Intersubjectivity and explanation in linguistics: A reply to Hinzen and van Lambalgen

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1. Introduction

Let me start by saying that I very much appreciate both the effort that Hinzen and Van Lambalgen (hereafter, H&L) have put into commenting on *Constructions of Intersubjectivity* (hereafter, *CoI*), and their comments as such. It is important for all cognitive disciplines studying language that representatives from different schools of thought try to address each other's work, in terms of both results and foundations. We may not reach agreement as a result of a discussion, but it will still be helpful in clarifying matters for ourselves and for other interested scholars, and thus for the future development of our common field of study. This is true even if the divide is deep—which is the case here in a number of respects, as H&L indicate themselves.

Another important preliminary remark concerns the nature and scope of our differences. Philosophically they are certainly far reaching, but from an empirical point of view it is useful to notice that H&L do not present counterexamples to the actual linguistic analyses presented in *CoI*. Rather, their main point is that such analyses can also be provided in other frameworks, which they label "more traditional" than cognitive linguistics, and which should in their view be preferred for other than empirical reasons, having more to do with general ideas about concepts such as "meaning", "communication", "grammar", etc., and the way these relate to even more comprehensive concepts such as "evolution" or "language". Below, I will actually dispute that H&L's comments show that the alternative, "non-cognitive", frameworks provide these explanations (and suggest that they are not forthcoming either), but it is good to note at the start that their own comments do not concern the empirical claims of *CoI*. In fact, in my own view, our main difference concerns the question what may count as an *explanation* in the analysis of linguistic phenomena.

Finally, as to the organization of this reply, I will not follow H&L's comments step by step, as this would lead me to repeat myself too much.

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Instead, I will first concentrate on the notion of "meaning", addressing mainly sections 2, 3, and 9 of H&L (section 2 below); then I will look at the grammar of negation and argue that the alternative analysis H&L suggest is *linguistically* unmotivated, which is partly due to them leaving out some pieces that constitute important components of the argumentation in *CoI*; it is at this point that the difference in what should be allowed to count as an explanation in linguistic analysis becomes most concrete. In this section (3), I will also deal with H&L's remarks about mental spaces, "cognitive significance" (their section 5), and formalization. Section 4 concerns H&L's sections 6–8, dealing with complementation, recursion, and some basic assumptions about grammatical structure. Section 5 concludes this reply.

2. What do we mean by "meaning"?

Perhaps the most baffling passage for me to read in H&L's comments was in the second paragraph of their Section 3. They first summarize the general programme of *CoI*: to demonstrate that the specific human ability to manage perspectives is systematically reflected in the meanings of several grammatical constructions, in the sense that these meanings are often related to the management of such perspectives—what I call "intersubjective cognitive coordination"—rather than to describing the world (specifying an *object* of conceptualization in some way). What baffled me was that they immediately add to this: "which entails that semantics cannot be understood as serving both functions simultaneously" (and then they set out to argue that this is a bad idea). How could it be that they see this as a core idea of *CoI*, while evidence against it is abundantly present in the book? Specifically, the first section (p. 210–212) of the *Concluding Remarks* is entitled, "Not everything is intersubjectivity (although intersubjectivity is widespread)", and it refers back to parts of the book where the meaning of different items was claimed to involve *both* the objective and the intersubjective level of conceptualization (cf. also *CoI* section 1.3, esp. p. 18). Moreover: why would it be an entailment? There must be something that I missed, and I assume it is to be found in what H&L conceive of as meaning, and hence as semantics.

H&L devote a separate section to meaning, but the points they make there are closely related to some they make at the beginning. In section 9, they contest the proposal that evoking inferences is part of the meaning of linguistic expressions, and defend a "context-independent" notion of meaning; in section 2, they oppose an argumentative view of language use (their picture of this view is a bit of a straw man; see the end of this section) to the "romanticist" view that language is used for the "free and

creative expression of thought'' (construed as ''reference, representation or the assertion of truth''), claiming that the latter function, unlike the former, is crucial for understanding what makes language differ from animal communication systems. We can safely equate these two oppositions, since ''argumentative'' in the Ducrot-sense adopted in *CoI* means ''evoking inferences'' (through associated ''topoi'', or ''defeasible rules''), and the context-independent meaning, as explicated by H&L, consists in the contribution of a linguistic (or logical) symbol to the reference or the truth conditions of an expression containing the symbol.

Just how close these two oppositions are connected also comes out in H&L's discussion of Ducrot's example of the use of *seats*, used in *CoI* to elucidate and specify the idea of ''argumentativity'': saying *There are seats in this room* invites the addressee to (i.a.) ascribe a certain positive degree of comfort to the room under discussion. H&L write: ''But obviously, there can be assertions about seats in rooms where these seats fail to be comfortable. *Hence* [my italics], the implicature is a mere contextual one, and ipso facto not an *inherent* [italics original] (non-contextual) aspect of the expression''. The implicit premise, necessary to complete this line of reasoning, can only be: ''If an aspect of the interpretation of an expression is not truth-conditional (does not have to represent something in the world of which the expression is predicated), then this aspect is not an inherent aspect of the meaning of the expression, but a contextual one''. First of all, this begs the question, the point of dispute precisely being how ''linguistic meaning'' should be construed: as (strictly) truth-conditional or as (at least also) argumentative. So in principle, we could stop the debate here, as this basic point of H&L contains a fatal fallacy.

However, I find it even more important to note that H&L overlook the fact that their observation has actually been used as an argument *for* the argumentative view (cf. *CoI* 11, and the Ducrot reference cited there). The point is that the utterance *There are seats in this room* has its argumentative value *regardless* of the actual degree of comfort, or lack thereof, of the seats in the room under discussion (the only condition is that the language users mutually share the idea that rooms with seats are normally more comfortable than rooms without). This is precisely the point that explains why the statement that the seats are uncomfortable can only be connected to this utterance by means of an adversative connective, e.g., *but*, and that something like *and moreover* is incongruent. Assuming, for the sake of the argument, that it is somehow established as true that the seats in a certain room are not exactly comfortable, this still does not make the text ''There are seats in this room, and moreover they are uncomfortable'' a coherent one. If we want to *express*, i.e., represent *linguistically*, both the presence of seats and their lack of comfort, then we have

to mark this as contrastive, and that is what makes a linguist, whose job is to account for the use and distribution of linguistic expressions and their constituent parts, conclude that the argumentative character is inherent in the *linguistic elements* involved.

H&L do say that semantics should account for both "inherent" and "contextual" aspects of linguistic expressions. But they equate these two notions with "truth" and "argumentativity", respectively, and then *also* with the sentence and discourse levels (their Section 3). So according to H&L, the following 1-to-1 relationships hold:

a) Inherent meaning : descriptive : sentence level (and presumably below)
b) Contextual meaning : argumentative/inferential : discourse level

It seems to be this relatively implicit—but contestable and contested[1]—view of meaning and the organization of semantic description that makes H&L conclude that the *CoI*-view of linguistic meaning as including aspects of discourse and argumentation gives up the possibility to account for relationships between language and the world. Not only do they first implicitly identify "inherent meaning" with "descriptive meaning", thus begging the question, they moreover connect descriptive meaning especially to the sentence level. Since sentence semantics presumably in their view precedes discourse and inferential semantics (sentences being taken as the building blocks of discourse), it follows from considering some inferential and discourse meaning as "inherent" that there is no possibility to account for correspondences between language and the world. In any case, this is the only way in which I can make any sense at all of their statement.

But, of course, nothing of this kind actually follows from the basic assumptions of *CoI*, or cognitive linguistics in general. It is knowledge of shared (i.e., cultural) cognitive models that is directly evoked by linguistic elements, not information about the world; but some of the inferences that knowing these models allows us to make, do involve the world. Thus, the primary meaning of *beautiful* is to express a positive evaluation, not to give a description of some sort (consider the task of specifying the truth conditions for H&L's example of *beautiful perfume* ...); knowing the culture, and especially having some relevant experience, allows many language users to make some inferences about actual properties of the perfume involved. But it is not necessary to make such descriptive inferences, and a person not (capable of) making them can still understand the utterance.

Another important point about conceptions of meaning relates to the role of convention. Section 9 of H&L contains many clauses of which

the noun *meaning* is a part, but it is not at all clear that it can be used in the same sense in all these statements; in other words, H&L do not seem to be aware of, or at least they do not at all worry about, a possible polysemy of the term *meaning*, which might affect the contents and consequences of their statements. They object to the "philosophy of meaning" they think they find in *CoI*, but do not explicate what specific sense of *meaning* they mean. They state one point of their own position, in relation to "compositionality" as: "meaning will not be conventional: meaning will follow by necessity from algebraic laws, etc.". But in a context like this (leaving aside the issue whether compositionality is indeed to be viewed as an algebraic phenomenon, independent of a particular cognitive system), *meaning* does not have the same sense as in, for example "The meaning of the word *banana* is: a category of fruit with (prototypically) characteristics X, Y, Z". The latter involves a relation between a sound and a concept that is conventional; *banana* means what it does because speakers of English mutually share knowledge of the rules for the proper use of the word. So in all larger expressions, the meaning of the whole is partly conventional, because of the words; moreover, the balance between conventionality and compositionality is not fixed (consider *banana republic*), and there are even complete sentences with a meaning that is mostly a matter of convention (*An apple never falls far from the tree*). This is very elementary linguistics, of course. The basic hypothesis of *CoI*, about intersubjectivity being a prominent aspect of meaning, is explicitly stated in terms of the meanings of "linguistic symbols (words and constructions)" (p. 4), i.e., conventional signs. The claim is that intersubjectivity is so important that several linguistic elements, especially a number of grammatical ones, are conventional instruments for intersubjective management (and that they have not sufficiently been recognized as such in the past). Nothing in the argumentation for this point hinges on a view of compositionality, which is an important, but independent issue. But H&L keep talking about "(philosophy of) meaning" as if it were a unitary concept, and then present compositionality "of meaning" as an argument against conventionality "of meaning". It will be clear that this is simply completely beside the point. Moreover, if all the senses of *meaning* are to be subsumed under one "philosophy of meaning", this philosophy is never going to be anywhere near coherent, so of little explanatory value.[2]

As a final remark on meaning, a word on H&L's terminology in relation to very general scientific and philosophical commitments. In their attempt to challenge the idea that argumentation and intersubjectivity are inherent aspects of linguistic meaning, H&L use "manipulation" and "control"—alluding to behaviourism—as terms for the function of

language use as viewed in *CoI*, while *CoI* itself uses "management and assessment" and "cognitive coordination". Manipulation normally goes *against* the interests of the receiver, and especially: *without* the receiver recognizing the intentions of the sender (usually, it involves deceit). The point of the use of "argumentation" in *CoI* is precisely to simultaneously express similarity to animal communication (it *is* an attempt to influence), and a difference: it is an attempt to *convince*, i.e., to influence the receiver's decision making process, by (i.a.) *displaying* one's communicative intention.[3] Of course, it is true that we can and do sometimes use language, and our brains, to ponder the truth of something, just as it is true that we can and do sometimes use our legs to run for fun or in an athletics competition, that we can and do use our brains to play chess and watch the stars, etc.. But *focussing* on these kinds of uses is not going to get us very far in understanding how the features involved (legs, brains, language, etc.) fit into the natural world, that is: in *explaining* them. The challenge is precisely to develop hypotheses, maximally constrained by what we know about evolution and communication in general, about the way the human communication system also got to be "usable" for some functions, such as reference and description, for which is was not, in all probability, originally an adaptation (cf. Verhagen forthcoming a).

3. Negation and connectives: Interaction between grammatical items and its explanation

For negation, the point H&L try to make is that a "more traditional" non-monotonic logical approach, enriched with clauses that introduce the possibility of exceptions, can account for the same observations and generalizations as *CoI* without introducing the notion of "mental spaces"; if that were true, then their analysis would be simpler (using at least one theoretical construct less than mine). Moreover, they have objections against this construct as they have doubts about its cognitive and formal status (see the end of section 3.2 for some remarks on this last point).

3.1. *Exception clauses and topoi*

As H&L notice, their use of "exception clauses" runs parallel to the use in *CoI* of Ducrot's concept of "topoi". The general template of the latter is "If P, then normally Q"; the template for H&L's defeasible rules is "If P and nothing exceptional is the case, then Q". Indeed, for the cases they discuss, their analysis produces the same account of inferences associated with negative sentences as the one in *CoI*; the descriptive adequacy of their account is thus not better than *CoI*'s, so the approaches might

be considered notational variants. But one important question is: How about cases they do not discuss, but which are part of the account in *CoI*? Does their analysis generalize to these cases? This amounts to a question of explanatory power; it will be taken up in section 3.2. Another question, also an issue of explanation, is: Does their analysis classify elements into categories that make sense linguistically? In other words: Does their characterization of the semantics fit the *distribution* of the linguistic elements involved? This is the issue for the remainder of this section.

In H&L's analysis, *barely* indicates the existence of an exception; for example in *He barely passed his first statistics course*, *barely* indicates that the passing was abnormal, so the clause "nothing exceptional is the case" is not satisfied, and therefore the subsequent derivation of a relevant inference Q (e.g., "he can pass other courses as well") is blocked.[4] The same result is produced in *CoI* by the assumption that *barely* invalidates the applicability of topoi associated with the content of the sentence ("the performance was so minimal that one cannot draw conclusions that one would otherwise draw from the fact that he passed"). First, it seems to me that there may be a serious conceptual problem with the exception-approach. "Exception" does not seem to be a primitive notion; it presupposes the notion of a rule, whereas the reverse does not hold. Rules (including those about what is "normally", not necessarily always, the case) can be experientially based generalizations (e.g., in terms of frequency: "What happens most of the time?"), but not the other way around. Indeed, exceptions *must* be defined in terms of rules (negatively), as they are not themselves generalizations (what makes something an exception is the background rule). Thus it seems to me that H&L's use of *ab* as "a proposition letter indicating some abnormality" as if it were something unanalysable, may mask the possibility that their analysis ultimately reduces to mine.

Second, it is clear that the exception-approach to *barely* implies that it belongs to a different class of linguistic elements than *not*: the first belongs to the "abnormality indicators", the second does not. Here we reach a fundamental difference between the logical approach of H&L and the linguistic one of *CoI*. The initial reason for reconsidering the semantics of *barely* in argumentative terms was that both *not* and *barely* license the *let alone* construction, i.e., their distribution is similar in a *linguistically* important way (grammatical behaviour). What the analysis of *CoI* shows is that this grammatical behaviour parallels the inferential (and discourse connecting) properties of both elements (not the real-world relations), and can thus be *explained* by assuming that the grammatical properties are determined by argumentative rather than "real-world" aspects of meaning. By putting *not* and *barely* in semantically different categories of

elements, H&L simply give up this explanatory power. On the basis of their account, if the grammatical behaviour of elements reflects their meaning, then one should expect *not* and *barely* to be grammatically very different, but in fact they are not; taking the programmatic idea of "language as a window on the mind" seriously should precisely lead one to taking the intersubjective analysis seriously, I maintain. As I said, I suspect the ultimate source of H&L overlooking this point is that their basic concerns are logical, rather than linguistic.[5]

3.2. *Explanatory scope*

The last comments in the previous section already indicate that H&L do not always take into account that an important part of the argumentation in *CoI* involves connections between different parts of the linguistic system, and that they focus their semantic analysis only on certain words and constructions in isolation. In fact, this is a more general tendency, that severely undermines the power of their criticism and their "alternative". For one thing, they do not discuss how their analysis of the defeasibility of the argumentative implications of *not* and *barely* can be applied/extended to *almost*, while this is, again, an integral part of the argumentation in *CoI*. The importance of the point can be demonstrated with example (1) (H&L's 4a*), showing the defeasibility of (at least some of the) argumentative inferences associated with a negated sentence:

(1) a A. Do you think our son will pass his courses this term?
b B-a. Well, he did *not* pass his first statistics course.
c A. But he got a very good grade for the astrophysics course!

Just like I invoked a wide-spread cultural model ("Statistics is a hard subject"), H&L invoke another one in the form of astrophysics to demonstrate that in a next move in the discourse, the initial suggestion "He is not going to pass" can be reversed again ("If he is smart enough to get a good grade for astrophysics, he may still pass"). Now the point is that the same reversal can also be established by certain sentences that contain the operator *almost*:

(2) = (1)a+b
c A. But he almost passed the astrophysics course!

In this case, A's utterance entails the *negation* of "He passed astrophysics": in actual fact, the student in question neither passed statistics nor astrophysics. But by means of *almost*, speaker A construes the latter as an argument for the conclusion that he might still pass the term, i.e., in the same way as the strongly positive statement in (1)c. This is straightforwardly

accounted for in *CoI* (just like *barely* is a relatively weak negative operator, *almost* is a relatively weak positive operator on the argumentative orientation of an utterance), which also explains why *almost* does not license the *let alone* construction (the argument from linguistic distribution again), despite the entailment of a negation.

How should this be accounted for in an exception-approach? First of all, H&L do not themselves indicate what such a generalization would look like. Perhaps we should say that *almost* also marks the event described in the sentence as an exception, so that otherwise licensed inferences cannot be derived? That would clearly not suffice, as it would then be said to have the same meaning as *barely*. In fact, we can now see that the characterization of *barely* as an exception-indicator is insufficient—minimally, the *direction*, i.e., negative, of the inferences involved should be included in this characterization. So suppose we characterize *almost P* as "not-P, and something abnormal is the case". Even though this might seem better, I don't think it is. The problem is to derive positive inferences from the negative statement. Recall that the general form of the defeasible rule, according to H&L, is "If (P and nothing abnormal), then Q". When we now have, due to the presence of *almost*, "not-P" as a minor premise, then it seems to me that nothing can be derived anymore. The conjunction of the rule (a) "If (P & nothing is abnormal), then Q" with (b) "something abnormal is the case" can lead to the derivation of (c) not-Q, even if P is the case (with "closed world reasoning"); but the conjunction of the same rule (a) with (b) "not-P and something abnormal is the case" cannot produce the derivation of (c) Q, as not-P by itself contradicts the antecedent clause of the rule ("P & nothing is abnormal"). In fact, it seems to me that in this case, too, not-Q would have to be derived, given the falsity of the antecedent clause. Thus, I conclude that there are good grounds for claiming that the exception-approach does not generalize to *almost*, and in that sense is also low in explanatory power, while *almost* fits naturally into the argumentative framework of *CoI*, as a weak positive argumentative operator, complementary to the negative *barely*.

A similar conclusion holds for H&L's discussion of the concessive connective *although*. While their reanalysis in terms of the exception-approach can provide an adequate semantic characterization of sentences of the type *p although q* in isolation from the rest of the linguistic system, they do not show that it accounts for interactions with other elements, in particular negation. Precisely this interaction is the key part in the argumentation in *CoI*: *although* cannot occur in the scope of negation, i.e., "not p although q" *must* be understood as "(not p) although q"; it cannot be interpreted as "not (p although q)", while its positive (causal)

counterpart *because* can occur in the scope of negation: "not p because q" can in principle be read both as "(not p) because q" and as "not (p because q)". In this case, I will refrain from elaborating H&L's approach myself to see how it might work, and simply observe that this is what they actually should have done in order to make their point, but they haven't.

The analysis in *CoI* of these phenomena crucially rests on the assumption that sentential negation introduces a separate representation ("mental space") of the viewpoint that the speaker of the present sentence opposes. This point is also not mentioned by H&L, who simply dismiss "mental spaces" as if they were only used in the analysis of negative sentences as such. On the contrary, chapter 4 of *CoI* shows that a mental space analysis of negation provides an explanation not only of the combinatorial restrictions between negation and *although*, but also of a number of such restrictions between negation and causal connectives—some of which exhibit scope restrictions similar to *although*. Moreover, the mental space analysis of negation is motivated in chapter 2 (as it is in the mental space literature in general) *independently* of the argumentative analysis of negation, viz. in terms of the interpretation of discourse anaphors following negative sentences, and the connective *On the contrary*. The greatest explanatory power of the mental space approach, according to *CoI*, lies in the possibility of this single idea to unify the analysis of the linguistic distribution of a number of phenomena. While it may be possible to construct an analysis of simple *although*-sentences without "special machinery for mental spaces", this analysis again does not naturally generalize to cases of interaction with other phenomena, as manifested in distributional and interpretive restrictions—a fundamental concern for a linguist with the ambition to provide explanations. But again, H&L's concerns seem to be located more in the dimension of logical rather than linguistic analysis.

It is in this context that H&L dedicate a separate section, with the title "Cognitive significance", to the status of the theoretical construct of "mental spaces", which in their view is rather dubious. To many cognitive scientists, this may appear somewhat puzzling, because the basic idea of mental spaces seems to be just a specific formulation of the fundamental human capacity of perspective taking and perspective shifting: to entertain the "same" object or idea in different ways, from different angles, etc., i.e., to combine difference and sameness, by means of "partitioned representations" (Dinsmore 1991). As it turns out, however, what H&L mean is that it is not (to their knowledge and/or standards) sufficiently *formalized*. In their view and invoking Chomsky's earliest work, the most important condition for a linguistic analysis to be called a

"cognitive" one, is to be explicit, which they immediately identify with to be given a "computational or algorithmic description". Firstly, notice that they move, very quickly, from what might be a necessary condition to a necessary-and-sufficient one. Secondly, it is quite strange, in view of the history of science (including recent cognitive science) to read that the integration of fields of inquiry should "depend" on the explicitness of computational descriptions. In actual fact, the possibility of operationalising generalizations obtained by one kind of research method in terms of another seems at least as important, and to my mind much more common ("If your distributional analysis says that A and B are basically the same/different, and if this is psychologically real, then the results of my reaction time/fMRI-measurements/etc. should look like this: ....."). But in the main stream of the generative enterprise, the focus has been on developing formalisms rather than on deriving such predictions from the theory and testing them. The confrontation with evidence, however, is the hallmark of empirical science; the generative preoccupation with formalisms at the expense of maximising evidence is thus indicative of the fact that linguistics is seen more like philosophy or mathematics than like science. It seems to me that H&L's point of view is only a recent carry-over of the unfortunate identification, in the 1950s indeed, of "language" with "formal language" (in the sense of "the set of well-formed strings of elements taken from some finite alphabet") that has hindered the understanding of human languages as historical and psychological phenomena that cannot be so defined, but that are still quite real (just like, to take a well known example, a biological species).

4. Complementation and recursion

4.1. *A minimalist "account"?*

There is a curious sort of complementarity in H&L's response to *CoI*. Their discussion of negation and connectives contains an alternative semantic analysis, but does not really pay attention to combinatorial and distributional (i.e., syntactic) aspects as sources of evidence for the semantics. Their treatment of complementation constructions exhibits the reverse pattern: it focuses almost completely on the issue of the proper syntactic analysis, and contains no more than two sentences about the semantics; in fact, for the sake of the argument they go along with the *CoI*-analysis (in brief: matrix clauses are perspectival operators, rather than event descriptions with other events as parts),[6] so here they ignore the possibility that the semantics may provide a constraint on the syntactic analysis (which, to be sure, is not to say that it would "ipso facto"

provide such an analysis). Be that as it may, their comments essentially come down on an argument against a constructionist approach to syntax, from the point of view of Chomsky's "minimalist program",[7] and I will accordingly also only comment on issues of syntactic analysis *strictu senso*. They do make some comments on perspective taking, and I will also have a bit to say about that, but they are unrelated to the syntactic analysis.

H&L suggest that "standard tests for constituency" provide evidence in favour of the idea that clausal complements should be analysed as verbal arguments, i.e., as bearing the same syntactic relation to the verb as a nominal complement. The problem is that these tests of constituency are never conclusive. They give the examples *George saw/knew/said what?* and *George saw/knew/said that X, and Bill saw/knew/said so too*, to suggest the generalization that complement clauses *in general* can be "replaced" by *what* and *so*. But that is simply not true, witness **George warned/was afraid what?* and **... and Bill warned/was afraid so too*, while *George warned/was afraid that his opponent would raise taxes* is fine. Thus, although the distribution of *what* and of *so* partly overlaps with that of complement clauses, there are also discrepancies. This makes "allowing replacement by *what/so*" basically worthless as "tests", as they sometimes produce the answer "no" and sometimes "yes" to the question "Does this complement clause bear the same syntactic relationship to the matrix verb as a noun phrase or a pronoun?". What H&L do, deciding that the "yes"-answer is the decisive one, is a clear case of the "methodological opportunism" in much syntactic argumentation exposed by Croft (2001: Ch. 1). As we saw previously, H&L have a tendency to overlook one of the most basic concerns of a (cognitive) *linguist*: to account for the distribution of linguistic elements, and to take the patterns in this distribution as the most reliable indicators for the precise way in which language provides a window on the mind.

H&L mention part of the more complete discussion of this issue in *CoI*, admitting that it is not really clear to them what the problem is, and then go on to provide an analysis of one type of complementation construction in terms of the minimalist program. They start with formulating a number of what they call "rather minimal assumptions". Leaving aside whether they are really minimal in the sense of "virtually a conceptual necessity" (I don't think so), I will restrict myself to the question whether this approach accomplishes what H&L claim it does. They state that these assumptions allow one to describe the similarities between nominal objects and clausal complements, and of course it does: any sufficiently abstract analysis does. They then say that because this analysis does not refer to syntactic categories—i.e., it *abstracts* from the differences between nominal and clausal phrases—it does not predict that nominal

and clausal phrases have the same distribution. But of course, without additional (presumably not so minimal) stipulations, that is precisely what the analysis *does* predict. If the claim is (and that is how H&L present it) that the minimalist approach can *explain* the occurrence of both nominal and clausal complements (and not only describe what, however minute and abstract, is similar to them), then the system as they describe it *must* predict the same distribution for the two (and more?) types of phrases that are instantiations of the fully general category label X (again, without additional stipulations). It seems to me that there is more of a "logical error" here than in *CoI* (cf. footnote 6).

Somewhat more mildly, one could say that, while it may be true that H&L's minimalist analysis does not strictly predict the same distribution for nominal and clausal phrases, it does not predict the differences either. Then the *CoI*-analysis would still have to be viewed as superior, since it *does* predict the possibility of clausal complements with *warn* and *be afraid* despite the fact that these predicates do not take nominal objects: they are both perspective markers (as a verb of communication, and a mental state predicate, respectively) and hence fully compatible with the hypothesized meaning of the complementation construction (notice that the analysis of form and function crucially meet here). But in any case, the minimalist analysis as provided definitely does not account for the observed distribution of clausal complements as only partially overlapping with that of nominal ones.

Surprisingly, the most detailed actual syntactic analysis in H&L's paper ultimately results in a full contradiction. For the rather sketchy analysis of standard object complementation (the *George saw/knew/warned/* ... examples above), H&L invoke the general principle that phrases are headed. They then attempt to give a minimalist account of copular complementation constructions of the type *The danger is that the middle class feels alienated*—which fit straightforwardly into *CoI* since being a danger is not an observable property in the world, but rather a subjective assessment; hence the matrix clause evokes a (perhaps unidentified) perspective, and thus satisfies the conditions for combination with a complement clause. In the minimalist account, the predication must count as symmetrical—only in this way is it possible for either element of the "small clause" allegedly underlying such sentences to "surface" as the subject of the sentence (cf. *That the middle class feels alienated is the danger*). H&L state explicitly: "Neither the CP [= clause] nor the DP [= nominal phrase] are the head". This directly contradicts their minimalist claim (iii) ("phrases are headed"), which was moreover necessary in the description of "object" complements. I conclude that their account is inherently inconsistent, and hence that it again does not accomplish what

it is claimed to accomplish, also not for copular matrix clauses of complements. And then we even have not yet touched upon all the theoretical machinery invoked, such as movement and empty structural positions, for which Occam's razor would require independent evidence—but that is a much more general issue than need concern us here.

4.2. *Perspective taking, recursion, and understanding false beliefs*

In *CoI*, it is observed that the hypothesis of complementation expressing perspective taking immediately accounts for the fact that complementation is a prototype of recursion in language (the possibility for a structure of type X to be embedded in another structure of type X), since conceptual perspective taking itself inherently allows for recursion. Thus, the *source* of this case of recursion in language is in a sense "placed outside language", but that is different from "placing recursion outside language", as H&L construe it. More importantly, they contest the *CoI* explanation on the basis of the argument that this explanation would only work if this conceptual recursivity is "propositional", for which they claim there is no evidence. They do not state very precisely what they mean by "propositionality", but they refer to experiments involving what is known in "theory of mind" research as false belief tasks; these are tasks in which subjects must be able to entertain another person's belief about the world and predict how s/he would act on that basis, while knowing simultaneously that this belief is false (hence the term), so that the subject's own response to the situation would be different. If recursion were restricted to this kind of "management of incompatible beliefs", then H&L would have a point, because (understandably) having a system of *secondary representation* for beliefs, i.e. a system on top of the primary sensori-motor system, seems to be a necessary condition for performing false belief tasks adequately.

However, managing false beliefs is simply not the *same* as perspective taking, it is one of its most abstract and complex forms. Human children develop several skills of social cognition before language, such as recognizing intentionality (distinguishing intentional acts from accidental events), sharing attention (e.g., in gaze following), and directing attention (pointing, showing). These basic skills all involve perspective taking, and their development is a necessary condition (given the arbitariness of connections between sound and meaning as children encounter them in the world) for the development of linguistic symbolic communication (Tomasello 1999): it is only through recognition of an adult's intention that a child can start to make guesses about the meaning of some sound. Moreover, these skills clearly already exhibit the potential for recursion; e.g., a

child can manage other people's attention to get them to show something to the child. More recently, is has been shown that very young children (and, to a limited extent, young chimpanzees) can also recognize other people's goals and desires, as evidenced by their propensity to provide help (Warneken and Tomasello 2006). These are complex social cognitive skills, and even in cases where our closest relatives can be argued to have similar abilities, humans are usually much better at them, also at a young age. Still, they are less complex than understanding beliefs, which are relatively permanent mental states not directly caused by the outside world (such as perceptions) but by other mental states or events—nor directly causing actions, but only indirectly so through guiding plans and intentions (cf. D'Andrade 1987). And they all involve simple alignment of the self with the other, not alignment *plus* dissociation, which, as mentioned above, requires a system of secondary representation.

Thus, understanding other people as having intentions and desires like oneself is simpler and more basic, also developmentally, than understanding beliefs, and especially false beliefs. Now language, being a system of symbolic communication, has the (fortunate) automatic "side effect" of also providing humans with a system of secondary representation (cf. Keller 1998: 127–128), so that it may, itself being based on capacities for social cognition, provide the scaffolding to enhance these capacities to a level like that of managing false beliefs. Thus, the acquisition of false belief understanding may very well be dependent on the acquisition of a representation system for perspectivization, such as complementation. As a matter of fact, Tomasello has been one of the scholars contributing some of the most compelling evidence for this view so far (Lohmann and Tomasello 2003). So it is not at all a matter of "putting the cart before the horse", but rather a matter of treating "perspective taking", "theory of mind", etc. not as monolithic concepts, but as configurations of features that constitute a family of related perspectivization capabilities of different degrees of complexity.[8]

5. Conclusion

H&L attempt to show that data adduced in *CoI* as support for an intersubjective, argumentative view of meaning in grammar, have an explanation in other approaches, which they consider "more traditional" and that they would in principle consider superior; but in general these attempts fail. The reasons for this failure are various, including misunderstandings and misconstruals, but the most important one is the fact that they ignore the precise character of the task of explanation in linguistics, which involves taking the *distribution* of linguistic elements seriously:

if several linguistic forms behave similarly with respect to one or more environments—grammatical ones and/or discourse ones—then an analysis should account for this (with a minimum of assumptions, of course), to be acceptable as an explanation (in some cases, H&L's overlooking of this crucial point even makes them leave out certain crucial parts of analyses in *CoI* from their own discussion). Indeed, it is only by taking the distribution of linguistic elements seriously in this sense, that the study of language provides an *independent* "window on the mind", such that certain conceptions of the nature of "meaning" and "mind" are not already built into the foundational concepts of a purported explanation.

It is thus ironic, in my view, that H&L turn "explanation" into their major point in their conclusions: I couldn't agree more.

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Notes

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1. The usefulness of drawing boundaries and connections in this way has been disputed by cognitive and functional linguists for decades now, and these alternatives have produced several important insights. In an important sense, Fauconnier's (1985) theory of Mental Spaces was motivated by the discovery of the inferential character of meaning at the level of the sentence, and even below; metaphors were demonstrated to be both inherent (in sentences ànd in words) and argumentative in Lakoff and Johnson (1980), etc.; an approach showing quite directly that the distinctions invoked by H&L must be called into question, is Levinson's (2000) theory of "presumptive meanings". So given the state of the art in cognitive linguistics, and in cognitive science in general, some more argumentation to still maintain the "traditional" view is highly desirable, to say the least. Even though, admittedly, one cannot cover everything in a relatively short commentary article, references to other work do not constitute such an argumentation (cf. Bierwisch's 2006 response to Hamm, Kamp and Van Lambalgen 2006).
2. The situation may even be worse. In English (unlike some other languages), the term *meaning* may also be used for a contextually derived, i.e., person and time bound, interpretation of a linguistic element or a piece of discourse, and even for what a speaker/writer "means" (i.e., intends to convey) with an utterance, and it seems to me that H&L include these senses in their notion of "meaning" too. Needless to say, all the senses are *related*, but they are certainly not identical. One important difference is that conventional meaning is, by definition, a social phenomenon, while speaker meaning is, also by definition, an individual phenomenon. Therefore, a theory of speaker-meaning and a theory of conventional meaning can never be the same, as a matter of principle (although they can and should inform and constrain each other).
3. H&L widen the gap between animal and human communication, not only by making humans very different from animals, but also by underestimating the cognitive and communicative capabilities of animals, especially when they say that these are "[s]tuck in the immediate here and now" (see Emery and Clayton 2004 on certain food caching birds), "only have a small number" of vocalizations (see Kroodsma 2004: 122 on some song-

birds having repertoires of thousands of songs), which are "all intrinsically linked to an immediate [...] purpose" (cf. Pepperberg 2004 on Grey parrots).

4. H&L also notice that the exception-approach entails the derivation of a variable for an abnormality. Rather than an advantage, I consider this somewhat of a problem, as I have no trouble understanding *I barely passed* and *I failed although I worked hard* without being committed to even the existence of a particular abnormality such as being sick on the day of the exam; so the occurrence of some abnormality does not seem to be a necessary condition for the occurrence of an exception. Rather, the inference of the *possible* existence of an abnormality seems to be a defeasible inference itself (which it is not in H&L's approach, as far as I can see).
5. In their footnote 2, they actually cite one of a number of passages from *CoI* stating this, but they seem to simply have missed the point. In a way, their analysis consists of a return to the position of Fillmore, Kay and O'Connor (1988), the problems of which precisely motivated the alternative analysis in *CoI*.
6. In one of these two sentences, they insert the proviso that the functional difference between their examples (5) and (6) is "not obvious" to them. But one of the (repeated) methodological points in *CoI* is that such differences are often not obvious when one looks at sentences in isolation, and only become visible when one looks at what are and are not coherent ways of fitting a sentence into a piece of discourse; it is that kind of evidence that is adduced in *CoI* to make the point. H&L do not recognize the validity of this kind of evidence, claiming it contains a "logical error", but it is clear that this opinion is entirely based on the fallacy of assuming a 1-to-1 relationship between "sentence" and "inherent meaning" as discussed in section 2, so not a matter of logic but of assumptions about the subject matter.
7. In fact, they devote a separate section to a very general discussion of this topic. The issue has been discussed in many other places in a more adequate way than I could do here, so I will restrict myself to two remarks. First, H&L call the idea that constructions can be reduced to "deeper" principles that are not construction-specific, a "claimed achievement" of Chomsky's two most recent research programmes. However, if there is one thing that work in constructional approaches over the last 10 years or so has established, then it is tons of evidence that the claimed result is not at all achieved, ànd in fact inachievable, for all practical and theoretical purposes. The alleged reduction of raising constructions and passive constructions to a single non-specific rule "Move NP" or even "Move", a few other principles, plus some construction-like stipulations (cf. H&L's idea of some *syntactic* heads "subcategorizing" for specific *semantic* categories) to take care of the details, turned out not to generalize to many other constructions, and meanwhile one construction after the other was found that has demonstrably unique, so irreducible, and yet productive features. Second, for conceptual reasons to doubt the general desirability of the Minimalist approach, I would like to point here to work within the generative tradition that H&L adhere to (though not the two research programmes mentioned above), viz. Jackendoff and Pinker (2005) and Culicover and Jackendoff (2005), esp. chapter 1.
8. A possible misunderstanding I have encountered in discussions of this point is that perspective taking would be the *only* source of recursion in language. It is true that *CoI* does not mention other possible sources, i.e., in other conceptual domains. As a matter of fact, I think that there are other such sources, independent of perspective taking (e.g., the specification of locations or referents, as manifested in embedding of prepositional phrases and relative clauses). But these still do not create an "overall" potential for recursion of *all* kinds of phrases; rather, recursion is restricted to its own functional "niches" (see also Verhagen forthcoming.*b*).

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