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Reply to comments

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Abstract. This short paper contains brief replies to the comments that Richard Hudson, Timothy Osborne, Paul Rastall, and William Benzon made to my focus article, "Linguistic structure: A plausible theory", in this issue of *Language Under Discussion.*

Keywords: relational networks, tokens, types, dependency, referents, activation, structure, Vygotsky, inner speech.

I am grateful to the commentators for taking the time to produce their thoughtful remarks to my focus paper (Lamb 2016). I believe they require only brief responses from me, and I set them forth in the order in which the commentaries were received.

Richard Hudson

Tokens and Types. In relational network (RN) theory a token is the result of activation of a node or group of nodes representing the type. It is thus accounted for, for either producer or receiver, in terms of the operation of a linguistic system. The problem Hudson (2016, 40) mentions, that RNs do not provide for two tokens of the same type, would seem to be a problem only at the conceptual level of the network, but at this level it is not actually a problem after all, since whenever a new concept is actually introduced into or constructed by a system, that introduction consists of recruiting a new conceptual node, even though its representation at the lexical level may include a lexical item that is also used in representing another concept.

Hudson's assertion that a new node is needed "for every word, sound segment, letter or whatever" (Hudson 2016, 40), that is, for items at levels lower than conceptual, is not given any support and in my view is not supportable within the framework of RN theory. The

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problem here is the test of neurological plausibility: There are far too few cortical columns or neurons in the brain to provide separate neural representation for all the tokens of such lower-level items that occur during a lifetime, or even during a year of normal mentally active life.

Of course, as with any scientific endeavor, what we put forth as a theory at any point must be regarded as tentative, and while it accounts for a certain body of observations it invariably opens up further vistas, raising additional questions. And so I don't rule out the possibility that there is more to the problem of tokens that needs to be taken care of by future developments; it may be that linguistic processing requires ad hoc construction of strictly temporary nodes using neural equipment that can later be reused in new situations.

Dependencies. As Hudson's (2016, 40–41) account demonstrates, the dependency notation in syntax, which I have always found rather attractive, is a notational device providing an abbreviated means of representing information from two separate structural levels. But, as the original theory of "Stratificational Grammar" argued, it is essential, for complete understanding of language, to explicitly distinguish the separate levels. Using his example of 'French house', at the lexical level we have two words, while at the conceptual level we have the concept of a "French house", a subcategory of the more general concept "house"; and 'typical French house' is the lexical representation of a subcategory of "French house". At the lexical level we are indeed dealing with combinations, while at the conceptual stratum such nouns and noun phrases represent conceptual categories.

Incidentally, one of the interesting consequences of the RN approach is that categories actually seem to be represented as cortical territories, with subcategories as portions of such territorial representations.

Referents. Hudson's (2016, 41) comments concerning referents are quite cogent in my opinion. Hudson would want to provide network representations to referents in the outside world. His argument is generally in agreement with my position, which evidently was not presented with sufficient clarity. Relational networks do not provide cognitive representations for referents generally, since they are external to the brain and it would not be in the interest of neurological plausibility to attempt to represent them since they are too numerous for the brain's capacity. But conceptual representations of those selected referents that come to attention *are* represented mentally; they correspond more or less closely to what is "out there" and in awareness, although the correspondence is not as close as we might imagine, because of the vagaries of perception.

Activation. On the point of spreading activation, which provides a means of accounting for priming, I also agree with Hudson (2016, 41–42), and I have written about it elsewhere. An example is the topic of unintended puns, treated in *Pathways of the Brain* (Lamb 1999: 190–194).

Exceptions. In his comments on exceptions (or irregular forms) vis-à-vis default representations, Hudson (2016, 42) mentions the method used in RN, and I have never seen an alternative that works as well. Hudson's method depends on his treatment of tokens, which as mentioned above I find unsatisfactory.

Another point worth touching on is the treatment of the mentally pervasive 'isa' relationship, the relationship of subcategories to categories, as in 'a poodle isa dog'. In earlier work (e.g., Lamb 1999) I treated this relationship (in RN) with a node, as Hudson does, specifically the OR node. Now I see it differently, as a matter of cortical territories, as mentioned above. For example, animals appear to be represented in a particular area in the left temporal lobe, which is subdivided into subareas for different animals, while these subareas are further subdivided into smaller subareas, and so forth. This hypothesis, with supporting evidence, is discussed in the focus article (Lamb 2016: 30–33).

Finally, I agree wholeheartedly with the last paragraph of Hudson's commentary (2016, 42), in which he emphasizes the importance of linguistic investigation for an enhanced understanding of the mind.

I might add one further point that has not been mentioned in my earlier writing. The fact that relational networks can be seen as representations of actual neural networks of the cerebral cortex may tend to suggest that I believe *all* mental phenomena can be reduced to brain structures. This seems to be the prevailing view in cognitive science and in neuroscience, but I believe that it is mistaken, and that, especially if we look beyond language, for example to the phenomenon of consciousness and to various other phenomena (see for example Kelly et al. 2007), we easily find aspects of mind that are not so reducible, and must therefore be recognized as having existence apart from brain or any other matter.

Timothy Osborne

Osborne's (2016) comments concern the topic of dependency, one of the issues raised by Hudson and therefore a point that I have already commented on (above). Thus the alleged parsimony of dependency representation is a matter of notation, since dependency is, as mentioned above, a handy (and indeed clever) notational device for abbreviating information.

I just add one further observation: Osborne associates constituency with the work of Chomsky, but Chomsky used it because it was the prevailing method of dealing with linguistic combinations in the environment of mid-century American structural linguistics, a method that was practiced by structuralists in general, including his principal teacher, Zellig Harris.

Paul Rastall

Rastall (2016) has several provocative ideas in his commentary, but some of his points have problems that I will mention even though not all of them are serious. My responses are written in the order of mention in Rastall's commentary.

In his introduction, Rastall (2016, 51) states, "…Lamb (2016) proposes that 'linguistic structure' is a 'plausible hypothesis'". No, this is *not* what I was proposing. What I proposed was a plausible theory of linguistic structure; that is, taking linguistic structure as a given phenomenon (see following passage), I proposed a plausible theory to account for it. Rastall's next sentence also has an error: "He seems to mean that any linguistic organization or 'relational network' is a 'structure'" (ibid.). Perhaps I was not clear enough. I was taking 'structure', without seeing a need to define the term, as denoting whatever it is that enables a person to speak and comprehend, and to read and write in the case of languages that have writing systems. It is what, for example, a Mexican has that makes it possible for him to speak

and understand Spanish, an information system of some kind that ordinary Americans don't have until and unless they take Spanish language lessons. And I take it as the job of the theoretical linguist to try to characterize that information. The relational networks arise out of the attempt to so characterize it.

This clarification of what I meant by the term *structure* will make it unnecessary, I hope, to respond to some of the issues raised in subsequent paragraphs of Rastall's commentary, including his section 2.

Thus, Rastall's third sentence is likewise erroneous in stating, "he appears to be … defending the idea of linguistic structure as 'plausible'" (ibid.). Rather, linguistic structure, in the sense just stated, is taken as the given, the information that needs to be accounted for in a realistic way. In this case, I also defined what is meant by *realistic*: The account must satisfy the plausibility requirements.

Now to an issue of an entirely different kind. In his next paragraph Rastall (2016, 52) writes, "His position is clearly 'monist' (i.e. it rejects a 'mind-body' dualism in line with most modern thinking in philosophy and neuroscience), advocating an "internal system' … activating all brain and physical mechanisms." This assessment is basically correct in the context of the paper under discussion, but it may merit some clarification. What I did, in writing the 2016 focus paper and in *Pathways of the Brain* (Lamb 1999), was to adopt, *as a methodological assumption,* the view that linguistic structure can be accounted for in terms of the brain. The value of such a methodological assumption is that it increases the motivation of the investigator to find a neurological basis. Without such motivation it might be tempting to give up too easily on troublesome problems of the investigation and to fall back on non-physical mental explanations. Proceeding on that assumption I did indeed find a neurological basis for language, and so one sees that the assumption was justified. But that fact does not in itself allow a jump to the conclusion that *all* mental phenomena have a physical basis. (That is something I used to say to my students when they would ask whether there might be any mental phenomena without a neurological basis.)

Rastall asserts that a phoneme like /t/ is a class, following a notion that was alive in linguistics a few decades ago. That concept is alien to relational network theory, where the closest thing we have to a phoneme is a nection or a node in the phonological network.

He also uses the concept of class in adherence to an apparent acceptance of the notion of a language shared by members of a speech community. In my paper I intentionally avoided getting involved with such an overly abstract notion as language (cf. Lamb 1986/2004), in favor of treating the linguistic system of an actual individual as a far more tangible entity. Rastall then goes on to bring up the ideas of Carlo Rovelli (2015, 42) concerning the 'blurred vision' we have because of our limited perceptions and intellects. I agree with his suggestion that Rovelli's point may be extendable to the world of social communication. He is correct in pointing out that, as he says, "Lamb's position is actually not too far from this".

I find the rest of Rastall's commentary interesting but not directly related to the paper under discussion, more easily relatable to conceptions of 'structure' from earlier days in the history of linguistics. I would claim that the various kinds of complexity mentioned can be accounted for in additional network structures not covered in my paper, which was, after all, an introductory account.

William Benzon

Benzon (2016) considers my metaphor of the cognitive dome, pointing out that one of its instructive features is the principle that no boundary can be found to separate the neurocognitive structure that supports language from that which supports cognition in general. Also important to emphasize is that it is only a metaphor and as such is, as he writes, "a crude conceptual instrument" (Benzon 2016, 74), a simplified representation of what is actually there in the neurocognitive system. He is also quite correct in pointing out that the 'external' world includes one's own body, an important observation.

We humans, in our feeble attempts to understand our world, like to visualize, so we build visual metaphors like the cognitive dome; but we need to remember that such metaphors and such visualizations are simplified models and that they can mislead if pushed too far. Some of the most interesting and challenging aspects of mind are doubtless beyond visualization.

Benzon's description of thought as inner speech is indeed in accord with my more extensive treatment in *Pathways of the Brain* (Lamb 1999) and the parallel to Vygotsky's formulation is welcome. I would only add that, as 'thought' and 'thinking' are ordinary non-technical terms, they are vague and ambiguous, and we have to recognize other kinds of mental activity that may or may not be included under these terms as used by different people, such as listening appreciatively to music, or meditating. And so I wholeheartedly concur with Benzon's final paragraph (2016, 77) while urging caution in the use of these terms.

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