



# biolinguistics

volume 2, issue 1

winter 2008



ISSN 1450-3417

## A Brief Note on the Scope of *Biolinguistics*

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This issue is *Biolinguistics* 2.1 — the first issue of the second volume. Since the publication of our inaugural issue, we have received excellent submissions. We are pleased to announce that we are now in a position to fulfill our original plans to assemble four issues per volume, with the aspired publication times winter (March), spring (June), summer (September), and fall (December).

With the release of the first 2008 issue, we would like to highlight two key aspects of the journal. The first concerns the section called *Forum*. We intend this to be a space primarily devoted to state-of-the-art reports and position papers dealing with controversial issues. These reports are not always solicited; in fact, suggestions, ideally by potential authors, are always welcome. An example would be Jon Sprouse's contribution in *Biolinguistics* 1.

Concerning the position papers, we ideally envision an interactive platform in which colleagues are invited (but, as with reports, not necessarily solicited) to react on a given piece. For example, in this issue Bob Ladd, Dan Dediu, and Anna Kinsella raise important issues concerning our characterization of biolinguistic research stated in the Editorial to the first volume, in particular the “strong” and “weak” senses we understand current research to fall into.

We hereby cordially invite the readership of this new journal to respond to the forum contribution by Ladd, Dediu & Kinsella in any conceivable way — as a one-page reaction, as a full-fledged research paper, or anything in between. We would like to collect these and publish them in future Forum sections, and offer the authors of the original position paper to reply to their critics.

The second item we would like to stress here is our commitment to publish research in linguistic *theory*. In our views there already exist many excellent journals in which to publish detailed analyses of particular linguistic phenomena. We, at *Biolinguistics*, would like to focus more on studies that are concerned with fundamental theoretical constructs that help reveal the nature of the language faculty, and ultimately may help bridge the gap between biolinguistics in the weak sense and biolinguistics in the strong sense (our ultimate goal). We feel that the contributions by Juan Uriagereka in the first volume as well as those by Norbert Hornstein and Jairo Nunes and by K.A. Jayaseelan published in the

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We cannot help but thank Terje Lohndal for extremely valuable editorial support. Since we are not supported by any publisher, all editorial work (including marking up submissions and creating proofs for publication) is done by the editors and members of the task team. It is thanks to the devotion of individuals like Terje that we can pursue our goals for *Biolinguistics*.



present issue offer nice examples of the sort of theoretical work we would like to publish. And as Stephen Crain and Drew Khlentzos' article shows (as well as several of the contributions in *Biolinguistics* 1), such investigations need not at all be restricted to syntax.

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# From Spatial Cognition to Language

Boban Arsenijević

Similarities between aspects of spatial cognition and language are examined in the domains of type of computations (recursive, categorial), type of information used (descriptive and geometric), update procedures for the relevant context representations, and neuro-cognitive aspects (the role of the hippocampus). Striking similarities observed, and the fact that spatial cognitive capacities of all vertebrates are of approximately the same nature and complexity, narrow down the set of possible distinctive properties of the human cognition and the language faculty in the comparative cognition perspective. It is proposed that these properties are: (A) domain-general use of the otherwise similar computational capacities, (B) serialization of the computations of descriptive and geometric means of reference, and (C) increased importance of the update of mental representations by a group rather than just an individual.

*Keywords:* evolution of language, hippocampus, language faculty, spatial cognition

## 1. Introduction

This article compares two cognitive domains: spatial cognition and natural language. While the former is present in a quite sophisticated form at least in all vertebrates, and a number of other species, the latter is an exclusive characteristic of humans. From the aspect of evolution, this means that spatial cognition has been there in the animal world for a very long time, while language is a relatively new development. A look at some interesting similarities and differences between these two capacities may contribute to the theories of each of them. Yet, more can be revealed about a newer capacity by looking at an older one, than about an older capacity by looking at the one that is more recent. The interest of this article is set in this more informative direction: It aims at learning about language (its setting among other capacities, its origins, its structures) by comparing it to the spatial cognition capacity, more precisely to one of its

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The present work is a result of my engagement at the project "The Origins of Truth and the Origins of the Sentence" funded by NWO (The Netherlands Organization for Scientific Research). I am grateful to two anonymous reviewers for their insightful comments. The article benefited a lot from my discussions with Wolfram Hinzen, Aritz Irurtzun, Marieke Schouwstra, and Hedde Zeijlstra. The mistakes are of course mine.



components: cognitive maps.

The article is organized as follows. In sections 2 and 3, I briefly outline the major aspects of spatial cognition and language, respectively, i.e. those that compare in the most insightful way from the perspective of the article. Section 4 points to the relevant similarities, and section 5 presents and discusses some differences. In section 6, I discuss the results of the comparison, especially concentrating on the possibility that language has evolved from the spatial cognition capacity, with the crucial step being an extension of the spatial computation to a domain-general use. Section 7 concludes.

## 2. Cognitive Maps

The field of spatial cognition presents one of the better-explored domains of the cognitive neuroscience. It has acquired a significant body of knowledge, which establishes quite precise links between the functional, representational and neurological aspects of the domain. Experimental work on a wide variety of species has resulted in a broadly accepted functional architecture of spatial cognition, and in precise linking of some of these functions to particular brain areas. One of the central fields of the theory of spatial cognition is concerned with cognitive maps — a component that is prominent in spatial cognition of all vertebrates, including humans (as opposed, for instance, to dead reckoning or the so-called compass mechanisms). I briefly present those among the core elements of the theory of cognitive maps that are of particular interest for this contribution.

Cognitive maps represent territories and involve two main types of information: the map of the territory, involving places, paths between them and their geometric configurations (spatial cues), and descriptions (object-specific cues) of each of the places involved, expressed in terms of a number of associated features (Vallortigara, Zanforlin & Pasti 1990, Vallortigara, Pagni & Sovrano 2004).<sup>1</sup> A third emerging type of information is the geometric information about a place, i.e. the set of relations of a place with other places and the set of geometrically relevant properties of a place (length, height, shape): This type involves geometric information, but presents part of the description of a particular place. Together, they form the representation of the spatial context.

For instance, consider the inside of a box with a rectangular base, painted white, with one red corner in which there is a small piece of meat. A rat, placed in this box, represents this territory as a spatial context. The context representation involves a map with a number of places: two long walls, two short walls, the floor, the ceiling, two corners with a long wall to the left and a short

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<sup>1</sup> In this article, I group together the pure geometric cues and the landmark cues, since they both involve the component of a spatial structure, absent in pure descriptions. I am aware of the facts that imply that the two grouped types of cues are different and should be treated apart, but for the purposes of the contribution, their grouping does not have important consequences and is a handy simplification. In fact, it would even strengthen the point because (i) even in language there are global geometric cues (topic, focus, familiar) and the more local ones, relative to some prominent referents (e.g., proximity vs. distance in demonstratives and other elements) and (ii) introduction of one more category further stresses the categorical nature of spatial cognition, as a parallel with language.

one to the right, one of which with the additional properties of being red, emitting a smell of an edible thing and containing an object, and two corners with a long wall to the right and a short one to the left, distinguished by whether they have the 'red edible' corner to the left or to the right. As abundantly confirmed by experiments, ignoring the red color and the piece of meat, we get two pairs of indistinguishable walls and two pairs of indistinguishable corners.

The representation of a spatial context can be updated by new information acquired through a sensory input. It has been argued that this process goes via the match–mismatch procedure (Mizumori, Ragozzino & Cooper 2000), which can be briefly sketched as follows. At any point, the animal has a spatial context representation, constructed as a set of expectations for the territory it is located in. The sensory input is continuously matched with this set of expectations, leading to the preservation of the matched and correction of the non-matched expectations. The spatial context representation is thus subject to a constant update. In the given example, removing the animal from the box, moving the piece of meat to a neighboring corner and then bringing the animal back into the box would result in the update of its spatial context representation by specifying that the properties of being red and of having an edible object are now distributed over two corners, and the corner with the latter property now has a long wall to its right and a short one to its left, while the rest of the context stayed the same.

Finally, the spatial context representation serves as a background for different behavioral actions, such as movement, eating, drinking, removing obstacles, etc. These actions depend on motivational aspects, and most of them introduce the need for an update of the spatial context representation. This update may involve the integration of a path that the subject is moving on, the change of the subject's location in the map or the change of features of some place in the map (after the food is eaten, the place where it used to be loses the feature of containing an edible object).

Animals can compute complex structures from a spatial context representation, among which most prominently paths. Paths involve the source, direction and goal, but also possibly a number of places via which they reach the goal, and which may serve as intermediate cues for navigation while moving along the paths. During movement, the path needs to be regularly recomputed, updating the position of the subject on the path, and hence also on the map. Note that this involves computation of paths and places (and their features), and as such is a different mechanism from dead reckoning (vector-based computation of the position of a moving animal with respect to the starting position), although dead reckoning may be involved in the computation of paths.

When two places have the same description in terms of non-geometric features, their position relative to some other, unambiguously defined place may serve as the distinctive feature (as with the pair of corners without meat above). This implies that spatial computation involves hierarchical structures of the type '[THE\_SHELTER [BETWEEN [THE\_TREE AND THE\_ROCK [ALONG [THE\_WATER [BEHIND [THE\_HILL]]]]]]]'. It takes a hierarchical structure to represent one place as specified by a description that involves another place (and its description).

One of the major roles in the computations producing the discussed

representations, and dealing with their update and use in other capacities, is played by the hippocampus, a brain area that can be identified in a broad range of animal species. There is evidence that this is where the spatial context representation and its update are handled (Nadel, Willner & Kurz 1985, Anagnostaras, Gale & Fanselow 2001). In addition, the hippocampus appears to have a role in the coordination between this representation and the peripheral modules: the sensory input, the motivational aspects, and the behavioral actions (Jakab & Leranth 1995, Markus *et al.* 1995, Wood *et al.* 2000). This means that among other things, the hippocampus is responsible for matching the sensory input with the spatial context representation, and for selecting parts of the spatial context representation to be matched with motivational aspects and patterns of behavioral actions. In other words, (i) it pairs the sensory input with a segment from the spatial context representation, where the latter can be seen as the interpretation of the former, and (ii) triggered by different motivational aspects, it matches segments of the spatial context representation with the adequate patterns of behavioral actions, usually realized by the motoric system.

### 3. Language

Natural language grammar is traditionally defined as the system that maps between the meaning and its physical carrier. The physical carrier is produced by the motoric system and perceived by the sensory system, while the meaning is taken to be some mental representation, directly or indirectly related to the real world. Arsenijević & Hinzen (2007) argue that there is no separate level of representation reserved for the meanings of linguistic expressions. Syntax *is* the specification of the compositional structure of the complex concept that we recognize as the intuition about the meaning of an expression. And syntax directly interfaces the discourse, and drives the integration of the expression, securing that the concepts associated to the terminal lexical units are integrated in the proper discourse domain and in the proper relation with respect to other such concepts in the expression. To sum up, semantics, as the intuition about the concepts derived by linguistic expressions, relates to two empirical domains: the syntactic structure that derives these concepts, and the effects of the integration of the expression into the discourse.

The use of language always involves a discourse: the representation of the aggregate body of information relevant for the current language use. This information includes contributions of the explicit linguistic expressions uttered so far in the communication situation, the immediately relevant presupposed material, and the directly relevant parts of the non-linguistic sensory input (the communication situation). The discourse consists of referents, their descriptions in terms of different predicates, and discourse functions (topic, focus). Discourse functions mark the position of a referent within the internal organization of the discourse — is it within the speaker's, the hearer's or some remote domain, is it part of the (recent) old information or not, is it a member of some relevant set, etc. The discourse also involves paths: A remote referent can be reached via the more proximal ones: an expression like *the dog of my friend's sister* would be used

if the speaker's friend in question is more topical in the discourse than the friend's sister, and the friend's sister is more topical than the friend's sister's dog, hence deriving the path among referents: *the friend – the sister – the dog*.

The discourse can be updated from the sensory input. If the discourse involves some relevant bit of information, which is denied, or corrected, by some expression uttered within this discourse, the discourse gets updated. Even if a bit of information became part of the discourse by the contribution of an earlier linguistic expression within the same communication situation, it can be denied or corrected (e.g., someone realizes that what he said a minute ago was wrong). This means that the discourse consists of expectations, which can be modified by a strong enough sensory input.

The discourse can trigger certain behavioral actions, falling into two important classes. The first is the production of a linguistic expression: an action effecting in yet another contribution to the discourse. This is a consequence of the fact that the discourse is usually shared, and those sharing it normally want to enrich it. A direct contribution to the discourse is achieved by producing a physical entity that presents a sensory input for the other persons sharing the discourse, thus making them update their discourse representations with the relevant material. The second class is simpler: Some updates in the discourse may introduce a direct instruction for the subject to take a certain behavioral action, assuming that a sufficient motivational support is provided (like the sentence *Leave me alone!*). These updates are referred to as speech acts.

In the domain of pure sensory input, linguistic expressions, serving as one of the possible sensory inputs leading to the discourse update, present (sets of parallel) linear strings (*the-dog-of-my-friend's-sister*). They have to be assigned hierarchical structures on their way to the discourse: [*the dog [of [my friend's] sister]*]. The hierarchical structures mediating the discourse update fall in the research domain of syntax. The choice of the hierarchical structure to be matched with a linear string is usually restricted by the relatively restricted expectations in the discourse. The proper one among them is in the default case uniquely determined by two types of information: the sequencing of units forming the linear string, their ordering, and their categorical and selectional properties memorized in the lexicon.<sup>2</sup>

It has been shown that in language comprehension, hippocampus plays a central role in the syntactic integration. Recording of Event-Related Potentials (ERP) shows that syntactically incorrect sentences elicit a negative deflection of 500–800 ms in this brain area (Meyer *et al.* 2005). This does not necessarily imply that hippocampus is directly involved in the processing of the syntactic structure (see Opitz & Friederici 2003 for arguments that the rule-based aspects of grammar are computed by other centers). It does mean, however, that the hippocampus plays a role in the discourse integration, and that this role is sensitive to whether the expression that is being integrated is assigned a valid syntactic structure or not. The special activity of the hippocampus may, for

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<sup>2</sup> The phonetic string is in fact phonologically computed as not linear but hierarchical, with a relatively shallow hierarchical structure (compared to that of syntax, to which it imperfectly matches), but since this article concentrates on the structures with direct semantic effects, the issue is slightly simplified.



instance, be related to trying to use the information from the discourse to identify the most likely update — in the lack of precise information provided by syntax, but I refrain from going into speculations of this kind.

#### 4. Parallels

This section presents some striking similarities between cognitive maps and the faculty of language, concerning their general architectures and the core structural properties of the computational mechanisms behind them.

Let us consider first the general architecture of the two capacities. Grammar, the core component of the faculty of language, has for more than a century been defined as a system that maps between physical objects (carriers of messages) and meanings (contents of messages). In the language use perspective, this renders three domains:

- (A) the mental representations corresponding to the meaning;
- (B) language production, as one aspect of the physical carrier pole;
- (C) language perception as its other aspect.

Grammar maps from the perceived linguistic material to meanings, and from meanings to the behavioral patterns engaged in the production of a message by speaking, signing or writing (or in other possible ways). Moreover, it has been argued that the notion of meaning should be dispensed with, in favor of a more precise model, in which it resolves into the lexical and syntactic material of a linguistic expression on the one hand, and the effects of the integration of the expression into the relevant discourse (Arsenijević & Hinzen 2007, Hagoort & van Berkum 2007). This gets us to the following picture: There is a computational module, grammar (plus lexicon), which drives the integration of the relevant type of sensory input into a special mental representation (the discourse) and triggers the adequate patterns of behavioral actions for the retrieved segments from the mental representation. The behavioral actions are sensitive to communicational and other motivations, and may be of two types: (i) language production and actions affecting the immediate context of communication and (ii) other behavioral actions coming as a response to the changes in the discourse. As mentioned above in section 3, the hippocampus has a central role in the coordination between the discourse and the sensory aspects of language, especially in the process of discourse integration of new material.

With a high degree of parallelism, the standard model of spatial cognition involves coordination between a mental representation (the spatial context representation), the relevant sensory input and the adequate behavioral patterns (Cheng & Newcombe 2005). The context is viewed as the continuously present background stimuli, but it also includes internal aspects such as plans, goals, motivation, types of behavioral activities involved (Markus *et al.* 1995). The sensory input that does not match the relevant segment of the spatial context representation gets integrated, leading to an update of the spatial context representation (Mizumori, Ragozzino & Cooper 2000). Patterns of behavioral

actions involve two types of motivation: the general curiosity about the spatial environment and the independent, non-spatial motivations such as hunger, thirst, fear, etc. (Voicu & Schmajuk 2001). They can be divided into behavioral actions with an immediate controlled effect on the spatial context (removing an obstacle, storing food in some place, changing the landscape by, e.g., digging) and those without such effects. As mentioned above in section 2, the coordination between the sensory input, the spatial context representation and the behavioral actions is shown to involve a significant role played by the hippocampus.<sup>3</sup>

The two mechanisms of update are characterized by one important difference: The update of a cognitive map by the sensory input is unconditional, while the update of the context representation by the linguistic input is not — the subject may as well decide to discard it if in conflict with some well-established old part of the representation (in effect similar like bees do it in the experiment described below on this page<sup>4</sup>). The choice that humans have in this respect can be attributed to their rich theory of mind. Humans can deal with a number of context representations, possibly embedded in one another. When talking to Bill, apart from her own immediate world-knowledge, Sue also deals with a representation of a context shared with Bill, and a representation of the context that she assigns only to Bill (including, e.g., points that they know they view differently). Any proposition contributed by Bill can be used to update any one of these representations, a combination of two, or all three of them. Even if Sue thinks that Bill is lying, she has to update the shared representation in some way. Hence, the necessity of update is common for the two capacities, but the linguistic update is characterized by the multiple possible target representations.

One of the phenomena most frequently identified as a characteristic property of language is the capacity of reference. By use of language, humans can talk about a particular object in reality, and assign it certain relevant properties, even when this object is out of the reach of any of their senses ('displaced reference'). A counterpart of this capacity can be identified in the domain of spatial cognition, in a number of different examples. For instance, the so-called homing species can compute paths leading to a particular location even when this location is far out of the reach of their (visual, olfactive, and other) senses. Examples like this allow for the possibility that the animal does not operate over a representation of the place of homing, but instinctively computes some complex paths that bring it to a territory where it can use other navigation mechanisms.<sup>5</sup> However, an experiment with bees, referred to in Gallistel (in

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<sup>3</sup> The parallels outlined do not entail, of course, that the two capacities do not also have computational components specific for each of them, shared with other capacities, or engaged by spillover when the centers engaged are overloaded. In any case, it is beyond the goal of this contribution to make more concrete claims about the neurological aspects of the parallel. Also, the architectures as presented might reflect a more general architecture of any module involving sensory input, and not only between cognitive maps and language, but other similarities point to a tighter parallel between the two capacities discussed.

<sup>4</sup> I thank an anonymous reviewer for pointing this out to me.

<sup>5</sup> But even in this case, the difference assumed between the human awareness of a place and the animal's instinctive mechanical computation of certain geometrical and other properties is an empty one. Ultimately, our awareness of a place, or other referent, also has to be represented in terms of neuro-cognitive mechanisms.

press), proves that animals can indeed compute, using cognitive maps, information involving a particular remote place, about which they are currently receiving no sensory input. In the experiment, an artificial source of food, new in the territory, moved in three steps: from a flower field to the water, and to another flower field. Different groups of foragers visited each of the three places, and then went back to inform the community about the source of food. Their dance only had effect when they were informing about the first and the third step, i.e. when the source was in places already known as possible locations of sources of food. When informing about the source on the water, the bees ignored the dance. This shows not only that bees have representations of remote places, but also that they are able to evaluate information as true (or useful, trusted) or false (or useless). Similarly, the ability of animals to create new paths within the territory, such as yet unexplored shortcuts (Taylor, Naylor & Chechile 1999), implies that they have a representation of the place presenting the goal of the path. This all suggests that the core of computation of cognitive maps at least in some species involves a counterpart of reference: animals can compute the representation of a certain place even when it is absent from their immediate perceptive input. The fact that humans can perform more complex activities with respect to reference may be a consequence of the absence of restriction of the computation of reference to a small number of domains (e.g., space, social relations) and of the higher processing and memory human capacities of computation of the context and of referents. To the exclusion of domain generality and memory and processing capacities, the essential ingredients seem to be shared between a number of species.

The problem with reference is that there is no standard theory of it, and hence it is difficult to make a deeper comparison between linguistic reference in humans and that found in spatial cognition in a wider variety of species. At least at the descriptive level, however, some more substantial parallels can be made.

In language, referential expressions usually involve two ingredients. One is a specification of a discourse domain, and of a discourse function that the intended referent has in that domain, and it is achieved by different tools which include sentence typing, discourse function marking, and demonstrative pronouns. The other ingredient is a description, a complex concept built by a composition of a number of simpler ones. The description should be restrictive enough to reduce the number of possible candidates within the relevant discourse domain to one. The two ingredients act as the address and the name of the addressee: The discourse domain and the discourse function specify the city and the street, while the description singles out the addressee among the candidates with the same address. As an illustration, consider the passage in (1), and especially the underlined nominal expression.

- (1) Two girls went to the hairdresser. One of the girls was extremely tall. The hairdresser told the tall girl to come another day...

Its intonation, position within the sentence, and the definite article used suggest that the expression refers to an old, topical referent, part of the ongoing discourse of the preceding two sentences. The words *tall* and *girl* specify two properties, i.e.

two concepts; the syntax of the expression specifies that the two properties should be intersected, producing a more restrictive interpretation. This interpretation is the description of the referent — it is tall and girl at the same time (for sake of simplicity, I ignore the meanings of singularity and countability, also contributed by the expression). The discourse domain and the discourse function reduce the possible candidates to the hairdresser and the two girls. The description singles out the intended referent among the candidates, in this case the tall girl. Note that *girl* only would not be enough (hence the expression *the girl*, instead of the underlined *the tall girl*, would not be salient for the context).

In cognitive maps, as briefly outlined in section 2, two types of information are used: the geometric information about a place, specifying its location relative to different elements of the spatial context representation, and the descriptive information, specifying some properties that characterize the relevant place. Interestingly enough, it seems that animals with sophisticated spatial cognition, apart from humans, acquire the two types of information independently of each other, and do not combine them, but have to choose only one of them for every attempt to locate a particular place (and pick a pattern of a behavioral action); cf. Wang & Spelke (2002), Pearce *et al.* (2006). Apart from this aspect, which is discussed in section 5, there is a strong parallelism with language: Both cognitive maps and language use two types of information in locating a referent. One type locates the referent relative to the organization of the mental representation of the relevant context (the spatial context representation or the discourse representation), and the other involves features, i.e. non-geometric concepts, to specify a restriction that singles out the relevant referent from the set of suitable candidates. The size of the context representation, the possibility for it to involve domains currently inaccessible to the senses of the individual (including the abstract ones), and other mostly quantitative features of the particular system may lead to dramatic differences in their performance, but the ontological similarity remains a fact.

The parallel between cognitive maps and language related to the use of geometric and descriptive information goes even deeper. It appears that in familiar spatial environments (i.e. contexts), in a vast majority of (vertebrate) species, geometric cues are preferred to the descriptive ones (Gibson & Shettleworth 2003). This is explained by the fact that geometric cues are less likely to vary over time, compared to the visual, auditory, or other descriptive cues. For instance, a colorful flower may close during the day, or change its angle with the ground, but its location will stay the same. In new environments, however, descriptive cues are more important than the geometric ones. In fact, in a new spatial environment, geometric relations are still to be established by exploration — which is most naturally performed by using the more readily perceived descriptive cues.

In language, within an old discourse, the preferred way of locating its referents is through (repeated) personal and demonstrative pronouns, elements using geometric features of locality and proximity, rather than through repeated descriptions. Consider the example in (2), where pronominal elements are used in the reply in B, and descriptions in B'.

- (2) A: My friend Mary's math teacher wants her to be more active.  
 B: Did he tell her what exactly he meant by that?  
 B': # Did (your friend Mary's) math teacher tell (your) friend Mary what exactly (your friend Mary's) math teacher meant by telling your friend Mary that your friend Mary's math teacher wants your friend Mary to be more active?

It is a standard view in syntax and semantics that pronominal elements, including demonstratives, are directly related to the functional projection of the determiner, which specifies the features related to specificity and definiteness, both linked to the organization of the discourse (e.g., Kayne 2002). Moreover, demonstratives often involve the component of distance vs. proximity (*this* vs. *that*), which is in most cases related to the discourse organization and the abstract vicinity of the referent to the speaker or to the thematic background of the expression (Jayaseelan & Hariprasad 2001).<sup>6</sup> This shows that language also prefers the use of geometric features in familiar discourses, i.e. for discourse-familiar referents. New discourses, new referents, and new choices among old groups of referents are better handled by the use of descriptions. This is why the subject of (2A), *my friend Mary's math teacher*, is used, as an expression involving a description: The referent has probably not appeared in the immediately preceding discourse, and therefore has not yet been assigned 'discourse-geometric' properties in the relevant discourse domain.

Another characteristic property of natural language grammar is that it crucially relies on a set of categories, which can embed in one another under certain structural restrictions, referred to by a number of different terms: subcategorization, selection, projection, etc. So, for instance, the category verb (VP in syntax) can embed immediately under the category tense (TP) — such as in  $[_{TP} -ed [_{VP} walk]]$  — to give the interpretation of the location of the eventuality denoted by the verb in the past with respect to some reference time and/or with respect to the speech time (the tense operates over the meaning of the verb).<sup>7</sup> However, TP cannot embed immediately under VP ( $[_{VP} walk [_{TP} -ed]]$ ), to give, for instance, the interpretation of a past that has the property of walking, perhaps as opposed to running or flying (i.e. where the meaning of the verb operates over the tense). Even more importantly, TP cannot embed immediately under TP, nor can VP embed immediately under VP (two verbs can compose, but this is either a case of coordination or a more complex hierarchical structure than immediate embedding). All these embeddings become possible when mediated by other categories, and hence not immediate: The restrictions are local and target imme-

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<sup>6</sup> Imagine the following reply to (2A), in which the interlocutor switches to another world:

- (i) B'': In a fairytale, he'd be her evil stepmother.

The dynamics of the use of pronouns and the features *distal* and *proximal* indicates that they are sensitive to a geometry independent of any particular world that belongs to the discourse, unless some particular worlds are involved in specifying the property that singles out the referent from the relevant set of alternatives.

<sup>7</sup> The lexicalization of this structure as *walked* is an issue of morphology and phonology, it is not a source of controversies, and is not interesting for the current discussion.

diate embedding only.

Something very similar to this can be found in the domain of cognitive maps. As outlined in section 2, there are two major types of spatial objects: places and paths. These make two different categories. Moreover, it appears that paths are a more complex category, the definition of which may immediately involve places (paths must go from places, to places and/or via places, while places can be specified by descriptions that do not involve paths), such as, for example,  $[_{PATH} \text{ goal } [_{PLACE} \text{ home}]]$ . Places are defined based on the geometric properties of their position with respect to the territory and other places on it, especially landmarks, and based on the properties they have, such as color, smell, or shape (i.e. descriptive features). Paths are dynamically computed in every individual situation, because they directly depend on the current position of the subject, and the way in which it is changing. They are dynamic interpretations of geometric properties, actual in the respective situation. There are also other possible categories, such as landmarks (a subcategory of places) or geometrical structures, an issue I do not dwell on in this article because the two categories above suffice for the aimed argument: That categories, with the same type of embedding, and restrictions over the embedding are an important property of cognitive maps as well as of the language faculty.

The distinction between descriptive and geometrical features is another level of categorization. While animals seem to compute these two categories separately, each of them still embeds in the category of places: Places are determined by their descriptions and/or by their geometrical positions. Hence we get an even deeper hierarchical structure:  $[_{PATH} \text{ goal } [_{PLACE} [_{DESCRIPTION} \text{ home}]]]$ . Moreover, if the description involves an additional feature specified with respect to another place, a real recursive embedding takes place; for *the water near the home*,  $[_{PATH} \text{ goal } [_{PLACE} [_{DESCRIPTION} \text{ water, near } [_{PLACE} [_{DESCRIPTION} \text{ home}]]]]]$ , for example. Embedding of this kind is quite restricted: computation of cognitive maps probably can only handle structures with at most one round of embedding (one place described in term of one other place). Yet, this restriction may be imposed by the memory capacities, or by economy principles, rather than by the computational capacity, which is then genuinely recursive. If it is correct that the computation of cognitive maps involves structures with recursive embedding, this presents recursive computations as a much older development in the course of evolution than argued by Hauser, Chomsky & Fitch (2002).

It is possible to speculate in the direction of establishing parallels between places in cognitive maps and referents of nominal expressions in language on the one hand, and between paths in cognitive maps and eventualities in language. Both members of the former pair correspond to geometric points, and both members of the latter pair have linear structures. Moreover, both paths and eventualities include places and objects, respectively, as important defining elements in their structures. Without a neuro-cognitive, or at least a deeper cognitive support, this line of thinking remains in the domain of speculations.

In talking about the way language establishes reference, referents were discussed as located within discourses, but also within parts of discourses, discourse domains, which present the immediate thematic, temporal and spatial vicinity of the most prominent topical referents in a particular segment of the text. This im-

plies a hierarchical organization of the discourse, i.e. its division into domains, which are smaller and hence easier for retrieval and for locating referents in them. The very same has been argued to hold for cognitive maps: A territory is always divided into sub-territories, which may be further divided, in order to make the retrieval procedure faster and simpler (Schmajuk & Voicu 2006).

Finally, as presented in sections 2 and 3, in the computation of both cognitive maps and language, an important role is played by the hippocampus. In fact, some proposed descriptions of this role in the spatial domain are equally well applicable to its linguistic aspects. One of them defines the activity of the hippocampus as directly handling a coding of locations, events, behavioral strategies and their mutual relations into the context representation (Aggleton & Brown 1999). This could well serve as a description of the effects of discourse integration, if locations are taken to cover all referents, and the relevant context representation is taken to be the discourse. This touches on the important question of language-specific cognitive elements, supporting the view that most of the components of the language faculty are rather general and apply in other capacities as well. Such is the case with the procedures that integrate new material to some context representation, be it the spatial context, the discourse, or some other relevant representation, or with those which, influenced by motivational impulses, match segments of a context representation with behavioral strategies.

## 5. Differences

The major difference between natural language and spatial computation is that while in the latter only one individual integrates new information into the relevant mental representation, in the former the representation can be shared and updated by groups of individuals. In fact, the possibility that more than one individual shares the same discourse and participates in the process of its update is one of the properties of language that have influenced its current form the most, from the very existence of the phonology and phonetics (and hence the dual patterning of language), to a number of smaller differences at all levels at which spatial cognition and the language faculty can be compared, especially at those at which language involves an important role played by phonology.

Another interesting difference is briefly mentioned in section 4. While in language, descriptions and geometrical properties (i.e. those related to the organization of the discourse) appear as parts of one and the same unit of computation (a linguistic expression), in cognitive maps, these two types of information are separated and one unit of computation may consist of elements either only of the descriptive, or only of the geometrical nature. In fact, even in language, the two types of information are not really computed simultaneously. They appear strictly structurally segregated: The 'lower' structural domains are reserved for the descriptive content, while the 'higher' ones involve the discourse-related information (e.g., Rizzi 1997). This means that grammar performs serialized computations, where each unit of computation, a phase under the phase theory of syntactic computation (Chomsky 2001), consists of the

inner phase with the descriptive content, and the edge, whose contents are discourse-related (McNay 2006).

It is interesting that the acquisition of language in humans influences their spatial cognition. Some properties of the processing of cognitive maps, including the use of descriptive and geometric cues, undergo a drastic change around the age of six, which is also considered the critical period during which the individual rounds her acquisition of grammar (Hermer-Vazquez, Spelke & Katsnelson 1999).

A third difference, and the last one to be discussed in this section, concerns the domains of the two capacities. In the spatial computation, only spatial objects are categorized, all the other concepts falling in the category of 'the rest', i.e. of the material used only for descriptions. In natural language, referents are not restricted in any way; they do not even need counterparts in the real world: they can be abstract (as in: *The suspicion caused jealousy.*), non-existent (*A unicorn fell in love with Godzilla.*), or even impossible (*square circles on solid liquids*). This means that every concept can be used as a referent (jealousy, redness, distance) as well as (part of) a description. Referents are placed in an abstract space, the discourse, with its own organization that is only marginally influenced by the spatial relations in the real world. This makes the geometric properties of the discourse abstract and much more easily transformed than those of the (representation of) physical space. At the same time, it makes the process of updating and retrieving the context representation much more complex in language than in cognitive maps. One tool that language developed for this purpose is a richer set of categories. Instead of several categories that could be identified in cognitive maps, syntax disposes with several dozens of categories (at least according to the 'cartographic' approach to syntax, see, e.g., Cinque 1999).

The result, at the surface, where functional effects of the cognitive systems are observed, is a significant asymmetry. Spatial cognition, the domain in which animals show a high level of reference-based abilities, produces and deals with (retrieves, updates, combines) a set of SPATIAL contexts and sub-contexts stored in the long-term memory, amounting to the set of relevant territories in the life of an individual. Language, present only in humans, and based on their domain-general application of the recursive computational algorithms, produces and deals with a drastically larger set of discourses and discourse domains in the unified abstract macro-space of *all the available concepts and all their (possible) compositions*.<sup>8</sup>

A very important consequence of the domain-general application of recursive computation in humans is that they can not only embed (spatial) contexts in other (spatial) contexts, but also embed non-spatial contexts in non-spatial objects. In such a way, a powerful theory of mind can be derived: The description of each object potentially involves a context, or a set of contexts (her knowledge, views, beliefs). This may be the explanation for the universality and important role in grammar of the feature of animacy. Animacy marks objects that

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<sup>8</sup> The possibility that humans only differ from animals in their domain-general use of recursive computations is mentioned in Hauser, Chomsky & Fitch (2002), although it is not the claim they are directly arguing for.



can have their own context representations, i.e. 'points' in the discourse where another discourse can be embedded. And this altogether enables a multiple update, as the core of communication. While a unit of a spatial sensory input updates one spatial (sub-)context, a sentence in language may update a larger number of context representations, some of which can be embedded in the descriptions of referents of other contexts. Crucially, the context representation of one interlocutor contains representations of other interlocutors as objects, i.e. referents, and the description of these referents involve representations of their relevant context representations. Each of these embedded context representations is normally updated by each sentence uttered in the discourse, parallel to the update of the hierarchically highest context representation. On the surface, this looks like a *group update*: Each of the interlocutors represents a number of (sub-)contexts that have counterparts in all the other interlocutors, and all such sets of counterparts are updated in (nearly) the same way. Individuals of the group thus develop synchronized context-representations, which enable a synchronized functioning of the group. Apart from its cognitive and linguistic significance, this phenomenon plays an important role at the social level, which has probably been one of the ingredients in the selective pressure that pushed the evolution of language (Bickerton 1998).

## 6. Discussion

Similarities and differences between spatial cognition and language discussed in this article could be interpreted in three possible ways. One option is that the similarities observed are just a consequence of the methodological apparatus applied: Cognitive sciences deal with a set of general models, such as the division of different systems into computational and memory components. The fact that the same set of models can be fruitfully applied to different subjects of study does not guarantee that a deeper exploration would not uncover significant differences and require a modification of the models that would make them ontologically different from each other. This option presents a general danger for any theoretical work and hence will be ignored, leaving to the future research to prove it correct or wrong.

The second possibility is that the similarities are not more than that: (vague) similarities between two different systems. The weakest explanation would be that the similarities are accidental. A stronger one would be that they are a consequence of some general properties of cognition, i.e. of the neuronal systems in the brain, but that they still are disjoint systems. The strongest option under this interpretation is that the two systems share some components, for instance the computational module engaged in the retrieval and update, or the window to the long-term memory, and that this shared component is responsible for the shared properties between the two systems. This option agrees quite well with the neuro-cognitive data about the role of the hippocampus in both systems, as discussed in sections 2–4.

The third possibility is that the language faculty has evolved from the spatial cognition capacity. This is the strongest, and hence the theoretically most

interesting interpretation: It allows for the second possibility above as the description of the current relation between the two capacities, but it also hypothesizes on the origins of this relation. Therefore, but also because it is an attractive hypothesis, this interpretation receives a more extensive discussion in this section.

Different cognitive capacities have been suggested as the possible immediate origins of the complex computational patterns found in natural language, in the arithmetic capacity and in other sophisticated human cognitive capacities. Among them are the vocal production (Carstairs–McCarthy 1999), social cognition (Bickerton 1998), motorics (Jarvis 2007), and navigation (Bartlett & Kazakov 2005). In the remaining part of this section, I consider arguments in favor of spatial computation as a better candidate.

Virtually all animals, and even some plants, show some sort of sensitivity to aspects of space. Whenever this sensitivity is not a matter of a direct physical reaction, but requires the mediation of some biological process, it may be considered to involve computation. Hence, it is reasonable to think that spatial computation preceded any other kind of cognitive computation in animals. Moreover, it is a prominent possibility that other types of computation developed through the process of broadening, or shifting, the domain of application of the spatial computation, and of its gradual, or perhaps at times abrupt, sophistication. This is to say that all the types of computation that can be observed in animals today stem from the original purely spatial computation, which emerged very early in the animal evolution line.

Arguments in favor of this view are numerous. First, most other domains in which computation applies either can be seen as essentially spatial, or can be seen as metaphorically subjecting non-spatial data to spatial computation. Among the essentially spatial ones are the vision, the navigation and the motorics. Some others, like the cognition of time, planning, and language, involve such a high degree of spatial computations at each level, that they can easily be seen as originating from the spatial domain.

Apart from the similarities presented in section 4, there are many other spatial borrowings in the structures and computations involved in language. Even the metaphors used to talk about grammar are predominantly spatial. In phonology, an important role is played by the linearity of structures involved and by notions such as distance or adjacency, which are all essentially spatial. In syntax, again, there are syntactic trees, feature geometries, locality relations, movements, unifications, and so on. In semantics, operators have scope, variables get bound, predicates are bounded (e.g., with an upper bound), homogeneous, or scalar; even our intuitions about sets and quantification rely on spatial concepts. This not only illustrates the suitability of spatial relations in the theoretical modeling of grammar, but also suggests the possibility that the target of this modeling borrows a number of essentially spatial computational and structural patterns.

But more importantly, there are similar connections at the level of content. In lexical semantics, one can observe that for instance all prepositions, including the temporal ones, usually stem from words that had spatial meanings (see Lakoff & Johnson 1980 for the lexical semantic, but also for the general cognitive

status of spatial concepts). Other classes show similar, although usually less strong, relatedness to spatial meanings.

A second aspect in favor of the view that language has evolved from spatial cognition capacity comes from brain science. Jarvis (2007) reports about a series of experiments on different animal species with considerably complex computational capacity in the domain of vocal production. Their insights go in the direction of the generalization that brain centers engaged in vocal production in all the examined species are directly related to the brain centers engaged in motoric activities. The authors speculate that the former developed from the latter, by processes of specialization and adaptation to different tasks. Even the shapes and positions of the brain centers involved strongly suggest this conclusion: The brain center engaged in vocal production is either located within that engaged in motorics, or looks like its translated copy (i.e. it is located in the immediate vicinity and has approximately the same shape). It is very difficult to separate the centers engaged in motorics from those involved in spatial cognition. The entire motoric system has developed for functions directly related to space. Every activation of the motoric system has direct effects only in the spatio-temporal domain, and it is in space and time that they lead to the possible further effects, which achieve their actual function. Every possible function of an activity of the motoric system is a function from certain spatial relations. Jarvis reports about experiments designed to exclude the possibility that the activated centers are those engaged in navigation, and involved in the control of the motoric activities. However, navigation is only one specialized type of spatial computation and even a successful isolation of the navigation centers from the experiment does not mean the isolation of all aspects of spatial computation. In fact, conceptual considerations quite strongly suggest that no experiment can investigate the motoric cognition in full isolation from any aspects of spatial cognition, because the former does not exist without the latter. If Jarvis is right that at least some special types of computation, such as those of vocal production and learning, evolved from spatial cognition, by its extension into a particular non-spatial domain, then it is a prominent possibility that a change of the same type, but involving a larger number of domains, lead to the emergence of language. Consecutive development and adaptation of the newly emerged capacity lead to the language faculty as we have it today.

A third argument comes from an intriguing speculation by Krifka (2007), who argues that the subject–predicate, or topic–comment relation, which is central for the human language faculty, originates from the human property of handedness: the specialization of one hand for slow, heavy, rough tasks, and of the other for precise, quick, light tasks. In essence, this is an argument that a core property of language (but also of vision as a figure-ground distinction between the focused and the non-focused part) is argued to originate from an essentially spatially realized property of the cognition involved in motorics.

Pushing the hypothesis further, we may offer the following answers to some interesting questions of evolution of language. Language has evolved from spatial computation. The important changes that channeled this process are the following:

- (i) the extension of the spatial computation into non-spatial domains leading to a domain-general use of the computation;
- (ii) the serialization of the descriptive and geometrical domains, generalizing a sequence that specifies both the description and the geometric properties of a place, i.e. referent;
- (iii) the increasing functionality of a group update of the mental representations involved, mediated (or even pushed) by the development of phonological/ phonetic modules.<sup>9</sup>

Note that (i) and (ii) are well facilitated by the expansion of the number of categories. Principles of economy lead to the development of complex translation and (de-)compression procedures between segments from the discourse and phonological structures. In this view, syntax is to be divided into two systems: one, the 'conceptual syntax', determining the structure of (the concept specifying) the descriptive and geometric components of a discourse referent (close to the notion of conceptual semantic structures of Jackendoff 1999), and the other, the 'translation syntax', specifying the translation and (de-)compression rules between the structures generated by the 'conceptual syntax' and the corresponding phonological structures. Only one of the two, the 'conceptual syntax', is generative (engaged in producing and interpreting structures), while the other is only translational (interface computations). The former developed together with the development of spatial cognition capacity and its extension to other domains, while the latter is part of the development of language, and in particular of phonology.<sup>10</sup>

Out of the three important changes above, only the first one, the step of extending the spatial computation to a domain-general use, presents a qualitative change, which might have happened relatively abruptly, i.e. within a relatively short period of time, and a relatively small number of generations. Yet, it is equally possible that this change was gradual, originally involving an import of some pseudo-spatial concepts into the spatial domain, and then of the less spatial ones, until the full disappearance of domain boundaries for the application of spatial computation procedures. In any case, it may be a consequence of a fairly simple genetic change, or possibly just a cultural development: a series of breakthroughs of individuals incorporated into the culture and acquired by the entire community (due to its special organizational properties). The other two

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<sup>9</sup> Originally, a group update could have emerged when the situation in which a change in the immediate context was perceived by a group of individuals was utilized and became a part of the cultural load of a group, triggering some theory of mind effects in the individuals from the group. The next step is the emergence of behavioural strategies to trigger a group update in a controlled fashion, which became more and more systematic, and more and more phonological. The present view has nothing to say about whether this process was pushed by the group update or by some already existing system of vocal production and learning.

<sup>10</sup> 'Conceptual syntax' is involved in the generation of every non-atomic concept. This is not to say that every time we use a complex concept, we generate it from scratch. More frequent complex concepts are stored in the memory, and can be used without generation. Sometimes they are also associated with phonological material, whether single morphemes or complex constructions.

changes are more likely to have been gradual, possibly driven by probabilistic changes and rounds of reanalysis. The serialization of the descriptive and geometric components might have kicked off as a product of the planning capacity, aimed to guarantee efficiency in navigation, which was generalized during a period of time, eventually becoming part of the computational procedure. The group update of the spatial context representation, and later discourse, is another phenomenon which exists in a number of animal species (e.g., the coordinated hunting strategies of some dolphin species or the food-caching jays discussed in Gallistel, in press), but as domain-specific.<sup>11</sup> Its extent in the behavior of humans differs from that in animals in a number of properties, such as for instance involving a complex and sophisticated intentionality.

## 7. Conclusion

The contribution pointed out some striking parallels between cognitive maps and the language faculty, from their architectures, to the role of categories, to reference, but also some interesting differences between the two capacities. The article concentrates on the possible explanations for the presented facts, paying a special attention to the possibility that language has evolved from spatial cognition by the switch of the genuinely spatial computation involved — to a domain-general use. Although the present view of the evolution of language is highly speculative, it presents a hypothesis that deserves serious consideration.

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<sup>11</sup> One possibility is that some general ability to conduct domain-general computations opened space for the switch of a number of originally domain-specific computational procedures to a domain-general use, including, but not limited to the application of spatial computations and the group update of the spatial context and discourse representations.

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# Is Logic Innate?

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Arguments are presented supporting logical nativism: the conjecture that humans have an innate logic faculty. In making a case for logical nativism, this article concentrates on children's acquisition of the logical concept of disjunction. Despite the widespread belief to the contrary, the interpretation of disjunction in human languages is arguably the same as it is in classical logic, namely inclusive-*or*. The argument proceeds with empirical support for the view that the inclusive-*or* is the meaning of disjunction in human languages, from studies of child language development and from cross-linguistic research. Evidence is presented showing that young children adhere to universal semantic principles that characterize adult linguistic competence across languages. Several *a priori* arguments are also offered in favour of logical nativism. These arguments show that logic, like Socratic virtue and like certain aspects of language, is not learned and cannot be taught — thus supporting a strong form of innateness.

*Keywords:* disjunction, innateness, language acquisition, logic, semantic universals

## 1. Introduction

It is a contingent truth, in our view, that human language disjunction corresponds to inclusive-*or*, as in classical logic. In making our *prima facie* case for logical nativism, we will take advantage of this specific contingent fact about human languages, in the following ways. One way is to provide empirical evidence from studies of child language demonstrating that young children initially adopt the inclusive-*or* interpretation of disjunction despite the paucity of evidence for this interpretation in the primary linguistic data. Some of the relevant data demonstrating that children's interpretation is consistent with classical logic have been gathered in recent studies of two-year-old English-speaking children, and from studies of both English-speaking and Japanese-

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This research was supported in part by Australian Research Council Grant FF0456432 to Stephen Crain, and ARC Grant DP0879842 to Stephen Crain and Rosalind Thornton. We thank two anonymous reviewers for helpful input, and we thank the following friends and colleagues for assistance and insights: Ivano Caponigro, Gennaro Chierchia, Takuya Goro, Andrea Gualmini, Martin Hackl, Britta Jensen, Thomas Lee, Luisa Meroni, Utako Minai, Anna Notley, Tanya Reinhart, Francesco Ursini, Peng Zhou, and, especially, Rosalind Thornton.



speaking 4–5-year-old children. The finding is that children demonstrate knowledge of the semantic principles that characterize adult linguistic competence, across these and other languages. It turns out that Japanese-speaking children differ from adult speakers, by adopting the inclusive–*or* interpretation of disjunction even in simple negative sentences where, for adults, disjunction is governed by an implicature of exclusivity because of its scopal relationship with negation. Japanese-speaking children apparently ignore the input from adults, and maintain an inclusive–*or* interpretation in simple negative sentences. The studies from child language form one empirical argument for logical nativism.

Another empirical argument for logical nativism is based on cross-linguistic research. We show that in typologically different languages (Japanese, Chinese and English), the interpretation of disjunction is consistent with classical logic, again because disjunction is interpreted as inclusive–*or*. Three putatively universal linguistic principles are proposed, all utilizing inclusive–*or*. It is noted, however, that these principles are manifested in complex structures in which disjunction combines (i) with negation, (ii) with the universal quantifier (e.g., English *every*), and (iii) with focus expressions (e.g., English *only*). In view of the complexity of these phenomena, it is unlikely that young children have relevant evidence in their primary linguistic experience to inform them that expressions for disjunction in human languages conform to classical logic. This brings the empirical findings from studies of children’s interpretation of disjunction in line with logical nativism. We contrast logical nativism with a learning-theoretical account of children’s acquisition of the interpretation of disjunction. The learning account maintains that children’s acquisition of the interpretation of disjunction is based on witnessing speakers’ use of disjunction in conformity with certain inference rules (introduction and elimination rules). We argue that the learning account is highly implausible because the hypothesized input turns out to be an unlikely source of children’s interpretation of disjunction. To bolster our empirical conclusions, we end the article by presenting two *a priori* arguments for logical nativism. One is, surprisingly, based on work by Quine. The other is, not surprisingly, based on work by Fodor.

## 2. Circumventing Subset Problems

To avoid prejudice, let us admit the possibility that disjunction, e.g., English *or*, may have the meaning associated with exclusive–*or* in human languages. We will indicate this meaning using the symbol  $\oplus$ . If a statement of the form ‘*A or B*’ is true on this interpretation (meaning  $A \oplus B$ ), then exactly one, either *A* or *B*, is true. By contrast, we indicate the inclusive–*or* interpretation of disjunction using the standard wedge symbol  $\vee$ . In human languages in which disjunction means inclusive–*or*, a statement of the form ‘*A or B*’ (meaning  $A \vee B$ ), is true if either *A* or *B* is true, or if both *A* and *B* are true.

Let us consider the learnability of disjunction in human languages. Suppose there is a class of adult languages  $L_1$  with exclusive–*or* ( $\oplus$ -disjunction) as the unique interpretation of disjunction, and suppose there is another class of languages  $L_2$  in which disjunction is uniquely inclusive–*or* ( $\vee$ -disjunction). Due to

the truth conditions associated with  $\oplus$ -disjunction and  $\vee$ -disjunction, any disjunctive statement that is true in  $L_1$  will also be true in languages in  $L_2$  (with  $\vee$ -disjunction). The converse relation does not hold, however, because  $A \oplus B$  entails  $A \vee B$ , but not vice versa. In other words, statements with  $\oplus$ -disjunction are true in a subset of the circumstances corresponding to statements with  $\vee$ -disjunction — with respect to disjunction,  $L_1 \subseteq L_2$ .

Consider how learners decide whether the language they are exposed to is in  $L_1$  or in  $L_2$ . Suppose the learner guesses, without compelling evidence one way or the other, that the language spoken by members of the linguistic community is in  $L_1$  (with  $\oplus$ -disjunction), but in fact the local language is in  $L_2$  (with  $\vee$ -disjunction). Since  $L_1 \subseteq L_2$  there will be positive evidence for the learner to extend their language to include statements with  $\vee$ -disjunction. The circumstances that inform learners that their initial hypothesis about the meaning of disjunction ( $\oplus$ -disjunction) was incorrect will be circumstances in which someone utters 'A or B' when both A and B are true. Grammatical change could take two forms. Learners could add to the truth conditions for disjunction, converting  $\oplus$ -disjunction into  $\vee$ -disjunction, or learners could add a second meaning to disjunction to their grammars, making disjunction ambiguous, with both  $\oplus$ -disjunction and  $\vee$ -disjunction.

There is a second learnability scenario, according to which learners initially guess (wrongly) that the local language is in  $L_2$  (with  $\vee$ -disjunction) whereas, as a matter of fact, the local language uniquely uses  $\oplus$ -disjunction. Since  $A \oplus B$  entails  $A \vee B$ , learners who made the wrong guess will only encounter evidence confirming their initial (wrong) interpretation, at least in the absence of negative semantic evidence. This is the familiar learnability dilemma that arises whenever an expression has two possible values, one yielding an interpretation that makes a sentence true in a superset of circumstances that correspond to the other interpretation. If the learner initially guesses the superset language, the evidence they encounter will always be consistent with this guess if the local language is actually the subset language. This is appropriately labeled the *Subset Problem*.

There are two potential ways to avoid the Subset Problem. One is to ensure that learners start out with the more restricted meaning, the subset interpretation. In the case of disjunction, the more restrictive meaning is  $\oplus$ -disjunction. As we saw, if it turns out that the local language (also) uses  $\vee$ -disjunction, then there will be positive evidence informing learners that their grammars need to accommodate  $\vee$ -disjunction. The other solution is to deny the existence of a Subset Problem. Essentially, this amounts to claiming that learners initially guess that the local language uses  $\vee$ -disjunction, and they are always correct because, as a contingent fact, all human languages use  $\vee$ -disjunction, and no languages use  $\oplus$ -disjunction. Of course, it is conceivable that some languages have two meanings of disjunction, i.e. both  $\vee$ -disjunction and  $\oplus$ -disjunction. However, if learners initially hypothesize  $\vee$ -disjunction as their initial interpretation of disjunction, then statements that correspond to the truth conditions associated with  $\oplus$ -disjunction will be covered whether or not the language also has  $\oplus$ -disjunction. In fact, if this learnability scenario is correct, then it is unclear why any language would need to express both kinds of disjunction, since learners' initial guess,  $\vee$ -disjunction, already handles the subset of circumstances

associated with  $\oplus$ -disjunction.

Despite these observations, the hypothesis that OR is uniquely  $\vee$ -disjunction in human languages is not widely accepted. Many linguists and philosophers think that at best, disjunctive words like English *or* are ambiguous between  $\oplus$ -disjunction and  $\vee$ -disjunction and, at worst, that disjunctive words in human languages uniquely mean  $\oplus$ -disjunction and not  $\vee$ -disjunction. Our own position is, following Grice (1975), that disjunction in human language is (exclusively)  $\vee$ -disjunction — inclusive-*or* (cf. Gazdar 1979, McCawley 1981, Pelletier 1972). In the next section we consider simple counter-evidence to this position. The counter-evidence takes two forms: (i) objections based on mutual exclusivity, and (ii) situational contexts where OR appears to violate de Morgan's laws, which are based on  $\vee$ -disjunction.

### 3. How Many ORs Are There?

There are many human language constructions that require inclusive-*or*, i.e.  $\vee$ -disjunction. In English, simple negative statements with disjunction (in the scope of negation) require this interpretation. So, *Max didn't order sushi or pasta* means that Max didn't order sushi and Max didn't order pasta. We will refer to this as the conjunctive interpretation of disjunction in the scope of negation. In classical logic, this interpretation follows from one of de Morgan's laws:  $\neg(A \vee B) \Rightarrow (\neg A \wedge \neg B)$ . The critical point is that this law assumes that disjunction is inclusive-*or*. To the extent that human languages yield conjunctive interpretations in negated disjunctions, then disjunction is inclusive-*or* in human languages. If the sentence *Max didn't order sushi or pasta* meant that Max didn't order sushi  $\oplus$  pasta, then the statement would be true if Max ordered both sushi and pasta, clearly the wrong result for simple negative sentences with disjunction in English (cf. Barrett & Stenner 1971).

But what about the corresponding positive sentence *Max ordered sushi or pasta*? For most English speakers, this means that Max either ordered sushi or he ordered pasta, but not both. This is not evidence that *or* is  $\oplus$ -disjunction, however. Following Grice (1975), we can account for the appearance that human languages express disjunction using exclusive-*or* as well as inclusive-*or* by invoking pragmatic norms of conversation, which sometimes eliminate one of the truth conditions of inclusive-*or*, namely the condition in which both disjuncts are true. In a nutshell, the Gricean account maintains that sentences of the form 'A or B' are subject to an implicature of exclusivity, i.e. 'A or B, but not both A and B'. The implicature of exclusivity arises due to the availability of another statement, 'A and B', which is more informative. 'A and B' is more informative because it is true in only one set of circumstances, whereas 'A or B' is true in those circumstances, but it is true in other circumstances as well. Due to the overlap of truth conditions, the expressions *or* and *and* form a scale based on information strength, with *and* being more informative than *or* (e.g., Horn 1969, 1996). A pragmatic principle *Be Cooperative* entreats speakers to be as informative as possible. Upon hearing someone use the less informative term on the scale, *or*, listeners assume that the speaker was being cooperative and they infer that the

speaker was not in position to use the more informative term *and*. Therefore, the speaker's use of the less informative term is taken by listeners to imply the negation of the more informative term: 'not both A and B'.

Several challenges to this account of the 'not both' interpretation of disjunction have been offered, and we will briefly rehearse them now, indicating how Grice's account withstands the challenges. First, it has been observed that there are many circumstances in which the exclusive-*or* reading of disjunction is the only available reading, not just the preferred reading. Such cases are quite common in the input to children. Adults ask children many questions that make it clear that the disjuncts are mutually exclusive. Here are some examples from the input to Adam in the CHILDES database (MacWhinney 2000): *Was it a big one or a small one? — Did you find it or did Robin find it? — Is it a happy face or a sad face?* Assuming that English has inclusive-*or*, it might be suggested that such questions demand a second meaning for OR, expressing mutual exclusivity (e.g., Kegley & Kegley 1978, Richards 1978).

The force of this argument is weak. According to the truth conditions associated with inclusive-*or*, statements of the form 'A or B' are true in circumstances in which only A, or only B, is true. Contexts in which the disjuncts are mutually exclusive are therefore consistent with the inclusive-*or* reading of disjunction. Of course, such contexts are not consistent with all the truth conditions associated with inclusive-*or*, since faces cannot be both happy and sad at the same time. But, someone who poses the question *Is it a happy face or a sad face?* assumes that it was either happy or sad, and both of these truth conditions are consistent with inclusive-*or*. As we saw, the inclusive-*or* interpretation of disjunction is true in a superset of the conditions that are associated with exclusive-*or*, so any truth conditions that would be associated with an exclusive-*or* meaning (were this available to children) would be consistent with the inclusive-*or* interpretation of disjunction. So, if the basic meaning of disjunction is inclusive-*or*, there would be no need to coin a second term, or assign an independent meaning to OR, to be used in circumstances corresponding to exclusive-*or*.

A similar observation concerns the interpretation of disjunction in the presence of other logical operators, such as negation. An example is *Max did not order noodles — or (was it) rice?* The idea is that the introduction of a pause, or by altering the prosody, one can indicate an exclusive-*or* reading, in direct violation of de Morgan's laws. Since de Morgan's laws depend on the inclusive-*or* reading of disjunction, such violations appear to call for a second meaning, i.e. one corresponding to exclusive-*or*. In our view, the issue here is one of scope, not ambiguity. The introduction of a pause, or a change in intonation, is taken by hearers as indicating that disjunction has scope over negation, and not the reverse. It is as though one had said: *It was noodles — or (was it) rice that Max didn't order.* De Morgan's laws are not operative when the scopal relation between negation and disjunction are reversed in this way, with disjunction having scope over negation.<sup>1</sup> In section 6, we offer an account of the 'inverse

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<sup>1</sup> De Morgan's laws are not the only laws that fail for exclusive disjunction. The Distributive Law 'A or B and C is equivalent to A or B and A or C' is another notable failure. Thus,

scope' reading of disjunction that crops up in simple negative sentences in some human languages.

#### 4. 'Weakening' as Evidence for Exclusive-or

There is a more serious potential challenge to the claim that the unique meaning of disjunction in human languages is inclusive-or. The challenge is predicated on the observation that the introduction rule for disjunction (known appropriately as 'Weakening') is typically judged to be unacceptable by adults. The introduction rule permits one to validly infer a statement of the form 'A or B' from a statement of the form 'A'. So, if one has evidence for  $A$ , one can logically infer  $A$  or  $B$ , regardless of the truth value assigned to  $B$ . This rule of inference is only valid if the disjunction operator in the statement 'A or B' is inclusive-or, since  $A \vee B$  is a logical consequence of  $A$ , both when  $B$  is true and when  $B$  is false.

$$(1) \quad \frac{A}{A \vee B}$$

Similarly,  $A \vee B$  is a logical consequence of  $B$ , regardless of the truth value of  $A$ .

$$(2) \quad \frac{B}{A \vee B}$$

If the meaning of disjunction is  $\oplus$ -disjunction, by contrast, the introduction rule of Weakening is not valid. On this interpretation of disjunction, exactly one disjunct can be true, so  $A \oplus B$  cannot be inferred from evidence that  $A$  is true when  $B$  is also true. This contrasts with the formula using inclusive disjunction,  $A \vee B$ , which is true if both  $A$  and  $B$  are true. The upshot is, one way to explain why Weakening is not accepted by language users is to suppose that the meaning of disjunction in human languages is exclusive-or and not inclusive-or.

There is, however, another way to account for the observation that people do not find the introduction rule for disjunctive statements acceptable. This account appeals to the pragmatic norms people follow in discourse, as sketched above. It is simply odd, pragmatically, for language users to produce two statements, the first more informative than the second. This is exactly what happens with the simple introduction rule for disjunction. First, one encounters  $A$ , then  $A$  or  $B$ . But, someone who produces  $A$  or  $B$  implies that s/he was not in position to produce either  $A$ , or  $B$ . It is therefore, pragmatically infelicitous to find  $A$  followed by  $A$  or  $B$ .

To adjudicate between these accounts of the unacceptability of Weakening, we propose to recast the Weakening inference rule in a way that makes it acceptable to ordinary speakers, by reducing the pragmatic infelicity associated

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whilst 'Either Annie or Bob and Chris will come to the party',  $A$  or  $(B$  and  $C)$ , is true if *or* means  $\oplus$  when Annie and Bob turn up without Chris, the conjunction 'Annie or Bob and Annie or Chris will come to the party',  $(A$  or  $B)$  and  $(A$  or  $C)$  comes out false, since the first conjunct turns out false. Had Bob stayed away it would have been true.

with the inference rule. Adopting a similar perspective, McCawley (1981: 33) argues that Weakening is accepted if it is introduced in a sub-proof of a logical derivation, rather than in the main proof. We adopt a different strategy. It is possible to reduce or eliminate the pragmatic infelicity of Weakening simply by inserting a logical step between the statement *that A*, and the statement *that A or B*. The step is existential generalization. Existential generalization logically follows from certain statements *that A*, and it logically validates corresponding disjunctive statements *that A or B*. Crucially, by making the introduction rule for disjunction indirect, it becomes more palatable for English speakers. Here is a version of Weakening that people we have consulted find acceptable.

Consider a domain containing two people, Max and Jon. Suppose that Jon laughs, so  $L_j$  (*Jon laughs*) is true. But if  $L_j$  is true, then it follows that ‘someone laughs’ is true, so  $\exists x L_x$  is true. Yet, there are only two objects in the domain, Max and Jon, so the existential claim that ‘someone laughs’ is logically equivalent to the claim that ‘Jon laughs or Max laughs’. That is, from  $\exists x L_x$ , we can infer the truth of  $L_m \vee L_j$ . In short, we began with the statement  $L_j$ , and derived the disjunctive statement  $L_j$  or  $L_m$ . QED: Weakening holds for OR. Therefore OR is  $\vee$ -disjunction.

If disjunction is  $\oplus$ , it is not logically possible to begin with  $L_j$  and to derive  $L_j \oplus L_m$  by following a sequence of steps that are each logically valid. To see this, suppose that Max laughs along with Jon. That is,  $L_m \& L_j$  holds. Clearly then,  $L_j$  holds. As before this validates the existential claim  $\exists x L_x$ . But if  $L_m$  and  $L_j$  are both true,  $L_m \oplus L_j$  is false. QED: Weakening does not hold for  $\oplus$ -disjunction.

The indirect argument from  $L_j (= A)$  to  $L_j \vee L_m (= A \text{ or } B)$  shows that the introduction rule for disjunction is sound after all. And this, in turn, means that disjunction is inclusive-*or*, at least for English speakers. So the fact that Weakening is judged unacceptable by most speakers in its simplest form (i.e. moving directly from  $A$  to  $A \text{ or } B$ ) does not support the conclusion that human language disjunction is exclusive-*or*. Rather, as Grice (1975) proposed, Weakening is unacceptable simply because the conclusion is less informative than the premise. It is therefore jarring to encounter the premise immediately followed by the conclusion. However, by making the route from the premise to the conclusion indirect (via Existential Generalization), the validity of the Weakening introduction rule becomes apparent. In fact, this version of Weakening is an *a priori* argument that disjunction is inclusive-*or*, at least in English. What about in other human languages?<sup>2</sup>

<sup>2</sup> Jennings (2001) notes that if  $\oplus$  is to serve as an acceptable interpretation of *or* in English, then it cannot be a binary sentential connective since ‘Annie or Bob or Chris will come to the party’ is a perfectly acceptable, unambiguous sentence of English. Yet, bizarrely, if *or* means  $\oplus$ , then this statement will be true if all three turn up! If  $A, B, C$  are true, then  $A \oplus (B \oplus C)$  turns out true since  $(B \oplus C)$  will be false. In fact, as Reichenbach (1947) first observed, for  $\oplus$  an  $n$ -ary connective,  $\oplus (\alpha_1, \dots, \alpha_n)$  will come out true if and only if an *odd* number of the

## 5. Weakening Reconsidered

Despite the logical proof we have given that English *or* is  $\vee$ -disjunction, it is widely believed that disjunction in human languages is exclusive-*or*, i.e.  $\oplus$ -disjunction (e.g., Lakoff 1971, Braine & Romain 1983). For example, although Braine & Romain (1983: 291) acknowledge the view that “equates *or* with standard logic”, they ultimately reject this view on the grounds that “coherent judgments of the truth of *or*-statements emerge relatively late and are not universal in adults”. They conclude that disjunction is more often than not, exclusive-*or* even for adults. In the last section, we presented an *a priori* reason for thinking that OR must have an inclusive reading in English. Moreover, we believe that the inclusive-*or* reading of disjunction is no quirk of English. Rather, we believe, the introduction rule for disjunction, Weakening, is valid in all human languages. In section 5 we present further arguments from cross-linguistic research for believing that all human languages allow an inclusive-*or* reading of disjunction. First, we reflect further on the *a priori* argument we offered for the claim that inclusive-*or* is the meaning of disjunction, based on the validity of one form of Weakening in English.

We began by considering the possibility that Weakening is invalid because, as many researchers have claimed, OR means exclusive-*or* ( $\oplus$ ) in their idiolects, and Weakening is invalid for  $\oplus$ . To counter this, we offered a validation of Weakening which disproves this hypothesis. Here is a variant of our earlier argument. If it is valid, it proves that *or* is inclusive-*or*, not exclusive-*or*, in English.

- (3) (I) Jon laughs and Max laughs.  
 (II)  $\therefore$  Jon laughs.  
 (III)  $\therefore$  Someone laughs.  
 (IV)  $\therefore$  Jon laughs or Max laughs.

Clearly, if it is valid to infer that *Jon laughs or Max laughs* from *Jon laughs and Max laughs*, then *or* is not  $\oplus$ -disjunction, since  $A \oplus B$  is false if both  $A$  and  $B$  are true. By contrast,  $A \vee B$  is true if both  $A$  and  $B$  are true, so if the argument is valid, then English *or* is  $\vee$ -disjunction. Anyone who thinks that the inference is not valid, in any language, however, owes us an explanation as to which step in the inference is unsound.

Let us consider the steps in turn. Consider first the inference from (I) to (II). This is the elimination rule for conjunction, Simplification. This inference is uncontroversial. To deny that Weakening holds in any human language, then, one must either say that (III) does not follow from (II), or that (IV) does not follow from (III). Presumably, to deny that *Someone laughs* follows from *Jon laughs*, one must deny that *someone* can mean ‘at least one person.’ Putting it another way, denying that (III) follows from (II) amounts to the claim that *someone* must mean

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sentences  $\alpha_i$  are true. As Jennings quips, there is no natural use of disjunction in human languages which counts A or B or C or D or E true just in case either one or three or all five of the disjuncts are true.



'exactly one person.' That claim cannot be right, however. The reason is that (II) *Jon laughs* is derived from the hypothesis that (I) *Jon laughs and Max laughs*. Since (III) *Someone laughs* is supposed to follow from (II) *Jon laughs* by existential generalization, that sentence also must rest upon the same hypothesis, (I) *Jon laughs and Max laughs*. But we patently cannot conclude from the hypothesis that both Jon and Max laugh that exactly one of them laughs. So *someone* cannot mean 'exactly one' and must, as required, mean 'at least one'. So we think the transition from (II) to (III) is incontestable in any language.

This leaves the final step, from (III) to (IV), as the remaining inference to challenge. Supporters of the exclusive-*or* interpretation of *or* are already committed to denying the inference of (IV) from (III). But it is hard to see how this inference could be denied. For it is just bluntly true that, in the circumstance where Jon and Max are the only members of the domain, *Someone laughs* is logically equivalent to *Jon laughs or Max laughs*. This is the human language counterpart to the relationship between quantificational operators and logical connectives in classical logic: The existential quantifier is disjunctive, and the universal quantifier is conjunctive. In classical logic, in a domain with two objects,  $a$  and  $b$ ,  $\exists xPx$  expands to  $Pa \vee Pb$ ; and  $\forall xPx$  expands to  $Pa \& Pb$ . The same relationship holds in human languages. So in a domain with two people, Jon and Max, the sentence *Someone laughs* can likewise be expanded to the sentence *Jon laughs or Max laughs*. Anyone informed that the former sentence is true can infer that the latter is also true.

None of this is surprising to anyone who thinks, as we do, that first order logic is the innately given logic of human languages. For the reason that *Someone laughs* can be expanded to *Jon laughs or Max laughs* is because the underlying logical form of the first sentence just is  $\exists xLx$  and the underlying logical form of the second sentence just is  $Lj \vee Lm$ , so that if there are only two objects  $j$  and  $m$  in our universe of discourse, the existential formula can be replaced at the level of logical form by its disjunctive expansion  $Lj \vee Lm$ . For logical nativists, the logical entailments that hold between the sentences of a human language just are the formal ones holding between the logical forms corresponding to those sentences, so there is no problem of trying to find a human language analogue for logical concepts and relations.

Interestingly, human languages can even wear the relation between quantificational operators and logical connectives on their sleeves. Japanese is one such language. In Japanese, the disjunction operator is *ka* and the conjunction operator is *-mo*. These logical operators appear in quantificational expressions in Japanese, such that 'someone' is formed using the expression for disjunction, and 'everyone' is formed using the expression for conjunction. That is, the equivalent of English 'someone' in Japanese is *dare-ka* and the equivalent of English 'everyone' in Japanese is *dare-mo*.

We have seen that, by itself, the inference of *Jon laughs or Max laughs* ( $A$  or  $B$ ) from *Jon laughs* ( $A$ ) gives us pause, but it seems compelling when viewed through the intermediary of existential generalization. It is very hard to see how this could just be a quirk of English, however, since the reasoning that justifies Weakening makes no use of semantic properties unique to English words. Rather any language that contains an existential quantifier and a disjunction operator

will vindicate it.<sup>3</sup> To disprove the hypothesis that inclusive disjunction must be an admissible interpretation of disjunction in any human language, it would have to be shown that there is a language  $L_n$  for which either:

- (i) Existential generalization fails (the inference from II to III), or else
- (ii) Existential quantification over a finite domain of named objects produces an existential claim that is not logically equivalent to a finitary disjunction (the inference from III to IV).

Any such human language  $L_n$  would be logically unsound. The upshot is that human language disjunction must have an inclusive–*or* interpretation on pain of logical incoherence.

## 6. Linguistic Universals with inclusive–*or*

So far, we have produced an *a priori* argument supporting the hypothesis that all human languages allow an inclusive interpretation of disjunction. If sound, this argument establishes that OR, meaning  $\vee$ -disjunction, is a universal feature of human languages. There is considerable empirical evidence that confirms this hypothesis. One source of this evidence is from cross-linguistic research.

For a start, it is universally the case that negated disjunctions adhere to de Morgan's law for negated disjunctions:  $\neg(A \vee B) \Rightarrow (\neg A \wedge \neg B)$ . In human languages, this law applies universally only when negation is in a 'higher' clause than disjunction. An example is given in (4) where the clause that contains disjunction, *...John speaks French or Spanish*, is embedded in the clause with negation, *Mary didn't say...*. Semantically, the critical observation is that (4) generates a conjunctive entailment, as indicated in (4a); it does not have the 'disjunctive' truth conditions indicated in (4b).

- (4) Mary didn't say John speaks French or Spanish.
  - a. Mary didn't say John speaks French and she didn't say he speaks Spanish.
  - b. \*Mary didn't say John speaks French or she didn't say he speaks Spanish.

Remarkably, when (4) is translated into Japanese, Chinese, Russian, and so forth, its variants in these other languages also carry conjunctive entailments. Here are examples from Chinese (5) and Japanese (6). In both languages, these negated disjunctive statements generate a conjunctive entailment.

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<sup>3</sup> We are not claiming that every human language must contain a word corresponding to the existential quantifier and a word corresponding to disjunction. The concepts of existential quantification and disjunction could be made available to language users indirectly, e.g., using negation and universal quantification, as in *Not everybody laughed*.

(5) *Chinese*

Mali meiyou shuo-guo Yuehan hui shuo fayu huozhe xibanyayu.

Mary not say-PAST John can speak French or Spanish

瑪麗沒有說過約翰會說法語或者西班牙語

'Mary didn't say that John speaks French or Spanish.'

(6) *Japanese*

Mary-wa John-ga French ka Spanish-wo hanas-u-to iwa-nakat-ta.

Mary-TOP John-NOM French or Spanish-ACC speak-PRES-COMP say-not-PAST

'Mary didn't say that John speaks French or Spanish.'

As these examples illustrate, when negation appears in a higher clause than the clause that contains disjunction, i.e. not  ${}_s[A \text{ or } B]$ , such statements exclude the possibility of both *A* and *B*, in typologically different languages (cf. Szabolcsi 2002, Goro 2004). Notice that in the Japanese example (6), the statement takes a different form,  $[A \text{ or } B]_s \text{ not}$ , as compared to English and Chinese,  $\text{not } {}_s[A \text{ or } B]$ . This is because Japanese is verb-final and negation is attached to the verb. Nevertheless, the Japanese example has the same truth conditions as the examples from English and Chinese. It makes no difference that the disjunction operator *ka* precedes negation in Japanese, whereas *or* and *huozhe* follow negation in the English and Chinese examples. This shows that the interpretation of disjunction does not depend on linear order; what matters is constituent structure. In any event, we have derived one candidate for a linguistic universal (influenced by the work of Anna Szabolcsi and Takuya Goro): *When disjunction appears in a lower clause than negation, negated disjunctions license a conjunctive entailment.*

It is implausible to suppose that children learn that disjunction is inclusive-*or* in human language based on their exposure to sentences like those in (4), (5), and (6). Such sentences are too rare to ensure that every language learner is exposed to a sufficient quantity of them to guarantee convergence on the target grammar.<sup>4</sup> The conjunctive interpretation of disjunction is licensed only if disjunction words are interpreted as inclusive-*or*, as in de Morgan's laws of classical logic. De Morgan's laws apply, of course, if and only if the negation operator is acting upon disjunction. To illustrate, consider the following two sentences, and their associated logical forms, where 'Dx' stands for *x is a delegate*, 'Sx' for *x ate sushi*, 'Px' for *x ate pasta*, and 'Ix' for *x became ill*.

## (7) Not every delegate who ate sushi or pasta became ill.

$$\neg \forall x[(Dx \ \& \ (Sx \vee Px)) \rightarrow Ix]$$

## (8) Not every delegate who became ill ate sushi or pasta.

$$\neg \forall x[(Dx \ \& \ Ix) \rightarrow (Sx \vee Px)]$$

<sup>4</sup> As we discuss in section 5, a similar cross-linguistic generalization does not extend to simple negative sentences with disjunction, such as *Ted didn't eat sushi or pasta*. In simple negative statements, some languages license conjunctive interpretations (e.g., English, German), but other languages do not (e.g., Japanese, Chinese).

The role the disjunction ‘ $Sx \vee Px$ ’ plays in both of these formulae might look the same prior to analysis, but there is a significant difference. In the formula in (7) disjunction appears in the antecedent clause of a negated conditional, whereas in (8) it appears in the consequent clause of a negated conditional. Thus, when we come to reduce each formula further, as in (7’) and (8’) respectively, we see that disjunction is no longer in the scope of negation in (7’), but it is in the scope of negation in (8’).

(7’) Not every delegate who ate sushi or pasta became ill.

$$\begin{aligned} & \neg \forall x[(Dx \ \& \ (Sx \vee Px)) \rightarrow Ix] \\ & \Leftrightarrow \exists x \neg [(Dx \ \& \ (Sx \vee Px)) \rightarrow Ix] \\ & \Leftrightarrow \exists x[(Dx \ \& \ (Sx \vee Px)) \ \& \ \neg Ix] \end{aligned}$$

(8’) Not every delegate who became ill ate sushi or pasta.

$$\begin{aligned} & \neg \forall x[(Dx \ \& \ Ix) \rightarrow (Sx \vee Px)] \\ & \Leftrightarrow \exists x \neg [(Dx \ \& \ Ix) \rightarrow (Sx \vee Px)] \\ & \Leftrightarrow \exists x [(Dx \ \& \ Ix) \ \& \ \neg (Sx \vee Px)] \\ & \Leftrightarrow \exists x [(Dx \ \& \ Ix) \ \& \ \neg Sx \ \& \ \neg Px] \end{aligned}$$

Thus negation acts directly on a disjunctive clause only in (8). The reason is that in negating conditionals we affirm the antecedent and deny the consequent, since this represents the sole condition under which conditionals are false. So if the disjunctive clause appears in the antecedent of a conditional, as in (7), it does not get negated, whereas if it appears in the consequent, as in (8), it does get negated. In human languages, then, disjunction is negated if it appears in the predicate phrase of a negated universally quantified statement, but disjunction is not negated if it appears in the subject phrase of a negated universally quantified statement. Consequently, disjunction licenses a conjunctive entailment in the predicate phrase of a negated universally quantified statement, as in (8), but not when disjunction appears in the subject phrase of such a sentence, as (7) shows. So to say *Not every delegate who ate sushi or pasta became ill* is to say that at least one of the delegates who ate sushi or pasta remained unaffected, and to say *Not every delegate who became ill ate sushi or pasta* is to say that some delegate who became ill didn’t eat sushi and didn’t eat pasta (so these foods are ruled out as the source of the illness). De Morgan’s laws are thus preserved at the level of logic and also at the level of semantic interpretation in human languages.

We just noted that disjunction licenses a conjunctive implication in the predicate phrase in negated universally quantified statements. This is in striking contrast to sentences with the universal quantifier in pre-subject position, but without negation. In such cases, disjunction licenses a conjunctive implication in the subject phrase, but not in the predicate phrase. As (9) shows for English, when disjunction is in subject phrase of a sentence with the downward entailing expression *every*, the sentence yields the entailments (9a) and (9b). Therefore, the English statement in (9) generates the conjunctive interpretation indicated in (10), which is simply the conjunction of the two entailments (9a) and (9b).

- (9) Every student who speaks French or Spanish passed the exam.  
 a. every student who speaks French passed the exam  
 b. every student who speaks Spanish passed the exam
- (10) Every student who speaks French passed the exam and every student who speaks Spanish passed the exam.

It is worth noting, again, that the same linguistic phenomena are manifested across human languages. When (9) is translated into Japanese or Chinese (and any other language, as far as we know), the corresponding statements also generate conjunctive interpretations. This is illustrated in (11) and (12). Example (11) shows that the Chinese disjunction operator *huozhe* licenses a conjunctive interpretation when it appears in the subject phrase of the universal quantifier *meige*. Example (12) provides the corresponding sentence in Japanese.

(11) *Chinese*

Meige hui shuo fayu huozhe xibanyayu de xuesheng dou  
*every can speak French or Spanish DE student DOU*  
 tongguo-le kaoshi.  
*pass-PERF exam*  
 'Every student who speaks French or Spanish passed the exam.'

(12) *Japanese*

Furansugo ka supeingo-wo hanas-u dono gakusei-mo goukakushi-ta.  
*French or Spanish-ACC speak-PRES every student pass exam-PAST*  
 'Every student who speaks French or Spanish passed the exam.'

In view of this cross-linguistic generalization, a second universal principle is postulated (influenced by the work of Gennaro Chierchia): *Disjunction licenses a conjunctive interpretation when it appears in the subject phrase of the universal quantifier.*

In the next section, we report the findings of a study showing that children know this universal principle. But more importantly, children also know where disjunction does not license a conjunctive interpretation in human languages. Interestingly, when disjunction is in the predicate phrase of a sentence with the universal quantifier, it no longer generates a conjunctive interpretation. This is illustrated in (13), which has been formed from (9) simply by swapping the contents of the subject phrase and the predicate phrase.

- (13) Every student who passed the exam speaks French or Spanish.  
 a. # every student who passed the exam speaks French  
 b. # every student who passed the exam speaks Spanish

In (13), the predicate phrase (*speaks French or Spanish*) contains disjunction, but a conjunctive interpretation is not licensed, because neither of the relevant entailments, (13a) or (13b), are valid inferences from (13). This asymmetry

between the subject and predicate phrase of the universal quantifier extends to human languages around the globe and, again, experimental investigations have revealed that children are aware, at an early age, that disjunction generates a conjunctive interpretation in the subject phrase of the universal quantifier, and children are also aware that disjunction does not generate a conjunctive interpretation in the predicate phrase of sentences with the universal quantifier.

The question naturally arises: how do children figure out that human languages interpret OR in one way in the subject phrase of the universal quantifier, and a different way in the predicate phrase? As Chierchia (2004: 94) remarks “All the action concerns meaning. Morphology or distributional patterns play no role”. Chierchia concludes that the “generalization under discussion yields a particularly strong version of the poverty of stimulus argument. It is thus interesting to find out when exactly the child starts acting in an adult like manner [...]” (*ibid.*). Since poverty of stimulus arguments are the bread and butter of both linguistic nativism and logical nativism, it is important to find out if knowledge of the asymmetry in the interpretation of disjunction in sentences with the universal quantifier emerges early in language development, albeit without decisive evidence from experience. We return to this in the next section.

First, we offer further confirmation that disjunction is inclusive–*or* in human languages. This confirmation comes from studies of how speakers interpret disjunction in sentences with certain focus operators, e.g., English *only*, Japanese *dake*; Chinese *zhiyou*. The semantic contribution of such focus operators is quite complex. Consider the statement in (14).

(14) Only Bunny Rabbit ate a carrot or a green pepper.

This statement expresses two propositions. Following common parlance, one proposition is called the *presupposition* and the other is called the *assertion*. Simply deleting the focus expression from the original sentence yields the presupposition: *Bunny Rabbit ate a carrot or a green pepper*. For many speakers, there is an implicature of exclusivity (‘not both’) in the presupposition (see section 3). The second proposition is the assertion. To derive the assertion, the sentence can be further partitioned into (i) a *focus element* and (ii) a *contrast set*. Focus expressions such as *only* are typically associated with a particular linguistic expression somewhere in the sentence. This is the focus element. In (14), the focus element is *Bunny Rabbit*. Typically, the focus element receives phonological stress.

The assertion is about the contrast set. The members of the contrast set are individuals in the domain of discourse that are taken by the speaker and hearer to be alternatives to the focus element. These individuals should have been introduced into the conversational context before the sentence was produced; their existence is presupposed. In the present example, the contrast set consists of individuals being contrasted with *Bunny Rabbit*. The sentence would not be felicitous in the absence of such alternatives to Bunny Rabbit. The assertion states that the members of the contrast set *lack* the property being attributed to the focus element. In *Only Bunny Rabbit ate a carrot or a green pepper*, the assertion is the following claim: *Everybody else (being contrasted with Bunny Rabbit) did not eat a*

*carrot or a green pepper*. The critical observation is that disjunction is in the scope of (local) negation in the assertion: ... *did not eat a carrot or a green pepper*. Because disjunction appears in the scope of negation, it licenses a conjunctive interpretation: *Everybody else didn't eat a carrot and everybody else didn't eat a green pepper*. As far as we know, disjunction generates a conjunctive interpretation in all human languages when it appears in the assertion of sentences with certain focus expressions. So, Chinese sentences license a conjunctive interpretation when the disjunction operator *huozhe* appears in the scope of the focus expression *zhiyou*, and Japanese sentences license a conjunctive interpretation when the disjunction operator *ka* is in the scope of the focus expression *dake*. Therefore, a third linguistic universal has been postulated (based on joint work with Takuya Goro and Utako Minai): *Disjunction generates a conjunctive interpretation in the assertion of certain focus expressions in all human languages*.

This rests our case for concluding that all languages adopt the same meaning of OR, namely inclusive-*or*. We cited three structures that, across languages, invoke inclusive-*or*. In all three cases, moreover, it seems implausible that children learn that disjunction is inclusive-*or* based on their exposure to sentences with these structures. These joint observations are relevant for the long-standing 'nature versus nurture' controversy. A linguistic property that (i) emerges in human languages without decisive evidence from experience and (ii) is common to all human languages is a likely candidate for innate specification. A third hallmark of innateness, early emergence, will be discussed in section 7. First, though, we wish to consider one way in which languages vary in the interpretation they assign to disjunctive statements, in simple negative sentences. Since evidence of cross-linguistic variation often accompanies arguments against innateness and for an experience-dependent account of language development, it is important to show that cases of language variation do not weaken the case for logical nativism. Experience matters, of course. As child speakers grow up, they must eventually learn to use disjunction in the same way as adults do. But, as we will show, the cross-linguistic variation at issue is not compelling evidence that disjunction is exclusive-*or* in any human language.

## 7. Variation in the Interpretation of Disjunction

It is worth asking why we didn't derive a universal principle invoking simple negative sentences such as *Max didn't eat sushi or pasta*, with negation and disjunction in the same clause. After all, this sentence also licenses a conjunctive entailment that Max didn't eat sushi *and* Max didn't eat pasta, at least in English. Why was it necessary to add the proviso that negation had to be in a higher clause than disjunction in order to ensure that a conjunctive interpretation was generated? The problem is that, if we translate the simple English sentence *Max didn't eat sushi or pasta* into certain other languages, including Japanese, Russian, and Hungarian, the corresponding sentences in these languages do not generate a conjunctive interpretation. As example (15) illustrates, adult speakers of Japanese interpret (15) to mean that the pig didn't eat the carrot or the pig didn't eat the green pepper. Despite the appearance of the disjunction operator *ka* under

local negation in the surface syntax, *ka* is interpreted as if it has scope over negation.

(15) *Japanese*

Butasan-wa ninjin ka pi'iman-wo tabe-nakat-ta.  
*pig-TOP carrot or green.pepper-ACC eat-NEG-PAST*  
 'The pig didn't eat the carrot or the pig didn't eat the green pepper.'  
*lit.: 'The pig didn't eat the carrot or the green pepper.'*

Pursuing a suggestion by Szabolcsi (2002), Goro (2004) proposed that languages are partitioned into classes by a 'parameter'. According to this parameter, the disjunction operator is a *positive polarity item* (like English *some*) in one class of languages, but not in another class of languages (including English and German, among others). By definition, a positive polarity item must be interpreted as if it were positioned outside the scope of negation (OR > NEG), rather than in the scope of negation (NEG > OR). The Japanese setting of the parameter is (OR > NEG), so a paraphrase of (15) would be: *It is a carrot or a green pepper that the pig didn't eat*. On this setting of the parameter, negation does not take scope over disjunction, so no conjunctive interpretation is generated. On the English setting of the parameter (NEG > OR), disjunction is interpreted under negation, so (15) would be paraphrased in English as *The pig didn't eat a carrot or a green pepper*. In this case, a conjunctive entailment is generated.

Based on considerations of language learnability, Goro made an intriguing prediction — that young Japanese-speaking children would initially generate a conjunctive entailment in simple negative disjunctive sentences, in contrast to adult speakers of Japanese. The prediction was based on the observation that the two settings of the parameter are in a subset/superset relation. Setting aside the implicature of exclusivity, on the Japanese/Russian setting of the parameter, (15) is (logically) true in three different sets of circumstances; when the pig didn't eat a carrot, but did eat a green pepper, when it didn't eat a green pepper, but did eat a carrot, and when it didn't eat either one. These are the circumstances associated with the inclusive-*or* interpretation of disjunction when disjunction takes scope over negation (OR > NEG). On the English/German setting of the parameter, negation takes scope over disjunction (NEG > OR). On this setting, (15) is true in just one set of circumstances, namely ones in which the pig didn't eat either a carrot or a green pepper. This parameter setting also invokes the inclusive-*or* interpretation of disjunction. This means that disjunction has the inclusive-*or* interpretation on both settings of the parameter. What changes, according to the setting of the parameter, is the scope relations between disjunction and negation.

Notice that one setting of the parameter (NEG > OR; English/German) makes the statement of (15) true in a subset of the circumstances corresponding to the other setting (OR > NEG; Japanese/Russian). The *semantic subset principle* dictates that, whenever parameter values are in a subset/superset relation, the *language acquisition device* compels children to initially select the subset value (Crain, Ni & Conway 1994). The semantic subset principle anticipates that the subset reading (NEG > OR; English/German) will be children's initial setting (i.e. the default). Based on this line of reasoning, Goro (2004) predicted that children



learning Japanese would initially interpret (15) in the same way as English-speaking children and adults. The prediction was confirmed in an experimental investigation of 4- and 5-year-old Japanese-speaking children by Goro & Akiba (2004). They found that young Japanese-speaking children consistently licensed a conjunctive entailment in response to statements like (15). This empirical finding reinforces the conclusion that human languages invoke the inclusive–*or* meaning of disjunction, as in classical logic (Crain, Goro & Thornton 2006).

According to the parameter under consideration, there are two classes of languages. In one class, which includes Japanese and Chinese, disjunction is a positive polarity item; in the other class, which includes English and German, disjunction is not a positive polarity item. By definition, a positive polarity item must take scope over negation. English *some* meets this definition of a positive polarity item, as (16) illustrates. If *some* were to be interpreted within the scope of negation, then the sentence would mean Ted didn't eat *any* kangaroo. Instead, it means *There is some kangaroo that Ted didn't eat*.

- (16) Ted didn't eat **some** kangaroo.  
'There is some kangaroo that Ted didn't eat.'

Positive polarity items (e.g., English *some*, Chinese *huozhe*, Japanese *ka*) are interpreted as having scope over negation just in case the positive polarity item and negation are in the same clause. However, if negation appears in a higher clause than the one containing the positive polarity item, then negation takes scope over the polarity item, as long as negation c-commands disjunction (and there are no intervening quantificational expressions). Example (17) illustrates this for English *some*.

- (17) You didn't convince me that Ted ate **some** kangaroo.  
'You didn't convince me that Ted ate any kangaroo.'

If Chinese disjunction operator *huozhe* and the Japanese disjunction operator *ka* are positive polarity items, as Goro suggests, then Chinese and Japanese should be expected to adhere to de Morgan's laws in sentences in which negation appears in a higher clause than the clause that contains *huozhe* or *ka*, as we have seen.

## 8. Children's Interpretation of Disjunction

There are several studies showing that young children know that disjunctive words in human languages correspond to inclusive–*or*. We begin with disjunction in the scope of focus expressions. Recent experimental research has sought to determine whether or not children know the two meaning components of sentences with certain focus expressions. In a series of studies (see, e.g., Crain, Goro & Minai 2007), we investigated children's interpretation of *or/ka* to assess their knowledge of the semantics of *only/dake*. The research strategy was to investigate children's interpretation of disjunction *or/ka* in the presupposition of

sentences with the focus operator *only/dake* in one experiment, and in the assertion in a second experiment. One of the test sentences was (18).

- (18) a. Only Bunny Rabbit ate a carrot or a green pepper. *English*  
 b. Usagichan-dake-ga ninjin ka pi'iman-wo taberu-yo. *Japanese*  
*rabbit-only-NOM carrot or green.pepper-ACC eat-DEC*  
 'Only Bunny Rabbit ate a carrot or a green pepper'

*Presupposition:* Bunny Rabbit ate a carrot or a green pepper.

*Assertion:* Everyone else (being contrasted with Bunny Rabbit) did not eat a carrot or a green pepper.

As indicated, the disjunction operators *or/ka* in (18) yield *disjunctive* truth conditions in the presupposition. Suppose, then, that children assign the adult interpretation to *or/ka* in the presupposition. If so, children should accept sentences (18) in the situation where Bunny Rabbit ate a carrot but not a green pepper. This was Experiment I.

In the assertion, *or/ka* licenses a conjunctive interpretation — everyone else did not eat a carrot and did not eat a green pepper. Consequently, if children assign the correct interpretation to *or/ka* in the assertion, they should reject (18) in the situation in which Cookie Monster ate a green pepper (while, again, Bunny Rabbit ate a carrot but not a green pepper). This is Experiment II.

To summarize, if children understand both the presupposition and the assertion of *Only Bunny Rabbit ate a carrot or a green pepper*, then they should accept it in Experiment I, but reject it in Experiment II.

The experiments in English and Japanese were identical in design, with only minimal changes in some of the toy props. The experiment adopted the Truth Value Judgment task, in the prediction mode (Chierchia *et al.* 2001, Crain & Thornton 1998). There were two experimenters. One of them acted out the stories using the toy props, and the other manipulated the puppet, Kermit the Frog. While the story was being acted out, the puppet watched along with the child subject. In each trial, the story was interrupted — after the introduction of the characters and a description of the situation — so that the puppet could make a prediction about what he thought would happen. Then, the story was resumed, and its final outcome provided the experimental context against which the subject evaluated the target sentence, which had been presented as the puppet's prediction. The puppet repeated his prediction at the end of each story, and then the child subject was asked whether the puppet's prediction had been right or wrong.

The main finding was that both English-speaking children and Japanese-speaking children consistently accepted the test sentences in Experiment I in both languages, and children consistently rejected the test sentences in Experiment II in both. The two groups of children showed no significantly different behavior in interpreting disjunction within sentences containing a focus operator, *only* versus *dake*. The high rejection rate in Experiment II shows that children assigned a conjunctive interpretation to disjunction in the assertion of sentences with the focus expression *only/dake*.

Another series of experimental studies investigated children's knowledge of the asymmetrical interpretation of disjunction in sentences with the universal quantifier. Several studies have investigated the truth conditions children associate with disjunction in the subject phrase and in the predicate phrase of the universal quantifier. For example, in studies (e.g., Boster & Crain 1993, Gualmini, Meroni & Crain 2003) using the Truth Value Judgment task, children were asked to evaluate sentences like (19) and (20), posed by a puppet, Kermit the Frog.

(19) Every woman bought eggs or bananas.

(20) Every woman who bought eggs or bananas got a basket.

In one condition, sentences like (19) were presented to children in a context in which some of the women bought eggs, but none of them bought bananas. The child subjects consistently accepted test sentences like (19) in this condition, showing that they assigned a 'disjunctive' interpretation to *or* in the subject phrase of the universal quantifier, *every*. In a second condition, children were presented with sentences like (20) in a context in which women who bought eggs received a basket, but not women who bought bananas. The child subjects consistently rejected the test sentences in this condition. This finding is evidence that children generated a conjunctive interpretation for disjunction in the subject phrase of *every*. This asymmetry in children's responses in the two conditions demonstrates their knowledge of the asymmetry in the two grammatical structures associated with the universal quantifier—the subject phrase and the predicate phrase. The findings represent a challenge to the experience-dependent approach to language acquisition. The challenge is posed by the asymmetry in the interpretation of the same disjunction *or*, in the subject phrase versus the predicate phrase of the universal quantifier, since the distinction is one of interpretation, not the distribution, of lexical items.

The case for logical nativism is also supported by evidence that English-speaking children respect de Morgan's laws at an early age. If adults judge that negated disjunctions license conjunctive entailments, then children must acquire the capacity to make similar judgments as they grow into adulthood. But that leaves a lot of time for exposure to a lot of data. But if very young children demonstrate knowledge of the semantic principles that characterize adult linguistic competence, then that would compress the acquisition problem considerably. Of course, no-one can ever prove that 2-year-old children have not already utilized a vast range of data, but the case for logical nativism is strengthened if it can be demonstrated that 2-year-old children adhere to de Morgan's laws before they are *plausibly* exposed to the data needed by learning-theoretic accounts. We will discuss one candidate for a learning-theoretic account in the next section. First, we present further empirical evidence for logical nativism based on experimental studies of 2-year-old English-speaking children.

In an ongoing longitudinal study of four 2-year-olds, we have presented them with negated disjunctions, and have recorded their behavioral and verbal responses. On a typical trial in one condition, children are shown three dogs, a white one, a brown one and a black one. Kermit the Frog, who is manipulated by

the experimenter, indicates that he wants to play with a dog. The experimenter then holds up the three dogs. Then Kermit says: "I don't want to play with the white dog or the brown dog". If children adhere to de Morgan's laws, they are expected to give Kermit the black dog. In another condition, negated disjunctions are used in *wh*-questions, such as *Who doesn't have A or B?*. On a typical trial, an array of characters are introduced, some with yo-yo's, some with sponge balls, and some with strawberries. Then, the target question is posed to children: *Who doesn't have a yo-yo or a sponge ball?*. One of the youngest children consistently responded in conformity with the conjunctive entailment beginning on the very first trial, at age 2;3. Other children produced consistent adult-like responses later than this, but all four children consistently respond in ways that demonstrate knowledge that negated disjunction yield conjunctive entailments by age 2;10. The transcripts of parental input reveal that children experience little evidence that disjunction is inclusive-*or*. The vast majority of the input is consistent with exclusive-*or*, so this interpretation would be adopted by many children if it were a possible semantic option in human languages. The fact that all four of the 2-year-olds we have tested have reached the opposite conclusion, that disjunction is inclusive-*or*, supports our claim that inclusive-*or* emerges in children's grammars in the absence of decisive evidence from experience. Emergence in the absence of experience is one of the hallmarks of innateness.

In this section, we produced empirical grounds for believing the inclusive-*or* interpretation of disjunction is universal and innate. The evidence from young children regarding their understanding of negated disjunctions seems compelling. Once they understand the meaning of *or* and *ka* they assent to the conjunctive entailments supported by de Morgan's laws, even for statements that do not obey de Morgan's laws for adult speakers, as in Japanese. Obviously, children do not learn to obey de Morgan's laws by observing how adults interpret disjunction. We think the conclusion to draw is, therefore, that children do not learn the meaning of disjunction; they bring knowledge that the meaning of disjunction is inclusive-*or* to the task of language development.<sup>5</sup>

In the next section, we consider what it would actually take for children to *learn* the meaning of disjunction. We will consider how children might learn the meaning of logical connectives, including disjunction, by observing how people use these connectives in drawing inferences. Once it is laid out for examination, such a learning story seems to us to be highly implausible. Then we summon some *a priori* arguments against such learning accounts.

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<sup>5</sup> One common argument against the universality of *v*-disjunction is that there is at least one language, namely Latin, which has separate words for inclusive and exclusive disjunction, *vel* and *aut* respectively, so that there is no such thing as *the* meaning of OR in Latin — in this language OR has two meanings, depending on whether it is inclusive or exclusive disjunction one has in mind. But Jennings (2001) has convincingly refuted this "mythical supposition", as he calls it. For if *aut* really did mean  $\oplus$ , then negating a sentence such as *Timebat tribunos aut plebes* 'One feared the magistrates or the mob' ought to produce a sentence meaning that everyone either feared both or neither. But this is not what *Nemo timebat tribunos aut plebes* means at all — it means that no one feared either, precisely as the inclusive interpretation of *aut* predicts.

## 9. Learning by Inference Rules

Some claim that there is a straightforward solution to the language-learning problem for a finite logical vocabulary: learning the meanings of logical expressions is simply a matter of learning the inferential rules associated with these expressions. This is the claim of Conceptual Role Semantics (CRS).<sup>6</sup> Advocates of CRS attempt to explain our knowledge of the meaning of logical expressions by exploiting the role these expressions play in inferences. Thus, we can imagine that children learn the rules of logic in the same way they learn the rules of chess or any other game: someone instructs them in the rules or, more likely, they observe the linguistic behaviour of others who know the rules. On this account, there are no alternative hypotheses involved, just like the kinds of meaning-stipulations that are required for learning “a knight can move two squares vertically and one horizontally or two horizontally and one vertically”.

Admittedly, there is an undeniable appeal to this type of account. And, the CRS account could plausibly explain how even young learners come to use AND, based on experience. The requisite experience consists of observing the patterns of inference that involve AND, namely its introduction rule (&I):

$$(21) \frac{A \quad B}{A \ \& \ B}$$

and its elimination rule (&E):

$$(22) \frac{A \ \& \ B}{A \quad B}$$

All the learner needs to learn the meaning of AND is to be shown these rules, (&I) and (&E). No testing of hypotheses is involved, according to CRS.

However, an account of meaning via exposure to inference rules does not generalize to other logical constants. Consider how children would learn the meaning of OR. Earlier we argued that human languages validate the introduction rule for OR, Weakening (vI) — see (1)–(2). However, English-speaking adults find direct statements of Weakening unacceptable, so they are not likely to use Weakening in the simple form. Although adults assent to the validity of Weakening if this rule is validated for them via a step involving Existential Generalization, it is highly implausible that children learn that disjunction is inclusive-or by observing adults using this complex form of inference. Even if adults were to use disjunction in this way, this kind of input is just too exotic to be available in sufficient quantities to ensure that all children learning English, or any other language, reach the conclusion that the meaning of disjunction is inclusive-or. Therefore, this inference rule is not a likely source of evidence for children that the meaning of disjunction is inclusive-or. Yet, as we have seen,

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<sup>6</sup> The view originated with Wittgenstein’s (1953) thesis that meaning is use and was developed further by Sellars (1969). Proponents of CRS today include Block (1986, 1987), Brandom (2000), Field (1977, 2001), Harman (1987), Horwich (1998), Miller & Johnson-Laird (1976).

even 2-year-old English-speaking children seem to have reached just this conclusion.

This brings us to the elimination rule for disjunction, ( $\vee$ E):

$$(23) \quad \begin{array}{c} [A] \quad [B] \\ \cdot \quad \cdot \\ \cdot \quad \cdot \\ \cdot \quad \cdot \\ \hline A \vee B \quad C \quad C \\ C \end{array}$$

Disjunction Elimination ( $\vee$ E) works in the following way: To prove that some conclusion  $C$  follows from a disjunction  $A \vee B$ , we need to establish that  $C$  follows from each of the disjuncts  $A$ ,  $B$  in turn. If so, then  $C$  must follow from the disjunction  $A \vee B$  since it has been shown that, irrespective of which specific disjunct holds,  $C$  follows. The disjuncts are bracketed to indicate that we are not committed to them by the end of our demonstration — they are ‘discharged’, i.e. removed from the list of assumptions to which we are committed.

Here is a simple illustration. Suppose we wish to show *Alice did not hear the telephone* can be derived from the disjunctive claim *Alice was out of the house or Alice was fast asleep*. We proceed by first assuming the left hand disjunct (LH), *Alice was out*, showing that if she was out, then she would not have heard the telephone in the house ring. We then assume the right hand disjunct (RH) *Alice was fast asleep*. Knowing how soundly Alice sleeps, we are able to derive the conclusion that she would not have heard the phone from the assumption that she was fast asleep. We don’t know whether she was out at that time or fast asleep, but let’s suppose since on either alternative she would not have heard the telephone ring, we have established that  $C$  *Alice did not hear the telephone* follows from *Alice was either out of the house or fast asleep*. Clearly, we are not committed to believing categorically that she was out nor are we categorically committed to believing she was fast asleep. We’re committed only to believing that one or the other alternative held, i.e. we’re committed to believing the disjunction *Either Alice was out of the house or fast asleep*. So we discharge both disjuncts *Alice was out*, and *Alice was asleep*.

We can formally vindicate the requisite conjunctive entailment of  $\neg A \ \& \ \neg B$  by  $\neg(A \vee B)$  as follows:

1.	$\neg(A \vee B)$	<i>Assumption</i>	6.	$B$	<i>Hypothesis</i>
2.	$A$	<i>Hypothesis</i>	7.	$A \vee B$	6 $\vee I$
3.	$A \vee B$	2 $\vee I$	8.	$\perp$	1, 7 $\& I$
4.	$\perp$	1, 3 $\& I$	9.	$\neg B$	6, 8 <i>RAA</i>
5.	$\neg A$	2, 4 <i>RAA</i>	10.	$\neg A \ \& \ \neg B$	5, 9 $\& I$

The child whose knowledge of the meaning of OR consisted in knowledge of the inference rules of *or*-introduction and *or*-elimination would know that the meaning of the English word *or* is inclusive-*or*. Yet even if these particular

inference rules for *or* are constitutive of the meaning of OR, it is quite another matter to conclude that these inference rules are available in the primary linguistic data (PLD) to which children are exposed. Disjunction Elimination, ( $\vee E$ ), in particular is a highly sophisticated rule that young adults typically struggle with in the logic classes. Why would young adults struggle if, as children, they tacitly grasped this inference rule when they first learned the meaning of OR? It is even less plausible to suppose that young children should have any idea of the discharge of assumptions or sub-derivations. But such knowledge is a prerequisite to understanding the bare notion of disjunction using Disjunction Elimination.

A simpler Elimination Rule for disjunction, Disjunctive Syllogism, presents itself as a far more plausible candidate for something a child might learn that could serve to fix the meaning of OR:

$$(24) \frac{A \vee B \quad \neg A}{B}$$

Disjunctive Syllogism, unlike Disjunction Elimination, seems highly learnable. The 'elimination of alternatives' would seem to be a fairly primitive conceptual resource. It has been suggested that it is available even to creatures far simpler than humans. It is reasonable to suppose that a pattern of inference that plausibly predates the advent of language would be made explicit in the logic of human languages, and recognized as sound by young language-learners. If this were indeed so then there would no longer be any need for the child to acquire the concept of disjunction by learning the meaning of OR since s/he would already possess the concept in using elimination of alternatives. Even if this speculation were to prove wrong, however, Disjunctive Syllogism could not *by itself* fix the meaning of OR since it holds for *both* inclusive and exclusive disjunction and thus fails to distinguish between them.

So far, we have concluded that the child's PLD is unlikely to contain instances of those inference rules, such as Weakening and Disjunction Elimination, that could serve as the basis for learning the meaning of OR. We established earlier that language-users are committed to Weakening as a sound form of inference governing their understanding of OR *even if they do not in general recognize this fact*. But in light of this latter point, Weakening is highly unlikely to appear in the primary (or other) linguistic data available to the child, so it cannot serve to fix the meaning of OR. Of the two Elimination Rules canvassed for OR, Disjunction Elimination and Disjunctive Syllogism, the former is wildly implausible as a possible route for a child to acquire the meaning of OR, because of its sheer conceptual complexity. Disjunctive Syllogism failed for exactly the opposite reason: given the pre-linguistic child's reasoning proclivities, its conceptual simplicity suggests it might already be available to the child prior to any acquisition of the meaning of OR. Yet, regardless, it is too weak by itself to fix the meaning of OR since it does not distinguish the exclusive reading of disjunction from the inclusive one.

In sum, the account of learning offered by CRS seems implausible, with the possible exception of the acquisition of the meaning of conjunction. Such worries

pale into insignificance, however, when compared to Prior's (1978) famous problem for CRS accounts of the logical constants. Prior invented a logical constant 'TONK' with the following introduction and elimination rules:

$$(25) \quad \text{(TONK I)} \quad \frac{A}{A \text{ TONK } B} \quad \frac{B}{A \text{ TONK } B}$$

$$(26) \quad \text{(TONK E)} \quad \frac{A \text{ TONK } B}{A} \quad \frac{A \text{ TONK } B}{B}$$

Prior then used these inference rules to prove that any two arbitrary sentences were identical:

$$(27) \quad \text{(TONK I)} \quad \frac{[A]^1}{A \text{ TONK } B} \quad \frac{[B]^2}{A \text{ TONK } B}$$

$$\text{(TONK E)} \quad \frac{B \quad A}{A \leftrightarrow B} \quad \text{(1, 2 } \leftrightarrow \text{ I)}$$

Of course, TONK is an incoherent rule. It grafts the introduction rule for OR onto the elimination rule for 'and'. Prior's point was that a purely inferentialist account of the meaning of the logical constants, such as CRS, doesn't have the resources to say what is wrong with the acquisition of the meaning of TONK. We think what has gone wrong with CRS is that nature hasn't designed us to be CRS machines. Instead, it has engineered us through evolution to be creatures with rich conceptual resources to check the reliability of our mental representations. The upshot is that CRS does not, in principle, provide an adequate model of how language-learners acquire the meanings of logical constants such as OR. Of course, we think the meanings are not learned at all, but are innately specified. We now proceed to offer two *a priori* arguments for logical nativism, one based on Quine's (1979) critique of logical positivism, and one based on Fodor's (1980) argument for the innateness of primitive lexical concepts.

## 10. Quine's Critique of Truth by Convention

Like the rest of his logical positivist peers, Carnap (1937) sought to account for the necessity of logical and mathematical truths. Carnap offered a linguistic account of necessity: The necessary truths of a given language L are simply those generated by linguistic stipulations that determine L, stipulations that are purely conventional. Moreover, it is because necessary truths are really disguised linguistic stipulations that they can be known *a priori* to be true. For the logical positivists, necessary truths are true irrespective of how the world happens to be since they are *true in virtue of meaning*.

Take the logical truth known as the *Law of Non-Contradiction* (LNC): 'It is not the case that p and not p are both true'. According to Carnap, once we know what the logical operators NOT and AND mean, we have *a priori* knowledge of the



truth of the LNC: It simply *follows from* the meanings of NOT and AND.

Quine's objection was simplicity itself. The conventionalist account just rehearsed makes essential use of the notion of 'follows from', i.e. of *logical consequence*. So, according to the Carnapian account of logical necessity, our *a priori* knowledge of logical truths does not simply arise from our knowledge of the conventional linguistic meanings we have adopted to define the logical terms NOT and AND. To get from these linguistic conventions to the truth of LNC we must appeal to an already understood notion of logical consequence. Now either this nascent understanding of logical consequence is *a priori* or it is not.

- (I) If it is *a priori*, then some *a priori* knowledge cannot be explained conventionally.
- (II) If it is *not a priori* then our knowledge of at least some necessary truths cannot be explained by means of linguistic conventions.

Either way, the conventionalist account of logical truth breaks down. Meaning-stipulations (conventions) by themselves thus fail to secure *any* truths, even language-relative ones such as the logical truths were held to be. Quine admitted that so-called logical truths, such as LNC, had a different status from ordinary truths that are learned from experience. But, Quine had an alternative account of the special status of truths such as LNC. To characterize the contrast, Carnap endorsed (28), Quine argued for (29).

- (28) The reason we know that LNC holds, and holds of necessity, is that  $A \ \& \ \neg A$  is a contradiction, and thus cannot possibly be true.
- (29) The reason we know that LNC holds, and holds with such tenacity, is that it is a strongly held belief that experience would never force us to conclude that  $A \ \& \ \neg A$  holds true.

So there are two possible explanations about the epistemic status of LNC. Carnap's explanation is that, having learned the conventional meanings ' $\neg$ ' and ' $\&$ ', we know *a priori* that the LNC is true without investigating what the world is like. By contrast, Quine's explanation is that the LNC, along with other fundamental logical and mathematical truths, occupies a privileged position in our web of belief purely because that web is so structured that the logico-mathematical truths lie at its core, well-insulated from the impact of experience. Another way to frame the contrast in epistemic status is to consider it from the vantage point of the learner. Assuming that the learner comes to know somehow that  $\neg [A \ \& \ \neg A]$ , two possibilities arise for the learner:

- (i) Is  $\neg (A \ \& \ \neg A)$  true because it could *never be correct* to assert  $A \ \& \ \neg A$ ?
- (ii) Is  $\neg (A \ \& \ \neg A)$  true simply because I *never hear*  $A \ \& \ \neg A$  asserted?

The problem with (ii) is that it invites the learner to infer that p is untrue on the grounds of a (persistent) absence of evidence for p. That is a risky inference, to say the least. So the learner should guess that (i) is likely to be right — he never

hears  $A \ \& \ \neg A$  because there is something amiss with its assertion: It would never be correct to assert it. But the question is why, and Carnap and Quine return opposite answers, (28) and (29). Quine cannot endorse (i). According to Quine, knowing that you could not as a matter of principle ever hear  $A \ \& \ \neg A$  asserted sneaks in a notion of knowing that certain statements are semantically illicit — that is, it sneaks in an implicit notion of *logical incoherence* in the form of a *contradiction*. So, Quine could endorse (ii) which claims that, as a matter of fact, we never hear anyone saying  $A \ \& \ \neg A$ . Despite their differences, both Carnap and Quine agreed on one thing — that the meanings of logical expressions such as NOT and AND are learned. For Quine, these were learned through observation of the assent and dissent dispositions of speakers. For Carnap these were learned through understanding the implicit conventions (stipulations) governing the meanings of these terms.

In a further critique of Carnap, Putnam (1975) added another argument, similar in spirit to the argument by Quine, but more relevant for our purposes, because Putnam's argument challenges the common assumption of both Quine and Carnap — that knowledge of logical truths can be learned. According to Putnam, Carnap's account of logical truth in terms of meaning-stipulations or conventions cannot be correct for the simple reason that the entire set of meaning-stipulations M could only be finite (or else recursive), whereas the entire set of logical truths T is infinite. So the question arises as to how T is to be generated from M. The only way this could be done is by deriving the elements of T from the elements of M — that is, by making use of the notion of logical consequence. Since it was precisely the notion of logical consequence that was supposed to be explicated by the meaning-convention approach, that approach is viciously circular.

Putnam is obviously correct in pointing out that the learner can only ever receive finitely many 'instructions' (either as to the meaning-stipulations or as to the assent-dissent dispositions of adults with respect to logical expressions). But the critical observation is that the learner somehow develops an unbounded competence in logic, on the basis of fragmentary experience. In the present article we have demonstrated that this human logical competence is both universal and emerges very early in children. These arguments, of course, are nothing other than an instance of Chomsky's familiar poverty of the stimulus argument for the language faculty (see, e.g., Crain & Pietroski 2002, Crain, Gualmini & Pietroski 2005, and Pietroski & Crain, in press). The difference is that it is applied to logic competence, rather than linguistic competence.

As counter-point to Putnam, Noam Chomsky and his followers ask how learners acquire knowledge that an unbounded number of strings are associated with certain meanings, and could never be associated with other meanings. Take a familiar example. In the string *He danced while Max ate pizza*, the pronoun *he* cannot refer to Max; it must refer to some unmentioned male individual. Where does the knowledge come from about what such sentence cannot mean? Again, there are two possibilities for the learner:

- (iii) Is  $\neg (he = Max)$  true because it could *never be correct* that  $(he = Max)$ ?
- (iv) Is  $\neg (he = Max)$  true simply because I *never hear* strings where  $(he = Max)$ ?

The critical point is that children know an unbounded number of such linguistic facts, such as the disjoint reference facts about pronouns and referential noun phrases. And children acquire knowledge of such facts despite having only fragmentary and often misleading evidence, as we have seen in the case of OR. In the case of pronouns and names, misleading data are abundant, consisting of examples which are similar in meaning, but in which the name precedes the pronoun (*Max danced while he ate pizza*), and where the pronoun precedes the name, but appears in a subordinate clause (e.g., *While he danced Max ate pizza*). Of course, this is just one example. For others, see Thornton (1990), Crain & Pietroski (2001), and Thornton (2007).

It is remarkable, then, to find that young children implicitly conclude that certain sentence meanings are ‘necessarily’ correct, permitting children to make judgments about entailments, contradictions, paraphrase, and ambiguity. Augmented by evidence that such linguistic phenomena are universal, and mastered by very young children, it has been argued that there is an innate Language Faculty (for a recent statement, see Pietroski & Crain, in press). We have presented a similar set of arguments for an innate Logic Faculty, based on the universality and early emergence of knowledge that disjunction is inclusive–or in human languages.

## 11. Mad Dog Logical Nativism

Fodor (1980) produced a notorious argument that is purported to prove every primitive lexical concept is innate. This is often referred to as Mad Dog Nativism. Whilst Mad Dog Nativism may be mistaken for lexical concepts, there is something right about the form of Fodor’s original argument in favour of it: properly construed, the argument provides a good reason for believing, not that *all primitive lexical* concepts like TURNIP and CARBURETOR are innate, but that primitive *logical* lexical concepts like DISJUNCTION and NEGATION are innate. Fodor’s argument proceeded as follows:

1. *All concepts are either learned or innate.*
2. *If learned, a concept must be acquired through hypothesis-testing.*
3. *Any concept acquired via hypothesis-testing is a logically structured concept.*
4. *Primitive Lexical concepts are not logically structured.*
5. *So, Primitive Lexical concepts are not acquired via hypothesis-testing.*
6. *Hence, Primitive Lexical concepts are not learned.*
7. *Thus, Primitive Lexical concepts are innate.*

It is unclear from Fodor’s presentation whether he thinks that primitive lexical concepts are unlearnable due to their primitive status, or due to their acquisition through hypothesis-testing. We think the status of lexical concepts as primitive (or derived) is a red herring when it comes to issues of learnability. What matters for learnability is not whether the learning net catches the little fish of primitive or unstructured concepts, what matters is the composition of the net.

In our view, however, Fodor’s argument raises a genuinely significant issue

concerning hypothesis-testing and its relation to learning. First, an obvious point needs to be borne in mind in any discussion of concept acquisition. Without having acquired the concept, say FROG, a learner, say Ollie, can think no thoughts with froggy content. That is, Ollie cannot think thoughts like *frogs are slimier than mice* or *I'd much rather play with a frog than with a rat!* No frog concept, no frog thoughts. Without having acquired the concept FROG, Ollie also cannot frame a frog hypothesis like *I get it! Claude is talking about frogs when he says 'grenouille'!* Nor can he use any special tacit knowledge about frogs in testing various hypotheses about the meaning of the French word *grenouille* or the English word *frog*.

The reason we refrain from endorsing Fodor's argument in its full generality can be illustrated by the frog-case. Ollie can acquire the concept of FROG by linking the word *frog* to other non-FROG concepts he has which he can use to identify frogs — for example, *those funny pop-eyed green hopping things*, or even *those things* (Ollie is pointing at some frogs). Fodor himself allows that Ollie will have mastered the concept FROG if his mental tokens of the word *frog* are causally linked in the right way to frogs. So, at least when the primitive lexical concepts pick out recurrent features of the language-learner's environment, there seems to be no special reason why our language-learner has to deploy the concept itself in hypotheses designed to settle the meaning of the lexical expression denoting that concept. Perhaps for Ollie to acquire the concept of FROG it will suffice if he has *some* innate primitive concepts, colour concepts, natural kinds, motion verbs, and so forth. Based on such innate primitive concepts, we see no reason, in principle, to suppose that other concepts, like frog, turnip and carburetor, cannot be acquired rather than being innate.

So Fodor's premise (3) does not seem *generally* correct. Nonetheless, there may be specific cases for which it does hold, and we claim that logic is precisely one such case. So we need to recast Fodor's argument, applying it specifically to *primitive* logical concepts rather than primitive lexical concepts. The argument for Mad Dog Logical Nativism proceeds as follows:

- 1'. *All logical concepts are either learned or innate.*
- 2'. *If learned, a logical concept is acquired through hypothesis-testing.*
- 3'. *If a logical concept is acquired through hypothesis-testing, neither the formulation of the hypothesis nor the methods used to test it can invoke the concept.*
- 4'. *In determining the meaning of a term denoting a primitive logical concept, learners make use of the concept to be acquired, if not in framing the hypothesis then in testing it.*
- 5'. *So, primitive logical concepts cannot be acquired through hypothesis-testing.*
- 6'. *Therefore, primitive logical concepts are not learned.*
- 7'. *And, therefore, primitive logical concepts are innate.*

To many ears, perhaps even to most, the conclusion 7' may sound incredible. If it is wrong, though, there must be something amiss in the argument. The suspicious premises seem to be 2', 3', or 4'. Indeed, 2' looks a tad suspicious —

two-year-olds are not scientists; so whatever goes on inside their heads when they learn the meanings of words, it is not by theory construction. While 2' stands in need of defense, it is not overturned by such simple considerations: Two-year-old theory construction could be worlds away in conceptual sophistication from theory construction by scientists, yet it could still be, for all that, genuine theory construction. For example, if Ollie initially thinks *or* means the same as *and* and later corrects this, he has surely revised his conjecture about the meaning of *or*.

As for 3', if Ollie has not yet acquired the concept OR, he cannot frame a hypothesis that is an OR-thought (a disjunctive thought). He cannot think *I get it! Claude is talking about alternatives when he says 'ou'!* Neither can he use any special tacit knowledge about alternatives in testing various hypotheses about the meaning of the French word *ou* or the English *or*. Indeed, he cannot even recognize alternative hypotheses as alternatives.

What about 4'? Does the formulation of a hypothesis about the meaning of French *ou* or English *or* require the use of the concept of disjunction? It is not at all obvious that it does. Suppose Ollie hears *A or B* a lot, in circumstances in which only *A* is true, or only *B* is true. After a while, he could use indirect negative evidence (e.g., that he hasn't heard *A or B* used when both *A* and *B* are true, or when both are false) to infer that these circumstances make such sentences false. Then, Ollie will have learned that disjunction has the truth conditions associated with exclusive-*or*.

However, one persistent criticism of truth-conditional semantics has been that the truth-conditions of logically complex sentences are only intelligible to someone who already possesses the relevant logical concepts.<sup>7</sup> Thus the truth-conditions for OR statements that Ollie learns can be summarized as: "*A or B* is true if and only if either *A* is true or *B* is true". According to this view, these truth-conditions are not intelligible to Ollie unless he already possesses the concept of disjunction, which was precisely what he was supposed to have acquired through learning the meaning of the word *or*.

Let's consider the method Ollie used to acquire the meaning of OR. When Ollie worked out that *or* meant exclusive-*or*, we must suppose that he learned what *or* means without engaging an innate concept of disjunction. It follows that no consideration of alternative hypotheses could have played any role in his hypothesis-testing.

That is, he could not have recognized H<sub>1</sub>: "*A or B* means at least one of *A* or *B* is true", as against H<sub>2</sub>: "*A or B* means exactly one of *A* or *B* is true" as alternatives, weighed up evidence *pro-and-con* for each, etc. But then, if he did not do any of *that*, how was his acquisition of the concept of disjunction (or the meaning of OR) a case of hypothesis-testing? So, if learning really *is* hypothesis-testing, as is widely assumed, how did Ollie learn that *or* meant exclusive-*or*? The critical point is that, quite generally, it seems that rich logical resources must be ascribed to the linguistic novice for him to learn anything, meanings or concepts

<sup>7</sup> Cf. Dummett's (1977: 114) complaint that a 'modest' theory of meaning containing clauses such as the above for disjunction "merely exhibits what it is to arrive at an interpretation of one language via an understanding of another, which is just what a translation manual does, it does not explain what it is to have mastery of a language".

included.

The challenge to opponents of logical nativism is how to accommodate the learning-as-hypothesis-testing problem without supposing innateness. We have already seen that the most obvious alternative to learning-as-hypothesis-testing doesn't fly. This was the case of learning the logical operators using the primitive inference rules that govern their introduction and elimination. This approach was appealing, since the logical vocabulary amounts to little more than five or six essential words *not, and, or, if, all, some*, and the primitive inference rules only add up to about double that number. But, we have seen that this alternative account of learning is fraught with empirical and conceptual problems.

At present, we see no plausible alternative to logical nativism. Empirical evidence from child language (including 2-year-old children) and cross-linguistic research (from typologically different languages) supports logical nativism, and several a priori arguments provide additional grounding. *A priori* arguments that logic must be innate are even stronger than scientific 'abductive' inferences from the empirical evidence gathered in investigations of child language, and cross-linguistic research. If any such argument is sound, logical nativism would remain true even if language-learners were given unlimited time and input in which to acquire logical concepts and inference rules: logic would be transcendently unlearnable, not just contingently so. Whether a Logic Faculty is intrinsically tied to a Language Faculty remains an open question for future research.

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# Adjunction, Labeling, and Bare Phrase Structure

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This article argues for a version of bare phrase structure which maintains that — contrary to the standard view on phrase structure — adjunction structures are simpler than structures involving complements and specifiers. Assuming with Hornstein (forthcoming) that the operation *Merge* is to be decomposed into two basic operations, namely, *Concatenate* and *Label*, the article shows that whereas the building of complements and specifiers requires that the output of a *Concatenate* operation be labeled, adjuncts may only require concatenation to receive a proper interpretation at the interface. It is argued that taking adjunction structures to be label-less concatenates not only complies with Chomsky's (1995) Inclusiveness Condition, but also makes it possible to account in a principled manner for the dual behavior of adjuncts, which sometimes behave as integral parts of the target of adjunction and some other times behave as completely independent elements.

*Keywords:* adjunction, bare phrase structure, labels, Merge, X'-Theory

## 1. Introduction

It is fair to say that what adjuncts are and how they function grammatically is not well understood. The current wisdom comes in two parts: (i) a description of some of the salient properties of adjuncts (they are optional, not generally selected, often display island effects, etc.) and (ii) a technology to code their presence (Chomsky-adjunction, different labels, etc.). Within the Minimalist Program, adjuncts have largely been treated as afterthoughts and this becomes clear when the technology deployed to accommodate them is carefully (or even cursorily) considered.

Our primary aim in this contribution is to propose a phrase structure for adjunction that is compatible with the precepts of Chomsky's (1995) *bare phrase*

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Part of the research reported here has been presented at the Universities of Maryland, São Paulo, Connecticut, and Leiden and at the *IV Encuentro de Gramática Generativa* (Mendoza, Argentina). We are thankful to these audiences for comments and suggestions. Special thanks to Alex Grosu and Carol Petersen for insightful and challenging comments on a previous version of this article. The authors would also like to acknowledge the support received from NSF (grant # BCS 0722648) awarded to the first author and CNPq (grant # 308176/2005-7) and FAPESP (grant # 2006/00965-2) awarded to the second author.



*structure* (BPS). Current accounts, we believe, are at odds with the central vision of BPS and current practice leans more to descriptive eclecticism than to theoretical insight. We have a diagnosis for this conceptual disarray. It stems from a deeply held, though seldom formulated, intuition: the tacit view that adjuncts are the abnormal case, while arguments describe the grammatical norm. We suspect that this has it exactly backwards. In actuality, adjuncts are so well behaved that they require virtually no grammatical support to function properly. Arguments, in contrast, are refractory and require grammatical aid to allow them to make any propositional contribution. This last remark should come as no surprise to those with neo-Davidsonian semantic sympathies. Connoisseurs of this art form are well versed in the important role that grammatical (aka, thematic) roles play in turning arguments into modifiers of events.<sup>1</sup> Such fulcra are not required for meaningfully integrating adjuncts into sentences. In what follows, we take this difference to be of the greatest significance and we ask ourselves what this might imply for the phrase structure of adjunction.

A second boundary condition in what follows is that an adequate theory of adjunction comport with the core tenets of BPS. Current approaches sin against BPS in requiring an intrinsic use of bar levels and in using idiosyncratic labeling conventions whose import is murky at best. We rehearse these objections in the following sections. A goal of a successful theory of adjuncts should be to come up with a coherent account of adjunction structures that (at least) allows for a relational view of bar levels along the lines of Chomsky (1995), itself following earlier suggestions of Muysken (1982).

More ambitiously, one could require that the bar-level properties of adjunction structures play no grammatically significant role. Hornstein (forthcoming: chap. 2) proposes a strong version of the Inclusiveness Condition, one in which *only* intrinsic features of lexical elements can be used by the computational system. This excludes, among other things, bar-level information (which is relational) from the purview of the syntax.<sup>2</sup> Thus, syntactic rules cannot be stated in terms like “Move/delete XP” or “Move X<sup>0</sup>” or “never move X’”, etc. Relational information *may* be important, at the interpretive interfaces for example, but syntactic computations *per se* cannot exploit these relational notions (given a strong version of the Inclusiveness Condition), as they are not intrinsic features of lexical items.<sup>3</sup> In what follows, we will try to adhere to this strong

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<sup>1</sup> See Higginbotham (1986), Parsons (1990), Schein (1993), and Pietroski (2004) for extensive discussion.

<sup>2</sup> “Other things” plausibly includes grammatical and/or thematic role information, Case information, agreement, hierarchical information, and chains, all of which are relational and go beyond the information contained in lexical items alone.

<sup>3</sup> The exact interpretation of the Inclusiveness Condition is somewhat murky. Chomsky (1995: 225) puts it as follows:

Another natural condition is that outputs consist of nothing beyond properties of items of the lexicon (lexical features) — in other words, that the interface levels consist of nothing more than arrangements of lexical features. To the extent that this is true, the language meets a condition of *inclusiveness*. [footnote omitted — NH & JN] We assume further that the principles of UG involve only elements that function at the interface levels; nothing else can be “seen” in the course of the computation [...].

version of the Inclusiveness Condition.<sup>4</sup>

The article is organized as follows. In section 2, we review the general properties of adjunction structures assumed in the literature and show that their standard account in terms of Chomsky-adjunction is not easily accommodated within the BPS approach to adjunction in terms of a distinct labeling procedure. Section 3 discusses what goes wrong if adjunction structures are assigned the same label as non-adjunction structures. In section 4, we argue that the output of a Merge operation need not be labeled, and this is crucial for the distinction between arguments and adjuncts. Section 5 discusses some consequences of this proposal and section 6 offers a brief conclusion.

## 2. General Properties of Adjunction Structures

Prior to minimalism, adjunction was an operation that returned a phrase of the same type as the one the operation had targeted. (1) formally illustrates (Chomsky-)adjunction with respect to phrases.

- (1)  $[_{XP} [_{XP} [_{XP} \dots X^0 \dots ] \text{ADJUNCT} ] \text{ADJUNCT} ]$   
 (2)  $[_{VP} [_{VP} [_{VP} \text{read a book} ] \text{quickly} ] \text{in the yard} ]$   
 (3)  $[_{NP} [_{NP} \text{student of physics} ] \text{from France} ]$

(2) and (3) exemplify the structure in (1) with the adjuncts *quickly/in the yard* and *from France* adjoining to VP and NP, respectively, and returning VP and NP, respectively. Accounts differed on whether adjuncts adjoined to XPs or to X's. However, they agreed in assuming that the output of adjunction left the input labeling (and constituency) intact.

The labeling in (1)–(3) codes five important properties criterial of adjunction. First, adjunction conserves bar-level information. Note that in (1)–(3) adjunction leaves the maximality of the input structure intact and in this regard, it contrasts with complementation as the latter changes bar-level information. For example, in (2) a  $V^0$  *read* combines with an NP *a book* to yield a VP (not a  $V^0$ ). Second, adjunction leaves the category information intact: If the input is verbal, the output is verbal. Third, headedness is preserved. Thus, the head of the complex in (1) is  $X^0$ , the head of (2) is *read*, and the head of (3) is *student*. Fourth, the adjunction structure ‘inherits’ the bar-level information of the target. Thus, in (2), we have three maxV projections: *read a book*, *read a book quickly*, and *read a book*

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A strong version of the above is that *the computational syntax* can only manipulate lexical features, not relations among these established during the course of the derivation: relational notions like bar level, chain, phrase, specifier, complement, etc. There are, however, other readings of this condition, but we will refrain from exegetical combat and simply see if the strong version mooted here can be sustained.

<sup>4</sup> This version of the Inclusiveness Condition suggests a strong reading of the autonomy of syntax thesis. If correct, syntactic operations are blind to certain kinds of information that the interfaces may exploit. This makes the divide between syntax and the other components of the faculty of language (FL) rather broad.

*quickly in the yard*. Last of all, there is no apparent upper bound on the number of adjuncts. Once again, this contrasts with arguments where there is generally an upper bound of three.

These five properties are well grounded empirically. The preservation of categoricity and headedness tracks the fact that adjoined structures do not introduce novel subcategorization or distribution relations. For example, in (4a) below perfective *have* selects/subcategorizes for a perfective *-en* marked V. This selection requirement is unchanged in (4b) despite the adjuncts.

- (4) a. has/\*is [<sub>VP</sub> eaten a bagel ]  
 b. has/\*is [<sub>VP</sub> [<sub>VP</sub> [<sub>VP</sub> eaten a bagel ] quickly ] in the yard ]

On the standard assumption that only heads can be seen by elements outside an XP and that heads mark the category of a complex phrase, the data in (4) indicate that the complex complement of *has* in (4b) is a VP projection of the perfective head *eaten* (as in (4a)). The same argument can be made in the nominal domain. For example, (5a) shows that *these* demands a plural nominal head and (5b) shows that adding nominal adjuncts does not change this requirement.

- (5) a. these [<sub>NP</sub> students/\*student of physics ]  
 b. these [<sub>NP</sub> [<sub>NP</sub> students/\*student of physics ] from France ]

Nor does adjunction affect the distribution of expressions. Thus, if an XP can occur in some position, an XP modified by any number of adjuncts can, as well. For example, predicative NPs can occur in (6a) and the more complex NPs in (6b) can, too.

- (6) a. John is a [ student of physics ].  
 b. John is a [[ student of physics ] [ from France ]].

The conservation of bar level reflects a different set of facts — two kinds actually. If an XP can be target of a grammatical operation (e.g., movement, ellipsis, or anaphoric dependency), then adjunction does not remove this property. Thus, VP-fronting can apply to the VP *eat the cake* in (7a) and can still apply to it in (7b).<sup>5</sup>

- (7) a. John could [ eat the cake ] and [ eat the cake ] he did.  
 b. John could [[ eat the cake ] [ in the yard ]] and [ eat the cake ] he did [ in the yard ].

Thus, the VP status of *eat the cake* is not disturbed by adjoining *in the yard* to it. In addition, the VP plus adjuncts are also VPs as they too can be fronted.

- (8) a. ... and [[ eat the cake ] [ in the yard ]] he did [ with a fork ].  
 b. ... and [[[ eat the cake ] [ in the yard ]] [ with a fork ]] he did.

<sup>5</sup> See section 4 below for some discussion on head-to-head adjunction.

As shown in (9) and (10) below, similar effects are attested with VP-ellipsis, *do-so* anaphora, and *one*-substitution. These each target the head+complement (obligatory) plus any number of adjuncts (optional).

- (9) John ate a cake in the yard with a fork and
- a. Bill did (so) too.
  - b. \* Bill did (so) an apple in the hall with a spoon.
  - c. Bill did (so) in the hall.
  - d. Bill did (so) with a spoon.
  - e. Bill did (so) in the hall with a spoon.
- (10) this [[[ student of physics ] with long hair ] from France ] and
- a. that one
  - b. \* that one of chemistry (with long hair from France)
  - c. that one from Belgium
  - d. that one with short hair
  - e. that one from Belgium with short hair

The fact that the complement cannot be left out in (9b) and (10b) is attributed to the fact that the head sans complement is not an XP of the right ‘size’. The fact that any number of adjuncts can optionally be targeted follows if head and complement, plus any number of adjuncts, are all of the same size (measured in bar levels).

To recap, the classical approach to adjunction captures several salient properties: It preserves the bar-level information of the target, retains the category information and headedness of the target in the adjoined structure, returns a constituent with a bar level identical to that of the target, and can do this without limit. The labeling convention in (1) succinctly summarizes these facts by having adjunction label the output of the adjunction operation with same label as the target/input.

It is worth noticing that this standard account of adjunction structures is incompatible with BPS views concerning bar levels and so is not in accord with either BPS dicta or the Inclusiveness Condition. To see this, consider the fact that adjunction leaves the maximality of the target XP intact. In BPS, a projection is maximal if it no longer projects. However, the conservation of headedness in adjunction structures implies that the head of the input is also the head of the output. But this is incompatible with BPS if we also insist that the XP that projects still retains its XP status. Thus, from a strict BPS perspective, either head properties are not conserved in adjunction structures or the XP to which the adjunct has adjoined becomes nonmaximal after adjunction. Similar considerations apply to XPs associated with multiple adjunctions. Take (1), repeated below in (11), for instance. Given a BPS understanding of bar levels as relational, only the outmost XP can be maximal; crucially, the ‘intermediate’ adjunction structure cannot be maximal if conservation of headness is preserved in the larger structure.

(11)  $[_{XP} [_{XP} [_{XP} \dots X^0 \dots ] \text{ADJUNCT} ] \text{ADJUNCT} ]$

This would seem to present BPS with empirical problems for we noted above that there is interesting empirical evidence that each of the XPs in (11) can function as targets of the same operations (cf. (7)–(10)). We also found evidence that the selection properties of (11) are identical to those of the simple non-adjoined XP in (12).

(12)  $[_{XP} \dots X^0 \dots ]$

This suggests that the head of (11) is the same as that of (12). There is, thus, a *prima facie* incompatibility between BPS, the classical approach to adjunction in terms of Chomsky-adjunction, and the facts.

The Minimalist Program has a different account of adjuncts. It proposes that adjuncts are labeled differently from complements.<sup>6</sup> As Chomsky (1995: 248) puts it:

Substitution forms  $L = \{H(K), \{\alpha, K\}\}$ , where  $H(K)$  is the head (= the label) of the projected element  $K$ . But *adjunction forms a different object* [our emphasis; NH & JN]. In this case  $L$  is a two-segment category, not a new category. Therefore, there must be an object constructed from  $K$  but with a label distinct from its head  $H(K)$ . One minimal choice is the ordered pair  $\langle H(K), H(K) \rangle$ . We thus take  $L = \{\langle H(K), H(K) \rangle, \{\alpha, K\}\}$ . Note that  $\langle H(K), H(K) \rangle$ , the label of  $L$ , (...) is not *identical* to the head of  $K$ , as before, though it is constructed from it in a trivial way.

Given this notation, an adjunction structure would look like (13):

(13)  $[\langle X, X \rangle [ \langle X, X \rangle [_{X(P)} \dots X^0 \dots ] \text{ADJUNCT} ] \text{ADJUNCT} ]$

The passage above discusses segments versus categories, a distinction that we will return to anon. For now observe that the label of an adjoined structure is *different* from that of the element that it is adjoined to. Thus, the *head* of the adjunction structure is distinct from that of the constituent adjoined to. If one takes this to mean that the head of the target of adjunction has *not* projected, then one of the problems noted above for the classical theory can be addressed.<sup>7</sup> As the labels differ (i.e. the heads did not project), given BPS, the inner  $X(P)$  and the outer  $\langle X, X \rangle$  categories are both maximal, thus being compatible with the movement in (7b), for instance. However, this result is achieved at a price of

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<sup>6</sup> In fact, Chomsky's (2000) distinction between set-merge (for arguments) and pair-merge (for adjuncts) suggests that not only the output of the merger operation may be different depending on whether we are dealing with an argument or an adjunct, but the merger operations themselves may be of a different nature. From a methodological point of view, the best situation would be that there is nothing that distinguishes the operation that merges arguments from the one that merges adjuncts. See section 4 below for further discussion.

<sup>7</sup> Whether the head has projected is actually unclear given Chomsky's observation that the label of the adjunct is constructed from the head of the adjoined-to in a 'trivial' way. Still, given Chomsky's underscoring the fact that the two labels are distinct (not *identical*), it appears that he would not see the label of the adjunction structure as the same as that of the adjoined-to.

redundancy, as VP-movement now resolves into two different operations —  $\langle X, X \rangle$  movement as in (8a) and (8b) and  $X(P)$  movement as in (7b) — at least if operations are distinguished by the objects they apply to.

Moreover, the  $\langle X, X \rangle$  notation still leaves several unresolved questions. For example: What is the status of the inner  $\langle X, X \rangle$  projection in (13) — is it maximal or not? If it is, then how come it determines the label of the outer projection? On the other hand, if it is not maximal, we would expect it to function differently from the outer projection, but so far as we can test this, the two function identically. Thus, given that the outer adjunction projection in (8b), for instance, can move, so can the inner one, as shown in (8a). More generally, if the labels of adjunction structures differ from those of their targets, then how do we account for the fact that their distributional properties are identical? Why are they subject to the same selectional restrictions? Why do they behave alike with respect to grammatical rules like ellipsis, movement, or anaphora? To put this same point more baldly: If the labels of adjunction structures are not *identical* to the labels of the non-adjunction categories that they target, why is it that the properties of the two kinds of constituents are indistinguishable?

The issues reviewed here show that the BPS approach to adjuncts in terms of distinct labels misses the generalizations that the classical theory coded. The trouble seems to be that the labeling that has been proposed relies on bar-level information in a crucial way. But this information should not be available as it is relational and not intrinsic to the lexical elements involved. Put another way, the labeling one finds with adjuncts differs from that found with complements, but it is not clear how this labeling is to be interpreted. In the next sections, we will suggest that the critical difference between complements and adjuncts is that the former *requires* integration into structures with labels while the latter does not. This gives adjunction structures greater grammatical latitude, in some respects. But before discussing adjunction in detail, we need to outline some principles of phrasal composition.

### 3. Same Labeling

Let's assume a simple view of phrase structure in which adjunction is not marked by any special kind of labeling convention. Under this view an adjunction structure will look something like (14) given BPS assumptions.

(14)  $[_X [_X [_X X YP ] WP ] ZP ]$

Given conventional assumptions, the two innermost X-marked constituents in (14) will be understood as X's, while the outer one will be understood as an XP. In addition, it is conventionally assumed that YP can be read as the internal argument of X as it is the immediate projection of X. All these are *relational* notions and they can be defined for structures like (14) if they need to be. One place where this information may be important is at the interfaces, where syntactic configurations are interpreted. A strong version of the Inclusiveness Condition (which we are adopting here) allows such relational notions to *only* be



relevant at the interfaces and not in the syntax proper, where only the intrinsic properties of lexical items are manipulated or noted.

How does the syntax ‘read’ (14)? Let’s assume that the labels are understood conventionally (as in Chomsky 1955) via the “is-a” relation and that being bracketed together means that the bracketed elements have been concatenated. Given this, we read in (14) that X concatenated with YP ( $X^{\wedge}YP$ ) is an X. In other words, concatenation plus labeling delivers back one of the original concatenates. WP and ZP are read in the same way:  $[_X X^{\wedge}YP]^{\wedge}WP$  is an X and  $[_X [_X X^{\wedge}YP]^{\wedge}WP]^{\wedge}ZP$  is an X. In effect, repeated concatenation and labeling produce bigger and bigger X-objects. In each case above, YP, WP, and ZP interact with X (and only with X) via concatenation.<sup>8</sup> If the Conceptual–Intentional interface understands concatenation here in terms of conjunction, then each concatenative step introduces another conjunct. We will return to this point in a minute. For now, let’s consider how (14) fares with respect to the empirical properties noted in section 2.

The fact that adjunction has no effect on selection follows directly as the head of the adjunction structure in (14) is the same as the head of a structure free of adjunctions. What is less clear is how the ellipsis, anaphora and movement operations that seem to target specific projection levels (e.g., VP-ellipsis, VP-fronting, *one*-substitution targeting NPs, etc.) are to be reformulated given a phrase structure like (14). Let’s rehearse the basic facts and see precisely what role bar-level information played before we consider an alternative.

Let’s take examine VP-movement, for concreteness:

- (15) a. It was kick Fred that John did.  
 b. It was kick Fred that John did in the yard.  
 c. It was kick Fred in the yard that John did.  
 d. It was kick Fred in the yard that John did at noon.  
 e. It was kick Fred in the yard at noon that John did.  
 f. \*It was kick that John did Fred.

The paradigm in (15) can be described using bar-level information as follows:  $V_{max}$  (but no  $V^n$ ,  $n$  not max) can be clefted. Adjunction of modifiers is to VP and the output of adjunction is bar-level identical to the input. Thus, if the structure of the affected VPs in (15) is as in (16), then structure preservation constraints (conditions that require  $X_{max}$  in specifier and complement positions) lead us to expect the pattern in (15).

- (16)  $[_{VP} [_{VP} [_{VP} \text{kick Fred} ] \text{in the yard} ] \text{at noon} ]$

In particular, the reason that *kick Fred* plus any number of adjuncts can be fronted is that *kick Fred* in (16) is a  $V_{max}$ , and so is *kick Fred* plus any of the adjuncts. Moreover, the reason why (15f) is unacceptable is that *kick* is not a  $V_{max}$ , and so

<sup>8</sup> Hornstein (forthcoming: chap. 2) suggests that elements can only interact via concatenation and that labeling produces bigger and bigger atomic concatenative units. As atoms have no internal structure accessible to concatenation and the label defines an atom, concatenation is always between atoms. See Hornstein (forthcoming: chap. 2) for further details.

structure preservation blocks its movement to a Spec-position.

The problem with (14), given the paradigm in (15), is that the structure of *kick Fred in the yard at noon* would not be (16), but (17) below. If we assume that bar-level information cannot be used, then it is unclear why the data distribute as seen.

(17) [<sub>v</sub> [<sub>v</sub> [<sub>v</sub> kick Fred ] in the yard ] at noon ]

There are, to be specific, two problems with (17) — one more general than the other. The more general one is how to prevent targeting *kick* for movement, as in (15f). If *kick Fred*, *kick Fred in the yard*, and *kick Fred in the yard at noon* are all Vs and can move, why can't *kick*, which is also a V, move?

The more specific problem with (17) concerns structure preservation. Hornstein (forthcoming: chap. 3) argues that one can derive structure preservation given two assumptions: that morphology can only operate on lexically simple expressions and that movement must obey the A-over-A Condition (A/A).<sup>9</sup> The former assumption is of no moment here, so we put it aside (but see section 4 for discussion). However, the second is very relevant in at least two respects. First, we can use the A/A reasoning to explain why it is that (15f) is unacceptable. Note that the V *kick* moves out of the larger V *kick Fred*. This is an A/A violation and should not be permitted.<sup>10</sup> Second, given this exact same reasoning, the V-movements in (15b) and (15d) both appear to violate the A/A Condition and so should both be barred.

Clearly, these pair of points are related and it would be nice to figure out a way to preserve the positive effects of this and hence derive the unacceptability of (15f), while at the same time figuring out why (15b) and (15d) are fine. This is what we aim to do in the next section.

#### 4. No Labeling

How are phrases composed? There are two operations: concatenation (aka Merge) and labeling. When two elements are concatenated, one of the two marks this blessed event by giving the result its name. In (18) below, X and Y concatenate, and X names the resulting object X. Combining Chomsky (1955) and BPS, we read (18) as saying that X concatenated with Y is (an) X.

(18) [<sub>x</sub> X<sup>^</sup>Y ]

Concatenation is defined over a set of atoms and labeling turns a non-atomic complex concatenate into a (complex) atomic element suitable for concatenation. In other words, what labels *do* is allow concatenation to apply to previously concatenated objects by bringing these complexes into its domain. In

<sup>9</sup> The A/A Condition is itself reduced to minimality in Hornstein (forthcoming).

<sup>10</sup> Hornstein (forthcoming: chap. 2) argues that structure preserving constraints can largely be accommodated if a BPS conception of phrase structure plus a version of minimality defined on paths (thereby deriving the A/A Condition) is adopted.

(18), for instance, concatenation applies to the atomic units  $X$  and  $Y$  ( $X^{\wedge}Y$ ) and labeling yields a new complex atomic unit ( $[_X X^{\wedge}Y]$ ), whose content is no longer available for further concatenation. Assume that this is the correct way of construing Merge (see Hornstein, forthcoming: chap. 3 for further details).

We can now ask whether labeling is always required after concatenation. What happens if we fail to label? In other words, how should we read (19)?

(19)  $[_X X^{\wedge}Y]^{\wedge}Z$

Here the concatenate  $X^{\wedge}Y$  is (an)  $X$  but not so  $[_X X^{\wedge}Y]^{\wedge}Z$ . The two objects contrast in that the former is a concatenate *and an atomic object that can be input to further concatenations*, whereas the latter is a concatenate, but it *is not an atomic object* and so *cannot be input to further concatenation*.  $Z$ , as it were, dangles off the complex  $[_X X^{\wedge}Y]$  without being integrated into a larger  $X$ -like expression. Assume that adjuncts can so dangle, whereas arguments must be integrated into larger structures via labeling.<sup>11</sup> In other words, whereas  $Z$  can be interpreted as an adjunct in (19), it cannot be interpreted as an argument. Under this view, a syntactic object such as *eat the cake in the yard* may have the structure in (20a) below, where *in the yard* is just concatenated with a projection of  $V$ , or the structure in (20b), where the result of the concatenation is also labeled as (“is a”)  $V$ .<sup>12</sup> Furthermore, under the standard assumption that only labeled elements (syntactic constituents) can be targets of syntactic operations,<sup>13</sup> it should be possible to move *eat the cake in the yard* in (20b), but not in (20a).

(20) a.  $[_V \text{eat}^{\wedge}\text{the-cake}]^{\wedge}\text{in-the-yard}$   
 b.  $[_V [_V \text{eat}^{\wedge}\text{the-cake}]^{\wedge}\text{in-the-yard}]$

What does this buy us? Recall that syntactic operations like VP-movement can target a  $V$ +complement plus any number of adjuncts, but not a  $V$  alone, as illustrated in (21) (see (15) above).

(21) a.  $[\text{eat the cake}] \text{he did in the yard}$   
 b.  $[[\text{eat the cake}] \text{in the yard}] \text{he did}$   
 c. \* $\text{eat he did the cake in the yard}$

If adjuncts need not resort to labeling to be licensed, as proposed here, the two possibilities in (21a) and (21b) are due to the two different structures that may underlie *eat the cake in the yard*. That is, (21a) is to be associated with (20a) and (21b) with (20b). Notice (21a) cannot be associated with (20b), for movement of *eat the cake* would violate the A/A Condition as it is part of a larger  $V$ -projection.

<sup>11</sup> That adjuncts are non-labeled constituents has been previously suggested by Uriagereka (1998: chap. 4, in press: chap. 6) and Chametzky (2000). Our proposal can be viewed as a specific implementation of this suggestion.

<sup>12</sup> We abstract away from the internal structure of the complement DP and the adjunct PP. We treat them here as atoms.

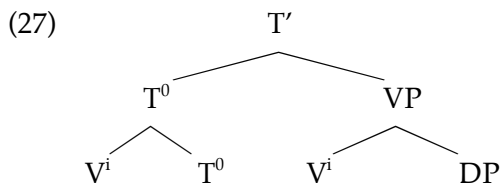
<sup>13</sup> Hornstein (forthcoming: chap. 3) derives this for any syntactic operation that involves concatenate as a sub-operation, e.g., movement.



movement by the A/A Condition.<sup>14</sup> When adjuncts don't move with the elements they modify, it is because they are not members of the *labeled* concatenate that has moved (cf. (24a,b)/(25a,b)). However, arguments can never be other than members of a labeled concatenate, for their interpretive lives depend on it. A side effect of this requirement is that bare heads become ineligible targets and the derivation of sentences such as (26) is ruled out by the A/A Condition.

The reader will have noted that this is not entirely satisfactory. We need an explanation for *why* there is this distinction between arguments and adjuncts, for otherwise haven't we simply recorded the facts? Though we agree that an explanation is needed (and we will provide one in a moment), it behooves us to note that if the above is tenable, then we have already accomplished something. We have attributed the label properties of adjunction constructions to structural ambiguity rather than to a novel labeling convention. What distinguishes adjunction structures is not a new *kind* of label but the absence of one. The V+complement in the non-labeled adjunction structure (cf. (22)) is clearly maximal for nothing with a different label dominates it in the relevant configuration. Where the V+complement plus a number of adjuncts move, the V+complement is not maximal (cf. (24)). When the V+complement+adjunct(s) moves, it is this V+complement+ adjunct(s) that is the maximal V. In other words, there is nothing amiss about labeling the whole moving constituent a projection of V in just the way that V+ complement is a labeled projection of V. Once one allows adjuncts to live within non-labeled concatenates, the standard facts about adjuncts are accommodated without running afoul of BPS conceptions. Clearly, more needs to be said about structures such as (22) or (24).<sup>15</sup> However, this is sufficient detail for the time being.

Let's now have a brief excursion on head adjunction structures. Take V-to-T movement, for concreteness. If we were to translate the standard Chomsky-adjunction structure in (27) below in terms of the proposal advocated here, we should get something along the lines of (28), with T concatenating with a projection of V twice. In one case, this yields a labeled constituent and in the other case, it doesn't.



<sup>14</sup> This reasoning extends to *one*-substitution cases and ellipsis on the assumption that A/A is respected here, as well. The logic is compatible with proposals that consider *one* to be thematically inert (unable to assign a  $\theta$ -role). If so, having *one* as an anaphoric head prevents its complement from integration into the proposition (cf. (10)). The same account presumably can extend to the *do-so*-cases if this is seen as the verbal counterpart of *one* (cf. (9)).

<sup>15</sup> For instance, one must determine the interface conditions that presumably motivate/license labeling in structures such as (24). Also, linearizing adjunction structures such as (22) and (24) appears to require special provisos. See, for instance, Chomsky's (2004) suggestion that adjunction might involve a different plane and Chomsky (1995) for arguments that the linearization of adjuncts is different from the linearization of arguments.

$$(28) \begin{array}{c} [_{T} T \wedge [_{V} V^i D ] ] \\ \wedge V^i \end{array}$$

Structures such as (28) raise several questions. First, why isn't the first merger between T and a projection of V sufficient to establish all the necessary relations between T and V? That is, why must T merge with (a projection of) V twice? Second, movement of the V-head appears to violate the A/A Condition, given that it is dominated by a larger V-projection. Third, when V concatenates with T for the second time, it does not target the root of the tree, thus violating the Extension Condition (Chomsky 1995). Finally, head adjunction structures do not behave like XP-adjunction structures with respect to movement possibilities. Descriptively speaking, XP-adjunction structures allow the adjunct and the target of the adjunction to move independently of one another. By contrast, in head adjunction structures movement of the adjoined element ('excorporation') is taken to be impossible (Baker 1988) or severely restricted (Roberts 1994). Moreover, it seems to be a point of consensus that the head of an adjunction structure cannot be excorporated, leaving the adjoined head stranded.

Let's consider two approaches under which head-to-head movement would be compatible with our proposal. Under the first approach, the problems reviewed above are not real because head movement is actually a PF phenomenon and not part of narrow syntax (see, among others, Boeckx & Stjepanović 1999 and Chomsky 2001: 38). If this approach is correct, the problems above actually provide a rationale for this gap in the computations of narrow syntax. Under the second approach, the problems are real, but tractable. A common assumption within minimalism is that if an expression X assigns a  $\theta$ -role to Y, then it cannot also check a feature, say Case, of Y (see Chomsky 1995, Grohmann 2003). So, for example, a 'transitive' light verb assigns a  $\theta$ -role to its Spec, but checks the Case-feature of the DP that is  $\theta$ -marked by the lower verb. In other words, the assumption is that the one and the same head cannot simultaneously  $\theta$ -mark and morphologically check the same expression. One could extend this division of labor to other morphological relations, as well. So, if T has both morphological and selection requirements to be satisfied by V, T must concatenate with (a projection of) V twice. Furthermore, it is arguable that morphological requirements must involve simplex (word-like) elements and not complex atomic elements (phrases).

That being so, the A/A Condition should accordingly be understood in a relativized manner. In other words, if a complex element such as the labeled projection  $[_{V} V D ]$  cannot satisfy the morphological requirements of T (it is not word-like), it does not induce minimality effects of the A/A type for the movement of the simplex verbal head (see Hornstein, forthcoming: chap. 2). From this perspective, excorporation of the adjoined head or the target of adjunction will cause the derivation to crash because T will not have its requirements satisfied later in the morphological component. So, if T is to undergo head movement later on, it must label the object resulting from its concatenation with the verbal head so that the latter is pied-pied when it moves.<sup>16</sup> And like the previous V-to-T

<sup>16</sup> In this case, the resulting structure would be as in (i).

movement, if  $[_T V^{\wedge}T]$  moves for morphological reasons, the larger complex projections of T will be inert for purposes of the A/A Condition. Finally, cyclicity (the Extension Condition) is not a problem if head movement proceeds via sideward movement (see Bobaljik 1995a, Nunes 1995, 2004, Bobaljik & Brown 1997, and Uriagereka 1998). That is, the verb can be copied from within  $[_V V^{\wedge}D]$  and concatenated with T prior to the merger between T and  $[_V V^{\wedge}D]$ , as illustrated in (29).

- (29) a. *Assembly of  $[_V V^{\wedge}D]$  + selection of T from the numeration:*  
 $[_V V^{\wedge}D] \quad T$   
 b. *Copy of V from  $[_V V^{\wedge}D]$  + Concatenation with T:*  
 $[_V V^i \wedge D] \quad T^{\wedge} V^i$   
 c. *Concatenation of T with  $[_V V^{\wedge}D]$  + labeling (cf. (28)):*  
 $[_T T^{\wedge} [_V V^i D]]$   
 $\quad \quad \quad \wedge V^i$

It is worth noting that none of the potential problems associated with  $X^0$ -adjunction structures arise in virtue of the specifics of our proposal. Rather, they also permeate Chomsky-adjunction representations such as (27) and their BPS cousins. So whatever is the ultimate solution for these problems, it is likely to be oblivious to the general theory of adjunction one adopts. We will leave the choice between the two approaches sketched above for future work.

OK, we have dallied long enough: Why the labeling differences between adjuncts and complements? What *conceptually* motivates the different treatment that we have seen is empirically required? We believe that the proposed difference tracks an independently required semantic contrast between the two, namely, the fact that to be predicated of events, arguments (in contrast to adjuncts) need a thematic pivot. Here's what we mean.

In a neo-Davidsonian semantics the core of the proposition is the event.<sup>17</sup> The V is a predicate of events and everything else modifies it. Thus, the logical form of (30a) is something like (30b).

- (30) a. John ate the cake in the yard.  
 b.  $\exists e$  [eating(e) & subject(John,e) & object(the cake, e) & in-the-yard(e)]

The crucial feature of (30b) for current purposes is that the verb *eat* and the adjunct *in the yard* apply to the event directly, whereas *John* and *the cake* modify the event via two designated relations, here marked *subject* and *object*. Whether it is grammatical functions like subject and object or thematic relations like agent and theme/patient is irrelevant here. What is important is that adjuncts can *directly* modify events, while arguments only do so *indirectly*. They need help in relating to the event and this help is provided by relational notions like subject, object, etc. In an event-based semantics, arguments — not adjuncts — are the

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(i)  $[_T [_T V^i \wedge T] \wedge [_V V^i \wedge D]]$

<sup>17</sup> For details, see Higginbotham (1986), Parsons (1990), Schein (1993), and Pietroski (2004), among others.

interpretive oddballs. They can only modify the event if aided by relational notions.

How does this bear on the requirement that arguments must be inside labeled concatenates while adjuncts need not be? If we assume the traditional definitions of subject, object, etc., then we need labels.<sup>18</sup> For example, objects are traditionally defined as the immediate concatenates of V, e.g., NP-of-V (i.e. [<sub>VP</sub> V NP ]) in the Standard Theory. Given the assumptions that subject and object relations must be marked so as to be of use at the Conceptual–Intentional interface (the place where the syntactic object is interpreted, viz. integrated into a neo-Davidsonian event-based proposition), we must provide the structural where-withal to define it. And if we understand notions like subject and object in classical terms, then labeling is critical for defining these relations. Thus, whereas arguments necessarily require being in a complex labeled structure, adjuncts can be licensed with simple concatenation.

Assuming that this proposal is on the right track, let's consider some of its implications for the computation of adjuncts.

## 5. Some Consequences

The traditional description of adjunction structures is that the adjunct somehow dangles off the target of adjunction. This accounts for the fact that when the target moves as in VP-fronting, for instance, it may pied-pipe the adjuncts or leave them stranded (cf. (23) and (25)). We have reanalyzed this optionality in terms of structural ambiguity. When an adjunct is left stranded, that's because its concatenation with the target was not followed by labeling, as sketched in (31a); on the other hand, if the adjunct is carried along, labeling has taken place, as represented in (31b).

- (31) a. [<sub>V</sub> V<sup>AD</sup> ]<sup>ADJUNCT</sup>  
 b. [<sub>V</sub> [<sub>V</sub> V<sup>AD</sup> ]<sup>ADJUNCT</sup> ]

In this section we will focus on structures such as (31a). Assuming that concatenation without labeling is a grammatical possibility for adjuncts, the structure in (31a) invites two inferences. On the one hand, the adjunct should be invisible to operations involving the labeled structure, as it is 'dangling off' the labeled V. On the other hand, given that it is not dominated by a labeled structure, the Extension Requirement does not prevent it from merging with another element. That is, the adjunct in (31a) may 'dangle onto' a different structure. We discuss each possibility below.

### 5.1. *Dangling Off*

One finds evidence from different domains that indicates that adjuncts may be

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<sup>18</sup> See, for example, Chomsky (1965).



invisible to certain grammatical computations. For instance, as opposed to arguments, adjuncts do not project focus (see Gussenhoven 1984 and Selkirk 1986, among others). A sentence such as (32a) below, for example, with *books* being prosodically prominent, can be a felicitous answer to *What did John buy?* (object focus), *What did John do?* (VP focus), or *What happened?* (sentence focus). Interestingly, addition of an adjunct, as in (32b), does not preclude any of these interpretive possibilities. By contrast, if the adjunct of (32b) is prosodically prominent, as represented in (33), it can only be an appropriate answer for *Where did John buy books?* (narrow focus) or *Did John buy books here?* (contrastive focus).

- (32) a. John bought BOOKS.  
 b. John bought BOOKS in that shop.

- (33) John bought books in that SHOP.

From the perspective explored here, the contrast between arguments and adjuncts with respect to focus projection is a byproduct of the fact that arguments must be fully integrated into their structure (concatenation and labeling are both required), whereas adjuncts are allowed to be dangling out (only concatenation is required). As arguments necessarily become integral parts of larger and larger structures, they allow focus to project to these structures, as exemplified by the simplified representations in (34a) below. In turn, as adjuncts are just concatenated, they are not very communicative with their neighbors and cannot project focus beyond their own projection, as illustrated in (34b).

- (34) a.  $[_T \text{John}^[_T T^[_V \text{bought}^{\text{BOOKS}} ] ] ] ]$   
 -----object focus  
 -----VP focus  
 -----sentence focus
- b.  $[_T \text{John}^[_T T^[_V \text{bought}^{\text{books}} ] ] ] ]$   
 $^{\text{in-that-SHOP}}$   
 -----adjunct focus

The contrast depicted in (34) in fact shows two points. First, it shows that labeling is not optional. If it were, the concatenate in (34b) could be labeled and the distinction between arguments and adjuncts with respect to focus projection would be lost. Second, if labeling concatenate structures involving adjuncts is not optional and must be triggered by some interface conditions (see fn. 15), focus projection is not one of them. If it were, it would license the labeling in (34b) and, again, we would have no principled basis to account for the different behavior of arguments and adjuncts regarding focus.

Thus, the lack of optionality of labeling illustrated by the behavior of adjuncts with respect to focus projection indicates that leaving a structure unlabeled is more economical (in the sense that fewer operations are applied) than labeling it. Say this is on the right track. Doesn't it contradict our proposal in section 4 that the multiple choices for VP-movement rested on structural

ambiguity, depending on whether or not a concatenate involving an adjunct is labeled? Not really. To say that a given surface string involving multiple adjuncts may correspond to different structural configurations depending on whether labeling follows the concatenation of the adjuncts does *not* entail that labeling is optional. All it entails is that whatever triggers/licenses labeling in these cases must have been enforced when adjuncts are pied-piped under VP-movement.<sup>19</sup> Our proposal in fact predicts that all things being equal, adjuncts should be able to project focus once the labeling is properly sanctioned. In other words, an adjunct should be able to project focus if it is pied-piped when the VP is fronted.

With this in mind, consider the contrast between (35) and (36).

- (35) [Context: *What will John do?*]  
 a. He will play SOCCER on Sunday.  
 b. #He will play soccer on SUNDAY.
- (36) [Context: *What will John do?*]  
 a. #Play SOCCER on Sunday what he'll do.  
 b. Play soccer on SUNDAY is what he'll do.

(35) replicates the contrast in (34): the object but not the adjunct allows focus projection. As mentioned above, a question such as *What will John do?* can be used as a diagnostics for VP focus and, therefore, the sentence in (35b) with high pitch on *Sunday* is expected to be infelicitous, as it only licenses narrow or contrastive focus (i.e. it would only be a felicitous answer to *When will John play soccer?* or *Will John play soccer on Saturday?*). Surprisingly, we get the reverse pattern in (36): The adjunct admits VP focus, but the object doesn't. Prosodic prominence on the object in (36a) triggers narrow or contrastive focus readings (i.e. (36a) is a felicitous answer to *What will John play on Sunday?* or *Will John play golf on Sunday?*). The crucial difference between (35) and (36) is that in the latter, VP-fronting under pseudo-clefting carries the adjunct along. According to our proposal, the fact that the adjunct is pied-piped in (36b) signals that labeling after its concatenation was licensed. Once fully integrated into the structure, focus can then propagate from the adjunct to the larger VP of which it became an integral part, as illustrated in (37).

- (37) [<sub>v</sub> [<sub>v</sub> play<sup>^</sup>soccer ]<sup>^</sup>on-SUNDAY ]  
 -----adjunct focus  
 -----VP focus

In turn, (36a) is infelicitous in the context given for the same reason (38a) below is odd in a VP or sentence focus context: There is no need to resort to an extraneous pitch accent if regular sentence intonation is sufficient to convey VP and sentence

<sup>19</sup> Interestingly, Johnson (2001) and Szczegieliński (2004) have argued that VP-movement underlies VP-ellipsis. If so, the several possibilities available for ellipsis involving multiple adjunction should fall together with VP-fronting, as far as the licensing of labeling involving the concatenation of adjuncts is concerned.



- (42) John *ate the cake* in the yard *with a fork* in the afternoon,  
 a. and eat the cake with a fork, he should have in the kitchen in the morning.  
 b. but Bill did (so) in the kitchen in the morning.

However, the fact that adjuncts can be left dangling provides an alternative analysis of data such as (41) and (42), which is compatible with the standard assumption that discontinuous objects cannot be targeted by syntactic operations. Recall that in section 4 we argued that structural ambiguity is what allows VP-movement, ellipsis, and *do-so* anaphora to also target any number of adjuncts without violating the A/A Condition. The idea is that the multiple possibilities for these grammatical operations are actually associated with different syntactic structures, depending on whether or not concatenation of the adjuncts is followed by labeling. The same can be said about the sentences above. That is, (41) is to be associated with the structure in (43), and (42) with the one in (44).

- (43) [<sub>v</sub> [<sub>v</sub> ate<sup>the-cake</sup> ]<sup>in-the-afternoon</sup> ]  
           <sup>in-the-yard</sup>  
           <sup>with-a-fork</sup>

- (44) [<sub>v</sub> [<sub>v</sub> ate<sup>the-cake</sup> ]<sup>with-a-fork</sup> ]  
           <sup>in-the-yard</sup>  
           <sup>in-the-afternoon</sup>

Given the structures in (43) and (44), the object that is targeted by the computational system in (41) and (42) is indeed a labeled concatenate (a syntactic atom) and not a discontinuous element. Rather than requiring some relaxation in the computational system, what sentences such as (41) and (42) actually do is show that the surface order among the adjuncts does not provide any information as to whether or not labeling has occurred. Or to put it in other words, the linearization of adjuncts in the PF component does not seem to be ruled by the same mechanisms that deal with the linearization of arguments (see fn. 15).<sup>21</sup>

There is an additional happy consequence of the approach we are proposing. Regardless of whether ellipsis resolution is to be ultimately accounted for in terms of PF deletion or LF copying, we have seen that ellipsis in (41b) and (42b) arguably disregards adjuncts that were merely concatenated into the structure. This opens a new avenue for the analysis of ellipsis resolution that may lead to infinite regress such as the ones in (45).

<sup>21</sup> Independent evidence for this claim is provided by production data. Rodrigues (2006) examined production errors with respect to subject-verb agreement in Brazilian Portuguese and found that for a target subject such as  $N_{SG} [_{PP} P NP_{PL}]$ , there were significantly more agreement errors (with a plural verb) when the PP was an argument than when the PP was an adjunct (average of errors for argument PPs = 1.7; average of errors for adjunct PPs = 0.74; maximal score = 6.0). These results suggest that as opposed to what happens to PP arguments, the surface position of PP adjuncts is determined after subject-verb agreement is computed.

- (45) a. John greeted everyone that I did.  
 b. John worded the letter as quickly as Bill as did.  
 c. John kissed someone without knowing who.

(45a) is a classical example of antecedent contained deletion (ACD) construction of the sort first extensively discussed in May (1985). (45b) is an ACD construction in which the major constituent containing the elided material is an adjunct (see Hornstein 1995). Finally, (45c) involves sluicing contained within an adjunct (see Yoshida 2005). In all of them, a simple-minded ellipsis resolution copying the matrix VP in (45a) and (45b) or the IP in (45c) into the ellipsis site will recreate a structure with elided material in need of resolution. This is not the place to discuss the intricate properties associated with each of these constructions. We would just like to point out that they appear to be amenable to the same analysis we suggested for (41b) and (42b).

More concretely, the infinite regress problem arises just in case the adjuncts in (45) are analyzed as forming a syntactic constituent with the target of the adjunction. Suppose that along the lines we have been exploring here, the simplified structures underlying the sentences in (45) are as in (46).

- (46) a.  $[_T \text{John}^[_T T^[_V \text{greeted}^{\text{everyone}} ]]]$   
 $\quad \quad \quad \text{^that-I-did}$
- b.  $[_T \text{John}^[_T T^[_V \text{worded}^{\text{the-letter}} ]]]$   
 $\quad \quad \quad \text{^as-quickly-as-Bill-did}$
- c.  $[_T \text{John}^[_T T^[_V \text{kissed}^{\text{someone}} ]]]$   
 $\quad \quad \quad \text{^without-knowing-who}$

In each structure of (46), there is a constituent that can provide the relevant template for ellipsis resolution without forcing infinite regress; namely, the V-labeled concatenate in (46a) and (46b), and the outer T-labeled concatenate in (46c). The crucial aspect in the structures in (46) is that the adjunct containing the ellipsis site is just concatenated with its target and therefore is not a proper part of the structure it modifies. As it dangles off the constituent with which it was concatenated, it is invisible for purposes of ellipsis resolution and this doesn't lead to the infinite regress trap.<sup>22</sup>

<sup>22</sup> Sentences such as the ones in (i), pointed out to us by Alex Grosu (p.c.), seem to be problematic in that the concatenation of the adjunct appears to be followed by labeling, given that the adjunct appears to have been pied-piped. However, if concatenation with the relative clause is followed by labeling, this gives rise to the infinite regress problem as the matrix VP includes the elided VP.

- (i) a. Everyone that I expressly didn't, Bill effusively invite.  
 b. How many people that you refused to did Bill nonetheless invite?

There is an alternative derivation for the sentences in (i), though. The derivation of (ia), for instance, can proceed along the lines of (ii)–(v) below. Given the two clauses in (ii), *everyone* is copied from K, as shown in (iii), and concatenates with L (an instance of sideward movement in the sense of Nunes 2001, 2004), as shown in (iv). Once no labeling

We would like to stress that it was not our intent to provide a detailed analysis of the several types of phenomena reviewed in this section. Our purpose was just to highlight empirical domains that may find a more streamlined explanation if our proposal that adjuncts may be just concatenated with their target is on the right track.<sup>23</sup>

## 5.2. *Dangling On*

There is one more aspect of adjunction structures that we haven't mentioned here. Grammarians distinguish between domination and containment (see May 1985). According to this distinction, XP in (47a) below is in the domain of  $Y^0$  but not in the domain of  $Z^0$  as it is dominated by all maxY projections. In contrast, XP in (47b) is in the domain of both  $Y^0$  and  $Z^0$  because it is not dominated by all maxY projections; that is, it is dominated by ZP but only contained by YP.

- (47) a.  $[_{ZP} \dots Z^0 [_{YP} XP [_{Y^0} \dots Y^0 \dots ]]]$   
 b.  $[_{ZP} \dots Z^0 [_{YP} XP [_{YP} \dots Y^0 \dots ]]]$

The distinction between domination and containment has been empirically useful in allowing expressions to be members of more than one domain. One interesting case that illustrates this possibility is provided by Kato & Nunes's (1998, 2007) analysis of matching effects in free relatives. In Portuguese, for example, free relatives allow a kind of preposition sharing between different verbs. The data in (48) below show that the verbs *discordar* 'disagree' and *rir* 'laugh' in Portuguese select for the preposition *de* 'of', whereas the verbs *concordar* 'agree' and *conversar* 'talk' select for the preposition *com* 'with'. When one of

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takes place in (iv), *everyone* can concatenate with the Top projection, as shown in (v), which surfaces as (ia) after deletion of the lower copy of *everyone* in K. Notice that the structure in (v) does not give rise to the infinite regress problem, as the VP of the matrix clause does not contain the relative clause. See fn. 24 below and Nunes (2001, 2004) for similar derivations.

- (ii) K =  $[_{Top} Top^{\wedge} [_{T} Bill\text{-}effusively\text{-}invite\text{-}everyone ]]$   
 L =  $[_C \text{that-I-expressedly-didn't}]$
- (iii) K =  $[_{Top} Top^{\wedge} [_{T} Bill\text{-}effusively\text{-}invite\text{-}everyone^i ]]$   
 L =  $[_C \text{that-I-expressedly-didn't}]$   
 M =  $everyone^i$
- (iv) K =  $[_{Top} Top^{\wedge} [_{T} Bill\text{-}effusively\text{-}invite\text{-}everyone^i ]]$   
 N =  $everyone^i \wedge [_C \text{that-I-expressedly-didn't}]$
- (v)  $[_{Top} everyone^i \wedge [_{Top} Top^{\wedge} [_{T} Bill\text{-}effusively\text{-}invite\text{-}everyone^i ]]]$   
 $\wedge [_C \text{that-I-expressedly-didn't}]$

<sup>23</sup> If movement is to be computed in terms of paths (see Hornstein, forthcoming: chap. 2) and if paths are defined in terms of traversed constituents (labeled concatenates in our terms), lack of labeling should block movement as paths can't be computed. In other words, lack of labeling may provide an account for why one can't move out of adjuncts. As for pied-piped adjuncts (which under our proposal must have triggered labeling), whatever accounts for why moved arguments become (CED) islands should in principle account for their island behavior, as well.

these verbs takes a free relative clause as a complement, the selectional properties of the matrix and the embedded verb must match, as shown in (49). Intuitively speaking, (49c), for instance, is ruled out because the matrix verb selects for *com*, while the embedded verb selects for *de*:

- (48) a. Eu discordei/ri **de**le / \***com** ele.  
*I disagreed/laughed of.him / with him*  
 'I disagreed with him.' / 'I laughed at him.'
- b. Eu concordei / conversei **com** ele / \***de**le  
*I agreed / talked with him / of.him*  
 'I agreed with him.' / 'I talked to him.'
- (49) a. Ele só conversa **com** quem ele concorda.  
*he only talks with who he agrees*  
 'He always talks to whoever he agrees with.'
- b. Ele sempre ri **de** quem ele discorda.  
*he always laughs of who he disagrees*  
 'He always laughs at whoever he disagrees with.'
- c. Ele sempre concorda \***com** quem / \***de** quem ele ri.  
*he always agrees with who / of who he laughs*  
 'He always agrees with whoever he laughs at.'

Assuming the traditional distinction between domination and containment, Kato & Nunes propose that the derivation of a sentence such as (49a), for instance, proceeds as follows. The computational system assembles the 'relative' CP and the verb *conversa* is selected from the numeration, as shown in (50) below. K and L in (50) cannot merge at this point because *conversa* does not select for a CP. The strong *wh*-feature of C then triggers the copying of the PP *com quem*, as shown in (51). Next, the computational system adjoins M to K, allowing the strong *wh*-feature to be checked, and merges the resulting structure with L, as shown in (52). Crucially, merger of the matrix verb and CP in (52) now satisfies Last Resort because the moved PP also falls within domain of *conversa* and they can establish the relevant syntactic relation ( $\theta$ -assignment).

- (50) a. K = [<sub>CP</sub> C [ ele concorda [<sub>PP</sub> com quem ] ] ]  
 b. L = *conversa*
- (51) a. K = [<sub>CP</sub> C [ ele concorda [<sub>PP</sub> com quem ]<sup>i</sup> ] ]  
 b. L = *conversa*  
 c. M = [<sub>PP</sub> com quem ]<sup>i</sup>
- (52) [<sub>VP</sub> *conversa* [<sub>CP</sub> [<sub>PP</sub> com quem ]<sup>i</sup> ] [<sub>CP</sub> C [ ele concorda [<sub>PP</sub> com quem ]<sup>i</sup> ] ] ] ]  
*talks with who he agrees with who*

In sum, the utility of distinguishing containment from domination is that elements contained within a projection are still visible beyond that projection,

while those dominated by a projection are not. However, this distinction crucially hangs on allowing XP in a structure like (47a) to be distinguished from XP in a structure like (47b) and this brings back all the questions we discussed in section 2. Note, for instance, that the assumption that the lower YP in (47b) determines the label of the outer projection but retains its status as a maximal projection is at odds with the notion of projection in BPS. In addition, it violates the Inclusiveness Condition in that bar-level information is tacitly being used as a primitive by the computational system. Moreover, notice that if these problems were to be fixed in consonance with BPS and the Inclusiveness Condition, (47b) should be reanalyzed along the lines of (53) below, where bar levels are not intrinsically distinguished. The problem now is that we lose the distinction between adjuncts and specifiers that was used to account for the matching effects in (49), for (53) would be the BPS rendition of both (47a) and (47b).

(53)  $[_Z \dots Z [_Y X [_Y \dots Y \dots ]]]$

The question before us is whether the useful distinction between domination and containment can be captured without friction with BPS or the Inclusiveness Condition in a theory that does not have specific labels for adjuncts such as the one we are advocating here. Recall that we suggested that adjuncts can concatenate with concatenative atoms, but the output need not project a label. Given this, we can represent the difference between domination and containment as the difference between (54a) and (54b).

(54) a.  $[_X Z^{\wedge}[_X \dots X \dots ]]$   
 b.  $Z^{\wedge}[_X \dots X \dots ]$

In (54a), Z has concatenated with the inner X-projection and the result has been labeled X again. (54b) exhibits a similar concatenation but the output is left *unlabeled*. If we assume that it is labeling that prevents all but a head to be ‘seen’ from outside the concatenate, then in (54b), Z can still be input to further concatenation.

To put it somewhat differently: Recall that in section 5.1 we discussed cases where adjuncts are disregarded by some operations because like Z in (54b), they are not part of a labeled constituent. Once an adjunct may be left dangling as in (54b), the converse situation may arise, as well. That is, the adjunct in (54b) may be targeted by some operation exactly because it is not a subpart of a bigger syntactic object. In particular, it is free to undergo merger in consonance with the Extension Requirement, as it is still a syntactic atom for purposes of concatenation.

Consider how our reworked version of the distinction between domination and containment operates in the case of the Portuguese free relatives described above. The derivation of the matching free relative in (49a), for instance, can be derived along the lines of (55).

(55) a.  $\text{com-quem}^{\wedge}[_C C^{\wedge}[_T \dots ]]$



- b. 
$$\begin{array}{c} \text{com-quem}^{\wedge}[_C C^{\wedge}[_T \dots ] ] \\ [_V \text{conversa}^{\wedge} \quad ] \end{array}$$

In (55a) *com quem*, which was copied from within CP, concatenates with CP and no labeling takes place. Once *com quem* is still an atomic element for purposes of concatenation, it can merge with the verb *conversa*. However, in order for *com quem* to be interpreted as an argument, such concatenation must be followed by labeling, as shown in (55b). *Com quem* in (55b) counts as two beads on a string, so to speak: it is an integral part of the V-labeled expression and a ‘mere’ concatenate to the C-labeled expression. If one assumes that Merge is just an instance of concatenate, then there is no reason why *some* parts of the phrase marker may not be string-like. Our suggestion is that this more adequately describes what happens for contained expressions. They are parts of mere concatenates, not labeled ones.<sup>24</sup>

<sup>24</sup> At first sight, our analysis fails to account for the acceptability of Portuguese sentences such as (i), for instance, where the free relative appears to have moved from the matrix object position. According to the derivation discussed above, such movement should not be possible, given that the PP and the relative CP do not form a constituent (cf. (55b)).

- (i) *Com quem ele conversa ele concorda.*  
*with who he talks he agrees*  
 ‘Whoever he talks to, he agrees with.’

However, upon close inspection there is a convergent source for (i), along the lines of (ii)–(vii) below (with English words and details omitted for purposes of exposition). That is, after K and L are assembled in (ii), the computational system copies *with who* and merges it with *talks* (an instance of sideward movement) to satisfy the  $\theta$ -requirements of the latter (see Nunes 2001, 2004), yielding (iii). After the stage in (iv) is reached, another copy of *with who* is created, triggered by the strong feature of the head of the relative CP, as shown in (v). After the relative CP adjoins to the copy just created, as shown in (vi), *with who* is still an accessible atom for purposes of structure building (no labeling took place after concatenation in (vi)). *With who* may then merge with the Top-labeled constituent, yielding another Top-projection, as shown in (vii), which surface as (i) after deletion of the lower copies of *with who* and further computations. See Nunes (2001, 2004) for discussion of similar derivations.

- (ii) 
$$\begin{array}{l} K = [_{Top} Top^{\wedge}[_T \text{he-agrees-}[_P \text{with-who} ]]] \\ L = \text{talks} \end{array}$$
- (iii) 
$$\begin{array}{l} K = [_{Top} Top^{\wedge}[_T \text{he-agrees-}[_P \text{with-who} ]^i]] \\ M = [_V \text{talks}^{\wedge}[_P \text{with-who} ]^i] \end{array}$$
- (iv) 
$$\begin{array}{l} K = [_{Top} Top^{\wedge}[_T \text{he-agrees-}[_P \text{with-who} ]^i]] \\ N = [_C \text{he-talks-}[_P \text{with-who} ]^i] \end{array}$$
- (v) 
$$\begin{array}{l} K = [_{Top} Top^{\wedge}[_T \text{he-agrees-}[_P \text{with-who} ]^i]] \\ N = [_C \text{he-talks-}[_P \text{with-who} ]^i] \\ O = [_P \text{with-who} ]^i \end{array}$$
- (vi) 
$$\begin{array}{l} K = [_{Top} Top^{\wedge}[_T \text{he-agrees-}[_P \text{with-who} ]^i]] \\ P = [_P \text{with-who} ]^i \wedge [_C \text{he-talks-}[_P \text{with-who} ]^i] \end{array}$$
- (vii) 
$$Q = [_{Top} [_P \text{with-who} ]^i \wedge [_{Top} Top^{\wedge}[_T \text{he-agrees-}[_P \text{with-who} ]^i]] \wedge [_C \text{he-talks-}[_P \text{with-who} ]^i]$$

Let's examine another potential example of an expression dangling onto a structure different from the one it concatenates with. Consider the contrast in (56) in English.

- (56) a. There is likely to be someone in the room.  
 b. \*There is likely someone to be in the room.

The contrast in (56) is the textbook example presented by Chomsky (1995) as evidence for the preference of Merge over Move. The reasoning is as follows. After the syntactic object in (57) below is built, the EPP feature of *to* may be checked by either merger of *there* or by movement of *someone*. Assuming that both options lead to a convergent result, they are eligible for economy comparison, for they share that same numeration and the same computations up to (57). The fact that (56a) trumps (56b) is then interpreted as showing that all things being equal, Merge in (57) is to be preferred over Move.

- (57) [ to be someone in the room ]

Under this analysis, the contrast in (58) below is completely unexpected, as it pulls in the opposite direction of (56). The problem with (58) is that if the movement of *books* to a position preceding the passive verb is to check an EPP feature, the computational system should then merge *there*, applying the preference of Merge over Move. This predicts that (58a) should preclude (58b), but we find the opposite.

- (58) a. \*There were likely to be put books on the table.  
 b. There were likely to be books put on the table.

Chomsky (2001) proposes that the derivations in (58) are subject to the same economy comparison as the ones in (56) and that the derivation that should result in (58a) is indeed the winner. The fact that it cannot surface as such is attributed to an "idiosyncratic rule of English" (p. 24) referred to as *thematicization/extraction (Th/Ex)*, which is an operation of the phonological component that moves the complement of a passive or unaccusative verb to its edge. Th/Ex is taken to be a phonological operation due to its "semantic neutrality" (p. 26). In particular, it is different from object shift in that the moved object is not associated with specificity. In fact, the moved argument of constructions such as (59) exhibits definiteness effects and therefore patterns like the *in situ* argument of (60a) rather than the moved argument of (60b).

- (59) There were likely to be some / \*the books put on the table.

- (60) a. There were likely to be some / \*the books on the table.  
 b. Some / the books were likely to be on the table.

Our proposal that concatenation is not always followed by labeling seems to provide a more elegant analysis to this set of facts. Let's see how it goes.

Following Lasnik (1992), assume that in English, *be* can assign (inherent) partitive Case (in the sense of Belletti 1988), but passive verbs can't. Being inherent, partitive Case is intrinsically linked to  $\theta$ -role assignment (see Chomsky 1986). So, *be* should not be able to assign partitive to the Spec of a predicative PP in a structure such as (61), for instance, as there is no such case as 'exceptional  $\theta$ -marking' (see Chomsky 1986, Belletti 1988).

(61) [ *be* [<sub>PP</sub> *books* [<sub>P'</sub> on [ *the table* ]]]]

The question then is how *be* can assign inherent Case to *books* in a simple sentence such as (62) below if *books* sits in the Spec of PP, as in (61). Extending Kato & Nunes's (1998) proposal, Avelar (2004) proposes that existential constructions actually involve adjunction small clauses and that in a configuration such as (63), *be* can assign inherent Case to *books* because they are in mutual c-command relation as *books* is contained, but not dominated by PP.

(62) There are books on the table.

(63) [ *are* [<sub>PP</sub> *books* [<sub>PP</sub> on [ *the table* ]]]]

In the terms of the system we are arguing for here, Avelar's proposal amounts to saying that *books* is only concatenated with the P-labeled expression, as represented in (64a), which in turn allows it to merge with and be assigned partitive by *be*, as shown in (64b).

(64) a. *books*<sup>[\_P on^the-table ]</sup>  
 b. *books*<sup>[\_P on^the-table ]</sup>  
 [<sub>V are</sub><sup>^</sup> ]

Let's get back to the contrasts in (56) and (58). If *be* assigns partitive Case, the two derivations in (56) do not actually compete.<sup>25</sup> After *someone* is Case-marked by *be* in (57), it becomes inactive for purposes of A-movement; hence, the only convergent continuation of (57) is to insert *there* and then move it later to check the EPP and the Case-feature of the matrix T. What about the sentences in (58)? Take the derivational step represented in (65a) below, after the participial clause is built. Assuming that Part has an EPP feature, the system can either move *books* or merge *there*. Notice however that if *there* is merged, it should induce minimality effects, preventing *books* from getting Case later on, when potential Case checkers are introduced in the derivation; hence the unacceptability of (58a). If merger of *there* does not lead to a convergent derivation, *books* is then allowed to move to check the EPP feature of the participial head. Crucially, *books* is active for purposes of A-movement as passive verbs in English do not assign partitive. *Books* is then copied and concatenates

<sup>25</sup> This does not entail that there is no Merge-over-Move preference. All we're saying is that it is not obvious that the contrasts in (56) and (58) are examples of the effects of this preference.

with the complex expression labeled Part in (65a), yielding (65b).

- (65) a.  $[_{\text{Part}} \text{Part}^{\wedge}[_{\text{V}} \text{put-books-on-the-table} ]]$   
 b.  $\text{books}^{\wedge}[_{\text{Part}} \text{Part}^{\wedge}[_{\text{V}} \text{put-books-on-the-table} ]]$

Once the concatenation in (65b) is not followed by labeling, *books* is still accessible for merger. It can then merge with and be Case-marked by *be*, as shown in (66), and *there* is inserted later in the matrix clause, yielding the sentence in (58b) after further computations.

- (66)  $\text{books}^{\wedge}[_{\text{Part}} \text{Part}^{\wedge}[_{\text{V}} \text{put-books-on-the-table} ]]$   
 $[_{\text{V}} \text{be}^{\wedge} \quad ]$

Needless to say that here we just touched on the tip of the iceberg that hides under existential constructions and much more needs to be said. But it is worth noting that our reanalysis of the notions of dominance and containment in terms of labeling provides a straightforward account for the fact that moved object in (58b)/(59) behaves like *in situ* objects of *be* in exhibiting definiteness effects. Its semantic neutrality, to use Chomsky's words, follows from the fact that like in simple existential constructions such as (62), it can merge with *be* in consonance the Extension Condition and be assigned partitive Case.

## 6. Concluding Remarks

Adjuncts are funny characters from a syntactic point of view, because they appear to be simultaneously inside and outside a given syntactic tree. Their double personality has led to the standard view in the literature according to which structures involving adjuncts are less trivial than the ones involving arguments. We have argued in this contribution that contrary to the traditional wisdom, exactly the opposite is true. Arguments — in order to be interpreted as such at the Conceptual–Intentional interface — require association with relational notions such as subject and object and the grammatical establishment of these relational notions is achieved through labeling. Hence, arguments must be parts of complex (labeled) structures. Adjuncts, on the other hand, may modify the event directly via concatenation and therefore need not invoke labeled structures to be properly interpreted. From this perspective, the addition of adjuncts into a given structure is achieved via the simplest possible operation, simple concatenation.

Our proposal for the distinction between arguments and adjuncts is conceptually couched on their distinctive role at the Conceptual–Intentional interface. But crucially, it accords well with both BPS (as we don't make use of bar-level information) and with the Inclusiveness Condition (as we don't introduce extraneous devices to code their difference). Rather, we rely on the unavoidable property that underlies the operation that builds complex syntactic objects (phrases) out of lexical atoms, namely, the concatenation procedure whose output is interpreted at the Conceptual–Intentional interface as

conjunction. Our proposal has been that examining adjunction structures through interface lenses both leads to a conceptually more appealing approach to adjunction structures, and opens new avenues for analyzing recalcitrant data.

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# Bare Phrase Structure and Specifier-less Syntax

K.A. Jayaseelan

It is pointed out that “specifiers” render the algorithm of projection overly complex. This consideration lends support to Starke’s (2004) reanalysis of specifiers as phrasal heads that project their own phrases — which makes phrase structure a simple sequence of head-complement relations. It is further pointed out that if head-complement relations are represented using dominance in place of sisterhood, to reflect the essentially asymmetrical nature of Merge (Chomsky 2000), a non-branching (partially linear) phrase structure tree is obtained that very naturally eliminates labels and projections. A simple Spell-Out rule then provides a linear ordering of the terminal elements. The linear tree preserves all the major results of antisymmetry.

*Keywords:* antisymmetry, label, linearization, specifier

## 1. Introduction

In this article, I suggest a notational innovation in the representation of phrase structure trees (henceforth, PS trees), taking as its background the assumptions of *bare phrase structure* (Chomsky 1995) and *specifier-less syntax* (Starke 2004). This innovation makes PS trees radically simple, and linear.

## 2. Traditional X'-Syntax and the Notion of Specifier

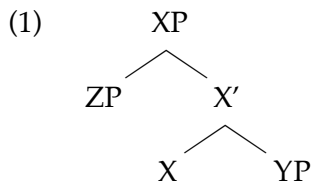
Phrase structure is represented by the following schema in X' syntax:

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For helpful comments at various stages of the growth of the ideas presented here, I wish to thank R. Amritavalli, Tanmoy Bhattacharya, Probal Dasgupta, M.T. Hanybabu, Ayesha Kidwai, P. Madhavan, Rosmin Mathew, Daniel Seely, Gautam Sengupta, and Michal Starke. I am also grateful to an anonymous reviewer of this journal for comments that helped me give this paper its final shape.

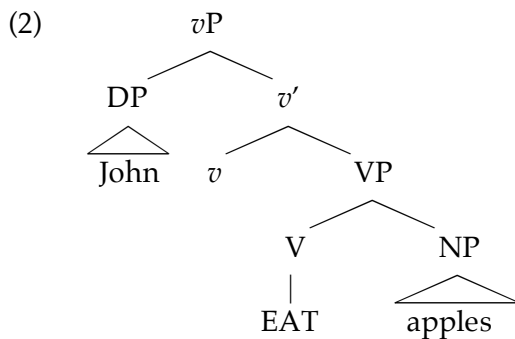




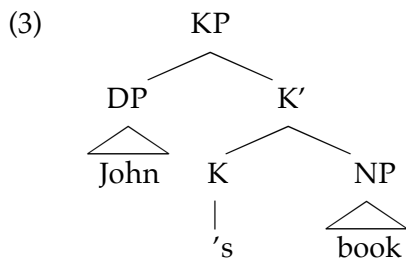


This embodies the claim that a head can be merged with two phrases, the first merge giving the head a complement (YP) and the second merge a specifier (ZP).

It is, as a matter of fact, not so straightforward to find a case where all three terms — head, complement, specifier — are lexically filled. *Prima facie*, a likely example of this might appear to be a verb phrase consisting of a transitive verb and two arguments, such as *John eats apples*. But this linguistic expression is now commonly represented as:



Here the lower verb (lexical V) has only a complement; and the higher verb (light verb *v*), which has a complement and a specifier, is (itself) an abstract element.<sup>1</sup> Outside lexical VP, auxiliary verbs have no specifiers; and if adverbial modifiers are in specifier positions of AdvPs (Cinque 1999), the AdvPs have abstract heads. PPs famously have no specifiers. TP and CP, commonly thought of as structural configurations with all three terms, may in fact not be such, as I presently show. If we leave aside TP and CP, the only examples one can readily think of which have all three terms are, in fact, phrases headed by inflectional elements; for instance, *John's book*, which can be argued to have the following structure:<sup>2</sup>



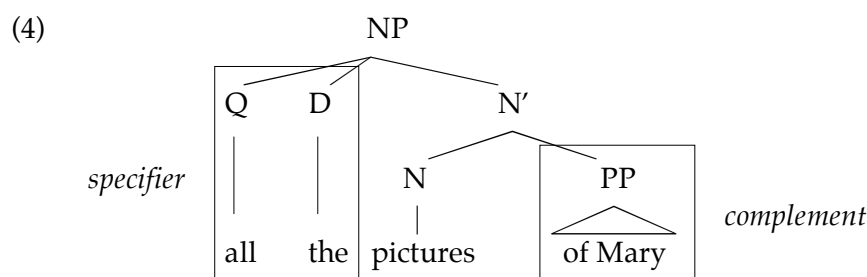
<sup>1</sup> A double object VP may or may not provide an example of all three terms lexically filled, depending on the analysis assumed. Thus, in *give Mary a book*, if [*Mary a book*] is analyzed as a small clause, it must have an abstract head (given antisymmetry). However, there are other analyses in which the structure proposed has (at least initially) all three terms lexically filled (see Larson 1988, among others).

<sup>2</sup> See Abney (1987) who, however, analyzes 's as a D<sup>0</sup>, and the whole structure as a DP.

Possibly motivated by this paucity of examples of phrases with all three terms lexically/phonetically filled, Koopman (1996) proposed a condition that in a phrase, the specifier and the head cannot both be lexically filled at Spell-Out, and tried to derive this result from a modified version of antisymmetry.

Koopman's concern is addressed in a different way by specifier-less syntax, which we come to directly.

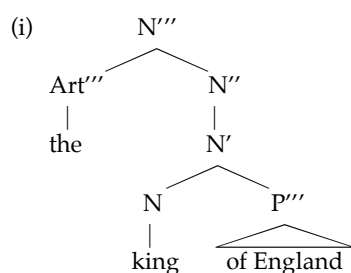
It may be useful to recall that "specifier", when Chomsky (1970) first introduced the notion into linguistic theory, was only a "residual category" consisting of all the phrase-internal elements to the left of the head. ("Complements", which were the categories that a head strictly subcategorized for, conveniently came — in English — to the right of the head.) It typically consisted of single-word elements; and when there was more than one of these elements, they could only be treated as "a concatenation of nodes" (Jackendoff 1977: 40).<sup>3</sup> For example, a phrase like *all the pictures of Mary* — if we took *pictures* to be the head of the phrase — could reasonably be represented only as (4).<sup>4</sup>

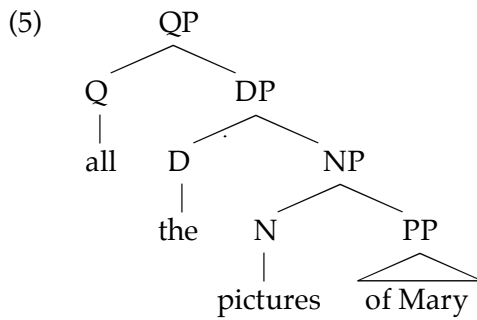


Arguably, it was Abney's (1987) "DP hypothesis" that changed this picture. Each of the single-word elements which were earlier grouped under the rubric of "specifier" now projected its own phrase, and took the phrase projected by the next element as its complement. For example, (4) became (5):

<sup>3</sup> As Jackendoff (1977: 14) points out, it is unclear if Chomsky considered the various elements in the specifier to be a constituent; although in his diagrams Chomsky does show them under a single node labeled "specifier", see Chomsky (1970: 211).

<sup>4</sup> Jackendoff himself, however — in obedience to his proposal of a "three-tier" X'-schema for every category — treated these single-word elements as phrases; cf. (i), which is adapted from one of his diagrams (Jackendoff 1977: 59):





The idea that specifier is phrasal had perhaps already been gaining ground prior to this development; cf. Stowell's (1983) "subjects across categories" and Chomsky's (1986) extension of the  $X'$ -schema to clausal categories — but the treatment of single-word elements like the definite article still remained a problem, until Abney's work enabled us to treat them as heads. The term "specifier" was now reserved for a phrase which occurred to the left of a projecting  $X^0$ -element; since it seemed inconceivable that a phrase could project, it was now analyzed as the specifier of the following  $X^0$ -element. If there was more than one such phrase, one had to say that they were multiple specifiers, or postulate an abstract  $X^0$ -element ("head") intervening between the phrases.

But in this schema, what interaction was postulated between a specifier and the head? And perhaps more relevantly, what interaction is postulated now? "Specifier" has a very ambiguous status in this regard at the current stage of the theory. Chomsky (2004: 111-112) claims that "a Head-to-Spec relation [...] cannot exist (nor the broader symmetric Spec-Head relation, in the general case)."<sup>5</sup> The only relation that is countenanced is a Spec-to-Head probe: Chomsky (2000: 124-125) suggests that an expletive merged in [Spec,TP] checks an uninterpretable feature of  $T^0$  by a probe. Bošković (2007) exploits the same device in his analysis of movement. But a probe only needs c-command and locality; it does not require the "special" relation of a specifier to its head. More specifically, a probe need not be contained in a projection of the goal. So in effect, in the current theory, there is no interaction between a specifier and the head as specifier and head.

Consider the claim that the subject in an English-type language is in [Spec,TP]. But the only relation that the subject has to  $T^0$  is that of fulfilling an EPP requirement of  $T^0$ . EPP is only a diacritic which says that a certain head "needs a specifier". Rizzi (2005) has suggested that we can make sense of this requirement in the case of TP if we say that a SubjP (i.e. a subject position) is obligatory in the functional sequence that constitutes IP.<sup>6</sup> We note that Rizzi's SubjP is a separate projection above TP, and that it has an abstract head. Given such an analysis, we can no longer cite TP as an example of a lexically instantiated specifier-head-complement sequence.

<sup>5</sup> This makes  $v^0$  "assigning" a theta role in its Spec position problematic; see Chomsky (*ibid.*) for discussion. Den Dikken (2006: 22-23) points out that  $v^0$ , as it is currently conceived, is a "hybrid element" which is partly functional (in virtue of its parametrically variable morphological features) and partly lexical (because it assigns a  $\theta$ -role); and he suggests that it should be treated as a purely functional category that does not assign any  $\theta$ -roles.

<sup>6</sup> See also Cardinaletti (2004) for the notion of a SubjP.

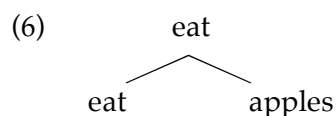
Consider another commonly cited example of a Spec–Head configuration, namely a *wh*-phrase in [Spec,CP]. The  $C^0$  here is in itself lexically null; but in English root questions, a tensed auxiliary verb is assumed to move into the  $C^0$ -position, either adjoining to it or substituting into it. Here then, one could say, is a clean example of an  $X'$ -configuration with all three terms — specifier, head, and complement — lexically filled. But unfortunately for this analysis, it has since been shown that there is no single  $C^0$ -head, but several functional projections, in the C-domain (Reinhart 1981, Bayer 1984, Rizzi 1997), and that the English *wh*-phrase (when it moves) moves into a Focus Phrase in that domain (Rizzi 1997). Now it is not certain that the auxiliary verb moves into the head position of this FocP. Depending on how high (in the C-domain) FocP is generated, and how many functional heads can be generated below it, the auxiliary verb has other possible adjunction sites, such as the head of Finiteness Phrase. (Incidentally, this FinP appears to never have a lexically-filled Spec; and FocP — unless we analyze the inverting auxiliary verb as moving into its head position — never has a lexically filled head.)

A third, at first glance strong, argument for a Spec–Head configuration, it might seem, is provided by phrases headed by inflectional elements; e.g., a Case Phrase (KP) headed by a Case morpheme that “requires” a nominal expression to its immediate left. We have already drawn attention to this type of evidence, in (3). Currently this is handled by moving a DP/NP into [Spec,KP]. But the dependency between the nominal expression and the Case morpheme can be expressed by a selectional relation between independent phrases, as Starke (2004) has shown.<sup>7</sup>

In section 5, I will show that the notion of “specifier” introduces a possibly unacceptable degree of complexity into any set-theoretical characterization of the operation Merge, making the notion costly and unintuitive.

### 3. Bare Phrase Structure (BPS)

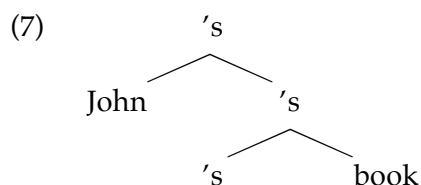
Improving on the traditional way of representing phrase structure, Chomsky (1995) proposed that category labels can be eliminated from syntactic representations. In his theory of “bare phrase structure” (henceforth, BPS), the head of a phrase is used as the label of its projections. Thus the VP *eat apples* of (2) will now be represented as:



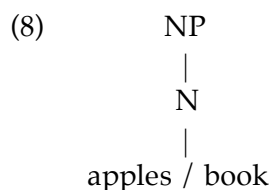
A phrase with a lexically filled specifier will be represented as shown in (7):

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<sup>7</sup> Starke has a notion of “dependent insertion” to cover these cases, and also such cases as the dependency between *wh*-movement and auxiliary inversion in English — more generally, the verb–second phenomenon of Germanic; see Starke (2004) for details.



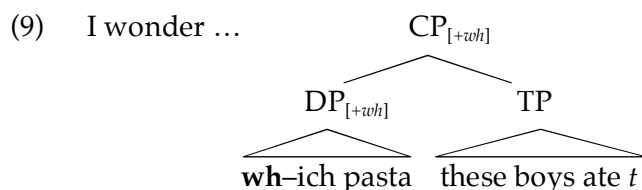
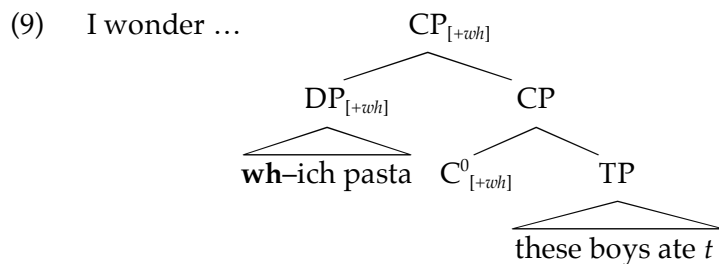
The representations in (6) and (7) are remarkable not only for the absence of category labels. Note that *apples* in (6), or *book* in (7), is both  $N^0$  and NP; in the traditional representation, this lexical element would be represented with at least the structure shown in (8):



But in BPS, there are no non-branching projections. Chomsky achieves this result by proposing a relational definition of “minimal” and “maximal” projections: A category that does not project any further is “maximal”, and one that is not a projection at all is “minimal”. By this definition, *apples* in (6) or *book* in (7) is simultaneously  $N^0$  and NP.

#### 4. Specifier-less Syntax

In a recent paper, Starke (2004) has argued that “specifiers” don’t exist, and that what has hitherto been analyzed as a specifier is a phrase which projects its own, independent phrase. An example is the following, taken from Starke (2004: 252), which shows *wh*-movement represented in the traditional way (9) and in Starke’s theory (10):



In (9), an “invisible head terminal” attracts a *wh*-phrase to its specifier position,

and checks its own [+wh] feature with that of the moved phrase. In (10), the [+wh] feature of the *wh*-phrase directly labels the projection. To legitimize (10), Starke argues, all we need to do is to discard a hidden assumption of the current theory that only  $X^0$  can project. Adopting (10), we eliminate two things: an invisible head and a duplication of features.<sup>8</sup>

In Starke's theory, the *wh*-phrase moves in order to conform to a universal functional sequence ("f-seq") which requires that there should be a phrase bearing the [+wh] feature above TP in a question. The mechanisms of Checking Theory (Chomsky 1993) — such as the uninterpretable feature [+wh] on the invisible head and the Extended Projection Principle (EPP) — can be dispensed with (but see fn. 9 below).

Note that in (10), the *wh*-phrase is a phrasal head that takes the TP as its complement. In Starke's theory, phrase structure is radically simple: "[...] syntactic structures are nothing but raw layers of head-complement relationships" (Starke 2004: 264).<sup>9</sup>

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<sup>8</sup> How can a phrase project? Note that the *wh*-feature, however deeply embedded it is in the *wh*-phrase, must be accessible from outside for selectional processes; otherwise the phrase will not have been "pulled up" into the C-domain in the first place, and it will not satisfy the checking requirements of  $C^0_{[+wh]}$  in the traditional configuration. If the feature is "salient" in this fashion, it should not be surprising that the *wh*-phrase can directly satisfy the English question clause's requirement of a *wh*-phrase in its left periphery by projecting this feature.

This should also answer the possible query why *which pasta* in (10) projects its [+wh] feature, and not, say, its D-feature. What the position requires is a [+wh] phrase.

<sup>9</sup> Starke's proposal about *wh*-movement is arguably too cryptic. It ignores many questions — for example: How does successive-cyclic movement take place?

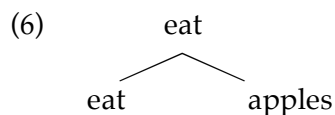
Let us try to fill this lacuna by considering Bošković's (2007) proposal, which contains a careful articulation of the problems involved and proposes a solution; and let us show that the Bošković-solution can be "adopted" into specifier-less syntax. Bošković suggests that movement is not target-driven but driven by an uninterpretable feature on the moving element. Thus, a *wh*-phrase has, say, an uninterpretable feature [*uK*], which must be deleted by an interrogative  $C^0$  with a matching interpretable feature. But [*uK*] must probe  $C^0$ , exactly as the uninterpretable features of  $T^0$  must probe an NP with matching features. Since probe is always "downward", [*uK*] must move to a position above  $C^0$ ; in Bošković's proposal, the *wh*-phrase moves to [Spec,CP]. Successive-cyclic movement is ensured as follows: Given Phase Theory (Chomsky 2000 *et seq.*), the *wh*-phrase must move to the edge of each phase it must escape from, before the complement of the phase head is transferred to Spell-Out; otherwise it will be "frozen in place", and [*uK*] will never be deleted.

In Starke's system, let us say that — modifying it somewhat by importing into it *some* of the mechanisms of Checking Theory — a *wh*-phrase can be marked with an uninterpretable focus feature [*uFoc*], and that this feature will be deleted when the phrase moves into a focus position in the left periphery of an interrogative clause (cf. Rizzi 1997). (The Focus position, we shall say, has an interpretable feature [*iFoc*].) Successive-cyclic movement will be ensured by the same considerations as in the Bošković proposal.

Note that we are saying that [*uFoc*] is deleted — that is, feature matching takes place — simply as part of the movement of the *wh*-phrase into the focus position. (In effect, Move and Agree use different mechanisms to delete uninterpretable features.) But we can also imitate the Bošković system more closely and employ a probe. We can say that the focus position in the left periphery of interrogatives into which the *wh*-phrase moves does not itself bear [*iFoc*], but that it is generated above a head — possibly the head hosting the question operator — which bears [*iFoc*], and that [*uFoc*] probes this head. (Recall our earlier point that a probe needs only c-command and locality; see also Jayaseelan 2007 for some discussion of a focus-above-question-operator configuration — although I use the notion of "specifier" in that paper for convenience.)

## 5. Eliminating Labels

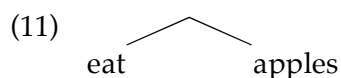
Returning to the BPS representation, consider (6) again:



*Prima facie*, *eat* not only takes as its sister *apples* in (6), but dominates the string *eat apples*. Similarly, in a phrase which contains a specifier, such as (7), the head dominates a string that contains the specifier as well as the complement. How should we understand this?

In the traditional way of representing phrase structure, domination — more correctly, exhaustive domination — signified an “is a” relation. For example, in (8), *apples* (or *book*) “is a” N(oun) and “is a” N(oun) P(hrase). What does domination signify in (6)? The lexical element *eat* contains the categorial feature [+V]. So the “is a” relation is recoverable in (6). Instead of “extracting” the categorial feature of the head and using it as a label, BPS uses the head itself as a label, which is arguably computationally simpler. As Chomsky (1995: 396) is at pains to point out, all the information needed for further steps in the derivation — e.g., in the case of (6), selection of *eat apples* by the higher head  $v^0$  (or whatever is the higher head that selects it) — is present in the label. Thus the label minimizes search.

However, in a proposal that is currently receiving serious attention, Collins (2002) argues that labels (and projections) ought to be eliminated from phrase structure representations.<sup>10</sup> For Collins, (6) should be replaced by (11):



In set notation, whereas (6) would be represented by Chomsky as (12), Collins wants only (13):

(12) { eat, { eat, apples } }

(13) { eat, apples }

Collins adopts a theory of “saturated” and “unsaturated” constituents from earlier researchers. In (11) (or (13)), there are two terms (besides the whole phrase, which is a term). Of these, one term, *apples*, is saturated, because it has no feature which is “unsatisfied”.<sup>11</sup> But the other term, *eat*, is (by itself) unsaturated, because it needs an argument to satisfy (what we can think of as) a “theta-role feature”. Therefore *eat* selects *apples*, and not vice versa. (This is what we mean

<sup>10</sup> See also Seely (2006) for an elaboration of this idea.

<sup>11</sup> An unchecked (unvalued) Case feature does not make a nominal phrase “unsaturated”, Collins maintains; therefore *apples* — even prior to being concatenated with *eat* and getting its Case feature checked (valued) — is saturated.

when we say that *eat* is the “head” of *eat apples*.) Now in any act of binary Merge, one member will be the selector (unsaturated) and the other will be the selectee (saturated). And the computation can tell which is which by only inspecting the two objects that are merged. Therefore, Collins argues, labels are not necessary.

But the computation’s task — one may want to point out — becomes more difficult when a specifier is merged with an intermediate projection  $X'$ ; because now it will have to look “into” the  $X'$  constituent to realize that this constituent is unsaturated. (It is a remaining unsatisfied feature of  $X^0$  — e.g., an EPP feature of  $T^0$  — that induces Merge of the specifier.) However, we can let this pass, because this is not our main problem with Collins’ proposal.

It seems to me that it is a function of notation, whether we are using the graphic notation of PS trees or the set notation, to express the *unequal* relation that obtains when two syntactic objects are merged. It is a relation which has directionality: One object is the “pivot”, it selects the other.<sup>12</sup> Neither (11) nor (13) expresses this. Observe that (13) is an unordered set. But what we need in this case is an ordered pair, in which the ordering reflects the directionality of the relation.

As is well-known, an ordered set can be represented in terms of unordered sets:

$$(14) \quad (\alpha, \beta) = \{ \{ \alpha \}, \{ \alpha, \beta \} \}$$

Consider the Chomsky-type representation (12), which we repeat here:

$$(12) \quad \{ \text{eat}, \{ \text{eat}, \text{apples} \} \}$$

It is tempting to make a small change in (12), as shown in (12'), and suggest that Chomsky’s “label” (or “head”) is simply a way of indicating that the set we are dealing with is an ordered pair:<sup>13</sup>

$$(12') \quad \{ \{ \text{eat} \}, \{ \text{eat}, \text{apples} \} \}$$

Such a suggestion becomes impossible, however, when we deal with a phrase which has a specifier. Consider (15), from Chomsky (1995: 398):

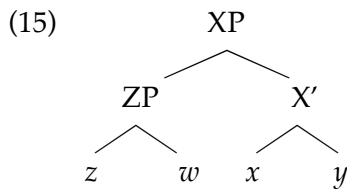
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<sup>12</sup> Cf.: “Set-Merge typically has an inherent asymmetry. When  $\alpha, \beta$  merge, it is to satisfy (selectional) requirements of one (the *selector*) but not both” (Chomsky 2000: 133).

To emphasize what is perhaps an obvious point: It is not enough that the native speaker, looking at any instance of merge, can tell apart (implicitly knows) the selector and the selectee. The function of linguistic representation is to make explicit the native speaker’s knowledge. The traditional phrase structure representation, and also Chomsky’s version of BPS, indicated the selector by means of projection and labels. With the elimination of labels, the unequal nature of Merge is unrepresented.

<sup>13</sup> Daniel Seely (p.c.) has pointed out that Chomsky could not have adopted (12'), for a good reason: In (12'), both occurrences of *eat* become “terms”, going by the “member of a member of the set” definition of “term” (Chomsky 1995).





Here  $z, w, x, y$  are terminals;  $\text{ZP} = \{z, \{z, w\}\}$  and  $\text{X}' = \{x, \{x, y\}\}$ . Up to this point, we can maintain — with a small change along the lines of (12') in the set representation — that the notion of “head” can be derived from the notion of an ordered pair.

But what is XP? If the notion of “head” is definable in set-theoretical terms as the first member of an ordered pair, we should get (16); but what Chomsky has is (17) (see the discussion of (15) in Chomsky 1995):

$$(16) \quad \{\{x, \{x, y\}\}, \{z, \{z, w\}\}, \{x, \{x, y\}\}\}$$

$$(17) \quad \{x, \{\{z, \{z, w\}\}, \{x, \{x, y\}\}\}\}$$

Therefore the notion of “head” is only a linguistic notion, not a set-theoretical notion at all.

How do we get (17)? Consider the stage at which ZP and X' have been merged, and we have still to find the label:

$$(18) \quad ?\{\{z, \{z, w\}\}, \{x, \{x, y\}\}\}$$

We cannot have an algorithm which copies “a member of a member of the set”, for this could as well copy “ $\{x, y\}$ ” or “ $\{z, w\}$ ”. We need (19):

$$(19) \quad \text{Copy a member (which is itself not a set) of a member of the set.}$$

If  $z$  is copied, the constituent shown as X' in (15) becomes the specifier of ZP. But in fact  $x$  is copied, and we get (15) (= (17)).

But (19) is overly complex.<sup>14</sup> Note that in a theory like that of Starke (2004) in which “specifiers” are phrases that project, we can have a very simple algorithm, namely the algorithm that generates an ordered set:

$$(20) \quad \text{Copy a member of the set.}^{15}$$

If (20) applies to (18), it can copy “ $\{z, \{z, w\}\}$ ”; in which case “ $\{z, \{z, w\}\}$ ” would be a “phrasal” head that takes “ $\{x, \{x, y\}\}$ ” as its complement. If “ $\{x, \{x, y\}\}$ ” is copied (instead), the relation would be reversed.

<sup>14</sup> Also, (19) by itself is inadequate, since we need the following rule for merging a head and a complement:

(i) Copy a member (which is itself not a set) of the set.

<sup>15</sup> More strictly: “Copy a member of the set and make it the member of a singleton set.”

What (19) points to is not really a difficulty about finding a label (which can be got around by doing away with labels), but a deeper difficulty that inheres in the idea of “second merge”. “Second merge” requires the activation, and accessing, of an element embedded in one of two phrases that merge to create the specifier configuration. This element — an unsaturated  $X^0$  element — can be an immediate constituent of the merging phrase that contains it, but it can also be very deeply embedded in that phrase if we are dealing with multiple specifiers. There is a plausibility argument here for doing away with the “specifier” relation. It is likely that Merge, the basic operation of syntax, only makes sets by looking at the immediate properties of the two syntactic objects that merge, that it does not also set in motion a search algorithm that looks deep into these syntactic objects.

## 6. BPS Further Simplified

The elimination of labels (and the consequent simplification of Chomsky’s version of BPS) can, in fact, be achieved in a radically simple way; (6) can be represented as:

(21) eat  
 \     /  
       apples

(21) has only terms, no labels. But unlike in Collins (2002), the unequal relation between the selector and the selectee is encoded in terms of dominance.<sup>16</sup> The “is a” relation is recoverable in (21), in the same sense in which it is recoverable in Chomsky’s version of BPS, i.e. (6): *Eat* contains the feature [+V]; therefore a structure “headed” by *eat* is a V(erb) P(hrase).

The standard PS tree has three relations: dominance, precedence, and (derivatively) c-command. But our representation (21) has only one relation, which we can think of in terms of dominance, or precedence (see fn. 16), or whatever other ordering device we choose.

But what happens if the “head” is a phrase, as can be the case in specifier-less syntax? Consider (22), which will be represented by Chomsky’s version of BPS as (23):

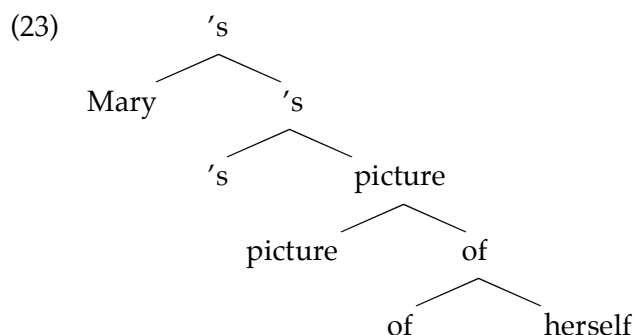
(22) Mary’s picture of herself

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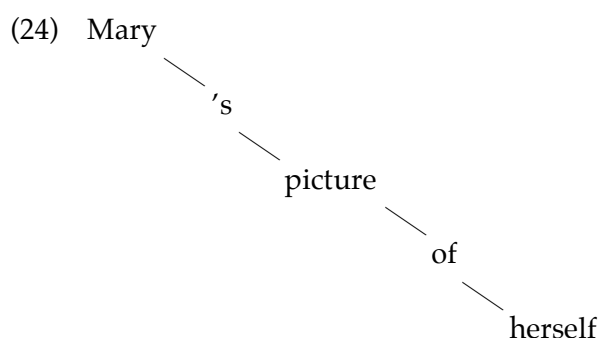
<sup>16</sup> Any way of indicating an ordering relation will do, including precedence:

(i) *eat – apples* (or: *eat^apples*)

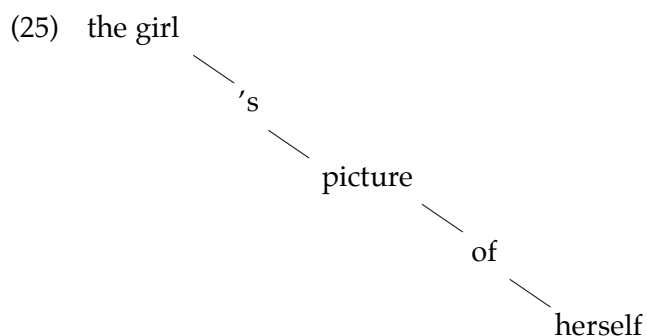
But we shall choose to use dominance in our illustrative examples. The notion of representing the head-complement relation as dominance has in fact a tradition in linguistics, see, e.g., Brody’s (1997) “Mirror Theory”. (Brody credits the idea to dependency grammar; see, e.g., Hudson 1990.)



Here *Mary* is treated as a specifier. But if *Mary* is a phrasal head, and if we apply the logic of (21) to this phrase, the representation that we get is:<sup>17</sup>



How about *the girl's picture of herself*? Note that *the girl* is not built up as a continuation of the “derivational cascade” (Nunes & Uriagereka 2000) that built up the rest of the phrase *'s – picture – of – herself*. It was built up in a different derivational space and merged as a phrase. We can encode this fact by representing it in the larger phrase as follows:



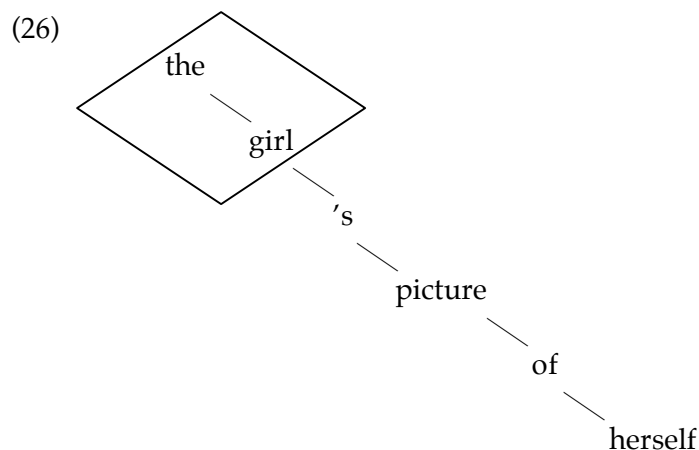
Let us stop to consider (25). It embodies a claim that there can be complex mother nodes, with internal structure. Two questions immediately arise: First, how do we make sense of the notion of a phrasal mother node? Second, how can this structure be accommodated to our declared target of a linear PS tree?

<sup>17</sup> We abstract away from the question whether *Mary* here is in its base position or moved up from a lower position in the phrase. *Mary* selects *'s*, perhaps in order to satisfy a Case feature. (See also fn. 7.)

To answer the first question: In the traditional PS tree, the mother node — bearing a categorial label — signified an “is a” relation with respect to the string it exhaustively dominated. (We said this earlier.) The Chomskyan version of BPS dispensed with any explicit representation of the “is a” relation; although, as we suggested, this relation could be recovered from the categorial feature contained in the label of the mother node. In contrast to both these systems, in our system the mother node–daughter node relation signifies the head–complement relation. Our departure from earlier attempts in the theory to use dominance to represent the head–complement relation (see fn. 16) is that — following the central claim of specifier-less syntax — we postulate phrasal heads. So it should not be surprising that we have phrasal mother nodes. This should be even less surprising if we think in terms of set representation: nothing prohibits the first member of an ordered pair being itself a set.

Now with respect to the second question: The tree in (25) is *not* linear — at least, not yet. While *the girl* stands in an ordering relation of dominance to the elements below it, the proper terms of that phrase — *the* and *girl* — stand in no relation to the elements below it. The total linear ordering of the terminal elements of the PS tree is a question that we take up in section 7, where it is implemented by a rule of Spell-Out. But in the meanwhile, what (25) achieves should not be lost sight of: We have here represented the head–complement relation in an asymmetrical fashion, correctly reflecting the asymmetrical nature of this relation; moreover, this representation very naturally eliminates projection and labels.

It should be pointed out further that the phrase *the girl* is internally ordered by the relation of dominance, so that we could equally well have represented (25) as (26):<sup>18</sup>



<sup>18</sup> The function of the box drawn into (26) is only to preclude the possible misunderstanding that *girl* takes (the structure headed by) *'s* as its complement. The box is not a theoretical construct that we need (or make use of); it is not “real”.

(26) already indicates why it is “easy” for the Spell-Out rule to achieve total linear ordering; all it has to do is to “wipe out” the box! ((26) ought to also dispel any possible suspicion that by admitting complex (phrasal) mother-nodes, we are covertly making use of the c-command relation.)

It will be recalled that in the theory of specifier-less syntax, the erstwhile specifier becomes a phrase that, as a whole, takes the phrase it is merged with as its complement; but, of course, none of its subparts (proper terms) takes the latter phrase as its complement. Thus, *the girl* can take the KP headed by *'s* as its complement — but that operation does not make the KP the complement of *the* or *girl*.

In order to implement this idea in terms of dominance, we can adapt Epstein's (1999) idea of "derivational c-command" and speak of "derivational dominance":

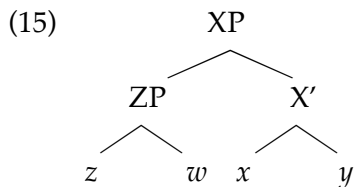
(27) *Derivational Definition of Dominance*

If  $\alpha$  is merged with  $\beta$ ,  $\alpha$  the selector,  $\alpha$  dominates all the terms of  $\beta$ .

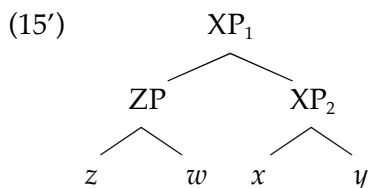
(27) does not mention the terms of  $\alpha$ ; so these do not dominate  $\beta$ 's terms. And since domination is an antisymmetric relation, no question arises of a reciprocal domination by  $\beta$  of  $\alpha$ 's terms. Also, it is important to note that any element which may now be merged above the structure shown in (25) or (26) will dominate *the* and *girl* separately; that is, a merged phrase is an unanalyzed unit (in effect, a "word") for the elements below it but not for the elements above it.

The definition (27) gives us the right result for an ungrammatical phrase like *\*Mary<sub>i</sub>'s brother's picture of herself<sub>i</sub>*, wherein *herself* cannot take *Mary* as antecedent. The explanation now is that only *Mary's brother* dominates *herself*, not *Mary*. The reader can readily see that the relation of dominance does all the work of the erstwhile relation of c-command.

In fact dominance does better than c-command, because it avoids certain problems created by c-command. Consider (15), repeated here for convenience:

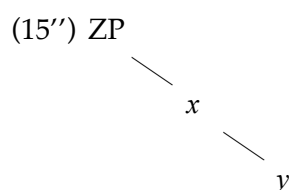


If we adopt the "first branching node" definition of c-command (Reinhart 1979),  $X'$  c-commands  $ZP$ ,  $z$ , and  $w$ . This is an unwanted set of relations; there is no positive evidence of the existence of these relations. For Kayne (1994), these relations also created counterexamples to antisymmetry, which is why he re-analyzed specifiers as adjoined phrases:



He claimed that a mere segment of a category — in (15'),  $XP_2$  — does not c-command. Chomsky (1995) (see also Epstein 1999) stipulated that an

intermediate projection does not c-command, but required that (nevertheless) the intermediate projection has to be present in the tree to prevent — in (15) —  $x$  and  $y$  from c-commanding the terms of ZP. All these complications arose, one can now see, because of an inadequate graphic representation that showed syntactic objects that merge in a symmetric relation (as sisters) on the one hand, and an analysis which claimed that “specifiers” are in a selectee relation to a following  $X^0$  category on the other. In our analysis, (15) becomes (15'')



There is no question here of  $x$  or  $y$  dominating the terms of ZP.

At this point, we wish to dispel a possible misconception that may have arisen. Our discussion so far led up from specifier-less syntax to a proposal about non-branching PS trees. But the two are, in fact, independent issues. Our non-branching PS tree is not contingent on the elimination of “specifier” from the grammar. Thus, consider (15'') again. In this configuration, ZP can still be analyzed as a specifier, if one so wishes. (One can define “specifier” derivationally as a “second merge”, or in some other way, exactly as before.<sup>19</sup>) That is, given the possibility of phrasal mother nodes, *any* analysis of phrase structure that does not crucially appeal to left-to-right ordering can be “translated” into a non-branching PS tree. All one has to do is to “push up” all the constituents on left branches into the “bole” of the tree. Therefore, a non-branching PS tree is not in itself a very interesting idea, and it is not the core of my claims about phrase structure representation. But note that the “pushing up” operation changes relations: The left-branch constituent is no longer in a symmetrical relation with the right-branch constituent, and this is what is significant. The substance of my proposal about phrase structure is, then, that Merge should be *asymmetrically* represented.

## 7. Linearizing the Terminal String: A Rule of Spell-Out

Note that while our theory yields a partially linear PS tree, we do not yet have a linear ordering of the terminal elements. To see this, consider again (25) or (26). In this structure, I insisted that, while they are ordered *inter se* by the relation of dominance, the terms *the* and *girl* of the merged phrase *the girl* have no dominance relation with respect to the terms of the constituent below the phrase. But linear ordering must be total; that is, in the present case, for any terminal elements  $x$ ,  $y$ , it must be the case that either  $x$  dominates  $y$  or  $y$  dominates  $x$ .

To obtain a total ordering of the terminal elements, let us propose a rule

<sup>19</sup> But now, of course, dominance will no longer uniformly represent the head-complement (selectional) relation.

that applies in Spell-Out:

(28) *Rule of Spell-Out*

If  $\alpha$  dominates  $\beta$ , the terms of  $\alpha$  dominate  $\beta$ .

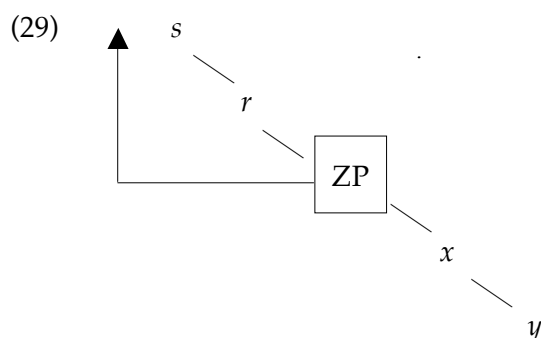
((28) in effect “wipes out” the box in (26)!)

Linearization of the terminal elements, then, is a matter of the PF component of the grammar (Chomsky 1995).

## 8. Movement in a Linear Tree

How do we do movement in a linear tree? In a traditional PS tree, a specifier “hung out” conveniently in a left branch, so that it could be moved (leaving a trace) without disturbing the rest of the tree. A head  $X^0$  also was on a left branch, and so could be similarly moved — if one wanted head-movement — without disturbing the rest of the tree. The movement of a complement presented no problem whatever, since one was only moving a constituent from the bottom of the tree.

In a linear tree, all but movement from the bottom of the tree (corresponding to complement movement) appears *prima facie* to be problematic. Consider (29):



Does the movement of ZP “disconnect” the tree? Actually, the problem with moving ZP in (29) is that it looks like the movement of a non-constituent. X and y “depend” from ZP. How can one move a node without taking along the nodes that depend from it?

Chomsky (1993) proposed that movement is “copy-and-merge”; this is now a standard assumption of minimalist research. But the traditional PS tree is so conceived as to facilitate our thinking in terms of the physical removal of a constituent (in cases of movement). All movement is from the bottom of a tree, albeit a sub-tree. (As just said, specifier and head “hang out” from a left branch and therefore are, in that sense, at the bottom of a sub-tree.) We can see that the traditional phrase structure notation is far from innocent.

If we graduate to thinking in terms of “copy-and-merge”, the question to ask is: What can be copied? Or, alternatively put: What are the constraints on copying? In this connection, let us adopt an idea of Collins (2002), that a

“saturated” phrase is spelled out.<sup>20</sup> Let us now build on this idea and say that a spelt-out phrase can be copied. Returning to (29), if ZP is a saturated phrase and therefore spelled out, it can be copied and merged without any problem.

## 9. The Linear Tree and Antisymmetry

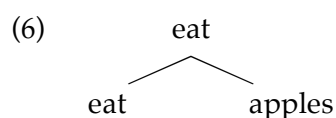
Specifier-less syntax, adopted here, is inconsistent with antisymmetry (Kayne 1994); for if XP takes YP as its complement (cf. (10), where the *wh*-phrase takes TP as its complement), XP and YP will asymmetrically c-command each other’s proper terms, and linear ordering will fail.

However, we now briefly show that all the major results of antisymmetry are unaffected within our framework; in fact, these results are also predictions of the linear tree.

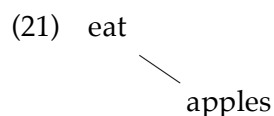
Thus consider the “Head Parameter” — that is, the claim that in UG, the head of a phrase has a choice between taking its complement to the left or to the right. The Head Parameter is inadmissible, given the antisymmetric framework. It cannot even be stated with respect to the linear tree.

In fact, no operation that crucially refers to “left” or “right” is now statable. Any seeming rightward movement of a constituent XP must be formulated (given the linear tree) as two movements: a movement of XP to the top of the tree, followed by the movement of a “remnant” to the top of XP. These are, of course, precisely the movements dictated by antisymmetry.

Chomsky (1995) pointed out that if “bare phrase structure” were to replace the traditional way of representing phrase structure, a problem would arise for the Kaynean framework: In every case where a complement is a single-word element, the linear ordering of the head and the complement will fail. Thus consider (6), and note the problem that there is no asymmetric c-command relation to invoke the LCA:



But the problem arose because of the representation of head and complement as sisters. The solution for the problem is the linear tree:



<sup>20</sup> See also Uriagereka (1999) and Nunes & Uriagereka (2000) for the idea that a moved phrase is spelled out prior to movement and that a spelled out phrase is treated like a “word” by the syntax.



## 10. Conclusion

I suggested that linguistic theory took a wrong turn when it postulated the X'-schema (1) that incorporated a relation of "specifier". I showed that the paradigm cases of a Spec-Head configuration allow, or require, other analyses. Moreover, the notion of a "second merge" introduces an arguably unacceptable degree of complexity into the algorithm of projection. Merge, the basic operation of syntax, can be maximally simple if we do away with "specifier".

We also suggested that Merge should be asymmetrically represented, to reflect the unequal relation between a selector and the selectee. We proposed that the selector-selectee relation be represented by dominance. This yielded a non-branching PS tree that imposed a partial linear ordering on the terminal elements, which could be converted into a total linear ordering by a simple operation of Spell-Out. Moreover, our partially linear tree yielded all the predictions of antisymmetry that didn't have to do specifically with the X'-schema (which I reject). The central claim of antisymmetry was that "if two phrases differ in linear order, they must also differ in hierarchical structure" (Kayne 1994: 3). This follows without stipulation in our schema, because here the linear order *is* the hierarchical structure.

Chomsky (2004: 112) has suggested that in natural language, displacement (internal Merge) is induced by "scopal and discourse-related (informational) properties". The cartographic analysis of sentence structure posits positions in a functional sequence which encode these types of meaning — e.g., TopP, FocP, SubjP. In the earlier way of representing phrase structure, we would have merged a null head marked Top<sup>0</sup>, Foc<sup>0</sup>, or Subj<sup>0</sup> and moved a phrase into its Spec position. But in the type of phrase structure representation argued for in this paper, we can let a phrase with the appropriate feature merge directly with the structure built up by the derivation up to that point, taking the latter as its complement, in a linear tree.

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## Mind Your Morals

**Hauser, Marc D.** 2006. *Moral Minds: How Nature Designed Our Universal Sense of Right and Wrong*. New York: Ecco/HarperCollins Publishers.

by Susan Dwyer

Morality is so steeped in the quotidian details of praise and blame, of *do's* and *don't's*, and of questions about the justifiability of certain practices it is no wonder that philosophers and psychologists have devoted relatively little effort to investigating what makes moral life possible in the first place. In making this claim I neither ignore Kant or his intellectual descendants, nor the large literature in developmental moral psychology from Piaget on. My charge has to do with this fact: Morality is an ineliminable feature of human life and human beings are *biological* creatures. Hence, what wants explaining is how a biological creature — a creature with an evolved mind/brain — can be a normative creature of a particular kind, that is, a creature that cannot help but engage in moral appraisal and evaluation. It does no good to try to wring such an explanation from the 'very concept' of agency (whatever that might be) à la Korsgaard (1996). Such a strategy merely delays the inevitable: How is it that biological creatures are agents? And while we can understand the practical value of charting the trajectory of babbling infants to toddlers to adolescents to adults, absent an account of the foundations of the capacities whose emergence constitutes this trajectory, we will still not have addressed the central question.

Sociobiology and evolutionary ethics fare no better. The apparent puzzle of cooperation amidst competition can and has been addressed via the notions of kin selection and reciprocal altruism. But these accounts are motivated by, and hence pitched at, the level of overt *behavior*. However, being a moral creature, in the sense that makes such entities apt subjects for deep intellectual investigation, has very little to do with whether they behave well (sometimes? often? on average? ever?) and everything to do with being capable of a certain kind of *cognition*.

Moral creatures are distinguished by the possession of moral *minds*. Or, to use Chomsky's preferred term, which serves to keep us honest, moral creatures are distinguished by their possession of moral *mind/brains*. Animals with moral mind/brains are built to cognize the world in a particular way; namely, as populated by objects of moral concern, by subjects of moral expectations, and by targets of moral evaluation. We replace description with explanation only when, taking the fact of our biological nature seriously, we come to know what capacities comprise moral cognition and then to discover what makes their



possession and operation possible. And while the story we tell about the underlying psychological mechanisms that constitute moral minds will have to be intelligible from an evolutionary point of view (once shorn of problematic adaptationism), that constraint leaves open more possibilities than are envisioned by extant sociobiology and evolutionary ethics.

This is a profoundly interesting and exciting research project. If it turns you on — which it should, or if it raises your skeptical hackles, which it may — then you can do no better than to read Marc Hauser's (2006) superb book *Moral Minds: How Nature Designed our Universal Sense of Right and Wrong*. It is a wonderful compendium of a vast array of empirical work — developmental cognitive psychology, ethology, neuroscience, experimental economics, and brain science — bearing on two major issues: (i) What is at the core of morality (i.e., what mechanisms and processes are distinctively involved in moral capacities) and (ii) which aspects of morality are unique to human beings? More importantly, *Moral Minds* is also philosophically sophisticated, engaging substantively with the long-standing debate in moral philosophy concerning the relative causal contributions of reason and emotion to the etiology of moral judgment and the action such judgment is thought to motivate. In the emerging field of empirical moral psychology, it is rare to find a work that is at once so comprehensive, accessible, fair-minded, and non-condescending to any discipline as *Moral Minds*. The bottom line: If you are new to the field, read Hauser's book before you read anything else.

In developing his central idea that humans “evolved a moral instinct, a capacity that naturally grows within each child, designed to generate rapid judgments about what is morally right or wrong based on an unconscious grammar of action” (p. xvii), Hauser exploits more fully than anyone to date the so-called linguistic analogy (see also Dwyer 1999, 2006, 2007, and Mikhail, in press). First posited, with a distinctly epistemological bent, by John Rawls in his *A Theory of Justice* (1971), ‘the linguistic analogy’ refers to one among several nativist approaches to moral psychology. For the empirically-minded moral philosopher, the striking parallels between the nature and development of moral competence and the nature and development of linguistic competence render the appropriation of certain concepts and a particular methodological approach from theoretical linguistics most appealing.

Very roughly, the parallels in question are:

- language and morality involve distinctive human capacities that appear to arise early in all individual members of the species relatively effortlessly;
- language and morality are both normative systems in the sense that they involve constraints on human judgment;
- moral creatures have moral intuitions that appear to be as natural, automatic and certain as speakers' linguistic intuitions (e.g., Trolley Problem data);
- despite the universality of morality and language there is diversity among the world's moralities and the world's languages.

In addition, what we know about the development of children's moral capacities — that is, their capacities to judge moral saliency and to attribute moral praise and blame — suggests that in the moral domain, as in the linguistic domain, we are faced with a set of phenomena that emerge relatively independently of variations in the child's environment. Children across the globe grow into moral creatures in human (thus morally-inflected) environments. However, the capacities they develop develop whether or not they receive lots of explicit moral instruction, whether or not they mature in a religious culture, and so on. Hence, as is the case with language, poverty of stimulus considerations appear to be apt (see Dwyer 2006).

The motivating idea behind the linguistic analogy, then, is not that morality is "like" language, presuming that notion even makes sense. Nor is it merely that morality and language appear to be two species-wide and species-specific phenomena. Rather, the deep reason for looking to linguistics for help in thinking about morality is that the fact of our being moral creatures — like the fact of our being speakers — is underpinned by a normative faculty.

We can usefully think of any normative faculty (and there might be such for logic and aesthetics, too) as a structure of constraints in the mind/brain that carves out a possibility space with respect to a certain domain. It may be characterized in terms of principles that 'express' the constraints it imposes. *Very crudely*, just as Universal Grammar constrains how a child acquires the grammar of her language and that grammar in turn constrains what meanings she can assign to what signals, so too, we might imagine a Universal Moral Grammar that constrains how a child acquires the grammar of her morality and that grammar constrains what evaluations she can assign to what bits of the world. And just as the acquisition of a particular grammar is dependent on local conditions (namely, the child's linguistic environment), we should predict that the acquisition of a particular moral grammar will bear the marks of the moral environment in which it occurs.

Given the success of the Chomskyan program in linguistics and the parallels between morality and language of the sort just sketched, inquirers would be crazy not to push the linguistic analogy as far as we can. For, this is the best going approach to addressing what I said at the outset is essential: To explain how biological creatures can also be moral creatures. That said, this approach is still very new. And while Hauser makes considerable progress, he is cognizant that, at present, pursuing the linguistic analogy sets up interesting research questions rather than answers them.

The capacity to judge that an action is permissible, obligatory or forbidden is just *one* capacity involved in moral competence. Others include attributions of praise and blame and (perhaps) the capacity to conform one's behavior to moral judgments in the face of significant temptation to do otherwise. Still, if we are trying to investigate the nature of an alleged moral faculty the above-mentioned judgments (which I shall dub collectively Permissibility Judgments) are a good place to start, for they are easily obtained in naturalistic and experimental settings.

Hauser and his collaborators have made good use of the Permissibility Judgments of subjects who have signed on The Moral Sense Test (see

<http://moral.wjh.harvard.edu>). The Moral Sense Test deploys familiar Trolley Problem thought experiments to elicit judgments from subjects and also asks subjects to provide justifications for those judgments. We may take such data as starting points for considering what principles (if any) people use in making Permissibility Judgments. For the past twenty-five years or so, many philosophers have pursued this project (see Fischer & Ravizza 1992), some emphasizing a morally asymmetric distinction between acts and omissions, others the so-called Doctrine of Double Effect, according to which an act with a good and bad effect may yet be permissible if the agent does not aim at the producing the bad effect and that effect is not a necessary means for realizing the good effect. The articulation of such distinctions and principles is useful, but it is just a start. We would like to know how it is that human beings conceive of actions and scenarios such that they could appeal (implicitly or explicitly) to such distinctions and principles in making Permissibility Judgments at all. Clearly, the capacity to make Permissibility Judgments is contingent on the possession and operation of other perceptual, cognitive, and (perhaps) affective capacities.

A central and crucially important contribution of Hauser's book is his careful exploration of what we can call the *parsing* of actions. "When [a creature with a moral mind] evaluates an action vis-à-vis its permissibility, it is unconsciously and automatically assessing the causal and intentional aspects of the action and its consequences" (p. 267). In Chapter 6, Hauser articulates the most basic principles whose possession is necessary for the very recognition of an action — as opposed to a mere happening. At the very least, such recognition involves the attribution of primitive agency and the disposition to identify the causal consequences of the operation of agency.

That the capacity to make Permissibility Judgments requires the possession of other capacities should strike anyone as a no-brainer as soon as the claim is noted. So the lack of attention to this fact by the vast majority of Anglo-American moral philosophers is breathtaking. But the real import of Hauser's work here is not the revelation of philosophers' inadequacies. (Indeed, Hauser has a deep and evident respect for the necessity of philosophical work in moral psychology.) Rather, his analysis allows us more clearly to address questions about what capacities had by the morally-minded are uniquely moral and about whether non-human animals are can be moral-minded. More generally, Hauser makes vivid the fact that moral philosophy simply cannot be an armchair enterprise. Progress in the discipline demands a methodology that integrates conceptual and empirical considerations. He is not the first to emphasize this point, but *Moral Minds* is the first work of this length to illustrate comprehensively how such a methodology is to be conducted and to reveal, with a suitably critical eye, its fruits to date.

At the very least, the making of Permissibility Judgments implicates the identification of agents and a theory of mind. Arguably, it also involves a particular suite of emotions or affective capacities, say, those required for the identification of relevant notions of harm. Now, some non-human animals clearly manifest some of these capacities, but these capacities are adjuncts to and not uniquely in the service of moral competence. Human beings, in contrast, possess all the relevant adjunct capacities. Still this alone does not support the existence

of a moral faculty, namely, a dedicated part of the human mind/brain. Indeed, one might think that once we have identified the cognitive and affective capacities a creature must possess in order to make Permissibility Judgments, we have effectively provided a reduction of sorts; there is no need to posit a moral faculty *per se*.

This thought would appear to be behind one of the late Richard Rorty's worries, as expressed in his review of Hauser's book for *The New York Times* (Rorty 2007). Rorty complains that, in order to argue for a moral faculty, one needs to show "a bright line separating [...] 'the moral domain' — one that nonhuman species cannot enter — from other domains". To my knowledge, no one has been able provide the asked-for criteria. Elliot Turiel (1983) attempts to do so in his much discussed posit, the moral conventional distinction. (See Kelly *et al.* 2007 for critique.) But, really, it is peevish to demand them. Human beings make moral judgments all the time. What we need to get a Hauser-like project going is a list of the explananda for moral psychology — namely, a list of the capacities, dispositions and so on that *characterize* our moral life.

Absolutely central here is the capacity for *judgment*. Human beings do not merely believe that certain actions are permissible or obligatory and others not. They *judge* them to be so — either when actually confronted with them or when considering them hypothetically. Moreover, human beings produce such judgments about indefinitely many cases in systematic ways, where the systematicity here has to do with the fact that *all* human beings make moral judgments, and that there appear to be culturally-specific differences in the content of moral judgments. And, finally, all 'normally' developing children acquire the capacity to make moral judgments in environments impoverished in crucial dimensions. (A child's socio-cultural and familial environment will undoubtedly influence the *content* of the moral judgments she is apt to make. But they do not determine the very *capacity* to make such judgments themselves.)

The virtues of adopting some form of the linguistic analogy seriously are manifest. We do not (or, at least *should* not) demand of linguists that they provide a list of necessary and sufficient conditions for what counts as the linguistic domain before we ready to take seriously various hypotheses about syntactic rules. This point is related to Chomsky's long-standing but remarkably overlooked admonition that there is no serious scientific inquiry to be done with respect to E-languages (Mandarin, French etc.). The targets of the relevant science is I-language (what is in the mind/brain of particular individuals that accounts for the acceptability judgments they make) and the language faculty (what is part of every human being's mind/brain that accounts for the universal acquisition of an I-language in relevantly impoverished environments). And the project is to uncover what principles characterize the operation of the moral faculty.

Now, one should not be misled by the mention of principles here into thinking that Hauser's idea is that principles like the Doctrine of Double Effect are innately encoded in the human mind/brain. As he himself notes (p. 295), these principles are far too coarse grained. And, as in the case of language, there is really no reason to expect that the principles that do characterize the operation of the moral faculty would be recognizable to the creatures with such a faculty. Ordinary speakers are not consciously aware of a principle about the violation of



island constraints. And professional linguists, who are perfectly familiar with such a principle, do not explicitly consult in speaking. (See Dwyer 2007.)

Admittedly, it is tempting to think that moral principles are readily accessible to the layperson and the professional alike. It seems to us that morality ought to be more articulable. However, I think this is merely symptomatic of the fact that contemporary moral philosophy is comprised of a good deal of normative ethics — the discussion of whether particular practices, like voluntary active euthanasia, say, are permissible. These discussions readily trade in explicit principles, such as that killing is morally worse than letting die. I have no doubt about the pragmatic importance of such talk for debating and formulating public policy and in the education of undergraduate philosophy students. However, it would be curious indeed if such principles were innately given in the human mind/brain.

Skeptics can, if they wish, deny the reality of morality altogether and insist that there is nothing to moral philosophy really except the articulation of some local conventions, that there is nothing to moral experience except the explicit inculcation of such conventions and of a fear of the sanctions attaching to their violation; in short, that there is nothing deep for science to uncover about moral minds, for there are no moral minds. Hauser's book will not appeal to such folks, but I do wish they would read it! Everyone else, however, should be stimulated by the empirical project Hauser has begun to explain the fact that human beings are both biological and normative creatures.

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# Languages and Genes: Reflections on Biolinguistics and the Nature–Nurture Question

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## 1. Introduction

With the launch of this journal, the term ‘biolinguistics’ gains new visibility and credibility, but a clear definition has yet to emerge. In their Editorial in the journal’s inaugural issue, Boeckx & Grohmann (2007: 2) draw a distinction between “weak” and “strong” senses of the term. The weak sense is understood to mean

“business as usual” for linguists, so to speak, to the extent they are seriously engaged in discovering the properties of grammar, in effect carrying out the research program Chomsky initiated in *Syntactic Structures*,

while the strong sense refers to highly interdisciplinary and broad

attempts to provide explicit answers to questions that necessarily require the combination of linguistic insights and insights from related disciplines (evolutionary biology, genetics, neurology, psychology, etc.).

We are concerned with the impact of rapid progress in genetics and cognitive neuroscience on linguists’ conceptions of the biological bases of language and on the overarching issue of nature and nurture in linguistics. The particular focus of our discussion is the recent claim (Dediu & Ladd 2007) that there is a causal relationship between genetic and linguistic diversities at the population level, involving two brain growth-related genes and linguistic tone. Our broader aim, however, is to consider the implications of such relationships — assuming that they actually exist — for those who are “seriously engaged in discovering the properties of grammar” and for those who are attempting to “provide explicit answers to [necessarily interdisciplinary] questions” about language as a biological phenomenon. We argue that broad biological findings and insights must eventually inform the work of those whose interests and activities in biolinguistics are covered by Boeckx & Grohmann’s weak sense of the term.

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We thank the editors for their invitation to comment on Dediu & Ladd’s work. DRL’s work on this paper was supported by an Individual Research Fellowship from the Leverhulme Trust, DD’s by an ESRC (UK) postdoctoral fellowship, and ARK’s by a British Academy postdoctoral fellowship.



## 2. Languages and Genes

It is now well established that genes affect speech and language *in individuals*. By this we mean that there are demonstrable associations between inter-individual differences in genetic makeup and inter-individual differences in speech and language abilities. The best known case to date is undoubtedly that of the *FOXP2* gene (Hurst *et al.* 1990, Gopnik & Crago 1991, Lai *et al.* 2001, Fisher *et al.* 2003), but it is well established that there are many other links between genetic variation and variation in abilities relevant to speech and language. The study of this type of correlation uses the tools of Behavior Genetics (Plomin *et al.* 2001, Stromswold 2001), which allows researchers to tackle three kinds of questions: First, to provide estimates of the *heritability*<sup>1</sup> of various speech and language abilities and disabilities (Stromswold 2001, Felsenfeld 2002, Bishop 2003, Plomin & Kovas 2005); second, to identify specific *genetic loci and alleles* involved (Fisher *et al.* 2003, Halliburton 2004, Plomin *et al.* 2001); and third, to dissect the complex relationships *between and within* aspects of speech and language (Plomin *et al.* 2001, Stromswold 2001, Plomin & Kovas 2005). The main conclusions from this fast-developing field seem to be (Dediu 2007: 125) that:

- (i) speech and language are quite strongly influenced by our genes at the individual level, but the nature and strength of this influence varies greatly across the particular aspects considered;
- (ii) the best model, both for disorders and the normal range of variation, is one involving many genes with small effects;
- (iii) some of these genes are generalists while others are specialists;
- (iv) most speech and language disorders simply represent the low end of the normal distribution of linguistic variability, rather than qualitatively distinct pathologies.

It also seems clear from this work that, in general, the causal links between genes and variability in speech and language are very complex and crucially involve the environment. We shall return to this point shortly.

In addition to connections between individual genetic and linguistic variability, it is also well established that genetic and linguistic diversity are correlated *at the level of populations* (Cavalli-Sforza *et al.* 1994, Dediu 2007: 125-187). That is, geographical inter-population differences in allele frequencies tend to match the distribution of language varieties (e.g., dialects, languages or linguistic families)<sup>2</sup>. This match, unlike the ones discussed in the preceding paragraph, is spurious, in the sense that it does not suggest any causal link between genetic differences and linguistic differences. Rather, it is due to past

<sup>1</sup> Defined as the proportion of phenotypic variation accounted for by genetic variation (Plomin *et al.* 2001: 85, Stromswold 2001: 652, Halliburton 2004: 539).

<sup>2</sup> It is true that some methods and datasets in the field have been heavily criticized, such as the tendency in earlier studies to make uncritical use of unjustified and/or controversial “historical linguistic” classifications and concepts, i.e. linguistic macrofamilies (Sims-Williams 1998, Bolnick *et al.* 2004), but the general approach is valid and fruitful.

demographic processes which shaped both types of diversities in parallel ways (Cavalli-Sforza *et al.* 1994, Poloni *et al.* 1997, Jobling *et al.* 2004, McMahon 2004): An ancient population split is reflected both in the present-day similarity between the genetic structure of the descending populations and in the close relationship between the language varieties they speak. One example of this approach is represented by the *language/farming co-dispersal* class of theories (e.g., Diamond 1997, 1998, Bellwood & Renfrew 2002, Diamond & Bellwood 2003), which try to explain the present-day world-wide distribution of genetic and linguistic diversities through the expansion of agriculturalists, carrying both their genes and languages in the process.

There is a third possible type of relationship between genetic and linguistic diversity that is not well established, namely between *population genetics* and *language typology*. This possibility was explored in a recent paper by two of the authors (Dediu & Ladd 2007), which proposed a connection between the *inter-population* differences in two human genes and the *inter-language* distribution of lexical and/or grammatical tone. The two genes are *ASPM* and *Microcephalin*, which are known to be involved in brain growth and development. In September 2005, two papers published by the same research group appeared simultaneously in *Science* (Mekel-Bobrov *et al.* 2005, Evans *et al.* 2005), announcing the discovery of two new alleles (haplogroups) of *ASPM* and *Microcephalin*, named “the derived haplogroup of *ASPM*” and “the derived haplogroup of *Microcephalin*”, and denoted here as *ASPM-D* and *MCPH-D* respectively. Both these haplogroups are fairly recent (approximately 5.8 thousand years ago for *ASPM-D*, and 37 thousand years ago for *MCPH-D*) and, strikingly, show a skewed geographic distribution and signs of recent or even ongoing positive natural selection (Mekel-Bobrov *et al.* 2005, Evans *et al.* 2005). Given that these haplogroups are potentially involved in brain size and development, the source of this geographical distribution and natural selection quickly became the focus of intense research. However, to date this research has failed to find the phenotype under selection, meaning that *ASPM-D* and *MCPH-D* probably do not determine obvious phenotypic effects; it has now been established that they do not appear to influence normal variation in intelligence (Mekel-Bobrov *et al.* 2007), brain size (Woods *et al.* 2006), head circumference, general mental ability, social intelligence (Rushton, Vernon & Bons 2007), or schizophrenia (Rivero *et al.* 2006).

The proposal of Dediu & Ladd (2007) is that the populations which have a *low frequency* of these derived haplogroups tend to speak *tone languages*. Impressionistically, this idea is supported by the apparent visual match between the map of tone languages (as given, for example, by Haspelmath *et al.* 2005) and the distribution of *ASPM-D* and *MCPH-D* (as given by the maps in Mekel-Bobrov *et al.* 2005 and Evans *et al.* 2005, respectively). Dediu & Ladd tested this hypothesis statistically using a database of 983 genetic variants (alleles) that sampled the human nuclear genome and 26 linguistic typological features that covered various aspects of phonetics, phonology and morphosyntax in 49 old-world populations. (Complete details on the populations, genetic variants, linguistic features and methodology are given in Dediu & Ladd 2007 or Dediu

2007.<sup>3</sup>) The statistical analysis showed that the distribution of the correlations between genetic and linguistic features strongly supports the hypothesized connection between *ASPM-D/MCPH-D* and tone. To rule out the likelihood that this correlation is of the spurious type discussed above, i.e. due entirely to underlying demographic and linguistic processes, Dediu & Ladd computed the correlation between tone and the two derived haplogroups while simultaneously controlling for geographic distances between populations (a proxy for population contact and dispersal) and historical linguistic affiliation between languages (a proxy for similarity through common descent); the proportion explained by these factors turned out to be minimal (again, details are to be found in Dediu & Ladd 2007 and Dediu 2007). It seems, therefore, that the relationship between tone and the derived haplogroups is not due to these standard factors; instead, it could reflect a *causal* relationship between the inter-population genetic and linguistic diversities.

### 3. From Individual Genetic Diversity to Population-Level Linguistic Diversity

How could such a relationship work? How could having or not having a certain allele in one's genome cause one's language to be tonal or not? We believe that any plausible mechanism relating individual genomes and typological variation in languages must consist of at least two distinct aspects: *individual bias* and *inter-generational cultural transmission of language*. We consider the second of these first.

The proposed influence of inter-generational transmission is based on the well-accepted notion (e.g., Lightfoot 1979, Lass 1997, Anderson & Lightfoot 2002, Hale 2003, Campbell 2004) that much language change is brought about when children acquire a subtly different grammar from that of their parents. In invoking cultural transmission as a mechanism for genetically influenced typological change, that is, we are simply proposing that a population whose speakers are linguistically biased — for whatever reason — may, over many generations, transform its language in ways that reflect the preponderance of individual biases among language acquirers. This general idea is supported by a number of computer and mathematical models, which show that even slight biases will affect the direction of language change. For example, Daniel Nettle (1999) studied language change and the threshold problem by including the impact of functional biases, and found that they are effective in influencing the trajectory of language change. Kenny Smith (2004) considered “innate” biases of agents (in favor of, neutral to, or against homonymy) and showed that these influence the evolution of vocabulary. A recent mathematical approach using Bayesian learners (Kirby, Dowman & Griffiths 2007) concludes that small learning biases can be amplified by the process of cultural transmission and expressed as universals. There are of course additional complications to be addressed: Human populations are rarely uniform in their genetic composition,

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<sup>3</sup> A readable account and links to other relevant sites is accessible online at <http://www.lel.ed.ac.uk/~bob/tonegenessummary.html> (March 2008).

and they are normally in contact with other populations who may be both genetically and linguistically quite distinct. Dediu (in preparation) computationally analyzes a complex population of heterogeneous agents and finds that an allele biasing the rate of learning of a binary linguistic feature can be amplified by the cultural transmission of language even for weak biases and low population frequencies. Given a relatively weak bias of the sort we discuss below, many factors might override its influence and impact on the trajectory of language change. Among other things, this makes clear that we are not proposing any sort of deterministic relation between genes and language, only a very indirect and probabilistic one; we certainly are not suggesting that there are “genes for Chinese”. But we believe that the broad outlines of an explanation based on the interaction of bias and cultural transmission are very plausible indeed.<sup>4</sup>

Now let us consider what we mean by individual bias. We intend the term very broadly to mean anything in a given individual’s genetic makeup that somehow inclines the individual to acquire, perceive and/or produce a given linguistic phenomenon in preference to some alternative. Such biases could include a range of cognitive/perceptual and anatomical/physiological factors. A relatively clear example is provided by the case of Italian and Yoruba vowels, discussed nearly thirty years ago by Peter Ladefoged (Ladefoged 1984; see also Disner 1983). Ladefoged noted the existence of small differences in formant values between Yoruba and Italian, which have otherwise very similar 7-vowel systems (namely, /i e ε a ɔ o u/), and noted that these differences are consistent with anatomical differences between Africans and Europeans:

Some of the differences between the two languages are due to the shapes of the lips of Italian as opposed to Yoruba speakers. [...] [W]ith the exception of /i/ and to a lesser extent /e/, the second formant is lower for the Italian vowels than for the Yoruba vowel. These differences are precisely those that one would expect if Yoruba speakers, on the whole, used a larger mouth opening than that used by the Italian. [...] The possibility of overall differences in mouth opening is certainly compatible with the apparent facial differences between speakers of Yoruba and Italian.

(Ladefoged 1984: 85-86)

It is uncontroversial that facial anatomy is influenced by genetic makeup and that vowel quality might be affected by facial anatomy. In our terms, the genetically inherited trait (the shape of various components of the face and vocal

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<sup>4</sup> Since the publication of Dediu & Ladd (2007), we have learned that a similar idea was proposed half a century ago by Darlington (Darlington 1947, Darlington 1955) and extensively developed by Brosnahan (Brosnahan 1961), based on the apparent correlation between the distribution of blood groups in Europe and the distribution in the European languages of interdental fricatives, front rounded vowels, and various other phonetic types. The idea was largely dismissed at the time — though Brosnahan’s book was reviewed in *Science* (Swadesh 1961) — partly because of the taint of racism in the general intellectual atmosphere of the time, partly because the proposal’s empirical underpinnings in genetics were necessarily primitive and its statistical approach elementary, and partly because there was no obvious way of ruling out a co-dispersal account even if the apparent correlation was valid. However, Brosnahan does give a very clear account of how variable individual biases or predispositions might affect the development of languages over many generations, which is identical in its essentials to the proposals discussed here.

tract) induces a linguistic bias (a tendency to produce slightly more open or less open vowels). However, this is only half the story. Indeed, Ladefoged goes on to say:

This does not, of course, imply that a Yoruba could not learn perfect Italian. Any individual speaker could compensate for the overall, statistical, difference in headshape [...]. (Ladefoged 1984: 86)

This is a critically important qualification. First, it makes clear that individual bias need not be manifested in the behavior of the linguistically mature speaker: It is perfectly obvious that all normal children acquire the language(s) they are exposed to during their first years. Second, and more important, it means that individual bias by itself will not necessarily have long-term effects on the language system. If any Yoruba child can learn perfect Italian or any Italian child perfect Yoruba, the putative effects of facial anatomy on phonetic realization can become manifest, if at all, only through the operation of some further factor.

That factor, we claim, is inter-generational cultural transmission. Ladefoged did not spell this out, but a hypothetical scenario will make clear the kind of thing he might have said if he had done so. Imagine that a group of Yoruba infants, as a result of some inconceivable but irrelevant cataclysm, are brought up in Italy away from any speakers of Yoruba. We can assume that their Italian will be phonetically indistinguishable from that of the Italian speakers with whom they live. Now let us further imagine that these unfortunate children go on to found an Italian-speaking community isolated from contact with other Italian speakers and remaining largely endogamous, i.e. genetically Yoruba rather than Italian. We suggest that, a number of generations downstream, the language spoken by their descendants will exhibit vowels having slightly lower second formants. Any individual Yoruba child of the founder generation, brought up in Italian surroundings, will have learned to produce vowels that acoustically match those it hears; over several generations, however, under the influence of the anatomically-determined bias, the community's phonetic norms will drift. This scenario also serves to make a further important point about gene-language links of the sort we are discussing: The linguistic bias in this case is *unrelated to any biologically selective pressures* that may have given rise to the differences in facial anatomy. That is, genetic differences can affect language without creating selective pressures, and without being due to selective pressures related to language. There is no reason to think that slight differences in vowel quality confer any selective advantage on speakers, even though they are causally related to anatomical traits that are themselves clearly heritable and that may be due to natural selection for some other reason. The linguistic differences can merely be indirect by-products of characteristics that have independently evolved.

The case made by Dediu & Ladd (2007) for a link between *ASPM*, *Microcephalin*, and linguistic tone is more complex and more speculative than the example based on Yoruba and Italian vowels, because the nature of the bias is considerably less obvious, but their basic proposal for the interaction of individual bias and cultural transmission is identical. Dediu & Ladd assume that



the bias is some sort of cognitive or perceptual preference arising from structural differences in the areas of the brain involved in language and speech. Detailed mechanisms remain hypothetical, but Dediu & Ladd sketch one proposal for the kind of structural differences that might be involved, and point to a range of studies showing that genes have an important impact on the normal inter-individual variation in brain anatomy and physiology, including the areas involved in language and speech (e.g., Bartley, Jones & Weinberger 1997, Pennington *et al.* 2000, Thompson *et al.* 2001, Wright *et al.* 2002, Scamvougeras *et al.* 2003, Giedd, Schmitt & Neale 2007). They concede that it is by no means clear what sort of cognitive or perceptual bias might induce a preference for or against linguistic tone, though they suggest that it may relate to a preference for having phoneme-sized units that are strictly linearized (as in a non-tonal language) or for allowing phonemes to occur simultaneously (as in a tone language) (Ladd, in preparation). Importantly, they also note that — as with the case of facial anatomy and vowel quality — the putative linguistic bias could be completely unrelated to the selective pressures that may be driving the spread of the derived haplogroups of *ASPM* and *Microcephalin*. There is no reason to think that there is any selective advantage to speaking a tonal or a non-tonal language, since both types of languages serve as supports for a wide range of complex human cultures. If we wanted to use the proposed bias to explain the strong natural selection of the derived haplogroups argued for by Mekel–Bobrov *et al.* (2005: 1722), the difference in biological fitness (however defined) between tonal and non-tonal languages would have had to be so obvious that Dediu & Ladd’s finding would be old news. Instead, it is most likely that the proposed linguistic effects of *ASPM-D* and *MCPH-D* are selectively neutral by-products, and that the naturally selected phenotypes of these genes must be sought elsewhere. The latter is a topic well beyond the scope of our brief remarks here.

#### 4. Nature, Nurture, and the Language Faculty

If genes can affect language through the mechanisms discussed here, what does this mean for the biological basis of language? We think that, most importantly, it provides a further illustration of the fact that there is a fundamentally complex and irreducible interaction between one’s genes and one’s language and culture — between *nature* and *nurture*. A clear example of this interaction, from a very different domain, is provided in a recent paper (Caspi *et al.* 2007): Caspi and colleagues found that breastfed children tend to have higher IQs than non-breastfed children, but only if they possess a specific variant of the *FADS2* gene, allowing them to actually process the human-specific long-chain polyunsaturated fatty acids present in mothers’ milk, which “are thought to be important for cognitive development because they are required for efficient neurotransmission [...] and are involved in neurite outgrowth, dendritic arborization, and neuron regeneration after cell injury [...]” (Caspi *et al.* 2007: 18860). Thus, if a baby is breastfed (nurture) but does not have the appropriate genome (nature), or does have the genome but is not breastfed, there is no positive effect on its IQ. For such an effect to appear, it is required that *both* nature *and* nurture are present

and “cooperate”. Genes are expressed *through* the environment, and not, as suggested by the unfortunate catchphrase “nature *versus* nurture”, in spite of the environment or independently of it.

The case of breastfeeding and the *FADS2* gene is just one example of the interaction between nature and nurture; many others can be found in the biological literature under the headings of “extended phenotype” (Dawkins 1982), “niche construction” (Odling-Smee, Laland & Feldman 2003), and “phenotypic plasticity” (West–Eberhard 2003). All this literature suggests that we have to move beyond simplistic slogans and embrace the complexity of genotype–environment interactions. For the specific case of genes biasing language, the causal chain leading from genes to their phenotype flows not only through the individual’s immediate environment and the individual’s effects on it, but through a temporally and culturally-mediated environment, including the individual, as well as the individual’s linguistic peers and their descendants *over many generations*. In the case of language, that is, the nature–nurture interaction fundamentally includes time, in the form of repeated transmission of cultural information across generations. This is the most obvious lesson to be drawn from cases like those discussed by Ladefoged (1984) and by Dediu & Ladd (2007).

A more subtle, and probably more important, consequence is that the capacity for language (in its broad sense) is not fixed and uniform across the species, but *diverse and dynamic*. It can vary from individual to individual, and it can change gradually over time. This would be a commonplace for anyone taking an evolutionary stance and regarding language as a biological phenomenon that has resulted from biological evolutionary processes, but sits uneasily with the idea of language as a perfect and economical system (Kinsella, forthcoming). There is a wealth of data showing that human evolution did not stop at a conveniently chosen moment in the past (be it around 200,000 years ago, when, presumably, the *Homo sapiens* species arose, or 10,000 years ago, when agriculture and civilization as we define it began). Rather, it continues to act on various aspects of the human body, brain and mind (see, for example, Mekel-Bobrov *et al.* 2005, Evans *et al.* 2005, Voight *et al.* 2006). The two linguistic examples we have considered both deal with phonetic and phonological aspects of language, but there are no principled reasons for excluding morphosyntax or semantics from the discussion. Linguistic theorizing in general, and biolinguistics in particular, must take into account and integrate the idea that human linguistic capacities are variable and probably still evolving.

This does not rule out the existence of genetically determined universals of language. Indeed, the existence of the type of genetic influence on typological linguistic features discussed by Dediu & Ladd would seem to *increase* the plausibility of claiming that some properties of language have deep cognitive, and ultimately genetic, causes — though, of course, the very lack of variation implicit in the definition of absolute universals makes it difficult to evaluate such claims empirically. That is, some linguistic universals may result from biases that are due to genes fixed or near fixation in the human species, a possibility that fits very well with the Chomskyan research program that forms the basis of Boeckx & Grohmann’s “weak sense” of biolinguistics (see also especially Anderson & Lightfoot 2002). At the same time, however, if we accept the possibility of genetic

explanations both for some universal properties of language and for some cases of typological variation, it is difficult to avoid the implication that the capacity for language has evolved through the standard mechanisms of evolutionary biology, in a gradual manner, and that it continues to do so. We therefore think that the most important task for biolinguistics is to inform linguistic theorizing by putting a strong emphasis on the *evolutionary adequacy of linguistic ideas* (Kinsella, forthcoming). This can only be achieved if we adopt Boeckx & Grohmann's "strong" sense of biolinguistics.

We are not suggesting that "business as usual" for linguists should be abandoned; this endeavor has yielded enormous results over the past decades. Indeed, we believe that a new and better account of the mystery of human language will only come from a truly interdisciplinary approach; one that brings together linguists and others *in equal measure*, making use of their respective methodologies with a full understanding of their assumptions, and trying to resolve any incompatibilities using shared standards of falsifiability and argumentation. Yet we also believe that we must keep in mind Theodosius Dobzhansky's (1973) famous dictum that "nothing in biology makes sense except in the light of evolution". Everything in biolinguistics must ultimately be confronted and eventually reconciled with known evolutionary theory. Unless evolutionary concerns are taken seriously, the point of proclaiming a new field of biolinguistics remains obscure.

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## Clarification

Henk van Riemsdijk

It appears that some passages in my interview with Kleanthes Grohmann that appeared in the first issue of *Biolinguistics* have given rise to unexpected and unintended interpretations. These concern specifically the passages on the second page about when and how the biolinguistics program was first mentioned in the fifties.

1. I observed that there was no explicit mention of the biolinguistic program in *LSLT* [Chomsky 1955] (modulo the introduction to its 1975 edition that was written much later), in 'Three Models for the Description of Language' [Chomsky 1956], and in *Syntactic Structures* [Chomsky 1957]. The observation as such, (as was pointed out to me by Paul Postal, p.c.) had already been made in J.J. Katz' 1981 book entitled *Language and Other Abstract Objects*, pp. 40-41).
2. I intended to leave open the question of whether Chomsky already had the biolinguistic program in mind at that point. The abovementioned introduction to *LSLT* states that the "realist position", the assumption that the structure of language is a "system with 'psychological reality'" (p. 36), is taken for granted in *LSLT* (p. 37). In actual fact, Noam Chomsky (p.c.) informs me these ideas had already come up in late 1951 in discussions with Morris Halle and Eric Lenneberg. I was not principally interested in the question as to when these ideas first came up, but rather that in the Skinner review that appeared in 1959 [Chomsky 1959] the biolinguistic perspective was explicitly and forcefully presented for the first time. Whatever the reasons, I feel in retrospect that this was a wise strategy.
3. In speculating about the reasons why Chomsky did not mention the biolinguistic perspective in the 1955–1957 publications, I (at least in part mistakenly) suggested that this had to do with the fact that audiences at MIT were more interested in the more mathematical aspects of Chomsky's research. This may have been true for *Syntactic Structures*, which was based on lecture notes from Chomsky's classes at MIT in 1956, but when *LSLT* was written, Chomsky was still a Junior Fellow at the Harvard University Society of Fellows. In the MIT lectures he addressed undergraduate MIT students who "had been taught that information theoretic approaches [...] were the answer to the problems of language and psychology" (Noam Chomsky, p.c.). Clearly it was important in those lectures to address these misconceptions and





then present the arguments for a transformational approach to language. In characterizing the audience I made the mistake of suggesting that the military, like the students, had an interest in the information-theoretic approaches that Chomsky was arguing against (that research was often financed by the military was quite common and in no way implies any pressures on the content of the research). By this I did not intend to suggest that Chomsky in any way adapted his work to the requirements of the military or that his choice of lecture topics was influenced by the perspective of an appointment at MIT. If the text permitted such an interpretation, I apologize — it was entirely unintended. (*Editors' note: Due to the publication schedule, we had to urge Prof. van Riemsdijk to postedit and proofread the entire interview at the very last minute.*) What I had in mind was that then and now certain choices of topics and certain ways of presenting them to the public can be influenced by strategic considerations. After all, the intellectual climate at the time was (and in many ways still is) quite hostile to many of Chomsky's ideas. Thinking about strategies to present these ideas in the most convincing and forceful manner is in no way unethical, in fact it is part and parcel of the generative enterprise, as it is in many other branches of science.

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Volume 2, Issue 1

Winter 2008

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biolinguistics

ISSN 1450-3417

*Biolinguistics* 2.1: 001–128, 2008  
<http://www.biolinguistics.eu>