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What Language Is

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I am honoured by the thoughtful considerations that have gone into all of the commentaries in this issue. Their respective origins in psychology, linguistics and philosophy indicate the ramifications and implications that the present research program is meant to have and the challenges it faces from all of these directions. It is only in this broader frame that the fruitfulness of the central hypothesis of the present framework can be demonstrated: that sapiens-specific thought rests on a linguistic foundation and grammatical distinctions mediate cognitive ones. Ultimately, this hypothesis raises the question of what language is – how it functions as a system, and whether or how it transforms the cognition of which it is an aspect. The point of the hypothesis, however, is to motivate new empirical and experimental research questions. **De Villiers** pointedly formulates one of them within developmental psychology: Do ‘children with autism fail to automatically “see” the world through language’? I agree with her that, in psychology more generally, ‘the empirical research to date has not focused on the kinds of questions that Un-Cartesian linguistics poses’. Theoretical linguistics, too, is not at present pursued as targeting the integration of language with a particular cognitive type, and as **Reboul** discusses, it still tends to separate language as such from the type of species-specific communication that it supports, a separation that the present framework urges to resist. None of this even touches on the deeper philosophical issues that **Wolfsdorf** raises, from the nature of meaning to the connection between language and metaphysics. These are barely addressed in my work (though see Hinzen & Sheehan [henceforth HS], 2015, chapter 9) and a fascinating enterprise to embark on in the future.

1 Articulating the framework

In his meticulously researched review, **Wolfsdorf** distinguishes functional, lexical and grammatical reference and suggests that I have ‘lapsed into an explanatory

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circle: grammatical reference is being defined in terms of grammar, and grammar in terms of functions such as grammatical reference'. However, the existence of grammatical reference as a natural kind is an empirical claim, not a definitional one: the claim is that the referential capacity discussed in the article depends on the existence of grammatical organization. The claim could be false, if, say, nonverbal humans or non-human species exhibited instances of the same forms, or if we saw different forms of such reference varying independently of forms of grammatical organization (for example, being regulated pragmatically instead). In short, such reference is grammatical by name and empirical fact, not definition. As for defining grammar, I take the standard view to be that reference is a 'semantic' function, not a grammatical one, often more specifically posited to hold between words and things, with grammar adding an only formally specified mechanism of recursion/Merge. Grammar in this sense is reference-free. That core grammatical relations such as Case or thematic roles are mechanisms through which different forms of reference are generated in language is, again, an empirical claim about the nature of grammar, which *The philosophy of universal grammar* (HS, 2015) and related work (Hinzen 2014) sought to substantiate.

Wolfsdorf also suggests that I recognize 'two types of descriptive content': lexical and grammatical. However, functional and lexical structures in nominal phrases cooperate in such a way that lexical-descriptive content provides a semantic condition on reference in the sense of a concept that identifies the relevant referent as belonging to a general kind, while functional structure in the edge of the nominal regulates the type of referential use to which it is put on an occasion of the concept's retrieval. This is the 'semantic' contribution of determiners, or the content of grammar, in this nominal case.

Crucial to the framework is not only that the forms of reference in question exist in a grammatical frame but that they exhibit an at least partial order (a hierarchy). In this regard, **Wolfsdorf** notes that along with grammatical differences between these forms of reference, there are a number of other features that distinguish them, which do not form a homogeneous set and appear, at first glance, difficult to synthesize. As he illustrates, this includes seemingly metaphysical features (e.g. abstractness versus concreteness; generality versus specificity; or denoting properties versus individuals); hybrid metaphysico-linguistic features such as indefiniteness versus definiteness; or even epistemological features such as being known or unknown. Yet in response to the general heterogeneity seen in almost any aspect of the natural world, we need to find systems that organize the diversity seen in a coherent fashion, making it intelligible to our understanding. It so happens, the claim is, that the *grammatical* specifications of the hierarchy do form such a natural system. That differences structured in this fashion will not also co-vary with other factors is not the claim,

and indeed this would be expected given how interwoven language is with cognition at large. How valid, on the other hand, the descriptive notions above are, is another question. For example, if abstractness vs. concreteness are said to be ‘metaphysical’, and they demonstrably are also both grammatical and lexical-semantic distinctions, how independent are our metaphysical distinctions really from linguistic ones? Aspects of grammatical organization in human language might be intrinsically relevant to the concerns of an epistemologist or metaphysician.

This can be illustrated with the grammatical first person, which in many languages including English involve overt lexical forms that involve no descriptive content whatsoever.¹ How epistemologically significant is this? Hinzen and Schroeder (2015) have pointed out that the absence of descriptive content implies that no act of reference involving such first person forms can involve the misapplication of a description, protecting such reference from failure due to misapplication of a concept (‘immunity from error through misidentification’, in traditional epistemological terms). While not questioning this argument per se, **Wolfsdorf** finds it ‘difficult to understand what kind of formal ontological object *I* could refer to’. This raises an interesting question about the connection between grammar and metaphysics. I do not see an empirical basis for suggesting that the shift from the grammatical third to the first person changes anything in whether a given NP in a given grammatical position is referential or not. It is equally a fact that forms of reference in the third and first person are distinct grammatically, semantically and epistemologically: it is not the case that one could be replaced for the other. For example, reference specifically to one’s body by saying ‘this body’ cannot be collapsed with saying ‘I’. Finally, it is a fact that referring to oneself as ‘I’ is not to give any descriptive features of whatever the object of reference here is – unlike in the case of ‘this body’ or ‘this man’, for example. Therefore, from the viewpoint of the formal ontology of grammar, the two kinds of referents are distinct. Does this mean they are metaphysically distinct as well, validating a form of metaphysical dualism (Lowe 2007)? This is a fascinating question in the realm of the interface between grammar and metaphysics, which unfortunately has been far beyond the scope of my own work to address (but see Miller 2014).

Wiltschko articulates a core prediction of my account in a slightly misleading way: ‘It appears that in ASD and SZ, reference is disturbed [...], which reflects a dysfunction of language and hence – given the [Language=Thought] hypothesis – amounts to a dysfunction of thought.’ I feel slightly uneasy of

¹ For discussion of other cases see Reichard (2013), Reichard and Hinzen (2016), and Hinzen et al. (2014).

reducing my framework to the programmatic claim in corner brackets above: the qualification in particular that many forms of ‘thought’ with different kinds of content – music, arithmetic, spatial navigation, etc. – may not depend on language, is crucial. **De Villiers**, too, makes the un-Cartesian claim unnecessarily strong by stating that ‘The further claim is that there is nothing either *before* [human-like thinking] in ontogeny or *like* [it] in a nonlinguistic mind’. The point is that our species has a cognitive type different from those of other species, even in our genus *Homo*, for which we need an explanation. But apart from that type of worry to do with formulation, I would prefer to state the prediction the other way around than **Wiltschko** does: ASD and SZ both involve a disorder of thought (virtually by definition, in the sense that they are neurocognitive disorders), and there is strong empirical evidence that they both empirically involve disorders of reference; given the un-Cartesian approach, then, these should amount to a linguistic dysfunction. As **Wolfsdorf** rightly puts it, ‘cognitive pathologies in humans should have linguistic symptomatology’.

Collins sees room for a more traditional position consistent with the un-Cartesian commitment that there is no ‘generative system of independently determinate thoughts’, and ‘propositional thought is largely structured by linguistic combinatory technology’. ‘Largely’, though, means ‘not completely’, on this alternative view, which **Collins** regards as the ‘standard position of generative linguistics’. Specifically, a substantial injection of pragmatics – i.e. extra-linguistic factors – is required ‘to fix the content of utterances on their occasion of use’. This position remains un-Cartesian in my terms, precisely insofar as the additional processes to yield ‘complete’ thoughts are pragmatic in **Collins’** sense: for if so, there is no syntax there, and the structural frame is in place. In different terms, the formal ontology of language – the potential for a thought system with referentiality and an ontology of objects, events, and propositions – is *set*. **Collins’** additional extra-linguistic factors remain formally unspecified, but they are postulated because of empirical considerations, not architectural ones. Since the ambition of the un-Cartesian program is investigating the neurobiological foundations of a cognitive type unprecedented even in the human lineage prior to our species, the addition of the envisaged pragmatic element to the claim that language lays the foundation of this type will not change the basic research hypothesis. My view here (Hinzen 2015) remains that the question **Collins** raises is empirical. While the weakened position may certainly be forced empirically, it should, in my view, only be adopted *if* forced in this fashion.

I will comment on relevant empirical data immediately below, but note first that the framework I outlined hardly amounts to a rejection of what **Collins** suggests is the standard position of generative linguistics. In its minimalist shape, the latter is a research program – it does not amount to any position.

This crucially includes the issue of the interface(s) between language and thought. Minimalism as such does not imply deciding this issue one way or the other, as Chomsky (2007) noted. As a sociological generalization, the Cartesian position is probably maintained intuitively by most formal semanticists as well as syntacticians. But it is rarely if ever explicitly discussed or defended (though see Jackendoff 2002), and Chomsky's own frequently re-stated commitment to the idea that a thought system arises from the operations of the computational system of language, without an independent Language of Thought (LoT) (e.g. Berwick and Chomsky 2011), lacks articulation: we need to *explain why* this thought system takes the form it does, i.e. how its empirical features (e.g. the referentiality that it inherently entails) fall out from the operations of language. Doing this is the ambition of HS.

2 Linguistic challenges

Let us now come back to the empirical facts that **Collins** adduces (though I stress that none of the following judgements can be made as lightly and require detailed empirical and cross-linguistic investigations). Their purpose, in **Collins'** exposition, is to prove the point that extra-linguistic (specifically, extra-grammatical) factors determine propositional meaning. Beginning from those of most relevance to what I was discussing in the article, **Collins** claims that (1) has an existential construal, equivalent to (2), which, if true, would entail that bare plurals in English do not map obligatorily to the generic reading seen in (3), creating an apparent mismatch between grammar (nominal syntax) and propositional interpretation:

- (1) Dogs are in the garden
- (2) Some dogs are in the garden
- (3) Dogs are four-legged

In response, I note that I have not claimed that bare plurals do obligatorily map to the generic reading. Indeed, (4) is immediate evidence to the contrary:

- (4) I saw dogs in the garden

(1), on the other hand, is less clear. It has the generic reading of *Dogs are in the garden, cats are in the house* (e.g. stating a rule), but it is not clear that it has the

existential reading of (2), and even if it does, then (5) (apart from (2)) is definitely a more natural way of stating this reading, which in turn strongly disfavours the rule-reading:

(5) There are dogs in the garden

If so, the example illustrates a classical un-Cartesian point: change the grammar of a sentence, and you'll change its propositional meaning. There is no synonymy, in grammar, ever: if (2) has an overt determiner, and (1) does not, available readings change.² (2) is grammatically different from (1), as is (5). These doubts continue with his example stated here as (6):

(6) A mini is black and yellow

Collins states that 'we naturally read [6] as being about some particular mini rather than minis as such, for minis are not so generally coloured'. But in the reading **Collins** has in mind, where a particular mini is intended, (7) is way better than (6):

- (7) a. Some mini is black and yellow
- b. There is a mini (here) that is black and yellow

(6) is more naturally read generically (in which case it would be false). Note further that Collins (problematically, on my view, as argued above) states that the difference between (1) and (3) that he envisages – i.e. (1) is existential, (3) generic – depends 'on the predicate being either stage- or individual-level'. If so, rather than making a point about the extra-linguistic influence on the fixation of propositional meaning, he directly specifies the linguistic factors involved. The referential hierarchy I formulated never entailed that NP structure viewed in isolation determines the nature of the referential act – it always is NP-structure together with the sentential configuration (see further Martin and Hinzen 2014). Collins' point, if correct, would thus only show that the difference between existential and generic uses of NPs depends on the sentential configuration as

² The exclamative version of (1) (e.g. a person shouting *Dogs are in the garden!*, where stress shifts to *Dogs*, may favour the existential reading. Hans-Martin Gaertner reminds me of the 'joke' in *Dogs must be carried* (Halliday, see Gussenhoven 2008: 94) as sign on the underground train, meaning either *Dogs that aren't carried aren't allowed* or *People without dogs aren't allowed*, suggesting that the existential reading is available to some extent with stress on *dogs*, similarly to what is seen in (5). **Wolfsdorf** (fn. 36) notes a similar difference in meaning arising from whether the indefinite determiner *some* is present or not.

a whole and what factors are involved in it. Indeed, stage-level vs. individual-level is not merely a lexical distinction, since many lexical predicates can be used in both ways, in the right grammatical configuration: for example, *I am red* can mean that I got a sun burn earlier that day, but it can also mean that I am red by nature (in terms of skin colour, political convictions, etc.) (see also Raposo and Uriagereka 1995).

This brings us to the second range of examples, more lexical in nature, that lead **Collins** to think that linguistic structure does not suffice to fix propositional meaning. *A red pen*, he notes, may ‘merely look red, given the lighting, or be classified as red due to the draw it belongs in’. Grammar, for sure, does not tell. I agree (Hinzen 2015). But this is the point: where grammar does not reign, context rules: *red* as such, irrespective of context, doesn’t refer to anything, virtually, not even a determinate general concept.³

3 Challenges from psychology

De Villiers is helping me to predictions for the one species of humans that I am not actually researching at the moment: neurotypical children. Will infants faced with nonverbal tasks such as individuating the instances of a kind or interpreting the thematic structure of an event behave differently depending on their progress with language? Will adults form different implicit concepts of what makes structured events ‘similar’, when the participants and actions are perceptually different but fall under the same linguistic descriptions (e.g. *Dog bump car?*), in dual task designs where their language faculty is not at their disposal? Is the same true in the absence of dual task designs of children younger than 2 years, who did not yet master the syntactic complexities of reversible event descriptions? Negative answers to the last two questions from recent experimental work of de Villiers (2014, 2016) and Shukla (2016) to the last question (and see Nordmeyer 2011), imply that the relevant tasks are in fact misclassified as ‘nonverbal’: they require verbal, not nonverbal thinking. In my own terms, these research questions and experiments precisely address to what extent the intuitive formal ontology of the world, whether in the case of objects or events, is linguistically configured. The idea that we first configure our thoughts non-linguistically via some formally unspecified mechanism, and then ‘map it onto’ a

3 Applied to **Collins**’ (3a), *Every student passed the exam*, this means what counts as a ‘student’ will contextually vary, while I take his (3d) to be a classic case of structural ambiguity, though argument structure is of course in part lexically encoded as well.

linguistic surface, may be a myth. Almost all of the work cited by **de Villiers** (see also Newton and de Villiers 2007) beautifully illustrates how an empirical agenda speaks to a foundational and abstract idea.

I can strongly relate to the worry that the un-Cartesian position becomes untestable when we cannot find experimental paradigms that manage to disentangle language-guided cognitive processes from others that may only be lower-level perceptual and behaviour-oriented. Implicit theory of mind (ToM) type of tasks shown to be mastered by very young children just seem to fit the bill, yet **de Villiers** notes recent evidence against the replicability of these early findings (Kulke et al. 2017). Independently of that, it is not clear which cognitive process, exactly, implicit ToM tasks with looking time as an outcome measure really tap into. In a formal fashion, putative cognitive processes underlying different ToM tasks have to my knowledge never been described – in the way, say, that the mental process of constructing a recursive sentence has been (the project of modern generative grammar). Typically, researchers simply use their own language faculty to provide a verbal description of the stimuli, e.g. *Sally thinks her marble is in the basket*. Based on these, reasoning of experimental subjects is then thought to unfold. But does this structure depict the thought of a 7-month old infant accurately? Can we ever fully tell? There is reason to believe that, in principle, we will not: any perceptually presented task will always underdetermine the concepts that we employ to cognize it, since thought is not controlled by what we see – a consequence of its inherent intensionality (with an ‘s’). Based on the very nature of thought, that is, a margin of doubt on any claim to the effect that a human being has a certain thought, when and as long as this thought is not and cannot be rendered verbally by that being, will always remain. For now we simply do not know what the cognitive mechanisms are that are said to fall under the label ToM, and how minimal they might be, perhaps failing to involve a notion of ‘mental state’ at all (Heyes 2015; Apperly and Butterfill 2009; Heyes and Frith 2014).

The worry should affect the Cartesian theorist as well: it is him who wants to *disentangle* two systems that the un-Cartesian predicts are necessarily *entangled*, once thought is conceptual, intensional, and referential in the sense described. Yet the systematic implication of non-aphasic language anomalies in cognitive pathologies can provide relevant evidence. Whatever cognitive process ToM tasks tap into, and however ill-described it may be formally when language is not presupposed, we can look at whether performance on such tasks ‘marches along’ with language development, as **de Villiers** puts it. Will completely nonverbal children and adolescents with autism master even implicit false belief tasks? This seems highly unlikely. While this deficit could then be credited to their autism or intellectual impairment rather than their lack of language, this

question can be addressed by asking whether IQ- or language-matched non-autistic individuals will fail the same tests.

Wiltchko raises another possible area to explore more specific predictions of the un-Cartesian hypothesis in the context of mental disorders. As she notes, reference is a deeply social phenomenon: it ‘doesn’t stop at establishing a relation between a concept and the world (by identifying a particular individual or proposition)’. This illustrates why the study of reference in populations such as people with ASD or SZ is vital: both disorders involve deficits in what can broadly be called social cognition, and ASD is virtually defined by these deficits. Languages however differ in how aspects of discursive interaction are encoded syntactically. How do social communication difficulties in these populations pan out linguistically, then, when we compare languages with different morpho-lexical and morpho-syntactic resources, such as confirmationals and other sentence final particles, discourse markers, or evidentiality? How do cognitive disorders affect language use and perception at the level of prosody and intonation? There is not only a dearth of serious linguistic work on schizophrenia, but of cross-linguistic work as well.

4 Language and communication

Reboul takes things in a new direction inspired by her own work (Reboul 2017), which conceptualizes human language as grounding sapiens-specific thought rather than merely being a tool of communication, more in line with a traditional postulation of a Language of Thought (LoT). However, **Reboul** simplifies and partially mis-states my account as resting on the claims that: ‘a) language is thought; and b) language corresponds to the external languages that we use to communicate (Chomsky’s E-languages).’ For one thing, having language in place, the human mind becomes capable of taking things further, probing other ‘languages of the mind’ such as mathematics, music, or logic. These, while on my view resting on the foundations that language lays, can take us further into mental spaces that may not be governed by linguistic laws and clearly have different kinds of inherent contents (lacking referentiality, and arguably lacking content altogether in the case of logic). Secondly, point b) invokes the distinction between I- and E-languages that does not figure in the conceptual framework laid out here. Sheer endless discussions have taken place in philosophy and linguistics over the exact meaning of this distinction and its implications. Rather than furthering this debate, which I distanced myself from since Hinzen (2007), I note here that there seems to be no evidence of the existence of any human-like minds that have language figuring in them

internally, yet do not externalize it as speech or in some other way (selective mutism would not prove the contrary). Abstracting from these other ways, language is implemented in the brain as a speech circuit: the standard picture of left-hemispherically lateralized perisylvian pathways connecting superior temporal and inferior frontal areas in the brain are interwoven with the sensory-motor integration that inherently characterizes speech. This suggests that the linguistic species we are, inherently, is a communicating one as well, with its mode of thought manifest as speech. What is ‘I’ and what is ‘E’ here? Could noun phrases exist ‘internally’ but not ‘externally’? Would they be referential in this case? If they were not, would they be noun phrases? The framework I have outlined has not so far required me to answer these questions, whose meaningfulness is not clear to me.

What is empirically assertible, however, is the triangular nature of reference in either language or thought: it is *my* thought that I think, identifying myself in 1st Person, where the thought (i.e. any thought) can be different from that of an interlocutor identified in 2nd Person, and is necessarily about something distinct from us both, the 3rd or non-Person. This triangulation is both inherently grammatical (since Person is, along with Tense and other such inherent features of thought) and it plays itself out in the public domain, where speech acts take place at times and as uttered by people. The I-vs.-E distinction again does not seem meaningful here. **Reboul** thinks that at best the second person gives us an external dimension, but a thought in first person without a content, and hence the third person, viewed as external to my first personal world, is not a thought; and if the thinker cannot fathom that it is *his* thought, possibly without it being a second or third person’s thought, could it be a thought at all or be recognized as such? This three-fold Person distinction appears to act as a system: we cannot abstract the first person out from it, identifying a radically privative mode of thought that does not seem to be the species-specific one.

Any teacher of Linguistics-101 type courses will realize the need to make students understand that language is not the same as communication, a critical insight foundational for modern linguistics as such. But having digested this insight, it is in my view timely now to concede that, with language, and apparently inherently and exceptionlessly, goes a form a communication that is as sapiens-specific as language is. Communication in that sense and language are inseparable and come as a unit. This is where pathologies can teach us so much: where this species-specific mode of communication is disturbed, it is in *linguistic* (or language-related gestural) communication that we see this happening. People with autism do communicate: it’s the species-specific forms of communication, which involve language inherently, where we see a disturbance.

5 LoT or no LoT?

Against this, **Reboul** champions a LoT type view, conceding that whatever LoT we are going to postulate in the human case, it is going to be different from the animal case. This raises the immediate question of which neurocognitive change triggered this step change in evolution, if that factor was not language, and it deprives us of a potential advantage of the LoT hypothesis, namely that it provides continuity between linguistic and non-linguistic species. The more the human LoT becomes language-like in its internal structure, moreover, the more we would have to ask: What makes it different from language? A language is a thought system, inherently. We cannot talk without producing thoughts, and indeed communicating them. So what distinction does **Reboul** insist in making, if she pulls these apart, suggesting that ‘LoT is for thought and external languages are for communication’? Thought, language and communication (in their sapiens-specific forms) universally co-occur in humans, factually; for some, their co-occurrence is a conceptual necessity as well (Davidson 2010).

6 ‘Concepts’

‘Concept’ is a problematic term in current discussions, used widely to stand in for whatever the author’s preferred notion of ‘meaning’ is. The notion of ‘word’ improves things, since words have identifiable empirical properties which can be studied in a controlled fashion. Since they have meaning as part of what makes them the words they are, I am also calling them ‘lexical concepts’. **Reboul** concurs that these as such only have general meaning, with forms of referential meaning only arising within relevantly complex grammatical configurations occurring in utterances taking place in contexts. But she also introduces the notion of ‘decoupling’ the concept from the referent, and states that in the referential hierarchy I describe, ‘decoupling and reference are in inverse ratios: the more referential a term is, the less decoupled (even weakly decoupled) it is, and the less referential a term is, the more decoupled it can be’. She notes that this suggests that decoupling as such should not depend on grammar: even if grammar is crucial to reference, decoupling is not, which she argues evidence from animal cognition on planning supports.

In response, I note that a conceptual capacity (having lexical concepts) in my sense requires both more and less than ‘perceptual independence’ in **Reboul**’s sense: less, because lexical concepts correspond to categories, I suggest, which are rooted in our perceptual capacities, which in turn are

partially shared with other species.⁴ Concepts corresponding to such categories remain perceptually dependent in their lexical *content*, even after they are lexicalized – they are never ‘decoupled’ in this sense. What changes is their grammatical meaning, and it is the latter, not the lexical concepts, which are perceptually *independent*. Referentiality and perception are independent dimensions. Grammar lies between them, turning lexicalized concepts based on perceptually rooted categories into referential expressions on an occasion of use. Grammar can only function in this way if it can access categories in a way that does not depend on perceptual stimuli: we can think about anything we like, at any point (which is presumably false even of planning in animals). It therefore presupposes lexicalization, a crucial reformatting step logically required for reference in the propositional, i.e. ‘disinterested’ (non-action related) sense that **Reboul** identifies, which in turn presupposes grammaticalization, i.e. adding a functional-grammatical ‘edge’ to the lexical concept in question.

Reboul is right that, in theory – though not in empirical fact, as noted above – an edge in this sense would not need to connect with any externalization at all, i.e. it could have led to a fully *internal* recursive system used independently of communication. A language/LoT system would then have evolved to which triangulation does not matter. The problem I have with this account is that the triangulation *does* matter: here is where pathologies are telling. Where the deictic frame is disturbed, the content of both thought and language change. In the schizophrenic case, truth and objectivity, which are inherent conditions of thought in the human-specific sense, are lost. If this aspect of the triangulation did *not* depend on language, **Reboul** would be right that we could go the old way: accept a language-like LoT, separating a Merge-based I-language from both communication and E-language. Truth could then be grounded in perception, as **Reboul** speculates, rather than in linguistic communication. But here we face the old problem: This will not do without an explanation of why other species with similar perceptual capacities lack the rationality and the thought system that we do. Whatever the way in which objectivity and truth are ‘grounded’ in perception, a difference remains with how truth figures in speech and the specific form of thought it conveys.

Overall, whether there is a problem for the LoT hypothesis to solve depends on what we think the language system is and what functions it involves. Here, for me, the discussion should be centred: how does language function

⁴ I do not see the alternatives, although the view is entirely open to the existence of ‘foundational abstractions’ in e.g. Gallistel’s (2009) sense, and I also suggest that other content can come from language and can lexicalize.

cognitively? The question is barely on the research agenda, especially with regards to grammatical organization. If language carries out the cognitive functions that I hypothesize it does, **Reboul**'s disqualification of language as 'merely for communication' loses motivation: There is no need to go to a still only obscurely identified and formally undescribed 'thought' system, which is paradoxically meant to be both very language-like yet different from the only language system we effectively know: language as such.

One of the deepest reasons for staying on the more conservative LoT-oriented side is a linguistic relativity scare: with only language left, how variable will thought be? A critic may grant the distinction independently assumed in much linguistic theory between lexicon and morpho-syntax, on the one hand, and grammatical relations, on the other. Yet I recognize that morphosyntactic variation may not be 'innocuous for an un-Cartesian view', as **Reboul** states. By postulating a universal LoT-style thought system unaffected by the vagaries of its externalization in different languages, we are seemingly out of this trouble. But at what cost? The problem of separating what is 'externalization' from what is 'language' is still with us now, as is the problem that the human-specific LoT remains unexplained; and language only is the explanandum that I think it is, if it involves externalization.

7 How promising is a new linguistic lens on autism spectrum disorders?

Durrleman asks whether the relation between grammatical competence and ASD is as strong as I envisage, mentioning a recent 'form is easy, meaning is hard' claim championed by Naigles and Tek (2017). These authors argue for 'generally good (or even intact) grammatical usage and development in children with ASD'. However, they equally emphasize that 'few extant studies of children with ASD actually include the relevant comparisons between language form and language meaning'. They also note evidence against their hypothesis from two studies (Tek et al. 2014; Park et al. 2012). Since these involved children with cognitive impairment, they subsequently restrict their claim to children with ASD without such impairment, thereby excluding almost half of the ASD spectrum. Negative evidence also comes from a third study of Eigsti et al. (2007), who found lower grammatical complexity in an ASD group matched with non-ASD groups on nonverbal IQ (see also Pierce and Bartolucci 1977). Naigles and Tek (2017) here comment that this finding may be due to the children with ASD talking 'less frequently about the kinds of situations that might elicit, for

example, past tenses and questions’, giving them less *opportunities* to show their mastery of grammar, rather than manifesting lesser grammatical capacities. However, the opportunities for producing such structures were presumably the same among the groups, hence lesser production of such structures still needs to be explained. A difficulty with producing such structures is a natural explanation, as Eigsti et al. (2007) discussed. An alternative is difficult to consider. For example, representing a past Tense is representing a relation between the utterance, event and possibly a reference time. The structure of such mental representations is linguistically well described. A non-linguistic alternative, such as a deficit in ‘mental time travel’, remains more vague and formally comparatively undescribed at present.

Positive evidence for Naigles and Tek’s hypothesis is mixed and needs to be carefully qualified. For example, positive evidence relying on crude quantitative measures like the growth of the mean length of utterance in the development of ASD and Down syndrome children (Tager-Flusberg et al. 1990) will ultimately be limited in force. Evidence on similarities in TD and ASD in the order of acquisition of morphemes from Waterhouse and Fein (1982) and Park et al. (2012) is in contradiction with evidence from another early study by Bartolucci et al. (1980). Some tasks purporting to assess syntax in ASD such as adding plural markers to nonsense nouns (e.g. Naigles et al. 2011) leave out the meaning dimension that all grammatical marking has in ordinary language use and which is critical to the present approach. The same applies to evidence for intact patterns of statistical learning of artificial grammars in ASD (Obeid et al. 2016), since these are devoid of meaning equally.

The most serious problem, however, with the ‘form is easy, meaning is hard’ hypothesis is a conceptual one: it only makes sense if meaning is independent of form. This is debatable for lexical meaning and unlikely to the extreme for grammatical meaning, i.e. the meaning expressed by grammatically structured utterances, since it would entail that all such meaning is available to a cognitive system no matter whether it masters principles of linguistic form or not. School-age children with ASD without language are one relevant population in this respect, which suggests that most of meaning and thought is in fact wiped out in this case (Norrelgen et al. 2015). Put differently, the ‘form is easy’ claim can’t be circularly based on a vision of syntax where syntax is completely meaningless – i.e. irrelevant to what types of meaning our mind can grasp. Indeed, on such a vision, the present view would *concur* with the ‘form is easy’ hypothesis. The level of grammatical meaning is crucial to the present approach, where grammar is not viewed as something free-floating of its use; this level is not considered by Naigles and Tek (2017), who only consider (i) pragmatic meaning, (ii) lexical meaning, and (iii) syntax.

As for (i), the authors are surely right that ‘a successful conversation with a social partner is not possible if one is not able to decode the intended meanings of words and utterances’, and hence that a difficulty in pragmatics widely granted to be universal in ASD will predict difficulties with conversational language. However, ‘decoding the intended meaning of words and utterances’ also requires mapping their grammatical meanings. A foreigner learning a new language will find it enormously taxing to follow jokes and non-literal speech, when he is still struggling even to catch the literal meaning. That difficulty is then clearly not due to a failure of pragmatics. What cognitive dysfunction triggers a failure in processes loosely described as pragmatic is an entirely open question: pragmatics is a descriptive, not an analytic category, and not necessarily an explanatory one. As Wittgenstein famously noted, ‘intended meanings’ are not things that can be perceived. But deviant uses of words in particular contexts can be, like a speaker saying that John is a dog. Not penetrating the speaker’s mind to identify an invisible ‘intention’ is required to understand the intended meaning here, but understanding what *dog* on this occasion and in this grammatical configuration is taken to *refer* to: not an actual dog, but a man who behaves in a dog-like way. *John* is the referential expression, *dog* part of the sentential predicate, which is why the speaker refers to a dog-like man, not a man-like dog or both a man and a dog. It thus seems natural to hypothesize that a difficulty characterized loosely as pragmatic could have its roots in this mechanism of detecting what refers to what in fluent speech. Is there evidence for a pragmatic impairment independent of a referential impairment in ASD, when it looks as if the two have to go hand in hand?⁵

Some studies have provided evidence for them going together. For example, as Naigles and Tek (2017) note, even high functioning adults with autism or Asperger syndrome use fewer referential expressions and form sentences that are not linked in a temporal order while narrating a story (Colle et al. 2008). Use of referential expressions and of tensed verbs is to use phrases of particular grammatically identified classes. In our study of narrativity in high functioning ASD (Schroeder et al. 2017), overall narrative scores correlated with such grammatical measures. Claims cited by **Durleman** about ‘subgroups with spared grammatical profiles’ are problematic, when such claims are necessarily relative to the standardized clinical language measures used, which often do not involve narrative tasks, in which language impairments can appear even when they don’t show in standardized measures (Norbury et al. 2014). The claims become circular when ‘grammar’ is identified from the beginning to simply *exclude* aspects of language

⁵ See Davies et al. (2016), for similar doubts in rooting pragmatic difficulties in a social cognition rather than language impairment.

contradicting the predictions, such as noun phrases, which are an inherent aspect of grammatical organization and as such go with certain kinds of uses. How can the understanding and use of NPs *not* be affected, if pragmatic difficulties are universal in ASD, and they involve grasping what NPs refer to in context?

A referential disturbance linked to grammatical organization ties in well with the early claim that reference often becomes too rigid in autistic language, featuring denotation but not connotation – the kind of flexible and generalizable meaning that is factored into normal NP use (Fay and Schuler 1980; Boucher 2012). It also connects with one of the most classical disturbances in autistic language, namely reversals of personal pronouns, which simply are NPs positively specified for grammatical Person. **Durleman** points out that misuses of personal pronouns may be less frequent than sometimes assumed, and that they are also found in typical development, where they clearly do not correspond to atypical forms of selfhood. Nonetheless, extended difficulties with *I* and *you* beyond the few months where we expect these in TD would make any mother rightly worried; and the pattern also may not be quite the same in TD and ASD, with a preference for 3rd Person forms of self-reference is seen together with personal pronoun reversals (Jordan 1989; Lee et al. 1994; Mizuno et al. 2011). These would seem even more worrisome to such a mother, and the recent finding of such personal pronoun avoidance patterns *across* the oral and visual modalities should certainly make us think (Shield et al. 2015).

8 Do mind-reading capacities in non-human primates challenge the un-Cartesian hypothesis?

Is the un-Cartesian position threatened by evidence from non-human species passing false belief tests? As discussed above, the first question here should be what this means, and what is at stake. I will assume in what follows that what is *not* at stake is that apes, ever, have a concept figuring in their mind, equivalent to our concept *belief*, which figures in an explicit thought of the form *He believes she is smart*. Rather, I will take it that the notion of passing a false belief test is operationalized simply by the criterion that certain choices are made in certain conditions. That these conditions are labelled as the ‘false belief’ and ‘true belief’ conditions says little as such about which neurocognitive mechanisms are actually involved. The labels reflect descriptive choices of the verbal creatures that are the experimenters, which are questioned when other mechanisms are equally plausibly involved, which do not involve the concept of belief.

If no explicit concept of belief is involved, no concept figures as part of a thought whose identity conditions are partially determined by what exact concepts it contains. If so, whatever thought takes place is different from the human type, and it is not clear whether an issue even arises in the present context. But let us ignore this problem here and address the evidence from non-human primates. Buttelmann et al. (2017) found a variety of non-human primates behaving very similarly to 16- and 18-month-old human infants in a ‘false belief’ task requiring an active behavioural response (Buttelmann et al. 2009). This new study comes after a sequence of others by the same group that failed to find evidence that the great apes can pass false belief tests (Call and Tomasello 1999; Kaminski et al. 2008; Krachun et al. 2010). One other study by the same group (Krupenye et al. 2016) tested great apes on an implicit ToM paradigm using anticipatory-looking time as an outcome measure, finding similar performance as in 2-year old humans (Southgate et al. 2007). Yet as Buttelmann et al. (2017) explicitly note, in the Krupenye et al. study, ‘apes could have passed the test simply by predicting that the actor would go to the last place he saw the object’. If so, the study in fact leaves open whether any concept of belief, or of false belief, was involved. The question, therefore, reduces to whether the Buttelmann et al. (2017) study fundamentally changes the situation that apes do not understand false belief.

Buttelman et al.’s (2017) finding is that apes facing an experimenter (E) who has hidden an object in one box (box 1), will tend to help him open *another* box (box 2), in which the object was re-located in his absence, when they see him struggling to open box 1 upon returning to the scene. If the object is relocated in his *presence*, on the other hand, the apes chose randomly between the two boxes. This asymmetry paradoxically suggests that the apes can make sense of *false* belief, but not *true* belief, which raises a number of questions. One immediate way of making sense of the results in the false belief condition, however, is based on long-standing evidence that apes can track goals (Call et al. 2004) and are aware of intentional and attentional relations of agents to objects of their interest. Based on this, the ape might ‘reason’ in the false belief condition: 1. E returns to retrieve his object; 2. But the object is now elsewhere; 3. Therefore let’s help him to open the box where it is. The concept of belief does not enter. In the true belief condition, by contrast, the agents’ actions ‘make no sense’ in this light: if E wanted his object, he would go for it, since he has seen where it now is; but he goes to the other box nonetheless, struggling to open it. To what purpose? Lacking an answer, the ape’s choice becomes random, which makes good sense.

This conclusion, together with (i) scepticism from other groups regarding the understanding of beliefs in apes (Penn et al. 2008), (ii) longstanding methodological problems discussed by Heyes on finding evidence for animal mindreading (Heyes 1998, 2015), (iii) the methodological problems mentioned initially in this

section, (iv) recently emerged doubts about the language-independence of even *implicit* false beliefs in young infants including the findings of Southgate et al. (2007) mentioned above (Kulke et al. 2017), and (v) the general incapacity of apes to understand even imperative forms of referential meaning, never mind reference in false belief contexts (Tempelmann et al. 2013), I conclude that the case of false belief reasoning in non-linguistic primates is weak.

9 Conclusions

The un-Cartesian hypothesis lays out a framework inviting new predictions for how language and human-specific cognition are linked. These are testable in different clinical and non-clinical settings, and they have implications for our understanding of both language and mind at a foundational level, exploring the cognitive function of language. The hypothesis may also help addressing such open issues as the brain basis of reference as such, and how close the explanatory circle is in which grammar, reference, and mental pathologies connect.

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