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Genes, Language, and the Nature of Scientific Explanations: the Case of Williams Syndrome

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Introduction

Simply put, the goal of science is to understand the natural world. Often though, nature only reluctantly lets us peer behind the curtain, forcing scientists to deploy ingenious measures to take a peek at its true face. Occasionally, nature is more charitable and reveals itself by orchestrating its own experiments. Such experiments of nature, as they are called, are cherished by scientists because they can bear on questions that would be practically or ethically impossible for them to answer otherwise. For scientists interested in the structure and development of the human mind, few puzzles are as tantalizing as the question of the relationship between genes and cognition. A little over 20 years ago, a fascinating natural experiment was brought to the attention of the Cognitive Science community with the potential to begin unraveling the causal chain linking genes and cognition.

This experiment of nature was Williams syndrome -- a relatively rare genetic disorder that results in a highly unusual cognitive profile, with individuals showing severe impairments in a range of spatial functions but strikingly fluent and well-structured language. This profile has placed Williams syndrome at the center of a major controversy within Cognitive Science over some of its foundational questions, including the modularity of mind, the nature and role of innate knowledge in language acquisition, and the relationship between language and other aspects of cognition. What makes Williams syndrome so interesting is that the unusual cognitive profile arises as a consequence of a well-defined genetic condition, with about 25 genes missing from chromosome 7. Perhaps not too surprisingly given the issues at stake, accounts of the cognitive profile of individuals with Williams Syndrome and its implications for the sciences of the mind tell the tale of two strongly conflicting viewpoints.

One set of conclusions regarding the cognitive phenotype of individuals with Williams Syndrome is that knowledge of language – and in particular syntax – is ‘spared’ or ‘intact’ relative to other cognitive systems and therefore that Williams Syndrome represents one side of a double dissociation between language and general intelligence. To quote Pinker (1999): “The genes of one group of children [children with specific language impairment] impair their grammar while sparing general intelligence; the genes of another group of children [children with Williams syndrome] impair their intelligence while sparing their grammar” (p 262). Thus, Williams Syndrome is often cited as a compelling demonstration of the modularity of mind (Jackendoff, 1994; Pinker, 1999; Piatelli-Palmarini, 2001).

A radically different position holds that knowledge of language – and in particular syntax – is not ‘spared’ or ‘intact’ in this atypical population, and therefore that Williams syndrome does *not* provide evidence for modularity. Moreover, scientists holding this position conclude that the linguistic profile of people with WS shows exactly the opposite-- that even a distinct genetic disorder cannot result in cognitive systems cleaving along the lines

suggested by modular theories of mind. This competing view, most closely associated with a broader framework known as Neuroconstructivism, holds specifically that knowledge of language in individuals with Williams Syndrome is only superficially good because it is achieved through the use of cognitive mechanisms different from those operating in normal individuals (Karmiloff-Smith et al., 1997; Karmiloff-Smith, 1998; Karmiloff and Karmiloff-Smith, 2001; Thomas and Karmiloff-Smith, 2003). To quote Karmiloff and Karmiloff-Smith (2001): “It has become increasingly clear, therefore, that the superficially impressive language skills of individuals with WS may be due to good auditory memory rather than an intact grammar module” (p.202-3).

What shall we make of such contradictory claims and conclusions? And, more generally, how should we approach the relationship between genes and cognition in the first place? In this article, we address these questions as follows: As a first step, we articulate the outline of a framework that we believe represents a productive way of approaching these questions. In doing so, we highlight three important components of such an approach. First, we ask how we should think about the relationship between genes and cognition, focusing on the question of what it might mean for genes to *cause* a particular cognitive profile. Here we argue that because the causal link between genes and cognition is extremely complex and indirect, it is crucial to proceed by selecting a clear level of analysis as our focus. Specifically, we argue for the importance of the *cognitive* level in understanding the WS cognitive phenotype. Second, in order to address the question of how a specific cognitive system – in this case language – may get damaged in WS, we need a *theory* of the cognitive structure in question, or at least a theory of the empirical phenomena under investigation. Third, we emphasize the role of development itself – in this case typical development – as a crucial benchmark against which to evaluate the atypical profile found in WS (Landau, 2011; Landau & Hoffman, 2012). These ideas may not be new, but we believe that they are important, and thus worth emphasizing.

After developing this framework, we use it to explore the nature of the conflicting perspectives described earlier. What is new, here, is what follows from the proposed framework. By far the most common assumption is that the tension between the modular and neuroconstructivist approaches stems from a meaningful debate between two sides (e.g., Brock, 2007; Mervis and Becerra, 2007; Zukowski, 2009). A recent formulation of this thesis holds that interpretations conflict because the data themselves are ambiguous: look at them through a generative/modular lens and you will reach one set of conclusions; put on neuroconstructivist goggles and you will reach a radically different set of conclusions (Thomas et al., 2010). Implicit in this perspective are the assumptions that the two competing frameworks address the same questions, and that they provide explanatorily equivalent accounts of the relevant empirical phenomena.

Here, we turn the tables on the ‘meaningful-debate-assumption’, which underpins much of the literature on this topic, and we show that this assumption rests on a fallacy. In doing so, we focus on the status of *grammatical knowledge* in individuals with Williams Syndrome – perhaps the most contentious issue within this ‘debate’. We argue that there is ‘the illusion of a debate’ between the two views described above, and we pinpoint the factors that give rise to this illusion. We identify two main sources: The first is substantial, and has to do with what constitutes a scientific explanation in the first place (Musolino and Landau, 2010). The second, which is a direct consequence of the first, is semantic in nature and involves the use of the same terms, e.g., ‘selective sparing’, ‘impaired’, ‘intact’ with different intended meanings.

With these important distinctions and clarifications in place, we revisit the empirical terrain, showing that the data are no longer ambiguous. Instead, the data strongly support the

conclusion that grammar – qua *knowledge* of core linguistic rules and representations – is, in its essentials, present and engaged in individuals with Williams Syndrome, just as it is in typically developing and mature individuals. This conclusion, in turn, raises a serious challenge for neuroconstructivism, as acknowledged by some of its proponents (Thomas et al., 2010), which, so far, has not been met (Musolino and Landau, 2010). Finally, we propose a new solution to an apparent paradox that has been claimed to undermine conclusions regarding ‘intact’ syntactic knowledge in individuals with Williams Syndrome (Thomas et al. 2010).

Our Framework

In this section, we introduce the key ideas underlying our framework. We begin by asking how we should think about the relationship between genes and cognition, focusing on the question of what it might mean for genes to *cause* a particular cognitive profile as in the case of WS. Next, we discuss the need to select a clear level of analysis as our focus, in this case the cognitive level, and the need to have a *theory* of the cognitive structure in question, or at least a theory of the empirical phenomenon under investigation. Finally, we discuss the role of development itself – in this case typical development – as a crucial benchmark against which to evaluate the atypical profile found in WS.

Genes and Cognition

Williams Syndrome (WS) is a relatively rare genetic disorder (prevalence of about 1 in 7,500 live births; Strømme et al., 2002), which is due to a micro-deletion of approximately 25 genes on chromosome 7 (Ewart et al., 1993). WS gives rise to physical abnormalities, which include cardiac anomalies and unusual facial features, as well as cognitive abnormalities. What makes the WS cognitive phenotype so interesting for cognitive scientists is that it is characterized by a strikingly uneven profile with areas of relative strength, such as language, alongside severe weaknesses in domains such as space, number, planning, and problem solving (e.g., Bellugi et al., 1988; Karmiloff-Smith et al., 1997). Consider the classic example of an 18-year-old individual with Williams syndrome who was asked to draw a picture of an elephant and describe what he knows about elephants (Galaburda, Wang, Bellugi, and Rossen, 1994) (Figure 1, reproduced from Bellugi, Wang, & Jernigan, 1994).

On first glance, there is a glaring asymmetry between the verbal description of the elephant--fluent, with well-formed sentences that convey coherent thoughts--and the impoverished drawing which preserves virtually nothing of the spatial structure of parts, and only the barest structure of spatial relationships among parts. Could such a striking example represent a clear case of a direct link between genes and cognition? In other words, is it possible -- as conjectured by Bellugi and colleagues (Bellugi et al., 1988; 1994)-- that the absence of 25 genes on a chromosome can cause the derailment of one major system of knowledge (spatial cognition) while leaving another one (language) intact? More generally, how should we approach the relationship between genes and cognition in the first place?

As Marcus (2004) explains, it is patently clear that genes do shape our mental lives, just like they can predispose us to cancer or diabetes. In the case of Williams Syndrome, it is also trivially true that the missing genes somehow cause the attested uneven cognitive profile. However, there is a chasm between individual genes and even strands of DNA and the fine details of syntactic structure or spatial representations at the cognitive level. How, then, should we approach the study of cognitive deficits caused by a genetic disorder?

In his important book *Understanding developmental disorders: a causal modeling approach*, John Morton (2004) offers some important suggestions. He begins by asking us to consider

the nature of explanation, using as an example the question of what caused Romeo's death. Consider the hypothesis in (1).

(1) Romeo was killed by the poison that he took.

This hypothesis is *a priori* plausible and constitutes a valid scientific explanation given what we know about poisons and human bodies. In principle, gaps in the causal chain can be filled by finding out which poison Romeo took, and how it acted on his body. However, Morton argues that people would not consider this to be a very good explanation because, in a sense, it is too specific and thus not very informative. Consider now the hypothesis in (2).

(2) Romeo's genes caused him to commit suicide.

In this case, Morton suggests that even if we were to assume that suicides run in Romeo's family, (2) would be a deeply unsatisfying explanation because it leaves far too many gaps in the causal chain. It is unlikely that anyone would want to posit that the act of committing suicide is coded in Romeo's genes; and the hypothesis in (2) does little to specify the details of the causal chain between genes and the act of suicide.

The same argument applies to the relationship between genes and the cognitive profile of people with WS. If we simply say that the missing genes are causing the uneven cognitive profile, this account would be deeply unsatisfying, even if it is trivially true, because it is uninformative and therefore lacks scientific usefulness. It is also clear that the link between genes and cognition is indirect and enormously complex. Moreover, at present, the causal chain between the genetic and cognitive levels is massively underspecified at many points. Given this state of affairs, how then should we proceed to study the link between genes and cognition? Many writers have suggested that in order to fully understand the effects of genes on cognition, scientists in different disciplines will need to coordinate their efforts as they focus on different ends of the causal chain.

At the genetic end, we need to understand what genes do at the molecular level and how this affects the development of the brain and mind (Marcus, 2004). At the cognitive level, we need a cartography that is as detailed and precise as possible. As Ramus (2006) points out, while the causal chain is clearly anchored in both ends, the genetic analysis of any disorder “can only be as good as the characterization of the phenotype, and cognitive phenotyping is (or should be) in the hands of cognitive scientists (p.249)”. Thus, at the cognitive end, progress can be made by being clear about exactly what part of the causal chain is being studied – and at what level of analysis – and by laying out as clearly as possible the structures and mechanisms involved.

Levels of Analysis and the Importance of Theories at Each Level

In this regard, it has become commonplace in work on the theoretical foundations of cognitive science to distinguish among three levels of analysis at which information processing systems may be described. In his influential book *Vision*, David Marr (1982) dubbed these levels the *computational*, the *algorithmic*, and the *implementational*. Pylyshyn (1984) calls these levels the semantic, syntactic, and physical, and they are also sometimes called the levels of content, form, and medium (Glass, Holyoak, and Santa 1979). Roughly speaking, the computational level corresponds to the level at which the system's goal is described (in terms of mental representations). The algorithmic level corresponds to the level at which the system's method can be described (in terms of cognitive processes); and finally, the implementational level specifies the physical means by which the representations and algorithms are realized (e.g., neurons in the case brains, or silicon chips for computers).

This division of labor, in turn, entails that one should strive to formulate, or at least use, explanatory theories *at the relevant level of analysis*. This point should be obvious, but as we will discover later, it is often overlooked, leading to unnecessary confusion. For example, if the goal is to try to understand *language* abilities in individuals with Williams Syndrome at the *computational level*, as will be the case here, we need to use theoretical concepts from linguistic theory –e.g., noun, verb, quantifier scope, etc. – and not notions such as plasticity, interactivity, or redundancy which may very well apply at a different level of analysis¹. Likewise, no-one would seriously try to explain how chess pieces move on a board by using quantum-mechanical rules and justifying such a move by pointing out that, after all, chess sets are made out of atoms.

The Importance of Development

We now have discussed two of the three ingredients in the proposed approach: the need to identify a clear level of analysis in the causal chain between genes and behavior, and the related need to rely on explanatory theories at the chosen level of analysis. The third ingredient that we believe plays an important role in our understanding of genetically induced developmental disorders such as Williams syndrome is the role of development itself, and in particular *typical* development. Since Williams syndrome is a genetic deficit that is present at birth, the syndrome is, by definition, developmental. The fact that cognition unfolds over developmental time entails that hypotheses regarding the nature of a linguistic deficit – or lack thereof – will have to take into account aspects of the developmental process (Karmiloff-Smith, 1998).

There are two views on precisely why development matters. One view is that to the extent that the mind is modular, it doesn't start that way, but rather, becomes modular as a result of the developmental process itself (Karmiloff-Smith, 2007; Elman et al., 1996; Karmiloff-Smith, 1998). However, if the developmental process operates within an atypical brain, as in the case of Williams Syndrome, then this altered brain, in turn, will lead to altered learning mechanisms, and ultimately, qualitatively different knowledge representations at the cognitive level (Karmiloff-Smith, 1998).

An alternative view is that while studies of developmental deficits must certainly take into account the developmental process itself, and not prejudge representation solely by what is known about adult brain damage studies, there are nevertheless very strong constraints on what a given cognitive system—such as language-- could look like. This view is rooted in the idea that cognitive systems normally develop from origins that themselves embody strong constraints on their architecture. This idea stems from the notion that there are core aspects of human knowledge such as object, space, language, number, and social understanding that emerge very early in development, without formal instruction or otherwise highly specific experiential constraints (Spelke, 2000; Carey, 2009).

These two views can be empirically tested. For example, one can examine in detail whether the language that is produced and comprehended by people with WS is best understood as a system that is characterized by the same underlying linguistic structures that are thought to characterize normal language; or alternatively, whether that language is best characterized by missing components of the theoretical system and/or completely different units, structures, and rules. Indeed, this has been at the heart of the debate about language in people with WS, and we will provide a concrete example of how this debate gets played out in the literature.

¹Or at least, we should not do so until one set of concepts can be meaningfully reduced to the other.

Note that the focus on linguistic structure can be separated from the question of whether language in people with WS (or any special population) is “normal”-- by which we usually mean, on par quantitatively and qualitatively, with typically developing children and normal adults of the same age. But this is not the only question about the endpoint of development that one can ask, nor even, perhaps, the most interesting. For example, it is possible (as we will discuss in more detail later) that people with WS develop the same linguistic system as normally developing individuals, but do so at a much slower rate, and undergo developmental arrest at some point, perhaps around adolescence (Landau, 2011; Landau & Hoffman, 2012). This would generate a language profile that looks qualitatively the same as a normally developing person *at a much younger age*. If a normally developing 4 year-old child has control of rich linguistic structures, then under the scenario of slow development and arrest at an early functional level, so too could a person with Williams syndrome.

We will return to this hypothesis later in our paper. For now, suffice it to say that the hypothesis is a likely candidate not only for many aspects of language, but also for many aspects of spatial representation, ranging from object recognition to perception of motion coherence to some aspects of navigation (Landau & Hoffman, 2012). If it proves true for language as well, it would solve the problem of how one can have a system that emerges under tight constraints-- not disrupted under even conditions of genetic deletion-- but at the same time, does not reach the functional level of a chronological age-mate.

To review, in our framework, we emphasize the importance of the cognitive level in understanding the cognitive phenotype in Williams syndrome; the importance of choosing theoretical frameworks pegged appropriately to the particular aspect of cognition being considered; and the importance of considering developmental trajectories in thinking about whether and how the theoretical entities under investigation emerge. Let's now move to a discussion of the two conflicting perspectives with which we opened our paper. In the third part, we will focus on a concrete example, apply the notions we explained in our framework, and compare the two views.

Two Conflicting Perspectives: Modularity and Neuroconstructivism

On a general level, the two views described earlier, namely the generative/modular view and neuroconstructivism, illustrate a perennial controversy over the structure and development of the human mind. These conflicting viewpoints have led to important disagreements in the case most relevant to us here, the acquisition of language. One view, most famously articulated by Noam Chomsky and Jerry Fodor, maintains that the mind contains a domain-specific set of principles dedicated to the acquisition of language — a language ‘module’ or language ‘organ’ to use classic metaphors. Mental modules, in Fodor's (1983) seminal formulation, are taken to meet a number of criteria. The ones most relevant to the study of language in Williams syndrome are domain specificity, and ontogenetic invariance (Musolino et al., 2010)². Many evolutionary psychologists have also come to view modularity as a form of functional specialization, a concept borrowed from biology (Pinker, 1997, 2005; Sperber, 1994, 2005; Tooby & Cosmides, 1992). For example, Pinker (1997) suggests that modules ought to be understood in terms of the operations they perform on domain relevant information, rather than through a list of necessary features (see Barrett & Kurzban, 2006 for a discussion of these ideas).

Another common assumption, underlying much work in theoretical linguistics, is that the language module itself has a modular structure and that it contains at least two submodules:

²Fodor's full set of criteria includes: domain specificity, obligatory firing, inaccessibility to consciousness, speed, encapsulation, shallow outputs, localization, ontogenetic invariance, and characteristic breakdown patterns.

a lexicon and a computational system. The former can be thought of as a mental dictionary containing a list of stored forms specifying category membership for entities such as nouns, verbs, etc., along with other idiosyncratic information. The latter is a set of rules used to combine lexical entries in order to construct larger structures such as words, phrases, and sentences (Chomsky, 1995). On this view of linguistic organization, advocates of modularity have suggested that WS differentially affects the lexicon and the computational system in the sense that the former is adversely affected while the latter is spared (Clahsen & Almazan, 1998; Clahsen & Temple, 2003; Pinker, 1999). For example, Clahsen and Temple (2003) express this view as follows: “They [Clahsen & Almazan, 1998] argued that these two core modules of language are dissociated in WS such that the computational (rule-based) system for language is selectively spared, while lexical representations and/or their access procedures are impaired” (p. 2).

Within the past couple of decades, an alternative to modularity has emerged, growing from work by Elman, Bates, Johnson, Karmiloff-Smith, Parisi, and Plunkett (2001). This new perspective has been applied to neurodevelopmental disorders under a framework known as neuroconstructivism (Karmiloff, 1998; Karmiloff-Smith, 1997, 1998; Karmiloff & Karmiloff-Smith, 2001; Thomas & Karmiloff-Smith, 2005; Westermann et al., 2007; Thomas, in press). One prominent case that has been used to illustrate this approach is Williams Syndrome. Two central tenets of neuroconstructivism are that individuals with WS learn language using cognitive mechanisms that are different from the ones used by typically developing individuals and that, as a consequence, *knowledge* of grammar is compromised in this disordered population.

The following quotes illustrate these two claims:

- “In sum ... Williams Syndrome also displays an abnormal cognitive phenotype in which, even where behavioral scores are equivalent to those of normal controls, the cognitive processes by which such proficiency is achieved are different (Karmiloff-Smith, 1998, p. 395).
- “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* are intact [our emphasis] (Karmiloff-Smith et al., 1997, p. 257), and finally,
- “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 ...” (Thomas & Karmiloff-Smith, 2003, p. 652).

Neuroconstructivism therefore explicitly rejects, on empirical, theoretical, and methodological grounds, the central conclusions expressed by advocates of the modularity view. To quote Karmiloff-Smith (1998), “this change in perspective means that atypical development should not be considered in terms of a catalogue of impaired and intact functions, in which non-affected modules are considered to develop normally, independently of the others. Such claims are based on the static, adult neuropsychological model which is inappropriate for understanding the dynamics of developmental disorders” (p. 390).

Instead, neuroconstructivism contends that language abilities in WS only *appear* to be impressive, mainly because individuals with WS have good auditory memory skills. This claim can be illustrated by the following quote from Karmiloff and Karmiloff-Smith (2001): “it has become increasingly clear, therefore, that the superficially impressive language skills of individuals with WS may be due to good auditory memory rather than an intact grammar module” (pp. 202–203). Furthermore, neuroconstructivism highlights the role of rote learning in WS and suggests that individuals in this population have difficulty extracting

underlying regularities and forming linguistic generalizations. The following two quotes from Karmiloff-Smith et al. (1997) illustrate these points.

- “This suggests that if WS children go about language acquisition differently from normal children ... they will end up — as they indeed do — with large vocabularies but relatively poor system building” (p. 257).
- “We challenge these claims [about modularity] and hypothesize that the mechanisms by which people with WS learn language do not follow the normal path. We argue that the language of WS people, although good given their level of mental retardation, will not turn out to be “intact” (p. 247).

To support their conclusions, neuroconstructivists often point to empirical studies by Karmiloff-Smith et al. (1997) suggesting that the comprehension of embedded clauses in English-speaking individuals is an area of weakness, and that French-speaking individuals with WS experience difficulty with certain aspects of grammatical gender. Volterra et al. (1996) as well as Capirci et al. (1996), who studied Italian-speaking individuals with WS, also report unusual syntactic and morphological errors. However, as Brock (2007) points out, further investigation is needed in order to determine the implications of such findings.

In more recent neuroconstructivist work, Thomas and Karmiloff-Smith (2003) discuss two additional hypotheses regarding potential sources of atypicality in WS language. According to what these authors call the conservative hypothesis, “... it is argued that the language we see in WS is merely the product of delayed development combined with low IQ” (p. 652). Alternatively, Thomas and Karmiloff-Smith (2003) introduce the semantics-phonology imbalance hypothesis according to which language development in WS takes place under altered constraints. Specifically, individuals with WS rely on a particular strength in auditory short-term memory while lexical semantics is a relative weakness. As a consequence of this imbalance, it is argued, individuals with WS are more reliant on phonological than on semantic information in language processing tasks, which may lead to certain patterns of atypical behavior.

It is worth pointing out that other investigators have found little empirical evidence supporting the semantics-phonology imbalance hypothesis. For example, this is the conclusion reached by Brock (2007) after an extensive review of the literature on language development in WS. Instead, Brock reports that, by and large, the available evidence is consistent with the ‘conservative hypothesis’, a conclusion echoed by Thomas (in press) who acknowledges, citing Brock’s (2007) study, that as research on WS progressed, the ‘conservative hypothesis’ seems to have eclipsed the ‘imbalance hypothesis’. It is also of interest here that other investigators have arrived at essentially the same conclusion, as illustrated by the following quote from Tager-Flusberg, Plesa-Skwerer, Faja, and Joseph (2003): “Despite claims to the contrary (Karmiloff-Smith et al., 2002), there is no evidence that children with WMS acquire language any differently than other children, although they may be delayed in the onset of first words and phrases, as would be expected given their mental retardation (Morris & Mervis, 1999)” (p. 20).

A Concrete Case of Cognitive Phenotyping

So far, we have sketched the outline of a general framework to study the relationship between genes and cognition, focusing as our case study on the development of language in individuals with Williams syndrome. Our approach emphasizes the role of three main factors: the need to specify a clear level of analysis, provide a theoretical account of the relevant cognitive structure at that level, and the importance of the (typical) developmental process itself. In order to apply these ideas, and make good on our promise to shed some light on the controversy surrounding the linguistic abilities of individuals with Williams

Syndrome, we now turn to a concrete case of cognitive phenotyping. In doing so, we will focus on knowledge of grammar in this disordered population, arguably the most controversial issue, as discussed above. Specifically, we will discuss an interpretive generalization involving the interaction of negation and the disjunction operator, *or*. By focusing on what might at first glance look like an arcane generalization, we will not only illustrate the general approach advocated here, but, more importantly, we will show that it can illuminate much broader issues in the study of syntax and semantics in typical and atypical development.

Following the approach outlined in the previous sections, we will begin by specifying a clear level of analysis for the phenomenon under investigation: the *computational* level in Marr's sense. Next, we provide an independently motivated theoretical account of our phenomenon and highlight the core grammatical principles at play. We then discuss the results of a set of experiments designed to test knowledge of these core grammatical principles in individuals with Williams syndrome. In doing so, we underscore the role played by typical development, our third factor, in accounting for the observed behavioral patterns. Finally, we bring those results to bear on the two conflicting views discussed above and show that the 'meaningful-debate-assumption', which underpins much of the literature on this topic, rests on a fallacy.

The Phenomenon

Consider the sentences in (1-2).

1. All of my friends liked the movie.
2. Some of my friends liked the movie.

Notice that whenever (1) is true, (2) is also necessarily true. In other words, if it is true that all my friends liked the movie, it must also be true that some did. Consequently, we say that (1) *entails* (2) but not vice-versa (that is, if it is true that some of my friends liked the movie, it doesn't necessarily follow that all of them did). More generally, we can say that propositions containing *all* entail equivalent propositions containing *some*, as shown in (3).

- (3) ALL A are B $\Rightarrow$ SOME A are B

Now consider the examples in (4-5)

- (4)
- a. John bought a house
 - b. John bought an expensive house

- (5)
- a. John didn't buy a house
 - b. John didn't buy an expensive house

Notice that (4a) does not entail (4b). That is, if it is true that John bought a house, it doesn't follow that it must have been an expensive one. John could very well have bought a cheap house. Interestingly, however, when the sentences are negated, as in (5), (5a) now entails (5b). In other words, if it is true that John didn't buy a house, it must also be true that he didn't buy an expensive house. Notice further that the entailment patterns created by the presence of negation have a directionality. That is, negation licenses inferences from sets (the set of houses) to subsets (the set of expensive houses), and for that reason, it is called a downward entailing expression.

Linguists have noticed that downward entailing expressions, such as negation, have an interesting set of properties. One such property concerns the way downward entailing expressions affect the interpretation of other logical operators, such as the disjunction operator, *or*. First, notice that in a declarative sentence like (6), *or* typically receives what is called a *disjunctive* or *exclusive* interpretation. That is, the most natural interpretation of (6) is that John bought one kind of car or the other, but not both. Another way to say this is that (6) does not entail (7). To see this, imagine that John bought a BMW. This would clearly make (6) true, but it would also make (7) false. In other words, it isn't the case that whenever (6) is true, (7) must also be true – (6) does not entail (7).

(6) John bought a BMW or a Mercedes

(7) John bought a BMW and John bought a Mercedes

Consider now what happens when (6) and (7) are negated, as in (8) and (9).

(8) John didn't buy a BMW or a Mercedes

(9) John didn't buy a BMW and John didn't buy a Mercedes

In the presence of negation, disjunction, *or*, is now interpreted conjunctively, or inclusively. What this means is that (8) is interpreted to mean that John bought neither a BMW nor a Mercedes. Crucially, unlike in the case of (6) and (7), when negation is present, (8) entails (9). This interpretive pattern, relating statements containing disjunction, *or*, to equivalent statements containing conjunction, *and*, is captured by one of DeMorgan's famous laws of propositional logic, as shown in (10). In plain English, (10) states that the negation of the disjunction of two propositions is logically equivalent to the conjunction of their negations, (11).

(10) $\neg (P \vee Q) \Leftrightarrow (\neg P) \wedge (\neg Q)$

(11) not (P or Q) $\Leftrightarrow$ (not P) and (not Q)

Consider now the examples in (12) and (13) and the apparent problem they pose. Both examples contain negation and the disjunctive operator, *or*, and yet, only (12) obeys De Morgan's law. In other words, (13a) entails (13b), but (12a) does not entail (12b). That is, we get a disjunctive reading of *or* in (12) and a conjunctive reading in (13). In order to understand why this should be, and how exactly negation interacts with disjunction, we now provide a theoretical account of the phenomenon.

(12)

- a. The man who didn't get a pay raise bought a BMW or a Mercedes.
- b. The man who didn't get a pay raise bought a BMW AND the man who didn't get a pay raise bought a Mercedes.

(13)

- a. The man who got a pay raise didn't buy a BMW or a Mercedes.
- b. The man who got a pay raise didn't buy a BMW AND the man who got a pay raise didn't buy a Mercedes.

The Theoretical Account

In order to understand how negation interacts with disjunction, we need to introduce three important and independently motivated theoretical notions: syntactic structure, c-command, and scope. The notion of syntactic structure should be fairly familiar. The idea is that linguistic representations of sentences are hierarchical and that the rules of grammar make

reference to this hierarchical organization. Thus, a sentence like (14) is not just a string of words but rather can be represented as being hierarchically organized, as shown in (15a–b).

(14) The man danced with the woman.

(15)

a. [S [NP The man] [VP danced [PP with [NP the woman]]]]

b.

C-command is a structural relation defined over hierarchical structure and it is defined as follows.

(15)x c-command y iff:

- a. $x \neq y$
- b. Neither x dominates y nor y dominates x
- c. The first branching node that dominates x also dominates y

A useful rule of thumb to calculate the c-command domain of an expression, for example the c-command domain of the verb *danced* in (15), is to start at the V node in the tree diagram, go up to the first branching point (i.e. VP), and then follow the descending path in all directions. Everything contained within the descending path from the first branching node (the blue box in the diagram in (16) falls within the c-command domain of the expression under consideration.

(16)

Hierarchical structure and c-command, in turn, play a crucial role in the notion of scope. In order to illustrate the notion of scope, we can use a simple mathematical analogy. Consider

the expressions $3 \times (2+1)$ and $(3 \times 2) + 1$. The scope of $3 \times$ (the number 3 followed by the multiplication sign) can be thought of as its domain of application. Thus, in $3 \times (2+1)$, the expression $(2+1)$ falls within the scope of the expression $3 \times$. However, in $(3 \times 2) + 1$, the number 2 falls within the scope of the expression $3 \times$ but the number 1 falls outside of its scope. Finally notice that different scope relations yield different results when the expressions are computed (i.e., $3 \times (2+1) = 9$ whereas $(3 \times 2) + 1 = 7$).

Let us now consider the notion of scope as it applies to language. Certain expressions, such as negation, for example, are “scope-bearing expressions”. Scope, in turn, is defined in terms of the notion of c-command, as given in (17).

- (17) Scope principle: an expression X takes scope over an expression Y iff X c-commands Y.

Going back to the diagram in (16) then, (17) simply means that the verb *danced* takes scope over the prepositional phrase *with the woman*, as indicated by the c-command relation. Returning to the way negation and disjunction interact, we can now say that *or* receives a conjunctive interpretation — i.e., that De Morgan's law applies — if *or* falls within the scope of negation; that is, if it falls within the c-command of negation. We can now return to the examples in (11) and (12), repeated here as (18), and see that negation, which is too deeply embedded within the subject NP does not c-command *or* in (18a) but that it does c-command *or* in (18b). This explains why only (18b) obeys DeMorgan's law. The reader can verify that for himself/herself by looking at the tree diagrams provided in (19a) and (19b) and applying the informal (or the formal) definition given for c-command.

(18)

- a. [IP [NP The man who didn't get a pay raise] [I' [VP bought a BMW or a Mercedes]]]
- b. [IP [NP The man who got a pay raise] [I' didn't [VP buy a BMW or a Mercedes]]](19a)(19b)

(19a)

(19b)

To recap, negation, a downward-entailing operator, can interact with disjunction, *or*, to give rise to a pattern of entailment relations known as DeMorgan's laws of propositional logic. Whether or not the interpretation of negation and disjunction is subject to DeMorgan's laws, in turn, depends on the kind of syntactic relation holding between these two elements. Specifically, for DeMorgan's law to hold, *or* must occur in the scope, that is, the c-command domain, of negation.

Testing the Linguistic Knowledge of People with WS

Given the ideas outlined in the previous two sections, we developed an experiment designed to test two issues (Musolino, Chunyo, & Landau, 2010). First, we wanted to know whether, at the *computational level* of analysis, there is evidence for the *theoretical* constructs described in the previous section in individuals with Williams syndrome. To the extent that this is the case, we then can assess the second question: How does the functional level reached by individuals with WS compare to the absolute levels of performance reached by normally developing children of different ages. This, in turn, will directly bear on the hypothesis of 'slow development followed by arrest' which we introduced earlier. The prediction from this view is that people with WS should, even as adults, show a quantitative and qualitative profile similar to normally developing children who are much younger.

More specifically, in our experiment we sought to determine whether knowledge of scope, c-command, and De Morgan's law is present and engaged in individuals with Williams Syndrome. We therefore devised an experiment to assess how such individuals interpret sentences containing negation and disjunction. The basic idea behind the experiment is this: Sentences such as the following will be very similar to each other but will have quite different meanings:

- The cat who meowed will not be given a fish or milk
- The cat who does not meow will be given a fish or milk

These sentences contain virtually identical sets of words, including negation and disjunction, and both have relativized subjects. As discussed above, however, these two sentences have different truth conditions. How then can someone know that one sentence is true in a situation in which the cat is given fish or milk while the other sentence is false in the same situation? The answer is that when people interpret such sentences, they engage the rules and principles outlined above (or their theoretical equivalent). In other words, it is *because* people have tacit knowledge of scope, c-command, and deMorgan's law of propositional logic that they can assign the correct meanings to closely related pairs of sentences containing negation and disjunction. Thus, to the extent that experimental participants can correctly distinguish such sentences, one can then infer that they must possess knowledge of the abstract principles described above.

Using a computerized Truth Value Judgment Task, we tested individuals with Williams Syndrome on their interpretation of sentences containing both negation and disjunction. As mentioned above, our primary goal was to determine whether knowledge of scope, c-command, and DeMorgans' law is present in individuals with Williams Syndrome and appropriately engaged during language comprehension. To that end, we tested participants on their interpretation of sentences like the ones in (20) and (21), in two experimental conditions. In the Precede condition, participants heard sentences in which negation only precedes disjunction – but does not c-command it – (21). In the C-command condition, participants heard sentences in which negation both precedes and c-commands disjunction, (20).

(20) The cat who meows will not be given a fish or milk

(21) The cat who does not meow will be given a fish or milk.

Participants heard pre-recorded versions of the sentences used as descriptions of animated vignettes were displayed on a computer screen. The table below illustrates our procedure and the fact that sentences like (20) and (21) have different truth conditions.

Our experiment also included four control conditions designed to isolate each of the elements that interacted in the experimental sentences. Specifically, being able to correctly calculate the truth conditions of sentences such as (20) and (21) also involves understanding (a) the meaning of *or*, (b) the meaning of negation (*not*), (c) knowledge of De Morgan's laws, and (d) being able to parse a relative clause. The four control conditions were therefore designed to independently test for knowledge of (a–d).

We tested 12 individuals with Williams Syndrome (mean age, 16;4 (year, month), age range, 11;10–21;11), a group of typically developing children matched to the individuals with WS on the basis of mental age (mean age, 6;1), a group of younger typically developing children (mean age, 4;3), and a group of 12 typical adult native speakers of English.

We found that the WS and MA control groups did not differ from each other on the control conditions and that both groups performed close to ceiling (90.8% vs. 94.5% correct responses on average, respectively; see Figure 2). In the experimental conditions, both groups performed above what would be predicted by chance; however, in this case, MA controls significantly outperformed the WS group (89.5% vs. 76% correct, respectively, averaging between the two conditions; see Figure 3). Thus, the analysis revealed a group (WS vs. MA) by condition (control vs. experimental) interaction.

From these results, one can draw two important conclusions. The first concerns the above-chance performance on the experimental conditions; this demonstrates that individuals with WS as well as MA controls have knowledge of the abstract principles of scope, c-command, and deMorgan's law of propositional logic³. Without these, they could not have drawn correct inferences about the meanings in the two types of experimental sentence, which had virtually the same lexical items, but in different syntactic configurations. The second conclusion is that whatever gives rise to the difference in performance between WS and MA controls in the experimental conditions is unlikely to be due to lack of knowledge of the individual elements interacting in the experimental conditions. This is because, on the control conditions, specifically designed to test for such knowledge, the performance of individuals with WS did not differ from that of MA controls.

One possible explanation for the fact that WS and MA differed only in the experimental conditions, is that interpreting sentences which contain negation, disjunction and a relativized subject — as opposed to having to interpret these elements one at a time in the control conditions — leads to more processing difficulties for WS than for MA. By this, we

mean that increased syntactic complexity might result in increased burden on processes such as working memory, which are required to hold parts of a sentence in mind while computing the meaning over all parts. We know that people with WS have verbal working memory limitations (Clarke & Clarke, 1974; Ellis, 1978; Hulme & Mackenzie, 1992; McDade & Adler, 1980; Vicari, Carlesimo, Albertini, & Caltagirone, 1994; Vicari, Carlesimo, & Caltagirone, 1995; Vicari, Carlesimo, Brizzolara, & Pezzini, 1995) and that normally developing children show systematic increases in verbal working memory during early and later childhood. Assuming that working memory does contribute to processing complex sentences, we would predict that certain combinations of operations could depress performance for both individuals with WS and normally developing children.

To further investigate this question, we used a multiple regression analysis to assess the contribution of each of the control variables to overall performance in the experimental conditions. We found that for both MA matched children and individuals with WS, performance on the control conditions was significantly related to performance on the experimental conditions. Moreover, in both cases, negation is a significant predictor of overall performance. We also found that the combination of negation and disjunction seems to create more processing difficulty for people with WS than for the MA matched group. One way to interpret these results is that individuals with WS can handle processing complexity – and therefore perform like MA controls – but that working memory limitations will at some point limit the WS individual to a greater degree than the normally developing mental age match. Once that limit is crossed, performance deteriorates more rapidly for individuals with WS compared to MA controls.

This hypothesis, in turn, makes an interesting prediction, namely that individuals with less mature processing skills-- such as younger typically developing children (i.e., children younger than our mental age matches, who were on average 6 years old)--should, like individuals with WS, experience more difficulty with the experimental conditions than the control conditions. In order to test this prediction, we compared the performance of individuals with WS with that of a group of typically developing 4 year-olds. Recall that comparing the performance of individuals with WS and MA controls revealed a group by condition interaction. In other words, while the two groups did not differ on the four control conditions (each of which tested a single property), individuals with WS did not perform as well as MA controls on the experimental conditions.

As predicted, the comparison between individuals with WS and normally developing 4 year-olds uncovered no such group by condition interaction. To the extent that the difference in performance between individuals with WS and 4-year-olds on the one hand, and MA matches (6 year-olds) on the other, reflects the greater complexity of the experimental conditions compared to the control conditions, we reasoned that we might be able to observe this effect when comparing the 4 and 6 year-olds directly. Indeed, this is precisely what we

³One of the reviewers raises the possibility that individuals with WS may interpret our sentences correctly not because they know about c-command, scope, etc. but rather because they are using simpler 'heuristics' such as, for example, "cat meows → no food" and "cat not meow → food". First, notice that because we used 16 different target sentences in our experimental conditions (precede and c-command), all using different combinations of lexical items, you would need 16 separate 'heuristics' like the one proposed here to account for the facts we report (e.g., a policeman/doughnut heuristic, a pirate/jewel one, etc.; see Musolino et al.'s original article for a list of the stimuli). Second, the proposed heuristic would not actually work in the 'cat meowing' case. Indeed, in the case of a sentence like (20) *The cat who meows will not be given a fish or milk*, it is not simply that the sentence will be true if the meowing cat gets no food. To be sure, the sentence will be true if the meowing cat gets neither a fish nor milk; however, it would also be true if the meowing cat gets food *other* than a fish or milk. In order to appreciate that, one needs to understand the meaning of disjunction, *or*, and how it interacts with negation. So one could try to construct a more detailed heuristic to take that into account, and also make that heuristic abstract enough to account for cases involving policemen and doughnuts, pirates and jewels, etc. Doing so however, would amount to formulating the kind of abstract notions that linguists take to be underlying people's ability to correctly interpret sentences containing negation and disjunction (e.g., scope, c-command, etc.). For a more detailed discussion of this point, see Musolino et al. (2010; p. 150).

found: 4-year-olds behaved disproportionately worse than 6-year-olds on the experimental conditions compared to the control conditions.

This pattern-- rough equivalence of individuals with WS and 4 year-old normally developing children-- is predicted by the theory of slow growth followed by developmental arrest that we discussed earlier (and is spelled out in more detail in Landau & Hoffman, 2012). Specifically, if people with WS show very slow development coupled with developmental arrest at an early functional level, their language should look very similar to normally developing children at a much younger age. It is noteworthy that an extensive review of findings on syntax and semantics in people with WS have shown just this pattern, with one exception—overall vocabulary (Landau & Hoffman, 2012). Indeed, standardized measures of vocabulary do show growth in people with WS through adolescence, leading to the common observation that people with WS have strong vocabularies, relative to other skills. This is not surprising, given that vocabulary in normal individuals continues to grow throughout the lifetime. By contrast, much of syntax and formal semantics-- such as we have tested here-- show effects of age at which they are learned. As is well known, young children acquire many properties of syntax, semantics, and morphology in their preschool years, but children and adults who start learning a second language later in life struggle to achieve native fluency (Newport, 1990; Munnich & Landau, 2009).

Comparing the Two Frameworks

As discussed earlier, studies of the linguistic abilities of individuals with WS have led to strongly conflicting conclusions regarding the nature of the cognitive profile in this disordered population. By far the most common assumption is that this tension stems from a meaningful debate between two sides (e.g., Brock, 2007; Mervis and Becerra, 2007; Zukowski, 2004). A recent formulation of this thesis holds that interpretations conflict because the data themselves are ambiguous: look at them through a generative/modular lens and you will reach one set of conclusions; put on neuroconstructivist goggles and you will reach a radically different set of conclusions (Thomas et al., 2010). Implicit in this perspective are the assumptions that the two competing frameworks address the same questions, and that they provide explanatorily equivalent accounts of the relevant empirical phenomena.

Another natural experiment, this one occurring through a set of publications and played out in the pages of the journal *Language Learning and Development*, gives us an opportunity to compare the two competing set of ideas in the equivalent of a controlled environment. Along with the publication of Musolino et al.'s (2010) study came a detailed commentary by Michael Thomas and his group, which spells out the neuroconstructivist perspective. For Thomas et al. (2010), the data presented by Musolino et al. (2010) were ambiguous and contained aspects that could be viewed to support one theory or the other. To quote Thomas et al. (2010): "...the behavioral data had aspects that could be viewed by both Position 1 and Position 2 [generative/modular vs. neuroconstructivist] 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). Moreover, Thomas et al. present their approach as an *alternative* to the one developed by Musolino et al.: "In this article, we consider the theoretical assumptions necessary to draw conclusions such as those of Musolino, Chunyo, and Landau (henceforth MCL) from the observed behavioral data and place them in the context of *alternative theoretical assumptions* that would lead to a different conclusion. [our emphasis]"(p. 163)

Let us now turn to the behavioral data that are claimed to have different possible interpretations depending on one's theoretical inclinations. What exactly are they? Recall that in our experimental conditions, we (MCL) made two observations. The first is that both groups of participants (WS and MA controls) were able to distinguish between statements in the precede and c-command conditions at levels above what would be predicted by chance. Let's call this observation 1. The second observation is that participants in the MA control group performed better, by about 13%, compared to individuals with WS. Let's call this observation 2 (Figure 4).

Thomas et al.'s claim now translates as follows: depending on whether one decides to focus on observation 1 or observation 2, one will reach different conclusions. Specifically, observation 1 suggests the presence of knowledge in both groups whereas observation 2, because of the differences in behavioral levels, suggests that the two groups are different. Hence Thomas et al.'s conclusion that: "Given a certain set of theoretical assumptions (e.g., generative/modular), fairly poor performance can nevertheless be viewed as indicating typical development. Given other theoretical assumptions (e.g., a neuroconstructivist view of constrained development), the same data can be viewed as indicative of atypicality." (p. 162)⁴.

One can indeed worry about labels here, i.e. whether we should say that WS development is typical or atypical, but this would be missing the whole point. Of course development is in some ways atypical in individuals with WS; nobody has ever claimed otherwise. The real question is not how we should label the data, but how we should *explain* them.

Let us begin with Observation 1. How can we account for above chance performance? On the account that we developed (MCL, 2010), there is a very precise reason for why both groups are able to distinguish c-command and precede statements: At the computational level, they possess and engage the detailed knowledge outlined in our theoretical section. This account is explanatory because it is not a mere restatement of the fact that precede and c-command statements are different. Instead, it specifies precisely *what* the difference is in terms of abstract and independently motivated principles, and *how* such a difference yields the observed interpretive patterns.

Recall now that Thomas et al. argue that neuroconstructivism provides an alternative explanation of the phenomenon under investigation. In this regard, it is informative to consider what these authors propose. According to Thomas et al the neuroconstructivism approach emphasizes: "... both (to) the use of particular methodological approaches ... 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) and "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).

So instead of concepts such as scope, c-command, and entailment patterns, Thomas et al. argued that we should use notions such as plasticity, adaptation, interactivity and lexical or

⁴It 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). Describing these results as "fairly poor performance" might be useful for rhetorical purposes, but it is hard to see how this performance is "poor", as it must reflect possession of a highly abstract knowledge system. From that point of view, we find it misleading to characterize the performance as "poor".

semantic/pragmatic compensatory mechanisms. What is important about concepts such as scope, c-command, and entailment relations is that they can be shown to play an explanatory role at what Marr called the computational level of analysis. Indeed, we devoted a whole section showing precisely how such an account works. On the other hand, notions such as plasticity, adaptation, and interactivity, because they are so vaguely defined in this context, are not useful at the computational level. To be sure, nobody knows how one could account for say, the interpretive contrast between the examples in (22) and (23) in terms of plasticity or interactivity.

(22) The cat who meows will not be given a fish or milk

(23) The cat who does not meow will be given a fish or milk.

One could of course always argue that notions such as plasticity, interactivity and the like were never meant to apply at the computational level, but perhaps at the algorithmic or the physical levels. If so, one could not say that they represent an alternative account of what was proposed at the computational level, as Thomas et al. (2010) seem to imply. More importantly, however, one would still be left with the non-trivial task of showing precisely how notions such as plasticity or interactivity at say, the physical or algorithmic levels, meaningfully relate to notions such as scope and c-command at the computational level.

We can now come back to our two observations. Take observation 1 again (above chance behavior in the experimental conditions). One can now see that while one side has an explanatory account of that observation, the other side does not. On the account developed here, participants can distinguish very similar pairs of sentences containing negation and disjunction precisely *because* of their knowledge of syntactic structure, scope, c-command, and de Morgan's law of propositional logic. In contrast, notions like plasticity and interactivity explain nothing about this finding. Turning to observation 2, Musolino et al. (2010) proposed that the small differences in behavioral levels between individuals with WS and MA controls on the experimental conditions do not stem from problems with linguistic knowledge since the two subject groups do not differ on the control conditions which test for knowledge of the individual components interacting in the experimental conditions. Rather, Musolino et al. concluded that the combined complexity of the elements involved is what is likely to give rise to the difference between WS and MA. Furthermore, they speculated that this may have to do with differences in computational resources such as working memory, and showed 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 explanation is consistent with our emphasis on the role of development, and with our hypothesis of slow development followed by arrest at the functional level of younger, typically developing children (roughly 5-year-olds). While this may not be the right explanation, or even the best one, here too, it is a testable hypothesis and the data are consistent with it. We do not believe that neuroconstructivism offers a meaningful alternative for this set of empirical facts.

Evaluating the Meaningful Debate Assumption

In the previous section, we focused on the details of the account developed by Musolino et al. (2010) and the neuroconstructivist alternative proposed by Thomas et al. (2010). Doing so allowed us to uncover a number of clues that have important implications for the 'meaningful debate assumption' put forth by Thomas et al. (2010) and found in most of the literature on this topic (see Brock, 2007; Mervis and Becerra, 2007; Zukowski, in press, inter alios). Recall that this hypothesis rests on two key assumptions, namely that (a) the two competing frameworks address the same questions, and (b) that they provide explanatorily equivalent accounts of the relevant empirical phenomenon. The upshot of our discussion is that these two assumptions are untenable. Regarding (a), neuroconstructivism seems to be

concerned with appropriate labeling – should we call this or that behavior (a) typical? – whereas Musolino et al. (2010) asked a different question – a standard one in the field of language acquisition – about the presence of linguistic knowledge. Regarding the second assumption, (b), our discussion reveals, as we have taken great pains to show, that the two frameworks *do not* provide explanatorily equivalent accounts of the relevant empirical phenomena.

This difference has profound implications for a range of issues surrounding the ‘debate’ over language abilities in individuals with Williams syndrome. For one thing, if one does not have a theory of the relevant empirical phenomenon (e.g., the interaction of negation and disjunction, relative clauses, etc.) at the relevant level of analysis (in this case Marr’s computational level), then one cannot say anything meaningful about questions pertaining to linguistic knowledge. Returning to our earlier analogy, this would amount to trying to understand the behavior of chess players (engaged in chess-playing, of course) without an account of what the rules of chess are. And so instead, as we have seen, Thomas et al. reframe the debate in terms of a different question about (a)typicality.

In this regard, another important point of contention between competing accounts of language development in individuals with Williams syndrome is the question of whether language, or aspects thereof, are ‘intact’ in this disordered population. In their discussion of these issues, Musolino et al. (2010) point out that the notion of “intact” grammar in WS can be defined in at least two ways:

Grammar is intact in WS if:

- a. *Grammatical knowledge* is present and has the same structure as posited in the typical system, or
- b. *Behavioral* levels on language tasks are at least as good as those of the relevant comparison group (e.g., individuals matched for chronological age, mental age, etc.)

Moreover, Musolino et al. argue that (a) should be relevant criterion for the area of cognitive science that is concerned with knowledge representations, while (b) is relevant for a different set of questions, perhaps concerning limits on working memory available to use grammatical knowledge. Thus, in mentalistic terms, a claim that grammar is intact is a claim that at the computational level of analysis, the mental representations found in say, individuals with WS, are the same structures as those posited in the typical, mature system. To quote Zukowski (2008): “The question of whether WS language is intact or not is most meaningful with respect to *grammatical knowledge* [our emphasis]. If people with WS are able to acquire normal grammars despite the fact that their brain development is slightly atypical, this would contradict expectations that have been articulated by some researchers working within a neuroconstructivist framework (Karmiloff-Smith, 1998; Thomas et al., 2001). On the other hand, if poor comprehension of relative clauses by people with WS is due to difficulty processing structures that are nevertheless part of the grammatical competence system, this would not bear on the debate.” (p. 3/4).

Returning to our chess analogy, the question of whether knowledge of chess is ‘intact’ is most meaningful when asked about knowledge of the rules of chess. Imagine for example two populations that differed in chess-playing behavior. Population A, compared to population B, would take much longer to play each move, would wipe each piece before putting it back on the board, would always use the same opening, and would never castle. Such differences in chess-playing behavior between population A and B might be informative at some level and for some purposes, but they do not bear on the question of whether knowledge of the rules of chess is compromised in population A. In fact, such

differences may very well have nothing to do with knowledge of chess itself. In contrast, suppose that population A, compared to population B, was found to move its rooks along diagonals or its bishops along straight lines⁵. Here too, one would observe a difference in chess-playing behavior between the two populations, but it would have a very different meaning. In this case, such behavior would indicate that knowledge of the rules of chess is compromised in population A. Finally, notice that without a theory of what the rules of chess are, one would have no way of distinguishing the two examples above, both of which involve differences in 'chess-playing behavior' between two populations.

What this means for our current purposes is that without a theory of the relevant linguistic knowledge at the relevant level of analysis (two of our core desiderata) one cannot meaningfully ask the question of whether grammar is 'intact'. One could of course attempt to do so and counter claims that knowledge is present and engaged by observing that behavioral levels between the groups of participants are different. However, as our previous analogy illustrates, and as Zukowski points out, this would amount to comparing generative apples with neuroconstructivist oranges, thereby only giving rise to the 'illusion' of a meaningful debate. Thus, in addition to different standards regarding what constitutes a scientific explanation, explored at length in the previous section, the 'debate' over language abilities in WS has been fueled by confusion over the meaning of terms such as 'intact' and the level of analysis at which they ought to apply.

To summarize, we have shown that the 'meaningful debate assumption' rests on a fallacy that stems from the use of different explanatory standards and the associated confusion over key notions (e.g., 'intact') that follows from such a difference. This conclusion, in turn, follows from the application of the criteria discussed as part of the approach advocated here. Recall now that in addition to being presented as theoretical 'alternatives', the generative/modular approach and neuroconstructivism also make competing empirical claims regarding the status of grammatical knowledge in individuals with Williams Syndrome. However, an important, albeit undesirable, consequence of the 'meaningful debate assumption' is that it has the effect of rendering theories immune from empirical falsification in spite of clearly incompatible empirical claims.

This is because any data presented as a potential refutation of neuroconstructivist claims regarding language abilities in WS (e.g. that knowledge of grammar isn't 'intact', that is only superficially good, etc.), as in the case of Musolino et al. (2010), for example, can be countered by the objection that those data are relevant *only when viewed from a particular theoretical perspective*. As we have seen, this is indeed the strategy adopted by Thomas et al. (2010). Thus, the 'meaningful debate assumption', in our view, has been a major contributor to the unproductive, polarized discussion found in the literature on language in Williams syndrome. However, our conclusion that the 'meaningful debate assumption' is untenable suggests a way out of the impasse described above. Following the approach developed here, we now have a clear criterion that can be used to assess competing empirical claims regarding the nature of language abilities in individuals with Williams Syndrome.

As discussed earlier, in order to decide whether grammar is spared or impaired in this disordered population, we need to empirically determine whether (a) is true. In other words, we need to ask whether *grammatical knowledge* is present in people with WS and has the same structure as posited in the typical system. Returning to our chess analogy, what we want to know is whether individuals with Williams syndrome believe that they should move their bishops along straight lines and their rooks along diagonals (which would violate the

⁵For readers unfamiliar with the rules of chess, rooks move along straight lines and bishops along diagonals.

rules of chess). To the extent that (a) is true, then neuroconstructivist claims about impaired grammar would be falsified (Musolino et al. 2010; Zukowski, 2008). On the other hand, if (a) is false, then generative/modular claims would need to be revised.

The results of Musolino et al.'s (2010) study demonstrate that knowledge of core syntactic and semantic principles (e.g., scope, c-command, de Morgan's laws of propositional logic) is indeed present and engaged in individuals with WS, as it is in typically developing and normal, mature individuals. In other words, the results support the conclusion that (a) is true. More generally, there is evidence that the same conclusion holds for knowledge of a number of other core grammatical principles that have been investigated in people with WS, including knowledge of the principles underlying the production and comprehension of relative clauses (Zukowski, 2004; Zukowski, 2008), binding and passives (Bellugi et al., 1990; Clahsen & Almazan, 1998; Ring & Clahsen, 2005; Perovic and Wexler, 2007), regular morphology (Bromberg, Ullman, Marcus, Kelly, & Levine, 1995; Clahsen et al., 2004; Pléh, Lukács, & Racsmány, 2003; Clahsen & Almazan, 1998; Penke & Krause, 2004; Zukowski, 2005), and core aspects of the syntax and semantics of spatial language (Landau, 2011; Landau and Hoffman, 2012).

Moreover, in his extensive review of the literature on language in individuals with Williams Syndrome, Brock (2007) finds no significant evidence that would suggest that (a) is not true. This conclusion contradicts neuroconstructivist claims that language in Williams Syndrome is only superficially good, that such individuals are unable to extract underlying regularities, or that they rely excessively on rote learning (Karmiloff-Smith, 1998; Karmiloff and Karmiloff-Smith, 2001). To quote Brock (2007): "In general, there is also very little evidence that syntax or morphology develop atypically ... There is also little firm evidence to support the claim that individuals with Williams syndrome rely excessively on rote learning when acquiring grammatical structures." (p.116).

Finally, we would like to explore two additional consequences of the approach developed here or the 'debate' over language in people with WS. The first has to do with our discussion of levels of analysis. A central tenet of the neuroconstructivist approach, as explained by Karmiloff-Smith (1998), is that because individuals with WS start out with a different set of genes, compared to typically developing individuals, and therefore slightly different brains, this must entail that structures at the cognitive level must also be qualitatively different in the two populations. In other words, missing genes give rise to abnormal brains, abnormal brains lead to altered learning mechanisms, and altered learning mechanisms yield different grammars. To quote Karmiloff-Smith (1998): "In sum, brain volume, brain anatomy, brain chemistry, hemispheric asymmetry, and the temporal patterns of brain activity are all atypical in people with WS. How could the resulting cognitive system be described in terms of normal brains with parts intact and parts impaired, as the popular view holds? Rather, the brains of infants with WS develop differently from the outset, which has subtle widespread repercussions at the cognitive level ..." (p. 393).

Notice that the same argument could be made by replacing 'genes' with 'environment' – both of which obviously matter – in Karmiloff-Smith equation. So environmental differences should lead to different brains, which in turn should lead to different learning mechanisms, leading, once again, to different grammars. However, it is obvious that such a statement should not be regarded as an a priori truth, but rather, as an empirical question. In fact, one of the central conclusions of half a century of research on language acquisition is that widely different environmental conditions can lead to strikingly similar cognitive structures in the case of language (Landau and Gleitman, 1985; Goldin-Meadow, 2003; Newport and Gleitman, 1990). Likewise, whether different genes and altered brains lead to

different grammars should be settled empirically rather than given as a necessary a priori conclusion.

What this means, in terms of levels of analysis, is that the mapping between the implementational, algorithmic, and computation levels, to come back to Marr's distinctions, cannot be as simple and rigidly deterministic as Karmiloff-Smith (1998) suggests. Besides, since different devices can implement the same algorithm, and the same computational outcome can be achieved via different algorithmic procedures (Marr, 1982), there is no a priori reason to believe that differences at one level of analysis will automatically lead to differences at the next level. Rather, the need to specify a clear level of analysis and engage in theory-driven cognitive phenotyping, as we have argued here, compels one to not prejudge this issue and treat the question of whether genetic differences may ultimately give rise to different cognitive structures as an empirical one.

The second consequence of the approach developed here that we would like to discuss involves our third criterion, namely the importance of considering the trajectory of typical development in attempt to understand atypical populations. In their commentary on Musolino et al.'s (2010) work, Thomas et al. (2010) point out that even if one were to endorse the set of theoretical assumptions made by Musolino et al., one could nevertheless arrive at opposite conclusions regarding the status of grammatical knowledge in people with WS. To quote Thomas et al. (2010): "We have argued that the authors' conclusion is partly dependent on a set of assumptions regarding how the experimental data should be interpreted. Nevertheless, even with similar assumptions and methods, other researchers have recently come to the opposite conclusion, that is, that aspects of syntax develop atypically in WS." (p.168). This kind of alleged theoretical inconsistency, in turn, invites the conclusion that the data are in fact ambiguous, as the 'meaningful debate assumption' suggests.

To support their conclusion, Thomas et al. point to a study by Perovic and Wexler (2007) on the acquisition of passives in individuals with Williams. Specifically, Perovic and Wexler report an interesting asymmetry between passives of actional and psychological verbs. While the former are handled well by individuals with WS, the latter prove to be much more challenging. These authors conclude that certain aspects of syntax develop atypically in people with WS. And that, according to Thomas et al. (2010), is a problem for the generative/modular approach. However, Perovic and Wexler's findings are in fact perfectly compatible with the approach developed here. The key, as we argued earlier, is to take typical development into account when interpreting the behavior documented by Perovic and Wexler (2007) in individuals with WS. Doing so reveals that the asymmetry observed in people with WS regarding passives of actional and psychological verbs is also found in typically developing children. Indeed, as Perovic and Wexler themselves point out, while passives of actional verbs are generally mastered by typically developing 5-year-olds, the same is not true for passives of psychological verbs which take an additional two or three years to mature. If we are correct in assuming that people with WS undergo overall slow development, followed by early developmental arrest at the functional level of a 4 or 5-year-old child, as discussed throughout this paper, then we are in fact predicting exactly what Perovic and Wexler report, namely that passives of actional verbs will be handled well by people with WS (because they are also handled well by typically developing 5-year-olds) but that passives of psychological verbs will prove to be a challenge (because they are also a challenge for typically developing 5-year-olds).

Conclusions

In this article, we discussed an experiment of nature and its implications for the sciences of the mind. The case of Williams syndrome bears on one of Cognitive Science's holy grails: the possibility of unraveling the causal chain between genes and cognition. Here, we have sketched the outline of a general framework to study the relationship between these two domains (the genetic and the mental), focusing as our case study on the development of language in individuals with Williams syndrome. In light of the complexity of the issues at stake, our approach emphasizes the role of three key factors: the need to specify a clear level of analysis, provide a theoretical account of the relevant cognitive structure at that level, and the importance of the (typical) developmental process itself.

Perhaps not too surprisingly given the issues at stake, the discovery of Williams syndrome has also given rise to two strongly conflicting theoretical approaches – modularity and neuroconstructivism – themselves offshoots of a perennial debate between nativism and empiricism. Here, we applied a proposed framework to explore the tension created by these two conflicting perspectives. To this end, we turned to a second experiment of nature—a published paper and reactions to it— which afforded us the rare opportunity of being able to compare the two competing perspectives in what comes close to a controlled experimental setting. From this comparison, we concluded that the ‘meaningful debate assumption’—a widespread assumption suggesting that neuroconstructivism and modularity address the same questions and represent genuine theoretical alternatives—rests on a fallacy.

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Figure 1. Contrast between visuo-spatial and language abilities in Williams Syndrome

Figure 2. Proportion of correct responses in control conditions

Figure 3. Proportion of correct responses in experimental conditions

Figure 4. Performance of the WS and MA control groups on the experimental conditions

Table 1
Sample Truth conditions for Precede and C-command statements

	<i>If given a fish or milk</i>	<i>If given something else</i>
C-Command The cat who meows will not be given fish or milk.	FALSE	TRUE
Precede The cat who does not meow will be given fish or milk.	TRUE	FALSE