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When Theories Don't Compete: Response to Thomas, Karaminis, and Knowland's Commentary on Musolino, Chunyo, and Landau

Julien Musolino
Department of Psychology and Center for Cognitive Science
Rutgers University

Barbara Landau
Department of Cognitive Science
Johns Hopkins University

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Thomas et al. (this issue) argue that someone interpreting our results through a generative prism would draw one set of conclusions, but that someone seeing the same results through a neuroconstructivist lens would draw radically different conclusions. We agree with Thomas et al. that there is a theoretical difference here, but it is not between one set of assumptions (i.e., generative/modular) versus another (i.e., neuroconstructivism); rather it is the difference between having a theory of the linguistic phenomenon under consideration versus not having one. Once this basic fact and its methodological and rhetorical implications are recognized, it is plain to see that what Thomas et al. have to offer falls far short of a genuine alternative to what we proposed.

INTRODUCTION

In their commentary on our work, Thomas et al. argue that someone wearing generative-colored glasses would draw one set of conclusions from our results, but that someone seeing the same results through a neuroconstructivist lens would draw completely different conclusions. Implicit in this claim is the assumption that the two competing frameworks both address the same question, and that they both provide equivalent and competing theoretical accounts of the empirical phenomenon under consideration.

However, an examination of Thomas et al.'s commentary clearly demonstrates that the equivalence described above does not hold, and thus that neuroconstructivism does not represent a meaningful alternative to what we proposed. We started with a clearly defined linguistic phenomenon for which we presented a detailed and independently motivated theoretical analysis. The challenge for dissenters, as explicitly acknowledged by Thomas et al., is to come up with an equally precise and explanatory analysis of the linguistic phenomenon under consideration.

Correspondence should be addressed to Julien Musolino, Department of Psychology and Center for Cognitive Science, Rutgers University, 152 Frelinghuysen Road, Piscataway, NJ 08854. E-mail: julienm@ruccs.rutgers.edu

However, Thomas et al., who certainly dissent, do nothing of the sort. Thus, there is indeed a theoretical difference here, as Thomas et al. argue, but it is not between one set of assumptions (i.e., generative/modular) versus another (i.e., neuroconstructivism); rather it is the difference between having a theory of the linguistic phenomenon under consideration versus not having one.

This theoretical difference has profound methodological ramifications. To be sure, having a theory leads one to pose certain questions and seek to address these questions using certain methodological tools. In this regard, the core question we posed — a standard one in the field of language acquisition — is whether individuals with Williams Syndrome (WS) have knowledge of the linguistic principles we identified. Since Thomas et al. do not have an account of the relevant linguistic facts, they cannot address our core question (even though they explicitly recognize the challenge it raises), and so instead they reframed the 'debate' in terms of a *different* question, namely the extent to which behavioral levels seen in individuals with WS are typical or atypical. In doing so, they argue that our approach and methodology are inadequate and they derive conclusions that are antithetical to what they (mistakenly) take ours to be. In the end, therefore, we have the illusion of a debate with competing 'explanations'. However, as we show below, once the theoretical smoke is cleared and the rhetorical mirrors are removed, what Thomas et al. offer falls far short of a genuine alternative to what we proposed.

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1. THE THEORETICAL QUESTION

Let us begin by giving Thomas et al. credit where they deserve it. These authors do recognize the fundamental challenge posed by our findings: "The strength of MCL's study of language comprehension in WS is to issue a challenge to those researchers who claim WS language development is atypical, to explain how the degree of successful behavior that was observed (in this case, above chance performance in the Truth Value Judgement Task) could have been produced using different underlying processes" (p. 168). Indeed, in order to explain how individuals with WS (or anyone else for that matter) correctly interpret complex sentences containing negation and disjunction (e.g., *The clown who is holding a flower will not be given a jewel or a coin*) and manage to distinguish closely related pairs of such sentences (e.g., *The cat who meows did not get a fish or milk* vs. *The cat who didn't meow got a fish or milk*), one needs a *theory* of the linguistic principles that must be present and engaged in the comprehension process. This much is not open to interpretation; it merely represents elementary standards of scientific explanation. Accordingly, we devoted several pages of theoretical background in our main article to the formulation of a detailed, formal analysis of the linguistic phenomenon under consideration based on independently motivated linguistic principles (see section 3 of our article).

Having recognized the fundamental challenge posed by our results, Thomas et al. offer the following alternative: "Such an account might presumably appeal to lexical or semantic/pragmatic compensatory mechanisms, comprise processes that contain some but not all of the grammatical properties outlined in the generative theory, or employ computational mechanisms that approximate formal syntactic systems under some processing conditions but not others (Christiansen & Chater, 2001; Rumelhart & McClelland, 1986)" (p. 169). They add that (in their alternative) "that emphasis leads both to the use of particular methodological approaches (e.g., research designs that trace developmental trajectories, rather than collapsing participant groups over wide age ranges; see Thomas et al., 2009), and to the formulation of explanations with certain characteristics

(e.g., theories that features concepts such as *plasticity, adaptation, interactivity, redundancy*, and *compensation*; see Thomas, 2005)" (p. 165).

In our view, these comments provide a compelling demonstration that Thomas et al. do not have an alternative to our account of the phenomenon we reported. By analogy, if the goal were to characterize human knowledge of the rules of chess, saying that the game involves computational mechanisms, and requires flexibility, adaptation, and interactivity is not equivalent to a detailed explanation of the actual rules that people use when playing. Thus, until one can specify how notions such as 'plasticity, adaptation, interactivity, or redundancy' or 'semantic/pragmatic compensatory mechanisms or computational mechanisms that approximate formal syntactic systems under some conditions but not others' can account for De Morgan's laws of propositional logic, the relationship between the inference patterns embodied in these laws and fine details of syntactic structure, etc., one simply does not have an explanation, let alone an alternative to what we proposed.

This point is of fundamental importance to understanding the differences between the account we proposed and the neuroconstructivist response. Because we have a precise idea of the kind of linguistic knowledge that must be present and engaged when one interprets sentences containing negation and disjunction, we can then ask whether such knowledge is indeed present and engaged by individuals with WS. Thus, having a theory leads us to pose certain questions, and seek to address these questions using certain methodological tools. Since Thomas et al. do not have an account of the relevant linguistic facts, they cannot account for individuals with WS performing above chance in our experimental conditions. Thus, Thomas et al. focus instead on the other aspect of the data, the fact that there is a difference in behavioral levels between our WS and control groups.

In this regard, the title that Thomas et al. chose for their commentary is quite revealing. We wrote an article entitled "Uncovering *knowledge* (emphasis added) of core syntactic and semantic principles in individuals with Williams Syndrome" — focusing on the question of presence of knowledge. Thomas et al. wrote a response called "What is typical language development?" addressing a totally different question about (a)typicality. This shift in focus is evident in the opening section of their commentary: "How did we get to a point where such poor performance on a language comprehension task can be viewed as evidence of normal processes of language development? If such poor performance can be viewed as evidence of normality, what does it take *not* to have normally developing language according to this task, or more generally?" (p. 162–163). First, it should be pointed out that accuracy levels for our WS group were 76% correct in the experimental conditions (compared to 89.5% for mental age controls) and 90.8% in the control conditions (compared to 94.5% for MA controls). It seems to us that describing these results as "such poor performance" is misleading. Much more importantly, however, Thomas et al. entirely missed the point of our studies. To be sure, we never asked whether language in WS is "normal" or "typical"; indeed, we think this is a completely different question, and in fact, one that is mired in much complexity and confusion (see our methodology section, below). Rather, our question was whether people with WS have *knowledge* of a particular set of syntactic and semantic structures and principles.

The question of whether individuals with WS are delayed in acquiring these structures is quite a different one, but it is the one framing Thomas et al.'s discussion: ". . . the neuroconstructivist would be focused on addressing why there should be such a large developmental delay in language comprehension in WS . . ." (p. 164). We are fairly certain that they *are*

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delayed developmentally: Many studies have shown that children with WS have initial delays in acquiring language, ranging from the early lexicon to pragmatics (see Mervis & Becerra, 2007, for a review). Studies also show that people with WS do acquire aspects of complex syntax, but exhibit asymmetries that are characteristic of much younger typically developing children (Perovic & Wexler, in press; Zukowski, 2009; our data). We agree with Thomas et al. that the notion of delay is highly underspecified in our field and that it can mean different things, depending on the cause of delay. For example, delay may be due to very slow acquisition, a possibility that is supported by evidence from Mervis et al. (1999), who found that syntactic complexity in WS was appropriate for M.L.U. Delay would therefore not be surprising for the aspects of syntax and semantics that we tested. Delay by itself is surely an indication that language is not "normal" or "typical," but the nature of the delay is crucial in determining what it means theoretically. Delay could reflect very slow acquisition, and could be caused by normal mechanisms that simply operate more slowly. Differential delay across domains might suggest other possibilities, as Thomas et al. point out, although the case for this has not received support (Brock, 2007). Another possibility, and one we have proposed for spatial representation, is overall slow development, followed by early developmental arrest at the functional level of a 4 or 5-year-old (Landau & Hoffman, 2007; Landau, in press). We actually suspect that this could explain the data from our paper — where people with WS perform at similar levels to normally developing 4-year-olds — as well as a wide range of other language data, a hypothesis we are currently testing. However, delay and its causes was not the principal topic of our paper.

Rather, we asked whether individuals with WS reach a state in which they can control the syntax and semantics of complex sentences. We used a method that is time-honored in the study of atypical populations: We tested for evidence of this knowledge. Other studies have used the same approach, with important results. For example, we now know that even though people with WS score worse than their mental age matches on a standardized test of relative clause comprehension (the TROG, Bishop, 1983), the same individuals can — under felicitous conditions — produce both object and subject relative clauses (Zukowski, 2009). Under these conditions, people with WS show the same asymmetries as much younger normally developing children, another hint that the delay plus arrest hypothesis may be true. Zukowski's studies prove that the representational system of people with WS includes a machinery that allows production of sentences such as "*The horse that the girl rode on jumped over the fence*." Our studies show that people with WS can comprehend complex sentences in a way that suggests a hierarchically structured representation in which subparts of the structure interact in specific ways.

We took this approach to see whether people with WS have knowledge of certain abstract syntactic and semantic principles. Thomas et al. nevertheless criticize us for the scientific approach they *think* we took: "Notably, MCL's argument did not proceed by direct falsification. Their study could have been constructed in the following way: Theoretical Position 1 predicts data A, Theoretical Position 2 predicts data B; the results turned out to be data A, therefore, Position 1 was supported and Position 2 was falsified. In contrast, the behavioral data had aspects that could be viewed by both Position 1 and Position 2 as supporting their theories, depending on the assumptions used in interpreting the results. The MCL study, therefore, revolved around a particular way of interpreting the data, rather than arbitrating between hypotheses from competing theories (p. 163)."

First, it is obvious that our experiments were designed carefully in order to evaluate several different hypotheses: Could the outcomes for complex sentences be due to weakness in

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sub-components of the structures? Could the differences between WS individuals and MA matches be due to overall developmental immaturity (in which case, we might expect greater similarity to typically developing children who are younger than the MA matches). Thomas et al. had something else in mind. They seem to believe that a better way to proceed would have been to directly contrast two different theories that could equally well explain the acquisition of the complex structures we tested. We wonder what theory they had in mind. Perhaps it is the one they advocate. In this case, we challenge them to provide a detailed theory that can account for our data. We suspect that if they attempt this, they will run head first into the question of what it takes to comprehend the sentences we used in our experiments; this will take them into a requirement to characterize the structures underlying the sentences. If they can explain our data using these terms, then this would lead to a real challenge and the possibility for testing competing theories. At present, we do not see that they have offered any alternative — no detailed theory of the language that is acquired, nor any detailed theory of how plasticity, adaptation, interactivity, redundancy, and compensation could lead to the acquisitions that we have documented.

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2. METHODOLOGY

Thomas et al. criticize our studies on several methodological grounds, each of which we believe reveals a misunderstanding of both the point of our controls and more broadly, the kinds of controls that are relevant for answering different kinds of questions.

First, they argue that our task is "greatly simplified (p. 162)." From their discussion of what constitutes "simplification," they seem to mean that the task required a simple yes/no judgment, which has a 50% chance of success. There are two separate issues here. One is that the judgment task requires a yes/no. This by itself does not make a task "simple," and is in fact a method having wide use in fields as diverse as psychophysics and psycholinguistics. We used standard statistical analytic techniques to evaluate whether performance among WS individuals (and controls) was better than would be expected by chance. It was, and in many cases the level of performance was quite high, between 75% and 90% correct, as we pointed out earlier. The second issue must be that Thomas et al. believe that above-chance performance can be achieved by some mechanism or representation or solution that does *not* require knowledge of the complex syntactic and semantic structures that we specified. As we argued, there is a detailed linguistic theory that specifies what would be needed to understand the test sentences, and to respond in an appropriate fashion to the truth value judgment task. If Thomas et al. have a different hypothesis about how the task was solved using some *other* representation, we invite them to provide it. Perhaps they can offer a solution that is "simpler" and propose how a person could use it to answer the questions with the accuracy we observed. In the absence of any concrete proposal from Thomas et al., our best guess is that they have vastly underestimated what is required to generate the results we obtained.

Thomas et al. also criticize our choice of control groups and claim that our approach was not developmental. Let us first reiterate that the comparison of WS individuals to both control groups we used was a secondary issue in our paper, with our emphasis on the absolute performance of people with WS. Still, we did compare the performance of individuals with WS to (a) mental age (MA)-matched children and (b) normally developing 4-year-olds. In a first pass,

we compared performance to MA matches, asking how absolute performance compared in the two groups, and we found that people with WS performed in some cases worse than these controls. We then asked whether WS performance was more like normally developing 4-year-old children (who are on average younger than the MA matches). We found that individuals with WS were overall like the 4-year-olds, consistent with delay or (more probably, we suspect) developmental arrest at this level.

Let us first point out that this approach clearly *is* developmental. It reflects the goal of establishing (a) what the best age equivalent is for WS performance and (b) whether any differences between the standard comparison group (MA matches) and individuals with WS might reflect general developmental immaturity, which includes important factors such as working memory. We found that the WS group performed somewhat better than 4-year-olds but showed the same relationship across subtasks, suggesting that the system was similar to a 4-year-old or slightly better. We also took independent measures, including verbal and nonverbal components of a standardized intelligence test (the KBIT, Kaufman & Kaufman, 1990); these allowed us to construct the mental age-matched (MA) group. We found that the WS performance was worse than would be expected for mental age, including both verbal measure (largely reflecting vocabulary) and nonverbal measures. Thomas et al. take this fact to be evidence of "atypicality," since our task and the KBIT did not align in the same way for the normally developing children and the WS individuals.

We find this logic to be quite misguided. Thomas et al. advocate taking a number of other measures of both language and nonverbal performance that can then be used to assess the *relationship* between the performance on the target task and these other measures. They point out that when this has been done, the results sometimes show that a given measure of language in people with WS is worse than predicted for mental age; or worse than predicted for level of vocabulary. We admire their trust in these measures, but unless one has a serious theory of what the measures reflect, whether the measures march together or do not is hard to interpret. For example, MA matching is widely used and is useful if one wants an overall look at the level of performance *in that task*. But what does the task measure? The KBIT (which we used) has a "Verbal" measure that includes a vocabulary test (matching of word/sentence to picture) and a "Nonverbal" measure that includes a variety of items such as "what belongs with what" and filling in matrices to fit a logical pattern. Individuals with WS are matched to their "mental age" matches using the raw scores on these tests, but what do the tests measure? We have much less faith in our understanding of what is being measured than Thomas et al. apparently do. In fact, the dissociation of measures such as vocabulary and syntax might reflect "atypical" development across domains but is also, in our view, powerful evidence for the presence of a modular system of knowledge.

3. BROADER IMPLICATIONS

Just as Thomas et al. did not discuss the question we actually posed, they also disagree with a set of conclusions that we did not reach. In the end, therefore, the conclusions that we did reach remain unchallenged.

First, consider again our core question: "Whether individuals with WS interpret sentences in ways that require that they engage the core properties of the computational system described above" (p. 128) and the main conclusion we reached, namely that "we presented new empirical

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evidence showing that knowledge of core, abstract principles of grammar is present and engaged in WS, just as it is in typically developing and mature individuals" (p. 155). By contrast, Thomas et al. assert that "the conclusion that the results are *evidence of normal language development in the disorder* [our emphasis] is undermined by the low accuracy levels exhibited by the individuals with WS . . . and the presence of at least one marker of atypicality, namely the relationship between component language skills" (p. 168). Again, we did not conclude that language develops "normally" in WS. As we discussed in section 1, we are fairly certain that development is delayed in WS, and in fact that it may be developmentally arrested at some point. However, notice that this observation is nevertheless perfectly compatible with our conclusion that abstract grammatical knowledge is present and engaged in individuals with WS.

Next, consider the broader implications of our results. According to Thomas et al., we took modularity to be the key concern. "Contra MCL, then, we argue that modularity is not the key concern; the key concern is specifying the nature of the developmental process" (p. 165) and "however, they maintained generally that *modularity* should be a central concept in explaining the uneven cognitive profile observed in WS . . . " (p. 164). As before, such claims represent a distortion of what we actually said. We certainly discussed modularity, but in a nuanced and tentative way, as seen in the following quote: "Thus, we would now like to discuss why we think that our results are at least *relevant* to the question of modularity. Since this topic remains highly controversial, we would like to underscore at the outset of the discussion the exploratory nature of the remarks to follow" (p. 152). Moreover, we were careful to point out that we took our results to be relevant to only *some*, but not *all* aspects of modularity (specifically, domain-specificity, robustness under deficit). Thomas et al. offered no good reason why we should abandon these conclusions. Since they did not offer an alternative analysis of the linguistic phenomenon under consideration they have no serious grounds for challenging our conclusion regarding domain specificity. The same holds for robustness under deficit. Our observation here is that in spite of an altered genetic potential and mental retardation, the *linguistic representations* that individuals with WS end up forming are indistinguishable from those of typically developing individuals. Again, since Thomas et al. do not specify what these representations are, they can have no serious objection.

Next, let us consider the implications of our results for neuroconstructivism. Here again, we find a sharp contrast between what Thomas et al. *believe* our conclusions to be and what these conclusions actually are. For Thomas et al., our criticism is leveled against a specific hypothesis within the neuroconstructivist framework, what Thomas and Karmiloff-Smith (2003) call the semantics-phonology imbalance hypothesis. To quote Thomas et al.: "The behavioral data were offered as favoring the modular view and sufficient to reject the neuroconstructivist Imbalance hypothesis" (p. 163) and "while MCL took their findings to falsify one neuroconstructivist hypothesis of language development in WS (the Imbalance Hypothesis), they took them to favor another (the Conservative Hypothesis)" (p. 165). However, we only refer to the imbalance hypothesis in one of our background sections (section 2.3) and for the sake of completeness. Not once in our paper do we make the kind of claim that Thomas et al. report regarding the fact that our results falsify the imbalance hypothesis. Instead, we argued against explicit neuroconstructivist claims about the nature of grammatical knowledge/rules/representations in individuals with WS, as well as other claims made by Karmiloff-Smith that knowledge of grammar is only superficial in WS, that language is acquired using different cognitive mechanisms, and that individuals with WS are unable to

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extract underlying regularities or form linguistic generalizations (see sections 5.1 and 5.2 of our article for relevant quotes and discussion).

In this regard, it is interesting to note that in their commentary, Thomas et al. claim that neuroconstructivism does not "embrace the distinction between competence and performance" (p. 164). Then again, Thomas et al. do not seem to be constrained by the need to actually come up with a concrete analysis of the linguistic phenomenon under consideration. So one can indeed dispense with the distinction between competence and performance, but if this means that one also has to dispense with an explanation of the relevant phenomenon, this hardly seems to be the right bargain. Moreover, the claim that neuroconstructivism does not embrace the distinction between competence and performance is difficult to reconcile with statements such as: "The results of the two present studies . . . challenge the often-cited claim that the particular interest of Williams syndrome for cognitive science lies in the fact that *morphosyntactic rules* [our emphasis] are intact" (Karmiloff-Smith et al., p. 257), or, to take another example from Thomas and Karmiloff-Smith (2003), "the final semantic and conceptual *representations* [our emphasis] formed in individuals with WS appear to be shallower, with less abstract information and more perceptually based detail . . . " (p. 652). How can one talk about rules and representations — which are not the same as behavioral levels — and yet deny making the distinction between competence and performance — or knowledge and behavior — at some level?

Finally, Thomas et al. observe that even if one were to make the same theoretical assumptions we make, and use the same methodology, one might nevertheless arrive at different conclusions. To support this observation, Thomas et al. point to a recent study by Perovic and Wexler (in press) in which these authors examined the acquisition of passives in individuals with WS and concluded that certain aspects of syntax develop atypically in this population. However, as already discussed, our core question was about the presence of certain specific linguistic knowledge in individuals with WS, not about whether syntax is "typical" or not. Perovic and Wexler argued that certain aspects of syntactic structure are acquired either very late, or possibly not at all, by people with WS. This conclusion is completely compatible with the possibility that we proposed for spatial representation (and in this paper, language) — namely the idea that people with WS undergo overall slow development, followed by early developmental arrest at the functional level or a 4 or 5-year-old (see previous section). This hypothesis can account for the asymmetry between passives of actional verbs and psychological verbs reported by Perovic and Wexler (K. Wexler, p.c.). Indeed, passives of actional verbs, which are mastered by typically developing 5-year-olds, are handled well by individuals with WS in sharp contrast to their performance on passives of psychological verbs, which are not mastered by typically developing children until the age of 7 or 8.

4. CONCLUSION

We agree with Thomas et al. that there are two aspects to our data: (a) the fact that individuals with WS performed above chance on our experimental and control conditions, and (b) the fact that they did not perform as well as MA controls on the experimental conditions. For Thomas et al., the main issue seems to be a descriptive one: based on this pattern of results, should we label language development as typical or atypical in this disordered population? Our question and approach, by contrast, focuses on a different set of questions and reaches a deeper, explanatory

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level. First, with regards to (a), our core question, we provided a detailed account of why individuals with WS performed above chance and were able to distinguish closely related pairs of complex sentences involving the interaction of negation and disjunction: they possess and engage the abstract linguistic knowledge that we specified. Thomas et al., who did not provide a meaningful alternative to our analysis, therefore have no way to account for (a). Regarding (b), we proposed that the difference in behavioral levels between individuals with WS and MA controls on our experimental conditions does not stem from problems with the linguistic knowledge responsible for above chance performance on (a) since both groups do not differ on the control conditions which test for knowledge of the individual components interacting in the experimental conditions. Thus, it seems reasonable to conclude that the combined complexity of the elements involved is what gives rise to the difference between WS and MA. We speculated that this may have to do with differences in computational resources such as working memory, and established that the same behavioral pattern emerges in younger, typically developing individuals (whose working memory capacity is not as developed as that of MA controls). This may not be the right explanation, or even the best one, but as was the case for (a), Thomas et al. did not offer an alternative. Thus, in the end, if what we care about is explanations, as opposed to descriptions and appropriate labeling, it is clear that our approach remains superior to that advocated by Thomas et al.

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