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Finding Signatures of Linguistic Reasoning

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Finding Signatures of Linguistic Reasoning

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Hinzen lays out the platform of un-Cartesian linguistics, and the ramifications threaten widespread beliefs about the relations between language and thought. The theoretical story is compelling but my commentary will address my concerns as a laborer in research.

A coherent account is presented by which human language makes possible lexemes, combinatorial concepts, propositional attitudes and intensional reference, and these together constitute human-like thinking. The further claim is that there is nothing either *before* them in ontogeny or *like* them in a nonlinguistic mind. Only humans with language can think in these ways. But theoretical coherence is not sufficient without empirical testability. The nature of thinking with and without grammar must be specified further. In particular, symptoms of such thinking must be identifiable without using language *as* the evidence.

The first issue concerns a signature of thinking with lexemes. The nature of concepts is one of the most fraught questions in the cognitive sciences. Categories exist in the world, and organisms recognize them as perceptual classes, but human concepts are not *just* perceptual classes (Ahn et al. 2001). How concepts develop is much debated (Mandler 2004; Carey 2011). Though infants may be able to tell one object apart from another at a very early age, and even form perceptual classes of like kinds (Quinn et al. 1993), there is a signature change at around the age of the first words. Xu and Carey (Carey and Fei 2001; Xu 2007) introduce the philosophical term *sortal* to capture an awakening around that time that permits infants to represent entities (ball, truck, Mama) as countable individuals, even while out of sight. That is, only at around 11 months can infants form an abstracted concept of an object that allows, among other things, the sharp separation of one entity (say a ball) from a different one (say a truck) while they come and go from behind a screen. Prior to that point it is as if infants seeing each toy register “Aha! A sortal!” without finer discrimination that would allow the computation that “there is at least a car and a ball behind the screen”. It is tempting to conclude that the change in children’s thinking at this time is a reflection of the language faculty coming on board to represent concepts that are relevant for language, namely the beginning of Hinzen’s *lexemes*.

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Unfortunately for this idea, monkeys can apparently pass the sortal task, thus without a language faculty (Uller et al. 1997). Does this mean that language is merely a “help” to sortal differentiation for human infants? In my view the results need re-examination. For example, might the sortal task for monkeys be too weak, given that it focuses entirely on their persistence in retrieving different foodstuffs? If monkeys are not interested in monitoring the identity and numerosity of other kinds of objects, does that not tell us something significant about the divide from human infant intelligence?

Lexemes are by definition part of human language, and there is nothing equivalent to them without language. Only a creature that could name the sortals, or be affected by their names in computing their numerosity, could be said to have lexemes. I would prefer to find a signature difference in the reasoning that human infants versus monkeys can engage in by virtue of lexemes.

Take another example. Verbs are also lexemes. Another fascinating finding about infants late in the first year is that they are starting to recognize the essential properties of actions in scenes. Gordon (2003) showed groups of infants repeated scenes of one of two nonverbal events: *hugging*, and *giving*. Then he asked if they would dishabituate to the videos when something changed: a teddybear that was in the actor’s hand. In the event of *hugging*, the teddybear was an accidental accompaniment, an adjunct, not part of the thematic roles. Infants did not respond to its removal. But in the *giving* scene, the teddybear was the object being given, so part of the thematic roles, and infants dishabituated, recognizing that it was no longer the same event. Could this prove to be another signature of infant cognition guided by developing lexemes? I predict that the effect would not occur at all with another species, though one would need to choose the events carefully to be of relevance. This could be wrong, but the theory needs such predictions.

A second issue of empirical interest is that of concept combinatorics without language. Lexemes have the essential property that they combine grammatically, as in “brown cow”, and the grammar dictates what the combination will mean. The crucial question then is whether any system other than the human language faculty can handle *concept* combinations, and if so, what the limit is.

Procedurally, a common procedure in cognitive science is it to ask adult participants to engage in verbal shadowing while they simultaneously engage in another form of reasoning or concept formation. Since verbal shadowing essentially ties up the language faculty, one can see if the “rest” of the human mind can do the task without it. To the extent that there are interference effects on

some kind of thinking but not others, one can infer that the thoughts require access to the language faculty (e.g. Hermer-Vazquez et al. 1999).

Dual task investigations in my lab have looked at implicit concept formation under verbal shadowing. In implicit concept formation, a participant merely watches pairs of static stimuli until one of the pair animates. Over trials, participants begin to look in anticipation at the one that will animate. An eyetracker tracks anticipatory eyegaze to investigate whether adults can acquire a rule governing the similarity across a class of events, with no explicit instruction. The target stimuli of relevance here are a class of transitive, reversible events sharing a common description such as “Dog Bump Car” (de Villiers 2014, 2016). The stimuli portrayed were all different dogs and cars, and their opposites: events of “Car Bump Dog” are not the target.

Crucially, to discover the rule linking the trials one must notice which is the agent and which is the object. Fundamental sentence structure is involved, namely the differentiation of hierarchical position of subject and object. It is usually assumed that this fundamental distinction would be established in the absence of language, then language mapped onto it. *Surface* language must entail a mapping, because languages differ in word order: SVO, OVS, SOV, etc. The question Hinzen’s work inspired me to ask was, is there anything non-linguistic at all about such concepts, or is language critically involved in this conception of event classes? In three different studies with different transitive events (de Villiers, 2014, 2016), adults who are not shadowing start looking at the targets in anticipation after three or four trials. However, adults engaged in verbal shadowing could not learn the rule after 30 trials.

The core of the theory concerns infants. The assumption in developmental linguistics is that the preparation even for sentences is being laid conceptually in the first year of life. But un-Cartesian linguistics posits that “There is no time when humans only ‘think’ and then eventually they also have language” (p. 21). Infants younger than a year have human minds, and language faculties. Is that enough to impose meaning on scenes, or does some surface grammar need to be developed?

Toddlers at 1–2 years have not yet solidified the surface forms for their language, but head direction is likely to be one of the first parameters to emerge (Wexler 1998). On the other hand, maybe the first sentences mark the point that linguistic representation for events arises, and first sentences very, very rarely take the form of reversible transitives. In fact, comprehending the way sentences refer to any of these reversible events appears to be a two-year-old achievement (de Villiers et al. 2016). The appropriate language then might not be present before age 2.

Shukla (2016) has tested infants and toddlers from 12–24 months on the “Dog Bump Car” task in an eyetracker (without shadowing!), and there is no sign that they can recognize the rule tying the events together. Of course it is necessary to prove that infants and toddlers can form implicit rules at all in this paradigm before deciding on the interpretation of the failure, and work is underway to test that they can attend successfully to more perceptually based rules. If they can, it seems plausible to conclude that the conceptual distinction between one set of transitive events and its opposite is actually only possible with the emergence of grammar. Monkeys and apes should not be able to do this even with operant training, but no one to date seems to have tried. The limited work on event recognition in the great apes suggests it is not easy.

In my lab we have pursued the question: what is unique about the concepts for which access to the language faculty is required? Jung and Hummel (2013) have suggested “relational concepts”, as have Gentner and Kurtz (2005). With the same dual task paradigm, what kind of implicit rules can adults form while shadowing? Nordmeyer (2011) demonstrated that it was easy for adults even with their language faculty tied up to recognize the difference between (extremely varied) natural kinds (stone, banana, waterfall, tree) and artifacts selected to resemble them perceptually (dime, shoe, fountain, statue). Margulis (2014) demonstrated that shadowing adults could very easily distinguish two sets of faces, each tied together only by family resemblances among their features (the Smiths versus the Joneses).

However shadowing adults fail everything that has anything remotely propositional about it. They not only fail to recognize “Dog Bump Car” versus “Car Bump Dog”, but they fail to form the class of “Not p” versus “p” (Nordmeyer 2011), and they fail “black cat and white dog” versus “black dog and white cat” pairs (unpublished). Floyd (2014) demonstrated that English speakers who were not shadowing could separate out a class of *telic* events from their *atelic* counterparts quite readily, rather surprising as the linguistic marking of telicity in English is scattered and unreliable. Nonetheless, the rule was there to be recognized by at least the mature language faculty. When adults were engaged in shadowing they failed to spot the similarity across trials.

In sum, there are ways to discover whether minds are sensitive to equivalences at the level of “concept combinatorics”. We have not found one yet that can be recognized even with an implicit measure like looking time, when the language faculty is tied up. But the empirical work must be extended beyond a single procedure, and new techniques are needed.

The third comment relates to the representation of Theory of Mind. For twenty years I have argued along un-Cartesian lines that propositional attitudes are inseparable from their linguistic representations (de Villiers and de Villiers

2000). Newton and de Villiers (2007) showed that tying up the language faculty with verbal shadowing rendered typical adults incapable of predicting the right ending for a nonverbal false belief task. We took the strong and less popular (read: hard to publish) position that this kind of thinking was inextricably linguistic, even after development.

But I have trouble with this passage (Section 2.1), “The Un-Cartesian hypothesis would explain why we never empirically find language developing without a particular mode of thought developing alongside. Even conceptually, though, it is not clear how we *could* find the one system without the other. Language without the relevant kind of thought expressed in it would be a parody and hence could not be language in the same sense; and thought that was not articulable in a language in some modality would not be thought of the same kind.”

My question has been the reverse: has there ever been evidence of a particular mode of thought developing without language developing alongside? Is the passage warning me against such pursuits? In Theory of Mind research, much effort has been made to devise behavioral tests for whether animals (say chimpanzees, crows) or pre-talking infants, or severely language delayed children, can reason about others’ false beliefs. None of these individuals can articulate their thoughts in any modality. The question is, if they pass the behavioral tests can it be said to be “thought of the same kind”? In our work with language-delayed deaf children, we reached the conclusion that the thought development marched alongside language development: you couldn’t pass nonverbal false belief tasks if you didn’t yet have false belief type (propositional attitude) language (Schick et al. 2007; de Villiers 2005).

But others have reached the opposite conclusion, based on experimental evidence using looking time or anticipatory gaze, that infants as young as 7 months, or at least toddlers, can reason about other’s false beliefs in the absence of any grammar (Baillargeon et al. 2010). Several attempts at compromise have been proposed, as Hinzen describes: perhaps there is a difference between implicit and explicit false belief understanding (Apperly and Butterfill 2009)? On the un-Cartesian view, it would be difficult to argue that these two forms of reasoning were connected. Or, perhaps infants are succeeding on the gaze tasks through some lesser, behavioral-type understanding that does not reach the standard of “reasoning”, that latter being only in the purview of language? One of the puzzles for me is why some of our very smart but language-delayed 8 year olds were not as clever as other researchers’ toddlers seem to be. Does this capacity die away?

There has recently been a sea-change in the view of the empirical results from the infancy research, with major questions being raised about replicability, and a collection of non-replications being amassed across many different

laboratories (Rakoczy and Kulke 2017). It is too early to tell what will come of this. Tremendous ingenuity has been employed to test the reasoning of non-verbal infants, and the prospect still exists that one can devise tests without language as the medium to test if thinking is the same. If we do not try, then the following statement becomes untestable: “There is no time when humans only ... have ‘social cognition’ but no ‘linguistic cognition’.” (Section 2.5)

My fourth comment is about the central claim of the paper that reference is a grammatical concept. Hinzen argues that children with autism reveal what it is for reference to go awry in development, and that persons with schizophrenia reveal its disintegration. The theory makes bold claims about both clinical disorders being centrally breakdowns of the human language system. But the empirical research to date has not focused on the kinds of questions that un-Cartesian linguistics poses.

The phenomena that are illuminated by Hinzen’s analysis of reference must generate new research investigations on children with autism, for example. But empirical issues abound here: on this account there would be little to study in that percentage (25 %) of children with no language. For those higher functioning children who do have language, the focus of psycholinguistic work has increasingly been on pragmatics, as the children frequently “pass” conventional tests of vocabulary and grammar. Hinzen makes the case for re-examining grammar, reference, and intensionality using more sophisticated tests. Might we say that children with autism fail to automatically “see” the world through language? For instance, what would be revealed in children at risk for autism on the sortal and event tasks described earlier?

The case of language breakdown in schizophrenia is a tantalizing one. Language appears to have come unmoored from its normal reference. Grammar no longer controls the meaning of sentences, and hence the sentences of such a patient can be interpreted by listeners in an infinite number of ways. Fifty years ago, R.D. Laing took the language of psychotic patients as metaphoric expressions, as genuine but poetic expressions of despair at the human condition. Hinzen would say that the expressions gain that meaning from the listener, not the speaker. Humans are interpreters *par excellence*: parents hear entire philosophies in the single word expressions of their toddlers; psychoanalysts like Freud can find “disguised” cues to the deep roots of neurosis in patients’ dream reports; the paranoid delusions of a schizophrenic can sound like a finely woven theory, until the listener recognizes how it departs from normal narrative; religious individuals impose linguistic messages even on nature around them.

The prediction being explored is that in the language of people with schizophrenia, the difficulties will be most apparent in those areas of reference that need the most grammatical support. The biggest problem here is how to test it.

Analysis of speech can only go so far, but performance on careful tests of comprehension may be hampered by other aspects of the patient's condition. Consider two phenomena at the very limit of my own grasp. One is indexicality, especially in how pronouns behave in embedded contexts (Roberts 2015). The second is intensionality, namely referential opacity in complements. There are well-constructed scenarios for production and comprehension that have been used to study children: if any phenomena will uncover the disintegration of reference in its most grammatically constrained form, these will.

Admittedly, my agenda for the first three points fails here: it is not clear that there is any such thing as intensionality outside of language. Hinzen may well say the same about the rest: there is also nothing like lexemes, nothing like syntactic combination, nothing like belief statements. I have suggested that we can find ways to test the conjectures of un-Cartesian linguistics wherever possible by specifying exactly how language changes forms of reasoning, and doing so will only sharpen the theory.

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