the parts, describe their interrelation, and you have *ipso facto* described the fusion. The trout-turkey in no way defies description. It is neither fish nor fowl, but it is nothing else: it is part fish and part fowl. It is neither here nor there, so where is it? - Partly here, partly there. That much we can say, and that's enough. Its character is exhausted by the character and relations of its parts.

I never said, of course, that a trout-turkey is no different from an ordinary, much-heard-of thing. It is inhomogeneous, disconnected, and not in contrast with its surroundings. (Not along some of its borders.) It is not cohesive, not causallly integrated, not a causal unit in its impact on the rest of the world. It is not carved at the joints. But none of that has any bearing on whether it exists.

If you wish to ignore it, of course you may. Only if you speak with your quantifiers wide open must you affirm the trout-turkey's existence. If, like most of us all the time and all of us most of the time, you quantify subject to restrictions, then you can leave it out. You can declare that there just does not exist any such thing - *except*, of course, among the things you're ignoring.

Doing away with queer fusions by restricting composition cannot succeed, unless we do away with too much else besides. For many respects of queerness are matters of degree. But existence cannot be a matter of degree. If you say there is something that exists to a diminished degree, once you've said 'there is' your game is up. Existence is not some special distinction that befalls some of the things there are. Existence just *means* being one of the things there are, nothing else. The fuzzy line between less queer and more queer fusions cannot possibly coincide with the sharp edge where existence gives out and nothing lies beyond. A restriction on your quantifiers, on the other hand, may be as fuzzy as you please.[3]

The distinction between what we admit in our ontology and what natural language quantifies over is important and should be kept in mind. I do not see for example why we should lose sleep over the question whether the situation ontology is atomic. What is relevant for semantics is whether the propositions expressed by natural language are atomic. These are two very different matters.

Dekker draws attention to an important point: adopting mereological fusion is incompatible with one assumption of Kratzerian situation semantics. There, worlds are defined as the maximal elements with respect to the part-whole relation. If we have free fusion however, there will only be one maximal element, the fusion of all the possible situations there are, something that fuses elements of all possible worlds into one big piece of gunk. Worlds as we know them have to be defined in a different manner. Lewis' alternative is to define worlds as the maximal sums of

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[3][Lewis' footnote:] For a fuller statement of this argument against restricting composition, see Lewis, *On the Plurality of Worlds*, pp. 212-213. For a rejoinder, see Peter van Inwagen, 'When are Objects Parts?', *Philosophical Perspectives*, 1 (1987), pp. 21-47, especially pp. 40-5.

spatiotemporally related parts. But there may be other ways out of the tension between free fusion and the existence of separate possible worlds. We could also take worlds as primitive.

The most pressing question for the situation-semantic treatment of adverbial quantification has to do with the individuation of the quantifier domain. Somehow we have to ensure that material is not counted more than once, something that is not easy when you are dealing with elements that can stand in a part-whole relation. The intuition is that we should only count elements that are "at the same level" in the part-whole hierarchy. The standard solution, as discussed in my paper, is to assume that natural language propositions have the property of minimality: there is a set of situations supporting the proposition that contain no proper parts that also support the proposition. We could then safely let our adverbs quantify over these minimal situations (with some desirable side effects as discussed in the literature). Unfortunately, minimality cannot be maintained. I discuss one reason in my paper: imperfective propositions. A running proposition contains no minimal running situations (unless we are willing to make risky ontological moves involving time and space, something that we should avoid). Dekker identifies another case where minimality will falter: any case in which propositions are involved that stand in a lumping relation. Take a typical case of lumping: my neighbor's house burnt down. His pantry burnt down as part of it. The proposition that his house burnt down, then, lumps the proposition that his pantry burnt down in the actual world. This is not a matter of entailment: the pantry could have been spared, or the house may not have had a pantry. But in the actual world, every situation that supports the house-burning-down proposition also supports the pantry-burning-down proposition. The problem is that if the pantry situation is part of the house situation then we get messed up when we want to count minimal situations of something burning down. "When something burns down, it is usually missed by the owner". Do we count both the pantry and the house?

Here, both the bound variable account and the classic Kratzer account will make wrong predictions. The bound variable account will count (at least) two cases of something burning down. The Kratzer account will find only one minimal situation of something burning down, namely the pantry situation. Intuitively, what we want to count is the house situation.

Now, I think that ontologically the Kratzer account is correct: there are (at least) two situations of something burning down and they stand in a part-of relation. The problem is how to ensure that only the bigger burning-down situation is counted. This goes against the minimality doctrine that is prevalent in situation semantics circles. My conclusion is that there is something fundamentally wrong with the minimality doctrine. Dekker's comments (and my musings about

imperfectivity[4]) have brought that into sharp relief. The alternative is to figure out some way of forcing quantifiers to quantify (i) only over objects at the same level of the part-whole hierarchy, (ii) only over "maximally uninterrupted" objects. At the moment, I can't see how to do that formally (I have tried at times to get something like that in the case of imperfective propositions, but things haven't worked out). Kratzer's "facts that make a proposition true" are more maximal than minimal, but her definitions still depend on there being minimal situations (I think there are no minimal running situations, unless you make some adventurous assumptions about time) and they are incompatible with free fusion.

But let me stress again: I think the two cases (house-pantry) are both legitimate objects in the ontology and they do stand in a part-whole relation. But, somehow a quantifier has to be prevented from treating them as separate cases. It is important that the problem we have identified transcends the debate between bound variable approaches and situation semantic approaches. Part-whole hierarchies are problematic for both approaches.

HAJICOVA & SGALL: FOCUS-SENSITIVITY

The dichotomy between semantic and pragmatic partition or between association-with-focus and association-by-anaphora is indeed too black and white, as Hajicova & Sgall observe. In fact, the purely semantic account and the pure association-with-focus account are impossible to maintain. We at least need the pragmatic account to deal with examples like *John is always late* where the domain of quantification cannot be determined internal to the sentence. The question is whether we can stick by a purely pragmatic/anaphora account or whether we have to retreat to a mixed/combined account that sometimes uses association-with-focus on top of pragmatic determination of quantifier domains. My project is to explore the simpler theory.

Some clarification on the import of the autonomy thesis. Grammatical features can be expressed in a myriad of ways across languages. Subjects can be marked by morphemes or by position. Likewise, topic/focus can be marked by morphemes, by position, and by prosody. As I wrote in my paper, things couldn't be otherwise: if topic/focus are features of language, they have to be expressed in some way or other. That is a legitimate area of grammatical research. Now, an entirely different question is what grammatical processes are sensitive to topic/focus articulation. The position that I am pursuing (inspired by Rooth) is that the import of TFA is purely pragmatic. TFA marks a sentence in such a way as to make it fit into some contexts and not into

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[4]Which were prompted by Angelika Kratzer.

others. TFA expresses presuppositions about contexts. TFA in this respect is very similar to the modal particles of languages like German, Greek, Sanscrit. In brief:

(i)   TFA is marked by grammatical means (morphemes, word order, prosody). It is part of the representation of any sentence.

(ii) TFA is interpreted as inducing presuppositions about the context.

The claim is that nothing else is needed. If an extraction constraint is sensitive to TFA, that should follow from the fact that TFA is expressed by a certain structural configuration. It is the configuration that has the effect and not TFA itself (the constraint does not have to mention TFA, just the structural configuration). If the meaning of some element is sensitive to TFA, that is because it is context-dependent and TFA tells us about the context (the semantics of the element does not have to mention TFA, just the facts about context).

The thesis is ideologically inspired by the dichotomy between sentence grammar and discourse analysis, which may or may not be a point in its favor. I see it as an attractively strong proposal that should be considered seriously. Whether it is realistic is another matter.

BERMAN: PRESUPPOSITION ACCOMMODATION

First a point of terminology: in my work I have followed Heim in distinguishing between global accommodation and local accommodation. In more recent work, people have introduced a further distinction between local and intermediate accommodation. The main bone of contention is whether there is accommodation of a presupposition into the restriction of the next quantifier up. I still call this local accommodation, others call it intermediate accommodation. I will stick by my usage, but nothing hinges on it.

There is much in Berman's comments that I cannot discuss here. In many ways, his comments are very close to my own views, closer even than Beaver's. However, his comments don't really concern my paper on adverbial quantification but other work of mine on presuppositions. I will address them elsewhere soon.

One issue is germane to the adverbial quantification paper. Berman seems to assume that in cases like *What's so special about cats? They always land on their feet*, there is no sense at all in which a new (subtly connected) discourse topic is accommodated, while intermediate accommodation is all that happens. I don't see how one can be so confident. Clearly, I can follow up by saying *And they use their tails to steer in the air*, which is anaphorically connected to a set of situations

of cats falling. So that set must now be salient in the discourse. So, how can we maintain that no new topic has been added?

DE SWART: THE STAGE/INDIVIDUAL DISTINCTION

If scrambling is a way of marking topics and if subjects of individual-level predicates are automatically topic-marked, then we have to admit situations where something is both a topic and a focus. De Swart objects: "I find it hard to accept such an analysis". Analyses that employ embedded TFA are not unheard of, but perhaps we can do without them. What we would have to do is crucially distinguish top-level TFA from embedded TFA. Call one discourse TFA and the other sentence or local TFA. De Swart leans towards drawing such a line in the sand. I would want to do without the distinction. Consider de Swart's example:

(3)   A: Who (of the students) knows French?
         B: Anne knows French.
         B': Canadians know French.

Here *Anne* and *Canadians* are in discourse focus, but they are also the local topic of the individual-level predication (which accounts for the generic reading of the indefinite). How can we represent that?

(4)
$D = \{x \text{ knows French}\}$
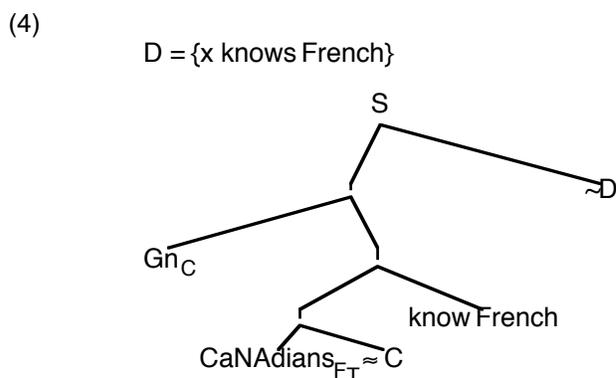
The discourse topic is a set of propositions of the form 'x knows French'. The answer 'Canadians know French' is focus-anaphoric to the discourse topic. The internal topic-marking of *Canadians* is interpreted as introducing a (second) discourse topic of propositions about Canadians. This can serve as the antecedent of the implicit generic quantifier. I do not see what is a priori wrong with such a picture.[5]

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[5]De Swart correctly points out that in my dissertation I erroneously use examples of *if*-clauses in discourse focus to argue against Haiman's claim that *if*-clauses are topics.

De Swart goes on to consider the very interesting question of why the subjects of individual-level predicates are topics. In my paper, I had assumed for convenience that this is a lexical idiosyncracy. But, clearly we would want to derive this intrinsic topic-marking from something else. One thing we need to explain is the generalization (due to Milsark and Carlson) that bare plural subjects of individual-level predicates are obligatorily read as generic:

(5)   a.   Firemen are available.
        b.   Firemen are altruistic.

De Swart argues that *altruistic* forces its event argument to be non-specific, since the predication cannot be used to characterize a spatio-temporal location. Now, since every sentence needs to have one strong/specific quantifier, this will have to be the one associated with the bare plural indefinite (the event quantifier is weak/non-specific).[6]

While this line of thinking is attractive, I do not see how it can deal with cases where more than one argument is involved. The differential behavior of the objects of expereiencer verbs and the objects of other transitive verbs is a case in point. In my paper, I discuss the following contrast:

(6)   a.   Usually, John dislikes vampire movies.
        b.   Usually, John complains about vampire movies.

The object in (6a) is obligatorily read as generic, while the one in (6b) is not (see Diesing for further discussion of such facts). Now, in both sentences there is a perfectly fine strong NP: the subject *John*. So, we cannot force a distinct behavior on the two objects based on the requirement that there has to be a strong quantifier in the sentence.

The notion of specificity as applied to the event argument of predicates is an interesting one, but it is unfortunately rather hazy. De Swart writes that specific predicates locate an eventuality in time and space. But we then find *be in a bad mood* classified as a non-specific predicate. Surely though, being in a bad mood is a state that is temporally located at particular times. I find more promise in McNally's (1995) idea that the crucial property is location-independence, for which only spatial location is relevant.

I don't think that aboutness can be reduced to conservativity. Here's why: Reinhart and, following her, de Swart correctly say that since weak quantifiers are symmetric, conservativity does not inherently single out the restriction or the scope as what the quantifier is about. So, the logical property of symmetry prevents us from using conservativity to deduce aboutness characteristics. Now, in the case of universal quantifiers there is a similar property that will

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[6]The idea that in sentences like (5a) it is the event argument that is the topic is worth further research. It is not a new idea: I floated it in a 1989 manuscript (von Fintel 1989) and I am sure there are other people who are thinking in this direction.

prevent the deduction. *All As are Bs* is equivalent to *All non-Bs are non-As*. Thus, the universal quantifier lives both on the A-set and on the non-B set. No deduction to aboutness is possible. I think that there is no intrinsic link between conservativity and aboutness, it's just that both of them are "left-leaning". I also don't understand how the notion of explicitly introduced sets can help in linking the logical property of conservativity with the pragmatic notion of aboutness.

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