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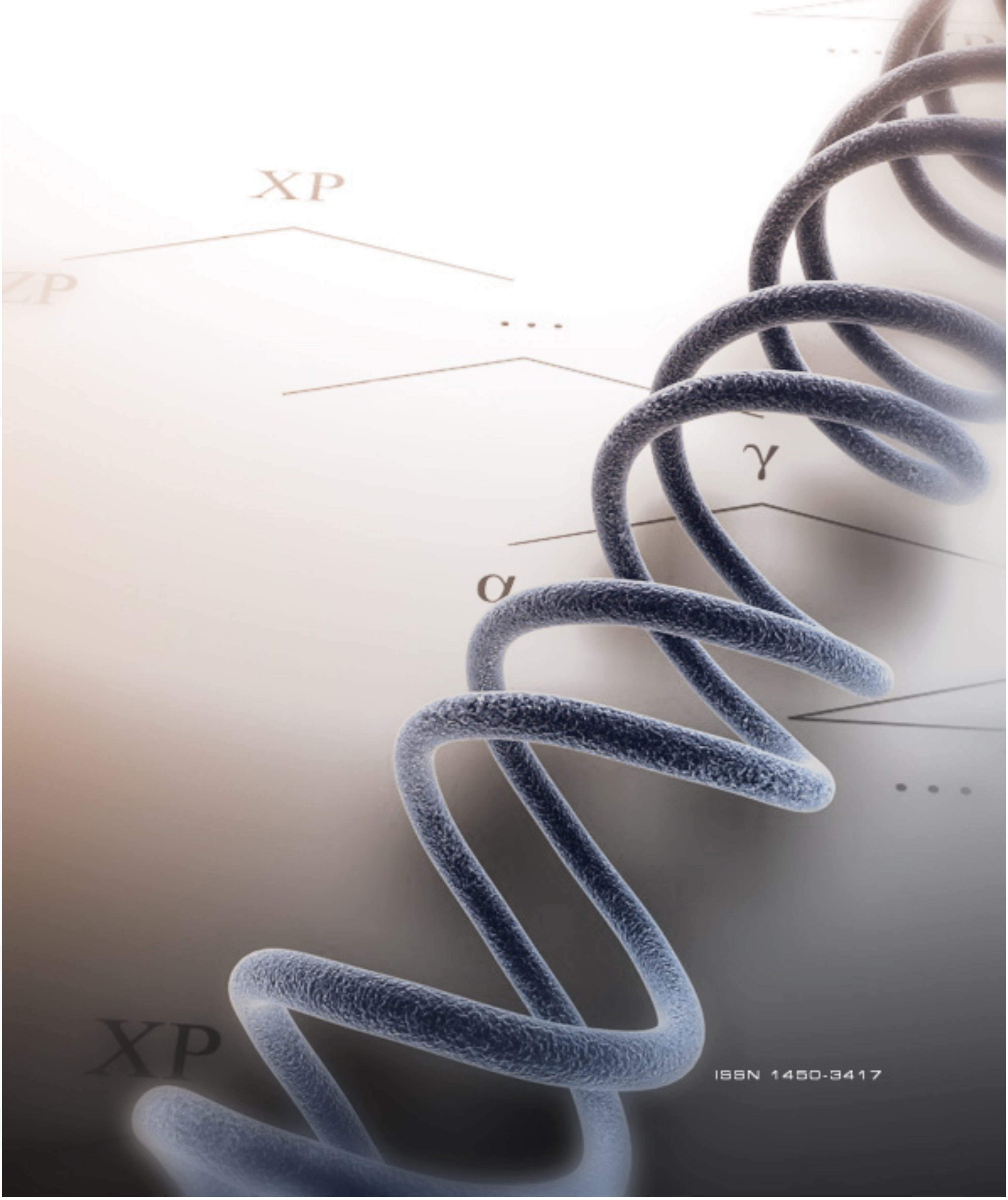

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★ EDITORIAL ★

Biolinguistics for Biolinguistics

Kleanthes K. Grohmann

When we started the journal *Biolinguistics*, our inaugural editorial began with the sentence (Boeckx & Grohmann 2007: 1): "Exactly fifty years ago Noam Chomsky published *Syntactic Structures* (Chomsky 1957), a slim volume that conveyed some essential results of his then unpublished *Logical Structure of Linguistic Theory* (Chomsky 1955/1975)." Now, with the journal going in its 9th year of publication, we could say: Exactly sixty years ago Noam Chomsky completed—or, exactly forty years ago Noam Chomsky published—*Logical Structure of Linguistic Theory* (Chomsky 1955/1975), a heavy tome that essentially gave rise to the emergence of the generative enterprise. And, to continue with Chomskyan anniversaries, we should perhaps present another important 50th, namely that fifty years ago Noam Chomsky published the arguably "most influential linguistics work of the 20th century", *Aspects of the Theory of Syntax* (Chomsky 1965). Though, surprisingly, neither anniversary seems to have made it much into commemoration activities, though see Geoffrey Pullum's piece just quoted from in *The Chronicle of Higher Education* (http://chronicle.com/blogs/linguafranca/2015/06/22/revolutionary-methodological-preliminaries) or Norbert Hornstein's post plus comments on the *Faculty of Language* weblog (http://facultyoflanguage.blogspot.com/2015/06/aspects-at-50.html, and soon the volume edited by Gallego & Ott (to appear).

In this context it could be said that the field of generative grammar took the opportunity to do some housekeeping and introspection. Earlier this month, I returned from Athens, where the widely talked about get-together 'Generative Syntax in the Twenty-first Century: The Road Ahead' took place, a round-table gathering with very interesting 8-minute feature presentations on all kinds of topics internal and external to generative syntax (https://castl.uit.no/index.php/conferences/road-ahead)—and *loads* of time and space for discussion with some 150 participants. (Correction: The three days were definitely not enough!)

Rather than launching into a critical discussion of the field—be it generative grammar in general, biolinguistics in specific, or the relationship between the two—I would like to use this editorial space to say a few things about *Biolinguistics*, the journal, and its current state. First off, you will notice that this editorial note appears on the cusp of June and July, halfway through 2015, its above-mentioned 9th year of publication. That much is great news. However, upon inspection of the table of contents for volume 9 (http://www.biolinguistics.eu/index.php/biolinguistics/issue/view/27), you will equally soon notice that this is in fact the current volume's first 'publication'. This is arguably an issue of considerable concern.

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2015 marks not only *Biolinguistics* 9, the journal's 9th year running, it also constitutes the first full year with me as sole editor. I believe that we had 7 good years of joint editorship, followed by an 8th year of transition, and we might now possibly be looking at new arrangements, but more of that below. In the years past, *Biolinguistics* has always been open—open to different topics (biolinguistics construed in the 'weak sense' as well as in the 'strong sense'), open to a variety of perspectives (generative and agnostic, 'pro-Chomskyan' and 'non-Chomskyan', etc.), and of course open to free access for everyone ('open access publication'). (It also embraces openly the internet as an important resource, as can be witnessed from the four URLs provided on the first page alongside four more traditional bibliographical references, and these URLs include even popular weblogs.)

The 'success' or 'impact' of a journal may be measured in many different ways. One option apparently not available to online-publishing journals without a major player behind them is the now infamous 'citation index' (more on that also below). The only tools we have at our disposal are citations in published works as calculated 'by hand' (for example, googling article titles or using authors' citation summaries on Google Scholar and such platforms)—or using OJS's report statistics, that is, the view and download counters integrated in Open Journal Software, an open-source operating system on which *Biolinguistics* runs. I refer to these below, but I readily admit that I am not completely familiar with the settings and hence cannot guarantee accuracy, further complicated by the fact that OJS had some internal system updates which changed the counting statistics; but the ballpark figures should be roughly as provided.

Using this measure then, I am happy to report that as of 30 June 2015, there are 5 articles that have so far been viewed and/or downloaded more than 7,000 times each, with the top-viewed piece clocking in at around 18,000 views. These are followed by 4 articles with more than 5,000 PDF downloads, 7 pieces with more than 4,000, 13 with more than 3,000, and 44 with more than 2,000 PDF downloads. In addition, *Biolinguistics* offers full issues as single-file downloads at the end of each volume/year; these have also each been downloaded several thousand times. Likewise, the least downloaded pieces are typically the most recent ones, but the numbers still range in the hundreds for each. I would think that for a journal that has been carried out without institutional support to speak of, and outside mainstream publishing that still holds sway over publication practices in our field(s), these are very good numbers indeed.

I haven't systematically carried out the above-mentioned former measure yet, that is, calculations 'by hand'. Nor have I carefully analyzed the ratio of number of submissions and acceptance (other than a steady increase of 'decline' from originally ca. 20% to now over 50%). But just looking at the numbers, one might still gain the impression that everything is hunky-dory: The journal is healthy, it is frequently accessed within the community, possibly even cited in many works published elsewhere, and it serves a well-defined field. But certainly the latter may not be so clear to some—'biolinguistics'? While a full treatment of the issues surrounding 'biolinguistics' are beyond the scope, or purpose, of this editorial, I would like to delve into the topic briefly, if only to return to the first apparent conclusion just drawn, namely that "[t]he journal is healthy". There is a lingering, more worrying aspect of 'health' that needs to be addressed as well.

In the above-mentioned inaugural editorial to the journal *Biolinguistics*, we provided a perspective on the field of biolinguistics, the study of the 'biological foundations of language' (Lenneberg 1967). Expanding on Jenkins (2000), we connected Chomsky's (1986) five questions on 'knowledge of language' to Tinbergen's (1963) four questions on 'the aims and methods of ethology'. Boeckx (2010) is a more recent attempt to flesh out this research program in (text)book length, and the five questions have been picked up by many researchers at different occasions, two also featured quite prominently in Athens (the highly influential #2 and the more distant #5). It may even be worth formulating them as specific 'problems', as did long-time *Biolinguistics* task-teamer Evelina Leivada:

1. *What is knowledge of language?*
(Humboldt's problem; cf. Chomsky 1965)
2. *How is that knowledge acquired?*
(Plato's problem; cf. Chomsky 1986)
3. *How is that knowledge put to use?*
(Descartes's problem; cf. Chomsky 1997)
4. *How is that knowledge implemented in the brain?*
(Broca's problem; cf. Boeckx 2009)
5. *How did that knowledge emerge in the species?*
(Darwin's problem; cf. Jewett 1914)

(from Leivada 2012: 35–36)

We further suggested that "these five questions constitute the conceptual core and focus of inquiry in fields like theoretical linguistics (the traditional areas of syntax, semantics, morphology, phonology), pragmatics, first and second language acquisition, psycholinguistics, neurolinguistics, and beyond" and that "[w]hat these research questions emphasize is the fact that language can, and should, be studied like any other attribute of our species, and more specifically, as an organ of the mind/brain" (Boeckx & Grohmann 2007: 1).

This led us to the perhaps unfortunate distinction of "a weak and a strong sense to the term 'biolinguistics'", which we characterized as follows:

> The weak sense of the term refers to "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 [(1957)] initiated […]. The strong sense of the term 'biolinguistics' refers to 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 regard [Lenneberg (1967)] as the best example of research in biolinguistics in this strong sense.
> (Boeckx & Grohmann 2007: 2)

In other words, we may indeed want to distinguish *biolinguistics in the broad sense* (language as a cognitive organ) from *biolinguistics in the narrow sense* (neurological and genetic bases of language), as Norbert Hornstein recently did in his blog (http://facultyoflanguage.blogspot.com/2015/05/what-invitees-to-athen-conference-are.html, comment date-stamped "May 14, 2015 at 8:20 AM"). Labels aside, Hornstein elaborates on the distinction:

> [In my opinion], we already have a lot to say about the latter and relatively little to say about the former. What I don't see is why being able to say something about the latter is not doing biolinguistics. In the local world in which I live, there is a smooth transition form questions that look like they belong firmly in the world of formal grammar to [those] that look like paradigm examples of work in psychology. There are even hints of some work relevant to neuroscience. [There] are good examples of how linguistically informed work can combine with techniques from other domains […] to fatten cognitive conclusions arrived at on linguistic grounds. It also open new questions[,] the answers to which will heavily rely on what we know about linguistic structure investigated using our standard techniques. In this world, we are all studying the same thing (FoL [the language faculty]) using different techniques. As FoL is a biological entity, we are *de fact[o]* doing biolinguistics even when we don't know a damn thing about genes or even much about brains. […] So unless one believes that humans are not animals then we are all doing biolinguistics, at least in the [broad] sense. When will we do narrow boiling? Well, ask that question about other domains (vision, audition, face perception) and you will find, I believe, that they are also very far from knowing anything biological in this sense. Why? Because we don't know much about brains and how they link to genes. We can't even [explain C. elegans]. So by the stringent criteria often adverted to, nobody is doing biology, i.e. linguistics is, once again, no worse than everything else in the cog[nitive]-neuro sciences.

I will not attempt a 'Where We Are Now'-type reflection of the field or idly ask '*Quo Vadis*, Biolinguistics?', but before sharing some worries for *Biolinguistics*, I would like to spell out, and hopefully dispel, some (mis)beliefs about the field from where I stand—speaking not only as editor of *Biolinguistics* but also as a linguist working on what I take to be relevant research in biolinguistics.

On staying with the theme when Cedric Boeckx and I started the journal *Biolinguistics*, we expressed our hope that "the term biolinguistics will make its way into institutional categories" and "that this journal will contribute to this exciting and rapidly growing field" (Boeckx & Grohmann 2007: 3). Eight years on, there's still some way to go, but we are on track, it seems, judging from the download success reported above, for example. Back then we wrote:

> We are fully aware of the fact that the uniquely interdisciplinary character of biolinguistics poses difficult problems of communication and misunderstandings, but we feel that a growing community of scientists of diverse background, including linguists, evolutionary biologists, molecular biologists, neuroscientists, anthropologists, psychologists, computer scientists, (language or speech and hearing) pathologists, and so on, are slowly overcoming these challenges. Only collaboration and mutual respect will make this type of research possible. We would be delighted if the contributions to *Biolinguistics* could clarify issues, unearth new data, and answer some of the questions that will help us understand the nature of language, and what it is that makes us human. (Boeckx & Grohmann 2007: 3–4)

Biolinguistics is one vehicle to transport such ideas. Beyond the journal, there are a number of very positive developments in the biolinguistics publishing world. More broadly construed, Pierre Pica and I edit the *Language Faculty and Beyond* book series with John Benjamins, which has so far brought out 12 titles (https://benjamins.com/#catalog/books/lfab); closer to the topic is perhaps the *Oxford Studies in Biolinguistics* series edited by Cedric Boeckx, with 4 titles to date (http://ukcatalogue.oup.com/category/academic/series/language/osb.do), but also several books and volumes, including our co-edited *Cambridge Handbook of Biolinguistics* (Boeckx & Grohmann 2013), which has already been critically examined (see, for example, the reviews by Stamenov 2014 and Truswell 2014).

Only time will tell which directions this research enterprise will take in the future. However, one hope we also harbored at the journal from the outset is that the terms 'generative grammar' and 'biolinguistics' are *not* taken to be synonyms, or that the theoretical perspective espoused in 'Chomskyan' generative grammar is the only valid underpinning of biolinguistic investigations. The journal is open to alternative views as it is, especially, to psychological and computational analyses and experimental research in the neurobiology of language. However, we can only publish what we get—and if there are no relevant submissions, such work cannot appear in the journal. Let this be my first plea to researchers to write up their research and submit it to *Biolinguistics*. (To be repeated.)

At this point, I would like to concentrate on my own worries, as editor, about *Biolinguistics* (the journal) rather than biolinguistics (the field as such or even as a perceived composite of diverse disciplines)—and sketch a few ideas for the future. The journal webpage still states, as it did from Day One, that "*Biolinguistics* is a peer-reviewed journal exploring theoretical linguistics that takes the biological foundations of human language seriously" (check out http://www.biolinguistics.eu/index.php/biolinguistics/about/editorialPolicies#focusAndScope for the full text). So, if any reader is engaged in this line of research, submit your work to *Biolinguistics* for peer review and consideration.

One publishing idea we also mentioned at the outset of the journal is that of a special issue. I happen to believe that special issues are a great idea, for many reasons; among others, when done well, they allow readers to get a good perspective on some hot topic from different directions. We published several in the past: *Biolinguistics* 2.2–3 (2008), 3.2–3 (2009), 4.2–3 (2010), 5.1–2 (2011), and 6.3–4 (2012) were all 'special issues' of some sort, either selected papers that arose from international conferences and workshops or real thematic issues. And we tried to ensure that these 'special issues' were actually conceived as such rather than 'conference proceedings', due to a perceived poor reputation of proceedings; on the one hand, all submissions were double-reviewed like any other article (and several such conference proceedings submissions were in fact rejected), and on the other, we had asked the guest editors to solicit additional papers within the theme, to really turn these into special issues proper. However, our experience with the leading journal impact factor awarding body were such that we were explicitly punished for having done this. The presumably highly decorated, very qualified evaluators concluded that "a major issue in the rejection was the predominance of conference papers over regular articles".

I can see at least two ways out of this dilemma, and I will consider both very seriously in the near future. One is to have a guest editor selected from the editorial board or of other high, interdisciplinary standing in the field. A second would be to ask for invited target articles and comments. So, to rephrase my plea: Please step forward if you feel you could contribute to this endeavor.

Another interesting idea that arose when I polled the *Biolinguistics* editorial boards for suggestions on how to attract more submissions was to create a new section in the form of a "republication of some 'classics' with a short new commentary/update by the original author(s) and/or present authors". I believe this is definitely worth considering. There are some other ideas out there as well, such as opening up additional sections in the journal in addition to 'Articles', 'Briefs', 'Reviews', and 'Forum' as well as perhaps clearly distinguishing theoretical from experimental papers, or more linguistically relevant from neurobiological ones, to mention just a few. I will use the remainder of the year to weigh my options as current sole editor of *Biolinguistics* and think about new strategies for the double-digit volume era.

The main reason I am sharing all of this with the readership is, of course, the now repeated main worry of the journal: *Biolinguistics* just does not receive sufficient submissions to keep publications at a steady flow. So, I would like to use this opportunity one last time for today to appeal to readers and researchers out there working on biolinguistic concerns—strongly construed or more weakly, in the narrow or broad sense, conceptual–theoretical or neuro–experimental—to swing that pen, type those keys, and send us your work using the easy online submission process you are automatically guided through when you click on "New Submission". Just to remind potential authors, you need to be logged in; that is, in order to submit to the free open-access journal *Biolinguistics*, you will have to be a registered user. However, even this procedure is relatively painless —and fully free of charge.

Please note that any article submitted will be vetted by an editorial team consisting of the journal editor and a specially recruited section editor. If we deem the submission to be appropriate in terms of form and content, we will send it out for review. With this volume, we will slowly move towards three peer reviews for each Article and Briefs submission. Forum contributions have, in the past, not been as stringently reviewed, but with the support of the growing editorial team, we will put additional measures in motion to ensure high-quality publications.

Likewise, if you are interested in putting together a themed special issue, if you have suggestions for a target article and commentators, of if you perhaps even would like to get started on the 'Classics' section, please get in touch with me. The same goes for any additional suggestions or ideas, whether to increase the journal's visibility or to attract more high-caliber submissions.

And in order to end on a high note, please allow me to share with you one more suggestion from the editorial polling: "You should post something I (and others) can tweet." Despite the email smiley not shown here, this is actually something we had thought about. With Bridget Samuels' help, we had set up a journal weblog and Twitter account right from the start. For a variety of reasons, however, neither really set off. The main culprits were the usual suspects, complete lack of time and honest absence of knowledge (both mostly on my part). But I do have hopes to reinstate the journal's social media exposure in the near future—and you can always visit and interact with us on Facebook at https://www.facebook.com/groups/BIOLINGUISTICS.Journal. See you there!

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Should It Stay or Should It Go? A Critical Reflection on the Critical Period for Language

Sergio Balari & Guillermo Lorenzo

This paper tries to shed light on traditional and current observations that give support to the idea that language is subject to critical period effects. It is suggested that this idea is not adequately grounded on a view on language as a developmental phenomenon which motivates the suggestion of moving from the now classic concept of language as a 'faculty' to a new concept of language as a 'gradient': i.e. an aggregate of cognitive abilities, the weight of which is variable from one to another developmental stage, and which exercise crucial scaffolding effects on each other. Once this well-supported view is assumed, the idea of 'critical period' becomes an avoidable one, for language can instantiate different forms of gradation, none of which is inherently normal or deviant relatively to each other. In any event, a notion of 'criticality' is retained within this view, yet simply to name the transitional effects of scaffolding influences within the gradient.

Keywords: critical period; faculty of language; behavioral gradients; cognitive hybridization; scaffolding

The end-state is not coded anywhere.
Thelen & Smith (1994: 49)

1. Introduction

When thinking about the suitability of the 'critical period' concept to the particular case of the acquisition of languages, there exist two preliminary questions that cannot be avoided: (i) What is (and what is not) a critical period for the development of any given organic capacity? (ii) Does language actually belong to the kind of phenomena to which the concept may be aptly applied? Surprisingly enough, an ample majority of the sources on the topic of the critical period for language seem to sidestep both questions. As for the second question, while language is customarily referred to as the target of critical period effects in the relevant literature, what one ultimately discovers is that what is suggested there to be subject to such effects are the putative organic bases underlying the acqui-

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sition, storing or use of languages; so on reflection, the corresponding approaches seem to implicitly adhere to the view that languages are not *qua languages* the locus of critical period effects. As for the first question, most views seem to conceptualize the critical period for language as a scheduling of sorts, which inadvertently introduces an unacceptable teleological bias into a developmental matter. In this paper, after reviewing the current consensus about why and how language is specifically thought to be a capacity subject to maturational control or critical period effects, we argue that by means of a clarification of the developmental character of languages and, in parallel, of critical period effects in developmental phenomena at large, one may avoid the cumulative odd implications of connecting these two issues.

In the second section of this paper we review the most important pieces of evidence that have in the last fifty years or so been collected in support of the idea that learning a language is subject to critical period effects. After that, we explain in section 3 that different contemporary views on language base their ideas about the individual process of internalizing a language on problematic assumptions about the boundaries between 'what acquires' and 'what is acquired' in this critical domain of human cognition. Such a critique is aimed at paving the way to a new conceptualization of language that, based upon invigorated versions of the ideas of 'behavioral gradient' and 'cognitive hybridization', we claim is better positioned than its traditional counterparts in order to test how language behaves regarding the nature of the maturational effects to which it is specifically subject. Our position on this particular concern is then unfolded in section 4, where we claim that avoiding some traditional preconceptions, language may unproblematically be incorporated into and treated within the parameters of an emergent theoretical trend that envisions development as the most basic manifestation of life and as a open-ended process in every single entity that manifests it. We thus conclude that despite the fact that certain developmental milestones typically punctuate the growth of language in the individual, it is not affected by any kind of critical period effect proper. We contend in section 5 that an important sample of neurobiological evidence supports such a view. Some other interesting conclusions follow regarding the existence of normal and deviant instantiations of language, with which we briefly close the paper.

2. The Received View: An Overview

It has been known for a long time that children are more apt than adults to learn non-native linguistic systems. To wit, Juan Huarte de San Juan, one of the founding figures of the field of Cognitive Psychology (see Chomsky 1966, 1968; Virués Ortega 2005), expressed with the following words in 1575 what for many continues to sound as a paradox (Newport 1990; Jackendoff 1991):

> The extent to which imagination and understanding seem to be improper skills in order to learn languages is clearly demonstrated by childhood, for while being the age at which men are the less gifted in them both, yet children, as already observed by Aristotle, learn any single language better than older men, in spite of the latter being more rational. And no one needs

> to remember us this, for common experience amply shows it, as when a thirty or forty years old native from Biscay [Basque Country] comes to Castile, and he never learns the Romance language, but if he is a child, within two or three years he looks as if born in Toledo.
>
> (Huarte de San Juan 1575/1991: 151; authors' translation)

> If one wants to learn Latin or any other language, she better does it while still a child, for if she waits until the body becomes rigid and gains its proper perfection, she will never succeed.
>
> (Huarte de San Juan 1575/1991: 60; authors' translation)

Efforts at scientifically clarifying this paradox had however to wait some four hundred years, after the focus was again put on the question by Wilder Penfield and Lamar Roberts (Penfield & Roberts 1959; see Lenneberg 1960), paving the way to the ground breaking work of Eric Lenneberg (Lenneberg 1967), to whom present conceptions and factual knowledge on the issue are profoundly in debt. As a matter of fact, Lenneberg's landmark postulation of a critical period for language acquisition, as an associated aspect to its maturationally controlled character, was a generalization based on his first-hand observations on the recovery patterns from traumatic aphasias at different age ranges, starting from very young children. Specifically, he discovered that children before 3 years (re)acquired their mother tongue almost as if they had not suffered any trauma, but that from that point on the following pattern was attested: From 4 to 10 years, gradual (re)acquisition without residual signs of impairment; around 15 years, gradual (re)acquisition with residual signs of impairment; and from 15 years on, unpredictable pattern of recovery, as it is typical of adult aphasias associated to strokes and so on. From these observations, Lenneberg concluded that a window of opportunity existed for first language acquisition that extended from age 2 until the onset of puberty, out of which normal levels of grammatical competence were not guaranteed at all. The indirectness of Lenneberg's method, far from problematic, was the perfect strategy to remedy the (fortunate) scarcity of related natural experiments—as children almost unexceptionally receive sufficient linguistic stimulation from the very onset of the relevant period, and the (obvious) ethical impediments to perform them in artificial conditions. Nevertheless, some new cases of feral children were known and studied with care after Lenneberg's untimely death, yet only to confirm his predictions (Curtiss 1977; see also Curtiss 1988 for a résumé). Moreover, a whole new field of linguistic research was concurrently being opened, namely the study of the signed languages used in deaf communities, which offered particularly valuable direct information on the impact of age on first language acquisition, for contrarily to non-hearing-impaired children, deaf children may start their contacts with signed languages at rather different ages due to their very different medical and sociological circumstances. Again, conclusions in this new field were in complete agreement with Lenneberg's hypothesis, at the same time that they served to refine the character of the decline along the age axis within the critical period (Newport 1984).

Incidentally mentioned by Lenneberg (1967: 176), but left unexplored in his book, was the question of how the hypothesis applied to the fact that not only the onset of first language acquisition may in certain exceptional situations vary from one to another individual, but that this is actually the most common pattern when people learn second languages beyond their native ones. A logical expectation regarding this would be that an endowment to successfully acquire linguistic systems is unlocked permanently if exercised with a first system at the right time. But as we all know well, this is not what is actually attested. The *locus classicus* of the experimental study of the impact of age effects on second language acquisition is Johnson & Newport (1989), where the following findings are reported. In highly competent bilinguals whose contact with the second language started at different ages, competence is almost indistinguishable from that of native controls when contact started from 3 to 7 years. From that age to 15, a lineal decline is observed as we approximate to this upper age limit; moreover, a strong correlation seems to exist between competence level and onset of exposure in that most people show a similar level within each particular age. Finally, when the contact started from 15 years on, levels of competence are generally (but not necessarily) lower than when it did at previous ages and, more importantly, the strong correlation between competence level and onset of exposure vanishes: People show extremely variable levels of competence at each particular age.

While Johnson and Newport's findings offer a certainly detailed and well-motivated image of the impact of the age factor on the human capacity to acquire non-native linguistic systems, a limitation of this study, obviously enough dictated by practical reasons, was that it exclusively concentrated on the domains of morphology and syntax (or 'compositional domains' in Newport 1990), thus excluding ('non-compositional') aspects of linguistic competence like mastery of phonology or lexical knowledge. In any event, the question of the critical period in relation to phonology had previously been touched upon by Asher & García (1969) and Oyama (1976), with not completely identical but nevertheless rather convergent results. According to both sources in populations not very dissimilar to the one studied by Johnson and Newport, 'foreign accent effects' were increasingly observed in parallel with the increase of the onset of the contact with the non-native system. Besides, a strong contrast was observed between those whose contact started before or after 12 years, while differences intensify only gradually within the 6- to 12-years range. A divergent result of the two abovementioned studies is that, while Oyama's informants whose contacts with the second language started around 6 years were judged within the range of native controls, Asher and García's counterparts were not. Whatever the reasons for this clash, more recent research concludes that the onset of the age of exposure to a second language may be critically reflected on foreign accent effects simply with a delay of one year relative to the first language (Hyltenstam & Abrahamsson 2003; Meisel 2013), with similar observations being applicable to the acquisition of a second dialect by in-migrant families with children (Labov 2010: 416–417). So it seems that the opportunity to attain native-like levels of competence that extends until ±7 years in the morpho-syntactic domain does not apply to the case of phonology, while the onset of puberty continues to be a critical frontier also in this domain. Complementing such observations, research conducted in the domain of lexical knowledge lead to the conclusion that competence levels are not significantly different between native speakers and those whose onset of exposure to the same language started no later than at 11–13 years of age (Weber-Fox & Neville 1996). So the onset of puberty continues to be a Rubicon of sorts also in this domain, but a window of opportunity extends until that age stronger than in the case of phonology and morpho-syntax that allows attaining native-like levels of competence for a longer period.

Of course, many details and discussions could be added to this overview. For example, it has been argued that some aspects of morpho-syntax show a behavior closer to that of phonology than to other aspects of the same domain (Weber-Fox & Neville 1996). In any event, the picture thus far presented is a reliable synthesis of the consensus view, which may be enough to reflect on how data should be interpreted respecting the best criteria currently offered by different fields of expertise devoted to developmental matters. So before closing this section, let us briefly summarize the most basic points that readers should keep in mind in the remainder of this paper: (i) The onset of puberty seems to be a crucial landmark regarding the human capacity to acquire linguistic systems; (ii) before that point, languages can be acquired unproblematically and attaining high competence levels. (iii) In any event, aspects of language seem to be differentially affected along this temporal axis: Namely, (iv) phonology and certain aspects of morpho-syntax seem to be exposed to a decay prior to other aspects of the latter domain, while (v) lexical knowledge related abilities seem to remain stable until the end of this critical period.

3. What Is Language, that It May Develop…[1]

Describing children's speech with adult grammatical categories […] automatically sets the developmental problem in terms of goals rather than in terms of origins.

Michel & Moore (1995: 370)

A complicating factor when dealing with questions like the subject matter of this paper is the multifaceted meaning of the word 'language', which different traditions and authors use to refer to rather disparate things. The opening pages of Chomsky (1986) contain a comprehensive analysis of this state of affairs, a source of difficulties that undermine productive discussions that could eventually lead to more agreed-upon conclusions in critical areas of our understanding of this distinctive feature of the human species. According to Chomsky's examination, concepts of 'language' range from physicalist interpretations, according to which language exists in materialized utterances (meaningful noises, printed material, and so on) to which linguistic properties somehow inhere, to psychological ones, for which such properties are derivative from the mind that projects them into utterances. Correspondingly, 'language' is understood as something 'given out there', in the world external to speakers/hearers ('E-Language'), or as something deeply rooted in the human mind, thus internal to speakers/hearers ('I-Language'). Followers of the respective views thereby understand that the human mind is either a more or less passive receptacle of the regularities

[1] The headings of sections 3 and 4 are inspired by the title of Piattelli-Palamarini (2009).

underlying the organization of utterances, or its source, actively devoted to the acquisition and use of particular instantiations of language according to *a priori* patterns common to them all.

The aim of the following pages is not to decide which one of these competing views is on the right track. As readers will note as the paper moves forward, our position is somehow in a middle ground, as it emphasizes the hybrid character of language (in a sense to be presently made clear), yet believing that it is a mind-based capacity to create and take advantage of such hybrid products. The specific goal of the next sub-section is to show that despite differences, such divergent views as those sketched out above share common or related shortcomings that have hitherto prevented them from offering satisfactory answers to the question whether the critical period concept applies or not to language: Namely, they both rely on problematic assumptions regarding how boundaries should be defined between biological and non-biological aspects of language, or between more or less central biological aspects of language. A subsequent sub-section introduces a new concept of language that seems better qualified in order to overcome these problems.

3.1. Does Language Develop? The **Whats** *and* **Whys** *of a Negationist Consensus*

Children start experiencing the world as non-linguistic beings, who nevertheless attain the mastery of the intricate properties of human languages within a few years, traversing along the way a series of distinctive stages and milestones. This is a seemingly innocent and uncontroversial claim, with which most laypeople would unhesitatingly agree. It is for this reason that the fact comes as a surprise that many past and present approaches to the question of language acquisition are based on theoretical models that incorporate the entailment that language does not belong to the kind of phenomena that properly follow a developmental path of individual growth. This is particularly clear in the case of the Vygotskian and Piagetian approaches that paved the way to the whole field of expertise that we recognize today as Developmental Psychology (Piaget 1962; Vygotsky 1986). In both cases, a gap of sorts is created between cognitive ontogeny proper, on the one hand, and the mental implementation of psychological contents, such as language, on the other hand. Clearly enough, the gap is somehow softened by the promiscuity and mutually reactive dynamics that the corresponding layers ultimately attain; yet it is clear that they belong to different realms, so to speak, for the former (Vygotsky's 'lower functions') can be unproblematically assimilable to other 'natural functions' that undergo normal development (up to the 'formal operational stage', in the case of Piaget), while the latter (corresponding to Vygotsky's 'higher' or 'cultural' functions, or to Piaget's 'intellectual development') are rather the outcome of the cumulative accommodation to such natural architectures of externally given and independently existing contents (corresponding, in the case of language, to what Chomsky 1986 critically refers to as tokens of an 'E-language').

But maybe more striking is the fact that the theoretical perspective that has hitherto adopted the most radically naturalistic stance on language (Chomsky 2000a, 2002) has at the same time encouraged a view on acquisition that under-scores the idea that languages, as a matter of fact, do not develop in the mind of children (Lorenzo 2013).[2] Based on logical arguments having to do with the poverty of the linguistic stimuli to guide *bona fide* processes of rule induction, which may be traced back as far as to Chomsky (1959), Chomskyan linguistics has traditionally embraced the thesis that children must therefore face their first contacts with the adults' linguistic utterances with a mental blueprint of sorts containing detailed information about the basic building blocks and structures of any possible human language; actually, so detailed and/or efficacious as to pave the way to full-fledged linguistic systems in a virtually instantaneous way (Chomsky 1975, 1980, 2000b). In parallel to this central tenet, it is also argued that the seemingly 'non-instantaneous' course that acquiring a language follows is just a deceiving appearance, due to 'difficulties' and 'delays' mostly explicable by the maturational path of the associated neuroanatomical systems that allow the individual to put into use her knowledge of language (Hyams 1986).[3] Attending to this 'consensus view' (Hornstein *et al*. 2005) within the nowadays mainstream 'biolinguistic' position (Boeckx & Grohmann 2013), it is not surprising that the most relevant among recent efforts to explain the maturational effects observed in relation to different modalities of language acquisition, actually point to some putatively language-associated systems as the locus of such effects, instead of suggesting more language-centered proposals. Let us briefly review two of them.

According to one such popular explanation, customarily referred to as the 'less is more' model (Newport 1990), the critical period effect on language is the direct consequence of the accomplishment of the mature version of the short-term (working) memory device on which analytical procedures are executed when trying to discover and fix rule systems from the incoming input in language acquisition processes. The more underdeveloped this device, according to Newport's argument, the better to the rule-extraction operations, for the device operates then on small chunks, which are much easier to analyze and make sense of than the chunks that the device retains in active memory when it attains maturity. Note that the hypothesis is in itself neutral regarding the question whether analyses are or are not based on a preexistent linguistic blueprint, but it nevertheless relies on the premise that the criteria on which analyses are based remain the same all along the process: In other words, what is subject to maturation and gives rise to the somehow deceiving appearance of decay in the capacity for acquiring languages, is actually a language-associated (but not language-specific) memory system, and not properly the language faculty. This

[2] The most explicit statement of this position is by Fodor (1985: 35), who wrote: "Deep down, I'm inclined to doubt that there is such a thing as cognitive development in the sense developmental cognitive psychologists have had in mind".

[3] Two main proposals have been made within Chomskyan linguistics in order to make compatible the strong 'aprioristic' stance of the trend and the obvious fact that languages develop anyway. The first one is Borer & Wexler's (1987) 'maturational hypothesis', according to which languages unfold following a schedule that is an added component part of the preinstalled program also containing the general guidelines of every single language; the second one is Yang's (2002) 'variational hypothesis', which holds that particular parts of the inborn universal grammar unfold as a function of the relative frequency of the corresponding environmental triggers. Both frameworks thus remain strongly anchored in the extreme nativism of mainstream Chomskyan linguistics.

aspect of Newport's thesis makes it particularly suitable to work in association with the Chomskyan nativist stance regarding the specifically linguistic dimension of the language acquisition device. Note, however, that the idea is also compatible with a Vygotskyan–Pieagetian reading, according to which Newport's device serves to decipher entirely public or external rule systems.

According to another serious attempt to explain the effect of age on language acquisition (Ullman 2001), it is suggested that the critical period is actually a side effect of the maturational tension between two memory systems, differently committed to the task of acquiring a language: Namely, a 'procedural' memory in charge of learning, representing and using automatized compositional routines (and thus implied in morpho-syntax), and a 'declarative' memory in charge of factual knowledge and arbitrary associations (and thus implied in lexical knowledge). According to Ullman's hypothesis, there exists a maturational mismatch between these systems, for the former is subject to decay from prepubertal ages, while the latter's decay only begins after the onset of puberty. Due to this mismatch, from the age the critical period is customarily supposed to end onwards, the declarative system is forced to apply in the learning of rule systems governing the composition of words and phrases on the basis of generalizations from models, thus basically treating them as idiomatically frozen items. So again, it is not the language faculty *per se* what is supposed to be subject to an age-associated decay according to this hypothesis, but systems external to this faculty (Chomsky 1995; Hauser *et al*. 2002), which impact on how speakers give language-particular contents to it and how these contents (i.e., language-particular rule systems and associated lexicons) are put into use in real settings.

This is not the place to evaluate the merits of these hypotheses at large. The point of the previous comments is that recent serious efforts to offer *bona fide* explanations to the critical period for language acquisition point to causes that do not even touch the alternative of languages being the true *locus* of such maturational effect, opting instead for explicating it as a delusory side-effect of sorts of the maturational schedule of certain language-related external systems. Such a position actually has a reinforcing effect on the two influential views on language referred to above: Namely, one that privileges the idea that languages are externally or public givens, not properly subject to organic development (but rather to 'acculturation' or 'intellectual development'); and another one that privileges the alternative view that languages are so deeply rooted in the human organism that they do not need to properly develop, being instead subjected to an almost instantaneous 'triggering' process. We are obviously aware that these two stances do not exhaust present-day conceptualizations of language, but we think that other representative efforts of somehow naturalizing language fall some point in between them, without really correcting the main shortcomings of such an intent that we are criticizing here.[4]

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[4] In a nutshell, current theoretical models of language describe a wide spectrum, ranging from cognitivist/functionalist oriented models that highlight the character of languages as socio-cultural achievements assimilable by general cognitive devices and principles (Croft & Cruse 2004), to Generativist oriented approaches that accentuate the biological character of language as a specific cognitive organ (Anderson & Lightfoot 2002). Middle-ground positions obviously exist (e.g., the rather convergent models of Construction Grammar and

For the time being, let us then concentrate on the Vygotskyan–Piagetian and the Chomskyan views thus far reviewed, which, despite their very obvious discrepancies, nevertheless share the rather shocking feature of protecting languages from being properly considered outputs of developmental processes. According to our diagnosis, this is so because both approaches embrace certain pervasive forms of dualism, each of a different pedigree, but both equally problematic: As for the first one, it has been criticized and referred to before in the literature as the 'culture-biology dualism' (Michel & Moore 1995; Oyama 2000a); as for the second one, not having to our knowledge received a specific denomination up till now, we will refer to it here as the 'underpinnings-capacity dualism'.[5]

The 'culture-biology' dualism, on the one hand, is the (usually 'implicit'; Michel & Moore 1995: 72) assumption that traditional forms of shared knowledge and/or behavior correspond to a distinct ontological realm and, correspondingly, that they are individually internalized by means also specific to the objects of that realm. Internalization, the story goes, obviously requires a biological machinery, which is also relevant in the long-term fixation, retrieving and practice of the related activities. In any event, the relation between such an organic ground and the superimposed cultural contents is one of accommodation of the latter to the former, even if more dynamic views have been also implemented where the cultural superstratum impacts on the biological layer, fine-tuning it as to pave the way to further cultural enhancements (Jablonka & Lamb 2005). In any event, the 'biological core' and the 'overlying cultural' stratum remain distinct despite such functional and developmental promiscuities. The most obvious present manifestation of this form of dualism is the common distinction between 'languages' as diversified cultural or traditional accomplishments, and '(the faculty of) language' as the uniform biological background that makes possible the acquisition and use of such cultural artifacts.[6]

The 'underpinnings-capacity' dualism, on the other hand, is the assumption (usually implicit as well) that a further distinction may also be made between 'language', as an organic faculty containing the bare essentials of any possible language-particular system, and the 'underpinnings', 'foundations', 'basis', 'equipment' or 'biological correlate' (see Lenneberg 1967; Lieberman 2006; Boeckx 2013; Piattelli-Palmarini 2013, among others) of such a cognitive organ (Anderson & Lightfoot 2002), notions that customarily, but rather vaguely, refer to aspects of the human anatomy and/or genotype. Actually, under the umbrella

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Simpler Syntax of Goldberg 2005 and Jackendoff & Culicover 2005), which nevertheless do not solve the tensions and dualisms (see below for details) on which modern linguistics is grounded. To wit, Tomasello's (2003) usage-based theory of acquisition, framed within the model of Construction Grammar, explicitly assumes a sharp distinction between the biological and the cultural aspects and processes of language acquisition (Tomasello 2003: Ch. 8) and not surprisingly, it locates the question whether a critical period for language actually exists within the former domain (Tomasello 2003: 286–287).

5 Maybe Kuo (1976: 94) approximates the most to what we have in mind, when criticizing a 'physiology-behavior dualism' typical of the developmental study of behavior.

6 For two recent sophisticated versions of this stance, see Balari & Lorenzo (2013) and Bickerton (2014). The former have however advanced towards a more biologically nuanced position in Balari & Lorenzo (2015b), based on the idea of 'scaffolding' (see below).

of this distinction the whole field of expertise customarily referred to as 'biolinguistics' has grown in the last years (Boeckx & Grohmann 2013), conceived of as "a branch of cognitive sciences that focuses on uncovering the biological underpinnings of the human capacity to acquire at least one natural language" (Boeckx 2013: 1). Such an approach, perhaps inadvertently to its practitioners, contains the problematic entailment that a 'capacity' (and an assumedly 'organic' one) can be dissociated from its 'biological underpinnings', and ultimately that language (an assumedly biological object) can be somehow taken apart from its biology (whatever that means, if it makes any sense at all).

3.2. An Alternative View: Setting the 'Gradient of Language' Concept

What follows is a batch of suggestions aimed at instigating an image of language as the outcome of normal developmental processes, the conceptualization of which does not require something along the lines of the two forms of dualism thus far reviewed. In association with the set of premises that will be put forward in the next section, these suggestions should help (or hopefully so) deciphering the observations introduced in the first section, avoiding the conceptual complications that accumulate around the idea of 'critical period' in developmental studies at large, and specifically in the case of language.

The first such suggestion implies recovering and applying Kuo's (1976) 'gradient' concept to the case of language. Kuo's main contention was that many and very different parts of an organism participate in any one of its various capacities, but obviously enough with "differences in intensity and extent of involvement for each of the different organs and different parts" (Kuo 1976: 92). Besides, the implication of parts as well as the intensity of single parts may vary in the temporal axis. To a certain degree this is due to the dynamics internal to the growing body (or 'maturation', in Schneirla's 1966 sense), but also crucially to the kind and amount of environmental inputs that it receives throughout the process (or 'experience', in Schneirla's 1966 also coincidental sense). Kuo's 'gradient' concept refers to the changing pattern of differently compromised pieces, both internal and external to the organism, which jointly compound the capacities that ultimately manifest in expected forms of behavioral displays. This idea is readily transferable to language.

There is enough consensus now around the idea that language, in a sense, is but a heterogeneous assembly of bodily resources, ranging from motor to intentional abilities, which takes advantage of the component parts of organic systems with rather disparate non-linguistic specializations (respiration, digestion, long term and short term memory, mindreading skills, and so on). This is, for example, the idea under the 'faculty of language in the broad sense' (FLB) concept put forward in Hauser *et al.* (2002). We however disagree with this FLB concept in that it was specifically suggested in a context aimed at preserving another sense in which 'language' names a subset of FLB that may be deemed a language specifically committed part of our brains—the 'faculty of language in the narrow sense' (FLN), which acts as a center of gravity of sorts that *a priori* guarantees the linguistic distinctiveness of the human brain. However, most recent research conducted in order to locate brain activity when executing linguistic tasks points to the direction that every single identified location is also routinely assembled for the execution of other non-linguistic tasks (see Stowe *et al.* 2005; Friederici 2011, for an overview), including the cortico-basal computational core that most reasonably corresponds to or contains Hauser *et al.*'s FLN (Balari & Lorenzo 2013, 2015a). So a more realistic picture than the one privileged by Hauser *et al*. seems to be one according to which no 'faculty of language' exists in the classical sense—not even a 'narrow' one, for such an idea problematically purports that human brains incorporate a language-specifically dedicated main component. Note that what is being questioned here is not a matter of localization, but of functional commitment, and that the more reasonable conclusion is one that supports the idea that language is, from root to branch, a collection of multipurpose components contingently recruited and developmentally stabilized into a coherent functional unit. Also relevant to our point is Hauser *et al.*'s complementary observation that not every organic system that is active when exercising language should automatically be included into FLB, even if it is a necessary condition for conducting such an activity (say, the circulatory system). We think that taking all these observations together, the 'gradient' concept is the one that less problematically can accommodate them, giving grounds to a concept of 'language' according to which the surrogate of the old 'faculty' is an array of interconnected capacities, each one differently involved ("in intensity and extent"; Kuo 1976: 92) in its linguistic specialization. According to this idea, the most involved a capacity in activities other than language, the less central its position in the linguistic functional system (Lieberman 2006), and the other way around. So the idea does not exclude the possibility that a core of highly specialized brain activity exists of a linguistic nature (misleadingly inviting to pinpoint it as the ultimate site of language proper), while at the same time predicting its enacting in other non-linguistic activities as well. The main point of the idea is, however, that language ramifies according to a complex pattern of bodily activities, even if each branch may be described as showing a different extension and a different thickness in the overall pattern.

Two relevant ideas still need to be added to this implementation of the 'gradient' concept to the case of language. The first one has to do with a shortcoming of Hauser *et al.*'s model, which entails a static and adultocentric view on language where each component belongs to the whole at every possible point in time in which observations could be possibly made. Obviously enough, we are not naively reading the model as if it purported that the parts that compound FLB do not undergo processes of growing, maturation, decay, and so on. What we actually mean is that a model is preferable in which, as it happens with the 'gradient' concept, parts are developmentally recruited, so not every one is present at every developmental stage, or not with the same intensity and extent. Thus in our opinion, an accurate image to render the process of the growth of language is one of different developmental paths more or less concurrently occurring, normally leading to increasingly interactive developmental dynamics, progressively bringing about more and more integrated and robust units of function. According to this view, language is not 'more or less' language at any given point of this constructive process. If anything, it is a different form of language, in which components that become very strong at a particular point are weaker or even absent at prior or later points.

The second point to be added has to do with the relevance of aspects external to the organism in Kuo's original formulation of the 'gradient' concept. It is our suggestion that respecting this aspect of Kuo's proposal contains the clue to overcome the two forms of dualisms that run, as argued above, against an integral developmental treatment of language. It is now perfectly known that the impact of verbal stimulation on children starts at an extremely early age, as a matter of fact prenatally (see Gervain & Mehler 2010 for a synthesis). Thus responses to particular aspects of such stimulation are precociously and rapidly being embodied by the growing organisms, in all likelihood starting with aspects of prosodic and categorical perception pertaining to the phonological domain, in such a way that prevents considering from the start that a distinction can be made between a biological agent that acquires and a cultural kit of contents that are acquired. In other words, a 'hybridization' of sorts materializes from the very beginnings of language development, considering which the 'E-language' against 'I-language' dualism vanishes, given the mutually scaffolding effects coming about throughout the process (Griesemer 2014; Balari & Lorenzo 2015b).

Within this new framework, moreover, the distinction between linguistic contents *a priori* belonging to human nature, on the one side, and linguistic contents resulting from historical processes and subject to social transmission, on the other side, lacks most of its original motivation. The growing organic capacity that successively becomes suitable to new scaffolding interactions, until attaining the overall domains of linguistic competence, appears to be a constraining enough force to limit the logical space of possible linguistic outcomes in the absence of *a priori* expectations about how the languages of the world are and differ (see Kajita 1997 and Lorenzo & Longa 2009 for two congenial approaches).[7] For the sake of clarity, let us elaborate this a little bit with some relevant illustrations.

Within the framework thus far presented motivation is lacking for positing a language-specific bias towards, for example, structure-dependent rules (*versus* linear-dependent ones that children never seem to consider; see Chomsky 1975, among other places),[8] which according to the Chomskyan view is literally coded in the brain of children prior to any linguistic experience. Alternatively, one may confidently conceptualize the robust observations gathered in this specific area of research by just considering what children know about language as they are particularly developing it at each developmental stage. In a nutshell, we suggest that from a very early age on they most probably scan different sequentially organized incoming stimuli on the grounds of a computational device with a working memory resolution that makes 'structural constraints' to be naturally

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7 We are grateful to an anonymous reviewer for suggesting us the connection between Kajita's framework and our developmental ideas.

8 The *locus classicus* in the study of this bias is question formation in 'subject-cum relative sentences', as in *Is the man who is tall happy?*, with a fronting operation of the *main* verb. In constructing these sentences, children never make errors that could be interpreted as if they were considering a linear alternative (e.g. 'front the *first* verb'), like **Is the man who tall is happy?* Children do commit errors when first producing this kind of sentences, but curiously enough they again have a structure dependent character: For example, sometimes they repeat the main (*Can the man who is tall can see Mickey?*), but never the first verb (* *Is the man who is tall can see Mickey?* or similar alternatives; see Crain & Nakayama 1987).

expected (Gervain *et al.* 2012). We specifically mean that from a certain degree of resolution on, such a device will be able to detect and to retain in working memory sequences of items (say, *x x x x … x x x*) for the time and/or with the intensity required in order to capture long distance dependencies (say, $x\ x_1\ x\ x\ \ldots\ x\ x_1$), of the sort instantiated in linguistic strings like *John claimed*$_1$ *that she was wrong emphatically*$_1$—where subscripts serve to annotate the points at which a *main* sentence is interrupted by an *embedded* one and restarted again. Such a degree of memory resolution is thus a computational requirement for the kind of nested relations through which structure dependent relations hold, obviating other putative linear/numerical constrictions. So, contrary to Chomsky's (2007: 7) suggestion, there seems to be a good reason for children to adopt the 'structural stance' even in the absence of a genetically coded UG instruction, for this is how they optimize a system of computation already in place for the scanning of incoming sequential stimuli—which if anything, might be conceptualized as a 'third factor' effect of sorts (Chomsky 2005).[9] Children consequently behave as if they were (so to speak) 'linear blind' (Longa & Lorenzo 2012). Our position is thus that as soon as children apply such a computational device to the flow of speech they are being exposed to, a linguistic hybrid of sorts is automatically created in their minds, the regularities underlying which are unavoidably interpreted as structure-dependent. Note that no piece of propositional knowledge establishing in advance a universal property of languages seems to be required for such a constraining effect to follow. Children may be capable of deriving it from the unique perspective of the language particular gradient that they are constructing.

A next logical expectation from this idea is that as development goes by, it successively creates the grounds for constraining further aspects of the hybridization process. For example, a language-particular case system (either following an accusative pattern or an ergative one) may within this framework be conceptualized as a hybrid outcome of the structure-dependent asymmetry detected among the verb's most prominent arguments, on the one hand, and a system of formal marks (case morphology proper, agreement, and so on), on the other hand, regarding which the incoming stimulation contains rich positive evidence (see, for example, Uriagereka 2007). Again, no particular expectation about how languages actually differ in this area of grammar seems to be required in order to constrain how the corresponding patterns are fixed. Thus the resulting image is one where over-arching or high-level principles of organization (like 'structure-dependence') pave the way to more nuanced or idiosyncratic ones (like the formal patterns chosen for marking specific structure-dependent relations), which in their turn probably reinforce the supporting operative principles (Balari & Lorenzo 2015b). For example, according to Crain & Nakayama's (1987) classic experiments, the bias towards 'structure-dependence' is fully operative in

[9] Chomsky actually incorporates into his list of third factor effects "principles of data analysis that might be used in language acquisition and other domains" and "principles of structural architecture and developmental constraints," but he emphatically adds that they correspond to "principles not specific to the faculty of language" (Chomsky 2005: 6). So ours is a welcome conclusion from a minimalist perspective, but one that clearly goes counter the classical view of language 'as a faculty'.

question formation tasks at 3;2 (years;months), but according to Lidz et al. (2003) it can already be attested at 1;6 in relation to other structure-dependent phenomena, like *one*-substitution. As for case marking patterns, a study by Elosegui Aduriz (1997) shows that full mastery of both the ergative (Basque) and the accusative (Spanish) patterns of case marking is attested on bilinguals at 3;3, with the respective key case distinctions ('ergative/absolutive' and 'nominative/accusative') emerging almost concurrently at 2;2–2;5. Such a partially overlapping chronogram between the precocious sensitivity to structure dependency and the subsequent fixation of a case system pattern, seems particularly fit to support Balari & Lorenzo's (2015b) reinforcing loops hypothesis.

Note that the reading according to which children's first language acquisition occurs as if they were respecting a 'logical flowchart' (Baker 2001) that they know in advance (see also Yang 2002), thus may be thought of as somehow motivated, yet it is simply the effect of viewing the process from the misleading perspective of the ultimately attained outcomes. We alternatively contend that only by inverting such a logic may one gain a true developmental perspective, for it is obvious that constraining influences on development must work the other way around: i.e. they must be derivative from the ongoing constructive process of the hybrid gradient, on which internal and external forces conspire with the hybrid-in-the-way itself to channel its own fate.

Turning to the main concern of this paper, we believe that the resulting 'gradient of language' concept fits particularly well with the basic maturational observations introduced in the first section of this paper and other, more recent findings to be reviewed in section 5. We note above that evidence has accumulated after years of intensive research that language is not monolithically affected by a single critical period effect. Effects seem to operate in a more selective way, with the ones touching the phonological domain affecting individuals earlier and being more noticeable afterwards, the ones touching lexical knowledge having a later chart of appearance and being less intrusive, and the ones touching morpho-syntax being in a middle ground both in timing and affectation of the acquired competence. Current research (Meisel 2013) even points to a more nuanced view, in which domains may eventually be parceled out in sub-domains motivated on maturational grounds. Such a state of affairs seems in perfect agreement with the ideas put forward in this section, which predict a complex pattern of maturational milestones as the gradient of language unfolds in time.

Moreover, development shows that the 'faculty-to-be' is not like a miniaturized version of the adult steady counterpart at every different stage that we may arbitrarily choose to study. So a more accurate approximation to these findings is one that envisions them as the chronological unfolding of an ever-changing gradient, in which a mostly phonologically biased capacity paves the way to increasingly complex units of function where the non-compositional lexical component and the compositional morpho-syntactic one take successively the lead in the complex. Correspondingly, the 'behavioral potential' (Kuo 1976) of the evolving capacity advances from its original link to social cognition (e.g. social attachment and maternal bond by means of acoustic cues; Locke 1993) to the open-ended functionality of adult versions of language (Chomsky 1975, 1980).

3.3. The 'Gradient of Language' and the 'Modularity of Mind'

A relevant question raised by an anonymous reviewer has to do with how the 'gradient of language' concept that we are entertaining here relates to the 'modular' view on the organization of mind, a framework within which the 'faculty of language' has traditionally been perceived as a welcome component. Two points of clarification are in order before trying to settle our particular take on this issue. The first is that the 'gradient of language' is a category that primarily belongs to the developmental analysis of language, while 'modularity' is a category that primarily pertains to the study of mind as a collection of full-fledged or steady cognitive components. So in a way, one is incurring in a category mistake of sorts when trying to evaluate them as competing hypothesis. Notwithstanding, and this is our second point of clarification, one may legitimately be interested in deciding whether they are or they are not coherent hypotheses from their respective (diachronic and synchronic) points of view. The more so attending to the fact that there is not a single or monolithic concept of 'modularity' (Robins 2015), so the door is clearly open to the possibility that one or another 'modularity' concept is the most congenial with the developmental view on language advanced in this paper. Let us briefly dwell on this.

Obviously enough, links between 'modularity' and 'development' have previously been suggested. To begin with, the ninth and last of Fodor's (1983) diagnostic features of modularity is a developmental one, according to which each module exhibits "a characteristic pace and sequencing": "[T]he neural mechanisms subserving input analysis [a.k.a. modules; SB&GL] develop according to specific, endogenously determined patterns under the impact of environmental releasers" (Fodor 1983: 100). Another well-known connection between 'modularity' and 'development' is the one suggested in Karmiloff-Smith (1992), according to which "the mind becomes modularized *as development proceeds*" (Karmiloff-Smith 1992: 4). But for different reasons, none of these seminal approaches appears to be congenial with the 'gradient' view advanced here. As for Fodorian modules, developmental determination obtains *via* a rich, pre-specified base of innate information at their disposal (Fodor 1983: 100–101), which contradicts the dynamic and contingent process of module construction that the 'gradient of language' concept should in any event require.[10] This is alternatively very much in the spirit of Karmiloff-Smith's model. However, Karmiloff-Smith's developmental perspective mainly boils down to the idea that module construction is a pace along a series of distinctive representational formats of increasing explicitness within particular mental specializations. The kinds of horizontal negotiations and dynamic accommodations between different bodily capacities that define the 'gradient' concept seem however alien to Karmiloff-Smith's idea.

Should we consequently quit trying to unite the 'gradient' and the 'modularity' concepts? Not necessarily, for versions of the latter exist that seem congenial with the former, particularly Carruthers' (2006) 'weak modularity', which envisions 'modules' as emergent and highly interactive functional units, maybe

[10] For the same reason, 'massive modularity' (Pinker 1997) is not an approach congenial with a 'gradient' concept of language either.

implemented with an enhanced version of Segal's (1996) 'diachronic modularity', namely one that privileges inter-domain penetrability as a developmental strategy to build functional modules proper as architectural units at relatively more stabilized stages. Along similar lines, Lieberman's (2006) 'functional systems' model also shows a desirable degree of compatibility with the 'gradient' developmental concept, for functional systems are modular in the sense of being well-defined specialized architectural components, without precluding their sharing specific sub-components. We don't see any in principle inconvenient in adding to Lieberman's 'weak modularity' the idea that by sharing components and activity, functional systems may help each other in their respective constructive processes.

But in the end, the substantial aspect of this issue revolves around the empirical consequences of bounding the fate of the 'gradient of language' concept with a particular vision of the architectural organization of mind—namely, a weak version of the modularity thesis. In this respect, two promissory areas in which predictions may be advanced and confronted with known facts are neurobiological findings regarding neural circuitry underlying putatively modular abilities and the study of breakdown patterns affecting them (Fodor 1983: 98–100). We devote sections 5 and 6 to each of these sides of the matter.

4. … and What Is Development, that It May Apply to Language

Development is a serially ordered process that is identifiable across time, but it is not defined by time.

Michel & Tyler (2005: 156)

This section inevitably requires a metaphysical opening. There exists a long-standing persuasion that the workings of nature are alien to the human system of categorization and explanation based on teleological categories: i.e. aims, goals, stages towards, expected paths and achievements, intermediary points, and so on, all of which entail the endeavors of rational/intentional agents (Dennett 1987). But as a matter of fact, the presence of such a system is pervasive in many domains of the natural sciences (for a critique in relation to current functionalist-oriented biological thinking, see Fodor & Piattelli-Palmarini 2010). Kant was particularly aware of this shortcoming of the life sciences and devoted to the topic most of the second part of his *Kritik der Urteilskraft* (1790). Kant's position was, however, somehow compliant with the teleological perspective, in that he understood that in as much as conscience is not lost that the rational/intentional categories are inevitably linked to the means by which natural causation becomes understandable from a human frame of mind, and not constitutive parts of the biological *explananda*, it may be maintained with no serious harm to the scientific enterprise. In any event, that the propensity of transferring the intentional stance from the explanatory strategy to the object being explained is a strong one is clearly attested by the fact that many functionalist-oriented approaches, particularly in the field of evolutionary biology, continue to take for granted that "teleological notions are a distinctive and ineliminable feature of biological explanations" (Allen 2009). The general position underlying the following pages is that approaches that cut off the 'look ahead' signature of teleology from interpretations of natural facts are better positioned to offer *bona fide* explanations in the corresponding domains than competing frames containing residues of the rational/intentional stance.[11]

We thus subscribe here a metaphysical framing for development along the lines put forward by developmental systems theoreticians (Oyama 2000a, 2000b; Oyama *et al*. 2001) or probabilistic epigeneticists (Gottlieb 1997, 2007), according to which nothing is contained (not even required) within organisms (plans, programs, blueprints, pre-installed structure, an so on) in order for development to unfold following highly predictable paths leading to highly predictable outcomes: It suffices to recruit resources anew and to repeat processes afresh in order to expected (yet not completely guaranteed) outcomes to obtain, given the chances that history offers to such contingent cycles to gain robustness and long term stability. Variability inevitably becomes a *sequitur* of such a take on development, ranging from the minor signatures of individuality to deleterious forms of teratology, through the generation of innovative morphotypes with a prospect of evolutionary stabilization. This general view also adheres to the idea, explicitly held for example by Minelli (2003, 2011), that development is to be taken as the most distinguishing feature of life, if not completely identical to it, and consequently that it may also be taken as an open-ended process, where no clear points of termination are to be searched and assigned. This is not in contradiction with observations inspiring the contrary conclusion that processes of maturation exist that lead to more or less durable steady states; but the default position within this general framework is that they do not, and that organic matter is an always evolving (i.e. developing) kind of stuff. It is also a consequence of this overall view on the organic realm that organisms do not evolve the means to plan, program, preview or prefigure their developmental fates; rather, organic resources at all levels of organization become liable to persist that directly benefit the reiteration of advantageous developmental cycles. According to Minelli's (2003) motto, development exists (primarily at least) just for its own sake.

4.1. The Position of the Gradient of Language within the Theory of Development

Modern linguistics has proven particularly refractory to the kind of non-teleological approach just reviewed when confronting the problems of language acquisition. Traditionally, the image of acquiring a language within the learning paradigm was one of 'successive approximations' (Skinner 1957) to the adult external models. But once the conclusion was settled that the primary linguistic data offered to children lack models and are very opaque in relation to crucial aspects of the grammatical competence already attested at very early ages (e.g. the structure-dependent character of most rules of grammar; see above), the consequence was not to abandon an adultocentric stance regarding language

[11] The position does not entail an eliminative stance concerning the status of 'intentionality' as a putative biological category in the domain of the mental, in the sense, for example, of Searle (1992). The position rather points to a stance according to which 'intentionality' is a putative biological *explanandum*, but is not a legitimate biological *explanans*.

acquisition. Contrarily to this, the common move was rather a generalized acceptance of the idea that the adult model is almost completely given from the start (see Chomsky 1981, Baker 2001, and Yang 2002, for some instantiations of the thesis), thus radicalizing adultocentrism with the extra assumption of a performationist stance, which adds to the 'aimed at' character of the process a strict 'determination' and 'tutelage' from the inside of the individual that acquires the surrounding linguistic conventions.

But things may become very different once the idea of a 'faculty of language', virtually preformed and fated from its very onset, is replaced with the alternative 'gradient of language' concept along the lines suggested in the previous section, as the view is particularly fitted to accommodate what may be seen as one of the central axioms of a theory of development (in the sense of Minelli & Pradeu 2014): "Development emerges *from* earlier conditions; it is not directed toward later conditions" (Michel & Moore 1995: 21). Earlier conditions are of course completely ignorant of their intermediary or ultimate fate (if at all), which may change radically, both in structure and functionality, as component parts appear, grow, associate with or dissociate from each other, gain or lose centrality within the whole, and so on. To wit, as pointed out above the gradient of language seems to have its starting point in an effective detector of the quasi-musical properties of adults' utterances (pitch contour, rhythm types, and so on). The fact that trials aimed at unveiling this ability customarily test them in experimental settings in which newborns are defied to tell apart stimuli belonging to different languages (Gervain & Mehler 2010), may help to create the image that it is a specifically language-devoted skill. But, as a matter of fact, it probably serves in most real situations to create and consolidate the newborn's affective and social bonds with her caretakers (Locke 1993). It is now a well-attested fact that later on this ability serves to the children as part of a phonological and statistical 'self-aid' kit with which they start breaking the continuous speech flow into component parts corresponding to 'word candidates', which they rapidly associate with presumptive meanings (or definitely discard as true words) (see Guasti 2002: 74–80, for a presentation and relevant bibliography). According to the point of view that we are adhering to here, it is a wrong conceptualization of such a developmental sequence that newborns' musical skills are there from the start 'in order to' facilitate segmenting the speech flow into word-like units, as a part of a 'program' of sorts in which language-specific categories (like 'word') are moreover anticipated. For the sake of the developmental explanation, it suffices to say that the corresponding perceptive abilities transform the incoming stimuli into one compounded of segments that children match very fast with meaningful associations (Carey & Bartlett 1978; Markson & Bloom 1997). Thus *from* abilities related to the musicality of sequences, a lexicon of arbitrary associations starts growing as an aspect of the child's declarative memory (Ullman 2001); a very different claim that saying that the former are *directed toward* the latter. The advantages that follow from having a catalogue of arbitrary pairings of sensory-motor and conceptual percepts may act, obviously enough, in the sense of entrenching the original underlying capacities, but they do not transform the latter into an anticipation of the lexicon to come in any meaningful sense. Similarly, it is a reasonable assumption that as soon as the child breaks the continuity of speech flow into component pieces, the stimulus now perceived as sequentially organized starts to instigate and strengthen procedural activity (Ullman 2001) capable of detecting and memorizing combinatorial patterns,[12] paving the way to productively using them in due time. The number of lexical items known by children at a given developmental stage serves as a good predictor of the moment at which syntactic abilities emerge (a schematic or telegraphic syntax appears when they are entering the hundreds, and around the four hundreds a more productive 'adult-like' one; see Guasti 2002, for a synthesis), which may actually be used for diagnostic concerns (Locke 1997; Bates & Goodman 1999). But again, the only sensible way of conceptualizing these facts is that *from* abilities related with the identification of discrete units in the stimulus and the increase of items within declarative memory, children obtain the opportunity of feeding the development of syntactic procedures or routines.

A final point of clarification is in order before closing this sub-section. As important as clarifying that there is no 'directed toward' development is the complementary task of explaining that when enumerating cognitive skills involved in the acquisition of languages by children, one is not really contemplating a number of instrumental means to acquire languages: What one is contemplating is just language, period. Language has no other reality and cannot be taken apart from the skills of concern, contingently recruited as development progresses and thus becoming part of a complex pattern of dynamics, where the position and the weight of each relatively to the others varies along the way. So similarly to how we previously saw that the distinction between 'languages as given out there' and 'language as a internal faculty' (as in the 'biology-culture' dualism) blurs within our developmental frame, we believe that it may complementarily also help blur the distinction between 'the faculty of language' and its putative 'underpinnings', for the resulting image of language as a developing phenomenon is one of a complexly evolving system that metamorphoses from (for example) an English acoustic and statistical detector successively into an English fragment of declarative memory, and into an English set of memorized compositional procedures, and so on (and the same with whatever other language one might be interested in observing from such a longitudinal perspective).

4.2. The Critical Period in Critical Condition

We are now in the position of answering the main question that motivates this paper, and in a way that does not differ too much from the answer given by Zing-Yang Kuo decades ago regarding the suitability of the critical period concept to developmental processes at large: "[…] the concept of critical period […] is of dubious scientific value" (Kuo 1976: 115). Sure enough, Kuo's is the most reasonable conclusion when one accepts the dynamic and ever evolving character of every single aspect of the cognitive/behavioral make-up of a species. So once the premises are settled (i) that language is a complexly growing system in which rather disparate skills are contingently recruited along the way (instead

[12] Gervain *et al*. (2012) have experimentally shown that newborns already display similar pattern-identification skills in relation to meaningless syllabic stimuli.

of a well-delimited faculty from the start), and (ii) that the development of language entails the 'negotiation' of predominance relations among the successively assembled abilities, corresponding to the upsurge of new emergent functionalities (instead of a monolithically given kind of functionality within the reach of children from the outset), then a very different idea of 'criticality' in the realm of cognition/behavior is due.

We also agree with Kuo that a 'criticality' concept may be nevertheless saved if conceived of in the physicist's sense in which it names 'points' at which certain states of matter undergo characteristic modifications (e.g. "the temperature above which a substance in gaseous form cannot be liquefied no matter how much pressure is applied"; Kuo 1976: 115). A congenial notion has also been implemented within the dynamic systems approach to the development of cognition and action advocated by Thelen & Smith (1994) under the interchangeable labels of 'critical' or 'transition' points and in their turn taking inspiration from the behavior of chemical reactions. In any event, the term boils down to the same idea of points at which continuously evolving complexes acquire the potential to engage in qualitatively new kinds of processes and/or exhibit qualitatively new patterns of activity. Irrespective of the label one prefers to choose, the distinguishing feature of this new concept of 'criticality' is that it puts the stress on the new kinds of events or states to come (given antecedent developmental events and present conditions), instead of focusing (as it is the case of the classical 'critical period' concept) on the potentialities left behind (as windows of opportunity close following more o less rigid schedules).

In the realm of language, similar 'critical (or transition) points' may be posited, for example, regarding the critical amount of lexical units that may already pave the way to a 'more procedural' than 'declarative' style of language, as observed in most children from their second year of life. It is our suggestion that this is the model of 'criticality' that ought to be generalized to the experimental evidence reviewed in the first section of this paper. Thus 'foreign accent' effects observed after minimal delays in the exposure to a second language, for example, should simply be seen as the ('normal' or 'characteristic') kind of reflex in production of a phonological system acquired *from* a certain maturational stage of the ongoing language gradient (and/or a certain degree of exposure), instead of as a 'deviant' outcome of an ill-timed exposure—the reading that one would be more prone to follow with the lenses of an orthodox 'critical period' concept. This interpretation fits in nicely with the observation that a complex of nervous fibers exists connecting posterior auditory and more anterior pre-motor left hemispheric areas, which is involved in phonological processing tasks and attains full maturity rapidly after birth (Friederici 2011). Thus, two partially different patterns of phonological assimilation naturally follow, due to the more or less earlier exposure to the relevant stimulation. Similarly, the 'decay' in the capacity of assimilating the morpho-syntax of a second language, customarily dated as concurrent with the onset of puberty, should rather be conceptualized as the point *from* which, as if mirroring developmental effects previously observed in prepubertal acquisition, the language gradient becomes more 'declarative' than 'procedural' in the relevant domain, so rules are now instantiated following a less automatized and more conscious style, maybe closer to the style with which words and idiomatic phrases are instantiated at previous stages of the gradient.

It has been suggested that similar corrective effects may be obtained by completely giving the 'critical period' concept up, and adopting instead an alternative notion of 'sensitive period' (Schneirla & Rosenblatt 1963; Bornstein 1989; see Locke 1993: 296ff., for the particular case of language). While we agree that the concept of 'sensitive period(s)' is a well-motivated and relevant one in every single developmental realm, yet we have an objection to rise concerning whether it truly is the right alternate to its purportedly classical antecedent. As explained in Michel & Tyler (2005: 160), the idea of 'sensitive period' offers an escape hatch to the "clock-like, built-in or predetermined periods in development",[13] replacing such teleologically connoted notions with a view according to which development itself produces distinctive stages, with an intrinsic 'variability in onset/offset (timing)', each being constructed *from* their (causally active) predecessors. We have not, as previously said, any conceptual objection to an idea of 'sensitive periodicity' thus defined; but we think that, if any, it may serve as a conceptual surrogate not of the 'critical period' concept, but of the idea of 'development' in itself, in case one decides to waive the old word with all its odd connotations and to introduce a brand new one to name what, in the end, is just development as usual. Conversely, it is our opinion that a concept of 'criticality' along the lines of this sub-section adds something substantive within such a renewed view of 'sensitive' development, so they are complements instead of competitors of each other.[14]

5. Some Neurobiological Evidence

It is important to see that there is not a cultural level above the *psychological above* the *biological, but many interpenetrating ones.*
William Wimsatt (2007: 136)

Throughout this paper we have been developing an alternative conception of language acquisition, based on the notion of 'language gradient' as a natural replacement of such theoretical constructs as 'the faculty of language' on which classical models of language acquisition are based. It has been our contention that with the notion of gradient a much more integrative view of cognitive development in general and language acquisition in particular is made possible, with the net effect of making these processes virtually indistinguishable from the onto-

[13] But maybe not always: For example, Bornstein's (1989) is an exhaustive, but rather conventional framework for disentangling 'sensitive period' effects, where 'sensitive' is perfectly interchangeably for 'critical' in the most traditional sense.

[14] Actually, Schneirla & Rosenblatt (1963) insisted on clarifying that their own concept of 'sensitivity' as applied to development simply boiled down to the idea (maybe the platitude) that developmental events are fuelled, *at every developmental stage*, by the conspiracy of the state already attained from previous events and the environmental inputs that the organism becomes reactive to given that particular state. Therefore, their 'sensitive period(s)' concept does not entail (but also does not exclude) the identification of characteristically 'critical' landmarks.

geny of the organism. In other words, our proposal promotes cognitive and linguistic development to the status of *bona fide* ontogenetic processes rather than ascribing them to the class of processes traditionally tagged as 'psychological development' taking place through the interaction with a properly articulated biological substrate. By blurring the biology / psychology-culture divide, we submitted that a novel interpretation is possible—from a non-teleological and non-adultocentric perspective—of an ample body of acquisition data concerning in particular the long-debated issue of critical periods. The pertinent question at this point is whether our view finds some independent support. This is the main purpose of this section.

In the last decade, a number of interesting works have been published on the matter of critical periods, with special reference to their molecular basis. It should be pointed out from the outset, however, that, as we suggested in the last paragraph of the previous section, critical / sensitive periods are the hallmark of development, in the sense that the emergence of virtually any developmental product is restricted to a more or less flexible time window (Hensch 2004). Thus, the question eventually reduces to what is meant by 'criticality': Either (i) a pre-specified or innately determined point in developmental time where the opportunity window is closed, or (ii) a stage in development in which, for whatever reason, the appropriate scaffolds are not present with the consequence of precluding the emergence of some expected developmental products and driving the process through a different, perhaps novel pathway.

Neurobiologist Eric Knudsen has contributed a number of important works to the understanding of critical periods. In a review article published in 2004, he proposed the following definition:

> The term 'sensitive period' is a broad term that applies whenever the effects of experience on the brain are unusually strong during a limited period in development. Sensitive periods are of interest to scientists and educators because they represent periods in development during which *certain capacities are readily shaped or altered by experience*. Critical periods are a special class of sensitive periods that result in irreversible changes in brain function. (Knudsen 2004: 1412; our emphasis)

Two points are of particular importance here: (i) the decisive role played by experience in defining the boundaries of the sensitive period, and (ii) inclusion of critical periods as a subclass of sensitive periods. A third factor also mentioned by Knudsen is that "although sensitive periods are reflected in behavior, they are actually a property of neural circuits". In this latter connection, Knudsen establishes a typology of neural circuitry on the basis of its inherent stability or plasticity. Thus, at one extreme of the continuum defined by this typology, we find those circuits that, for obvious reasons, possess an initial pattern of connectivity that is extremely resistant to change, due to the strengths of their connections. These circuits are built up essentially through endogenous processes, i.e. independently of experience, and are often those on whose functioning depends the activity of other, more plastic circuits, as it is the case of those "circuits located near the sensory or motor periphery, such as in the retina or the spinal cord" (Knudsen 2004: 1413). At the other extreme of the continuum are those circuits with an ample range of potential more or less stable patterns, attainable as development proceeds and experiential input through the sensory systems is supplied as a function of the availability of the appropriate stimuli in the environment. In between these two extremes, a variety of neural circuits exists with different degrees of stability and sensitivity to change.

To be sure, Knudsen's preferred metaphor to represent the development of neural circuits is that of the well of attraction or stability landscape, already familiar from dynamical systems theory or the developmental landscapes Conrad Waddington used to illustrate his notion of canalization. In this sense, less plastic circuits will be those with deeper wells of attraction and, consequently, only capable of changing if high amounts of energy are spent to perturb their strongly canalized pathways. Less stable circuits, on the other hand, will be reactive to weaker doses of perturbation, but will nevertheless be able to reach one or more stable patterns through the action of repeated experience (p. 1417) or, in the absence thereof, through the operation of "homeostatic mechanisms, intrinsic to neurons and circuits, which attempt to maintain a minimal level of impulse activity in developing neural circuits" (p. 1420).[15] Note already how close this idea of criticality is to the one Kuo urged us to adopt in the 1970s.

A number of conclusions can already be drawn from this. First and foremost, sensitive/critical periods are not series of windows of opportunity that open and close following a predetermined developmental schedule but rather stages characterized by variable degrees of plasticity in a global process tending to maximize stability.[16] Stability is not guaranteed, however, as it is highly dependent on the degree of plasticity of the system, on the one hand, but also on the intensity of the experience, on the other hand. Accordingly, even the most stable circuits are liable to change if perturbations are strong enough to make them revert to earlier stages or to follow alternative pathways, although, logically enough, the more deeply entrenched (Wimsatt 1986, 2001, 2007) the circuit the more difficult will be to perturb it, as highly entrenched systems often act as scaffolds to later-developing systems, and alterations of their functionality may have negative effects on the global stability of the whole system to the point of being deleterious.[17] Thus, the boundaries defining the onset and termination of sensitive/critical periods are not pre-established by some developmental clock, but are nevertheless more or less predictable given the very same dynamics of developmental processes seen as continuous chains of stabilization and scaffold-

[15] We will not offer here a detailed account of the neuroanatomical and molecular events associated to sensitive periods, but the reader may refer to Knudsen's paper for a general exposition. For a more detailed review, with special emphasis on visual and auditory circuits, the work of Takao Hensch and his team is particularly relevant; see Hensch (2004, 2005), Morishita & Hensch (2008), and Barkat *et al*. (2011).

[16] This is therefore perhaps the only sense in which one can say that developmental processes are goal-directed. This is nonetheless an interpretation of goal-directedness that is much closer to the physical notion of thermodynamic equilibrium than to the traditional teleological definition based, for example, on ideal adult models.

[17] In Balari & Lorenzo (2015b) irreversibility was highlighted as one of the hallmarks of developmental products. In light of the discussion in the text, this is clearly too strong, but the idea may be easily reformulated in terms of generative entrenchment and the degree of stability of the said developmental products.

ing effects.

This hierarchical organization of neural circuitry, with downstream systems dependent on other, more entrenched ones prompts, Knudsen's recommendation that researchers should be wary at the time of positing critical periods at the behavioral level (Knudsen 2004: 1421). The reason is that downstream systems tend to remain plastic, retaining the ability to compensate for potential abnormalities stabilized in the most entrenched ones. This situation may have the effect that irreversibility at the circuit level need not necessarily mean irreversibility at the behavioral level. In our opinion, this observation reinforces our gradient view of development, since behavioral/psychological categories like 'language', based on idealized adult models, tend to be much too coarse-grained to be applicable at all stages of the developmental process. As we pointed out in a previous section, if anything, 'language' is 'language' from the very beginning, but with the specific properties characteristic of each stage of the process attained through the participation of the different elements of the gradient.

We are ready to accept, however, that this does not really solve the question of 'language' in one direction or another. To be sure, despite Knudsen's advice, all the experimental data reviewed earlier could still be interpreted in the sense that some language specific system is susceptible to show criticality effects. For one reason: That sensory systems undergo maturational processes is well-known since, at least, the 1960s with David Hubel and Torsten Wiesel's experiments on the visual cortex of kittens (see Wiesel 1982 for an overview). Thus, one could argue, language is just another example of this, but one that does not contradict the idea that there are language-ready systems that require linguistic input to unfold or the idea that language, as a cultural phenomenon, requires a properly developed biological substrate to be acquired. For example, a celebrated experiment performed by Mayberry & Lock (2003) could receive any of these two interpretations. In a nutshell, Mayberry and Lock analyzed language performance of early *vs*. late acquirers of both spoken and signed languages in order to test "whether the onset of language acquisition in early life is related to the subsequent ability to learn any other language for the remainder of life, independent of the sensory and motor modalities of the first or second languages" (Mayberry & Lock 2003: 370). All tests assessed syntactic abilities through a variety of tasks comprising, for example, grammaticality judgments, sentence to picture matchings, etc. Perhaps not surprisingly, the final results suggested "that language experience during human development dramatically alters the capacity to learn throughout life" (p. 380). Mayberry and Lock's conclusions are somewhat puzzling. Thus, while they dismiss an interpretation in terms of a genetically specified ability and favor the idea of an epigenetic process "whereby environmental experience during early life drives and organizes the growth of this complex behavioral and neurocortical system" (p. 382), they characterize the critical period for language as "a time-delimited window in early life where the degree and complexity of neurocortical development underlying the language system is governed" (p. 382). The conclusion is puzzling, in our opinion, because, while the authors are ready to accept that cortical structures develop, nothing of the sort applies to language, which is 'out there' waiting to be learned by the appropriate structure. It certainly does not escape the teleological, adultocentric model we are arguing against in this paper.

So, the question is: Are there any acquisition data unequivocally or at least strongly supporting the gradient view? We believe there are, and we would like to close this section by briefly reviewing them.

In the early 1970s, Peter Eimas demonstrated experimentally that 1-month-old human infants are capable of perceiving speech sounds categorically (Eimas *et al*. 1970, Eimas 1974). Moreover, it was also shown that the ability to categorically discriminate certain distinctions at a very early age was lost as acquisition proceeded of a language in which such contrasts are not functional (Eimas 1975). These results were interpreted as evidence for the existence of a universal and innately specified human- and language-specific phonetic detector system operating in a selectionist mode as acquisition proceeded, stabilizing on a system of phonetic categories on the basis of phonetic evidence provided by the environment. Such interpretation was soon called into question when categorical perception of speech sounds was also experimentally observed in a non-human species (Kuhl & Miller 1975, 1978). This is an old story, and a well known one. Clearly, categorical perception of speech was not an ability based on some language-specific processing device, but rather the human system of phonetic categories was constructed on the basis of oppositions to which the mammalian or even the vertebrate ear is highly sensitive.[18] This story also signals the beginning of a fruitful research program on the acquisition of linguistic capacities that strongly supports the developmental view we have been defending in this paper.

In 1991, Kuhl (1991) described the 'perceptual magnet effect' in the processing of speech sounds by human adults. In essence, Kuhl's finding was based on the earlier discovery that speech categories have an internal structure and are organized around a prototypical center (Grieser & Kuhl 1989). The prototype then acts as a 'magnet' during perceptual tasks in such a way that all stimuli assigned to the category are interpreted in terms of the prototype. According to Kuhl (1991), this effect would explain the gradual loss of the ability to perceive categorically non-native sounds already observed by Eimas (1975) and later confirmed by Janet Werker and collaborators (Werker *et al*. 1981, Werker & Tees 1984). Another interesting finding of Kuhl's experiments was that the perceptual reorganization observed by Werker & Tees (1984) is a strictly human phenomenon—not observed in monkeys, for example—and partially completed around 6 months of age.

Summarizing so far, speech perception is driven by a deeply entrenched ability to perceive categorically which, in humans, acts as a scaffold to later construct a richly structured system of phonetic categories on the basis of the stimuli supplied by the environment. This system drives subsequent speech perception, acting as a filter where prototypes function as 'magnets' attracting those stimuli that are similar but not identical to the prototype and thus producing the typical effect observed in the processing of non-native sounds. The question now is what role does this early form of language plays in later stages of

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[18] Later experiments showed that the ability to perceive categorically is also found in many other nonhuman species, like apes, monkeys, and birds; see Balari *et al*. (2013: 497–499) for a brief discussion of these results and references.

the acquisition process.

New evidence supporting this view was reported in Kuhl (2000, 2004), where a number of experiments using neural imaging techniques are presented suggesting that perceptual reorganization is caused by what Kuhl calls 'native language neural commitment' (NLNC). In essence, NLNC would be an example of neural circuitry that has reached a relatively stable state for the processing of auditory inputs and thus showing a certain degree of criticality in the sense that non-native sounds are processed according to the stabilized patterns to which the system is sensitive. In Kuhl's (2000, 2004) words, early language experience literally "warps perception" of further linguistic input and thus "interferes with the processing of information that does not conform to the learned pattern" (Kuhl 2000: 11855). As later research demonstrated (Kuhl *et al*. 2005, Kuhl *et al*. 2008), NLNC, a form of entrenchment, positively acts as a scaffold for further language development, boosting the development of grammatical skills. Interestingly, this is a form of a critical period effect, in the sense that NLNC hinders the development of a second language once the neural network has reached a high degree of stabilization, a point previously raised in Marchman (1993). Concomitantly, however, variable degrees of neural commitment have been observed (Kuhl *et al*. 2008) such that lower levels of stabilization give rise to slower acquisition of further skills, but favor, should the environment include foreign language input, the eventual development of a second language, as it is the case with bilingual infants; see Kuhl *et al*. (2008: §5) for details.

Everything considered, we believe that the evidence reviewed clearly supports the gradient view of language development proposed here. One in which different components participate in different degree and intensity at different stages, in which a gradual hybridization is effected, starting with the development of prototypical phonetic categories, and proceeding through the development of later skills in a continuous chain of entrenchment and scaffolding effects (Dove 2012).

6. Final Remarks: Broadening Our Understanding and Appreciation of Linguistic Varieties through the 'Gradient of Language' Concept

In closing this reflection, we want to briefly stress two advantages of adopting a renewed view of 'criticality' along the lines put forward in the previous sections, each of a different character, but both ultimately related. The first has to do with the suitability of the idea of 'gradient of language', which replaces here the concept of 'faculty of language', as well as the associated 'critical point(s)' concept, to accommodate certain intriguing findings recently exposed in Hankock & Bever (2013). After following up an intensive research on groups with a relatively high incidence of familial sinistrality, the authors present the conclusion there that individuals belonging to such groups (not necessarily left-handed) show a certain advantage relatively to the outgroup in some linguistic skills, like the quickness with which they access and retrieve lexical (declarative) information. From this observation, Hancock and Bever raise some unorthodox, yet reasonable claims concerning the character of language: Namely, that it may display more than one 'normal' form of neurological/computational architecture, and correspondingly more than one 'normal' system of associated behavioral reflexes. Such a conclusion, anchored as shown in well-attested observations, agrees with the expectations of the view on language defended in this paper as a convoluted system of neurological/computational resources, contingently assembled as the corresponding gradient evolves and consequently open to a range of variation within which several architectural/computational styles may fit comfortably (for example, more or less 'declarative' *vs*. 'procedural' styles). As, according to Hancock and Bever, such an expectation seems to be fulfilled within the limits of what are considered 'normal' patterns of linguistic development and behavior, we suggest referring to such variants as 'computational styles', or 'computational dialects', respecting the traditional term in linguistics ('dialect') to refer to all classes of 'normal' variability.

Note that once such a move is made, nothing prevents us form extending the range of expected variability to slightly but increasingly deviant styles: For example, variants in which characteristic physiognomies and/or intellectual disabilities selectively impact on the language capacity, but with mild to moderate effects—as it is the case of Down syndrome, particularly in the expressive side (Martin *et al.* 2009); or variants in which other non-radically disruptive effects are observed—for example, a differential ability to deal with names as compared with verbs, in the context of certain developmental conditions, like Potocki-Lupski syndrome (Vares 2015).[19] Generalizing this, what obtains is a gradient of linguistic conditions, in clear agreement with the gradient character of language in the developmental sense that we have defended in this paper. Likewise, nothing prevents us from extending the gradient as to also cover the transition from monolingualism to different forms of multilingualism—actually conforming a complex spectrum with many transitional forms of multidialectalism in between, the cognitive/linguistic reflexes of which (differences in executive control tasks, visual and speech perception, etc.) are now becoming to be understood (Sebastián-Gallés & Díaz 2012; Sebastián-Gallés *et al*. 2012, Hernández *et al*. 2013). Developmentally speaking, the resulting gradient view purports that language is not circumscribed to a particular compartment of our mind/brain, but spreads on a complexly interactive system of bodily capacities subject to the impact of a correspondingly complex array of developmental influences, both endogenous and exogenous. Such a picture makes very unlikely the idea of a faculty of language as an epitome of sorts, from the point of view of which impaired, lessened or even enhanced variants must be deemed exceptionally deviant.[20]

Our second and final remark points to the added suitability of the developmental framework unfolded here to overcome the inconveniences of a vocabulary too much loaded with normative connotations, as the one displayed in prior passages (with all its occurrences of 'normal' within quotation marks). Forms of language are routinely referred to as 'abnormal', 'deviant', 'impaired', and so on, which obviously entails a 'normal' point of reference, as well as 'second', which

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[19] Verb–noun dissociations are also a well-known effect in the context of different forms of acquired aphasia. See Kambanaros & Grohmann (forthcoming), and references therein.

[20] We are grateful to Kleanthes Grohmann for sharing his thoughts on these ideas.

purports a question of priority, or 'foreign', an administrative condition completely alien to the way languages are acquired. We are not suggesting that labels like these are not necessary at least in some of the fields where they are applied. Our message rather is that they must be handled with extreme care and ideally even replaced when the aims of research have to do with purely developmental matters. Some of them (for example, 'impaired language') name exceptional courses of development (as when 'specific language impairment' is said to affect to 7% of people), which sometimes leave the individual in a more or less handicapped position; but some other times differences are negligible, revealed only after close technical scrutiny. Some other forms are however as common as so-called 'normal' language, like modalities of languages acquired and used in adulthood. In all these cases the developmental perspective, which must be ignorant of normative considerations, should see, again, just cognitive or computational dialects in such varieties (see Corder 1981, for some pioneering suggestions along similar lines; see also Boeckx & Benítez-Burraco 2014, for some recent observations). The move may perhaps prove also useful to remove many language-related stigmas and to promote more proactive attitudes in rehabilitation or reconstructive endeavors (Michel & Tyler 2005).

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Syntactic Networks as an Endophenotype of Developmental Language Disorders: An Evo-Devo Approach to Clinical Linguistics

Lluís Barceló-Coblijn, Antonio Benítez-Burraco & Aritz Irurtzun

Usually, developmental language disorders are defined either symptomatically (based on a constellation of linguistic deficits appearing recurrently within a population) or etiologically (on the basis of a common underlying deficit), or both. On paper, each of these clinical categories is expected to be distinguished from other close entities at several levels of analysis (phenotypic, cognitive, neurobiological, genetic, etc.). Nonetheless, this is not typically the case: Comorbidity, variability, and heterogeneity are in fact a common outcome of the clinical practice. Ultimately, different disorders may share the same underlying deficit (e.g., phonological dysfunction in dyslexia and SLI); conversely, different deficits may give rise to the same disorder (e.g., both visual problems and phonological deficits may contribute to dyslexia) (Benítez-Burraco 2013).

If we want to achieve a better—and earlier—diagnosis of these conditions, we should improve the tools we employ at present. A promising approach is one relying on the endophenotypes of disorders. Endophenotypes may be defined as cognitive, neuroanatomical, neurophysiological, endocrine, or biochemical quantifiable components of the space between genes and diseases (Gould & Gottesman 2006). Endophenotypes refer to more specific (and more physiological) aspects of the body function, therefore they allow us to gain a more accurate diagnosis of its dysfunction (Gottesman & Gould 2003). Here we would like to advance a putative endophenotype of language disorders that combines four factors: (1) linguistic analysis (syntactic computation), (2) information management (communicative strategies), (3) recent evo-devo insights in the nature of phenotypic variation, and (4) network approaches to emergent properties of complex systems (surely, language it is; Deacon 2005).

To begin with, we would like to note that, although the set of pathological conditions already described by clinical linguists is ample, it is not unlimited either. In other words, variation is constrained or canalized, even in pathological states. At the same time, we observe that language is both sensitive to damage (e.g., some aspects of language processing are perturbed in nearly all disorders, like the proper use of inflectional cues in verbal and nominal morphology) and resistant to perturbation (e.g., a nearly functional language faculty may emerge at the term of growth in spite of severe underlying deficits).

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Put simply, language is both plastic and robust, whereas language development is significantly canalized (Benítez-Burraco & Boeckx 2014). In evolutionary-developmental (evo-devo) approaches, the limited set of phenotypes that result from the interplay of the different factors regulating development are usually referred to as points within the morpho-space or adaptive landscape (McGhee 2006). Consequently, language disorders may well be characterized as possible—although dysfunctional—phenotypes within the whole landscape of language development potential. The real problem is that these phenotypes of the language faculty are still characterized in terms of the clinical categories we regard unsatisfactory (e.g., dyslexia, SLI, and the like). This may be optimized if we move downwards and consider instead some of their endophenotype(s). Because of their more biological nature, endophenotypes may reflect in a more reliable way how the impaired brain grows and how a more or less functional language capability instantiates in the pathological mind. More importantly, we expect (some of) them to be the axis delimiting the adaptive landscape of language development in the species (either normal or pathological). Nonetheless, not many confident endophenotypes of language disorders have been proposed up to now (see some exceptions in Neuhoff et al. 2012 or Peter et al. 2012). We believe that an evo-devo approach to disorders may further help to narrow and optimize the set of endophenotypes that are currently available.

In our opinion, one useful endophenotype of this sort may be the 'syntactic fingerprints' characterizing the child's ability to combine words at different stages of development and, specifically, the kind of networks resulting from the measurement of the combinations of syntactic items (words or morphemes) in real samples of speech (this ultimately reflecting the syntactic links among words within utterances). Because we expect these 'fingerprints' to confidently reflect how the typically developed faculty of language unfolds within the child's mind, we have hypothesized them to be language-independent. We further expect that different clinical conditions are characterized by different 'syntactic fingerprints' throughout development, as a result of different language faculties being implemented in the child's mind. In turn, this plausibly results from different brain architectures emerging from different molecular backgrounds (e.g. gene mutations, changes in protein homeostasis, and the like). Overall, we expect that our syntactic networks fulfill the set of properties that endophenotypes have to meet (see Gottesman & Gould 2003, Gould & Gottesman 2006). Although we are still testing many of the details of our hypothesis, some promising results have been achieved.

For starters, we developed a new analytical tool for measuring the syntactic complexity of the utterances produced by speakers in real conversations. Our tool follows the basic lines of dependency grammar (Hudson 1990), representing the direction of dependency relations as well as the nature of the dependencies themselves. Thus, we can label each syntactic item by its category (noun, verb, etc.) and capture the dependencies between pairs of syntactic items (say, between a noun like *dog* and a determiner like *the*)—whether it is a head–complement relationship, like in the phrase 'the dog', where *the* is the head (cf. Abney 1987, Longobardi 2000, and others), or a modification relationship, as in constructions like 'walk quickly', where the adjunct *quickly* is a modifier of the event of walking (cf. Pietroski 2005). Importantly, our technique allows us to treat each syntactic item separately (in a morpheme-by-morpheme fashion), which is essential for the analysis of agglutinative and polysynthetic languages. Once the syntactic analysis is done, the information is sent to the network program, which encapsulates words or morphemes into nodes and creates edges between nodes from the syntactic links between them (the program also imports the kind of syntactic relationship, e.g., subject, complement, etc.).

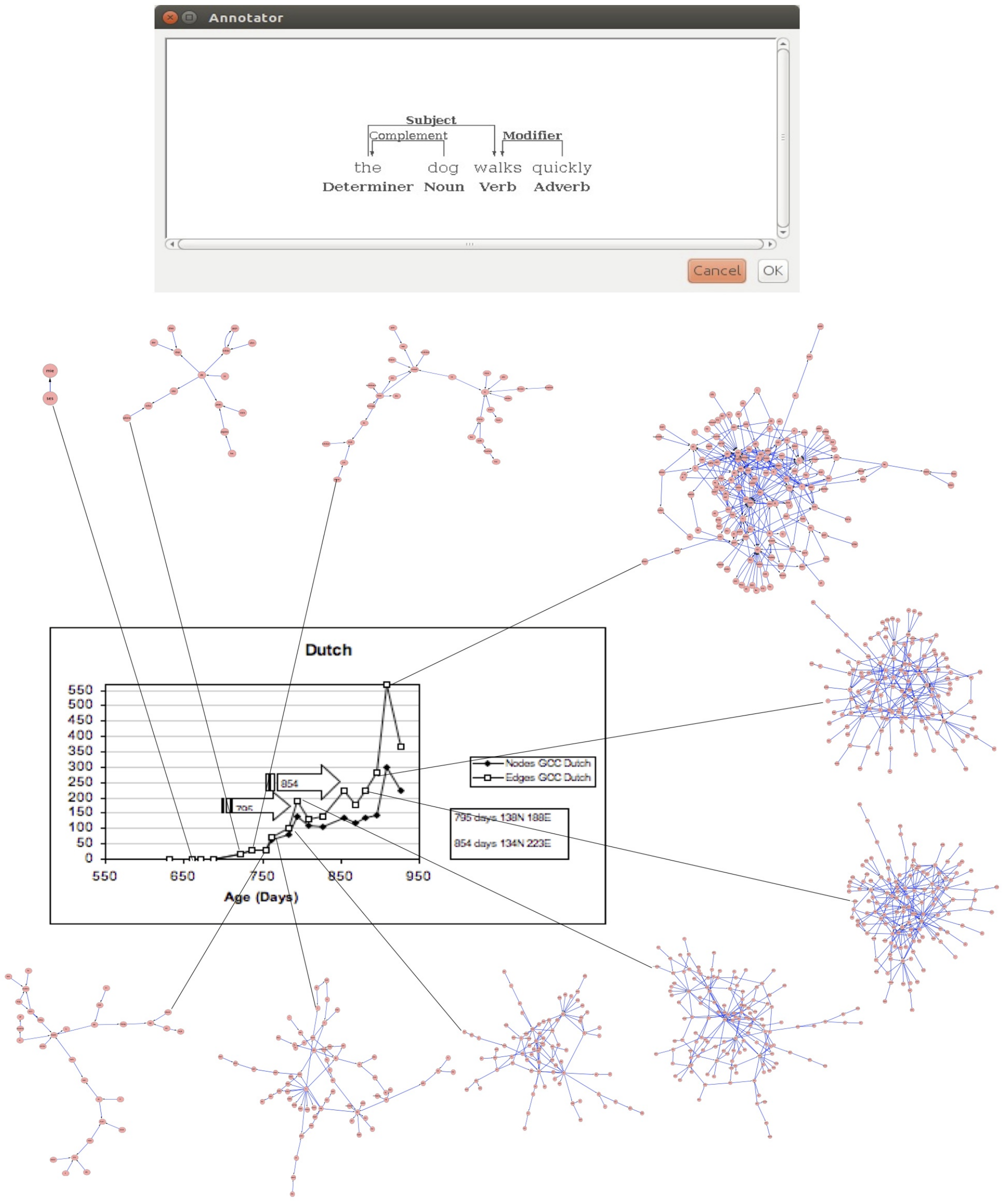

Figure 1: Language development as resulting from the network approach. On the top, the syntactic analysis is carried out. On the bottom, the development of the linguistic performance. In the pink networks, each word/morpheme is a node and each edge a syntactic relationship. This graphic belongs to the Dutch corpus Daan from CHILDES. In the graphic, white dots represent edges/syntactic relationships, whereas black dots represent words/morphemes. White arrows point to the abrupt transition and change in the topology of the resulting networks.

We have shown that this approach confidently characterizes the development in the child of her ability to produce complex utterances with the combination of multiple words or morphemes. Interestingly, we found abrupt phase transitions in the syntactic complexity of the child's speech as she grows (from chain networks to scale-free networks to small-world networks) (Figure 1 above). We take this categorical difference in production to be a reflex of the different stages in the acquisition of the child's syntactic knowledge (Corominas-Murtra et al. 2009, Barceló-Coblijn et al. 2012). Importantly, these patterns are also informative about the words that are center-stage in the child's speech and ultimately, about communicative strategies. Importantly, this analytical tool is not language-dependent: We found similar network profiles at similar developmental stages when applied to the speech of typically developing (TD) children acquiring languages which are typologically diverse and belong to different phylogenetic groups (Germanic: Dutch, German, English; Romance: Catalan, Spanish, French, Italian; non-Indo-European: Basque) (Barceló-Coblijn et al. 2012, Barceló-Coblijn et al. submitted). For example, at 27–28 months of life, the ability to syntactically combine words achieved by TD children acquiring any of these languages can be regularly identified by a small-world network with a ratio of words/nodes vs. syntactic links/edges of 1:2 on average.

More importantly, we also used this analytical approach to confidently characterize language growth in pathological conditions. Different developmental disorders entailing language deficits are known to display pretty variable patterns of linguistic behavior. For instance, whereas SLI or Down syndrome are typically associated with a sharp syntactic disability, the performance of children suffering from other conditions like Fragile X syndrome is closer to that of their TD peers (see, for instance, Martin et al. 2013). Similarly, other syndromes like Williams syndrome are characterized by fluent speech, which on the surface does not seem to display such a patent syntactic disorder (see Bartke & Siegmüller 2004 for discussion). Ultimately, the variability observed within pathological groups (in terms of language knowledge and use) is typically greater than the variability within the normal population. Overall, it is quite difficult to draw a distinctive linguistic profile of each disorder.

As we pointed out above, we expect that biologically-driven factors that affect typical development provoke a deviation from this regular pattern of network transition found in the TD population so that they are achieved differently or are never achieved. Our preliminary results (Barceló-Coblijn et al. submitted) confirm that the networks reflecting syntactic development in some pathological populations like Down syndrome differ from those observed in TD children in several aspects, including the kind of network (and hence several network parameters like the clustering coefficient or the path length), the lexical nature of hubs, and the ratio nodes/edges. Likewise, our first assessment of the Williams Syndrome discourse (Palmer 2014) is also indicative of an idiosyncratic pattern of language growth, which is characterized by the modular nature of the resulting networks, despite the appearance of a typical speech, as noted above.

Interestingly, it is the network technique that allowed us to capture and formalize the language deficits (and the deviant developmental pattern) characteristic of this group that may be otherwise difficult to identify or even to observe (obviously, Williams syndrome can be confidently diagnosed cytogenetically, but this is not always the case with conditions that are defined symptomatically, like autism or mental retardation; moreover cytogenetic analyses are expensive and may not be available under certain socio-economic circumstances).

We wish to end by briefly discussing the main translational values of our approach. First, the tool we have developed enables one to extract valuable information from real speech samples, which we feel is a more reliable source of information about the child's language knowledge and use (in contrast, tools currently used for the diagnosis of language disorders usually involve batteries of normalized tasks that have to be passed in controlled environments that may affect the child's performance). Second, because networks are characterized by a number of precise mathematical properties (like the clustering coefficient, the average path length, etc.), we further expect that the observed patterns are easier to quantify and have clearer diagnostic and prognostic correlates. Third, because we focused on a linguistic dimension that appears quite early in the child's discourse (syntax), we expect that our tool (and the kind of endophentype we propose) also allows for an earlier diagnosis of disorders (e.g., dyslexia cannot be reliably diagnosed until the child starts reading, at age 4–6, depending on the educational system).

Last, we expect our approach to be also of interest for the biological analysis of language (aka biolinguistics). On the one hand, because we heavily relied on a network approach for our analysis of how syntax emerges in the child's language, we expect to be able to accurately characterize how the properties of a complex system like language emerges during the child's growth. On the other hand, given that graph theory has recently been employed to define a two-dimensional morpho-space for complex networks (Goñi et al. 2013), we expect to be able to contribute as well to define the morpho-space of the available language faculties in the species. This latter approach focuses on two measures that capture communication efficiency within the network (routing and diffusion) and has shown that it is connectivity that matters and not just the *n* of nodes comprising the network. Under this view, two language networks may contain the same *n* of words/nodes but have a rather different *n* of syntactic links/edges or a different edge distribution—and hence a different kind of structure. This approach should help us to confidently characterize the complex networks resulting from the analysis of language growth in pathological populations.

Overall we expect that the whole set of language disorders (and the language faculty of non-affected individuals) can be translated into a constellation of complex networks located in different points of the language morpho-space, each characterizing a specific developmental itinerary for language, either normal or pathological (Barceló-Coblijn & Gomila 2014). Incidentally, all this conforms evidence that atypical language faculties also have their own developmental paths, although they grow in rather different ways. Actually, the network technique allows us to capture and formalize the fact that brains with linguistic disorders are not static entities. On the contrary, they are able to compensate damages at different levels and throughout growth—this probably explains why the linguistic profile of affected people varies in specific ways across populations and throughout development.

In sum, we regard this combination of syntactic analysis, complexity studies, and evo-devo theories as a promising approach to clinical linguistics. Specifically, we expect it to contribute developing better tools for diagnosing these complex conditions.

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X-within-X Structures and the Nature of Categories

Evelina Leivada

This paper discusses the existence of X-within-X structures in language. Constraints to same-category embedding have been the focus in a number of recent studies. These studies follow a long-standing tradition in linguistic theory that assumes a ban on the adjacency of same-category elements. In the present work, data drawn from a typologically broad variety of languages suggest that the postulated constraints are not so robust. It is shown that X-within-X structures do exist in language. In this context, an argument is made in favor of an unrestricted conceptualization of Merge, independent from category distributions, while recursion is taken to be a property of procedures and not of structures. The discussion of X-within-X patterns provides insights with respect to the attested category distributions, the nature of categories, and the language faculty, from a biologically plausible point of view.

Keywords: categories; complementizer doubling; demonstrative doubling; Merge; preposition doubling

1. Introduction

The nature of recursion in language is a topic frequently addressed in recent linguistic theory across different frameworks. Linguistic recursion, "the foundational linguistic universal" (Watumull *et al*. 2014), is defined as the ability to generate an infinite set of hierarchically structured expressions by iteratively using operations in syntax. This ability has been at the core of many heated linguistic debates and has received attention from a variety of disciplines and points of view (e.g., Hauser *et al*. 2002, Pinker & Jackendoff 2005, Fitch *et al*. 2005, Jackendoff & Pinker 2005, Chomsky 2008, Fitch 2010). Long before Hauser *et al*. (2002) revived interest in recursion, restrictions on the elements that Merge puts together have been discussed in a number of studies from the perspective of a

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well-formedness condition that precludes adjacency of same-category elements in order to avoid a linearization crash (Perlmutter 1971, Ross 1972, van Riemsdijk 1998, and, more recently, Grohmann 2000, Richards 2010).

The nature of the ban is not syntactic per se; as Manzini (2014) notes, the anti-identity condition in syntax has parallels in phonology and morphology. Lohndal & Samuels's (2014) conclusion is similar: "[N]on-distinctness is dispreferred across various linguistic domains, though the levels at which it is disallowed may vary from language to language" (p. 79). The present work is precisely an exploration of how non-distinctness is manifested across various spoken and sign languages. In the syntactic domain, Richards's (2010) formulation of the condition on linearization called *Distinctness* argues that "if a linearization statement <a, a> is generated, the derivation crashes". Discussing this condition, Alexiadou (2011, 2014a) correctly observes that Richards's exploration of different ways of reaching Distinctness raises concerns as to why morphological richness would affect syntax. In Alexiadou's (2011) words, "[t]his is especially unexpected under views according to which morphology merely interprets syntax" (p. 2).

Arsenijević & Hinzen (2012) discuss category distribution and argue that direct category embedding—not mediated by other categories—of a syntactic category X to another element of the same category X (henceforth, X-within-X or [XX]) is "surprisingly rare in human language" (p. 423), and possibly not existent at all. They approach counter-examples of X-within-X recursion of lexical categories, such as noun–noun compounds like $[_N [_N [_N [_N \text{war}_N\ \text{film}_N]\ \text{studio}_N]\ \text{committee}_N]\ \text{session}_N]$, by noting their counter-cyclic, 'anti-recursive' derivation, adopting the analysis in Roeper & Snyder (2005). Crucially, the absence of X-within-X with respect to functional categories is explicitly predicted, in line with claims in previous literature: In their words, "[e]mpirically speaking, counterexamples, which would involve adjacent articles (*the a book*) […] are not found in human language" (Arsenijević & Hinzen 2012: 425) and this absence carries, according to the authors, important implications for the nature of phasehood and cyclicity which bans unmediated X-within-X recursion in language.

In this context, it seems that the effort to lift combinatorial restrictions that predict the absence of X-within-X patterns is of key importance to linguistic theory because such a task is essentially an inquiry into the innately unconstrained nature of the combinatorial operation that lies at the core of grammar—namely, Merge. In what follows, I argue (i) in favor of viewing apparent restrictions on category distribution that predict the absence of X-within-X patterns as the consequence of interface filters, and (ii) against the robustness of such restrictions by presenting X-within-X patterns in a typologically broad range of languages. Highly relevant, in this context, is what one defines as X—in other words, what is a sufficient degree of difference to tease apart category X from category Y. Therefore, the present discussion also reflects on the nature of categories. Throughout this article, when I talk about 'X-within-X structures/patterns' or 'X-within-X recursion', which is a term used in the literature, I refer to [X(P)X(P)] structures. Recursion is not a property of structures but of procedures, as Watumull *et al.*

(2014) note, therefore I take 'X-within-X recursion' to be a misleading term and I use 'X-within-X structures/patterns' instead.[1]

The paper is structured as follows: In section 2, I illustrate the existence of X-within-X patterns for determiners (D), complementizers (C), and prepositions (P). Section 3 interprets the data in relation to (functional) categories and the treatment they receive, mainly in the cartographic enterprise. Section 4 concludes with a brief outlook.

2. X-within-X Patterns

Limits to same-category embedding boil down to the interaction of syntax with the interfaces: If the syntactic outcome amounts to strings such as [XXXX], the relevant utterances would quickly become uninformative, with zero information value (Boeckx 2014: 89). The fact that some structures are infrequent is in itself, of course, no evidence against the existence of computational mechanisms that would generate such structures. Miller & Chomsky (1963: 471) discussed this in a different context:

> There are many syntactic devices […] for the construction of sentences with nested dependencies. These devices, if permitted to operate freely, will quickly generate sentences that exceed the perceptual capacities (i.e., in this case, the short-term memory) of the native speakers of the language. This possibility causes no difficulties for communication, however. These sentences, being equally difficult for speaker and hearer, simply are not used, *just as many other proliferations of syntactic devices that produce well-formed sentences will never actually be found*. (emphasis added—EL)[2]

Arsenijević & Hinzen (2012) are right to claim that patterns like [XXXX] are not frequent in language but rather mediated by other categories, and usually so by phase boundaries. However, the fact that such patterns are not frequent does not entail that they are completely absent. In principle, Merge *can* put together elements of the same category giving rise to [XX(X)] patterns. In the following sub-sections, I show the existence of X-within-X patterns for three types of functional elements in different languages, arguing that Merge remains silent with respect to the types of elements on which it operates and that this is what allows for the existence of X-within-X patterns.

2.1. Determiners

If in the X-within-X pattern, X is D, then [D D] patterns should not be attested in the absence of any mediating category, but (1) shows that they are available.

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1 Lobina (2015) does a fine job pointing out issues related to the term 'X-within-X recursion'. Quite usefully, he also correctly draws the indeed "subtle distinction between process and generation", arguing that "Merge applies iteratively, but *constructs* a recursive [syntactic] object *recursively*" (p. 4, emphasis in the original).

2 I am grateful to an anonymous reviewer for helpful discussion on this point.

(1) afto to telos [Greek]
this the ending
'this ending'

(1) involves two adjacent D elements: a demonstrative and a definite article. The properties of this phenomenon of 'demonstrative doubling' have been addressed in the literature (see Grohmann & Panagiotidis 2015 and a host of references cited there). Following Panagiotidis (2002), both articles and demonstratives fall under the same category, that of D heads. If so, this example shows an instance of X-within-X for the functional category D. The same phenomenon is found, among other languages, in Hungarian and Javanese (Alexiadou *et al*. 2007).

Even if (1) is taken, under a different analysis, to show two functional elements that belong to two *distinct* functional categories (an issue to which I return later in this section and in section 3), in which case this would not be an instance of X-within-X, (2) involves two indefinite articles.

(2) ena kapço telos [Greek]
a a ending
'an ending'

Actually in (2), the plural forms of *kapço* are what substitutes for the plural of *ena*, which lacks such forms (Michael 2011). Both *ena* and *kapço* are indefinite articles; thus (2) seems to be a licit instantiation of the type of category distribution that Arsenijević & Hinzen (2012) make reference to when they suggest that "[e]mpirically speaking, counterexamples, which would involve adjacent articles (*the a book*) […] are not found in human language" (p. 425). Within the nominal domain, X-within-X patterns do not limit their existence to adjacent articles. Relevant patterns have been reported in Blackfoot for which Frantz (2009) and Bliss (2013) provide examples of adjacent demonstratives.

Beyond doubt, there is a limit to the number of unmediated determiners in a row one may find, but this limit is not syntactic per se. In other words, one can argue in favor of an unconstrained Merge which may give rise to X-within-X syntactic patterns, even assuming that any restrictions that preclude the overt realization of adjacent elements of the same category pertain to phonology and not to syntax. One such example is the phonological realization of one determiner, even if two determiners are assumed underlyingly; see, for instance, Lefebvre & Massam's (1988) surface filter rule on adjacent determiners and Davis' (2010) 'Determiner Deletion' PF rule.

At a syntactic level, the main difference between (1) and (2) relates to the fact that the two D elements in (1) can be shown to fall under different categories according to some analyses (e.g., D for determiner *to* and Dem for demonstrative *afto* in Leu 2008). (2), on the other hand, involves two indefinite articles and no demonstrative, therefore a syntactic analysis that places one element under D and another under Dem is not pursuable in this case. However, it can be argued that in (2) the indefinite article *ena* is a quantifier, introduced in NumberP and subsequently moving to DP (as argued in Alexiadou 2006). Following Alexiadou (2006) and Kariaeva (2009) in placing *ena* in the D position, the structure underlying (2) should be the following: [$_{DP}$ ena [$_{DP}$ kapço [$_{NumP}$ ~~ena~~ [$_{NP}$ telos]]]], giving rise to [$_{DP}$ D [$_{DP}$ D …]], an X-within-X structure. (3) presents another case of the [$_{DP}$ D [$_{DP}$ D …]] configuration, but crucially one that shows the two D heads being occupied by the same indefinite article *kapço*. (3), then, is not subject to any syntactic analysis that could present the relevant Ds as elements of different categories, as there are no semantic or intonational properties that would facilitate such a distinction between the two.

(3) (Kapçi) kapçi kala θa kanun na kitun ti ðulia tus. [Greek]
some some well FUT do.3PL SUBJ see.3PL the work POSS
'Some people would do well to mind their own business.'

Based on Ioannidou & den Dikken's (2006) analysis, where multiplication of definite articles in Greek polydefinites is due to different copies of the same article, I take (3) to show two adjacent copies of the same indefinite article. I agree with Ioannidou & den Dikken (2006: 4) that both the D head and the C head "represent abstract bundles of morphological features; the answer to the question of whether some overt element will end up spelling out C or D depends on whether something raises up to C/D in the course of the derivation" (see also Pesetsky & Torrego 2001). Multiple instantiations of this raising are bound to give rise to [XX] structures such as the ones in (2)–(3).

The idea that categories do not boil down to a property to be inherently found in the syntactic objects they characterize has been repeatedly voiced within the framework of Distributed Morphology (DM) as well as in Chomsky's recent work on labeling (e.g., Chomsky 2013). To this end, Marantz (1997: 215) argues that "[r]oots like √DESTROY and √GROW (to borrow notation from Pesetsky 1995) are category neutral, neutral between N and V. When the roots are placed in a nominal environment, the result is a 'nominalization'; when the roots are placed in a verbal environment, they become verbs". In other words, a 'category' feature is not intrinsic to mergeable items according to standard assumptions of DM.

It is significant for the purposes of the present discussion that Chomsky (2013) points to the same direction when he argues that Merge yields a set {X, Y} without a label and that the syntactic object SO receives its label through a labeling algorithm: "We assume, then, that there is a fixed labeling algorithm LA that licenses SOs so that they can be interpreted at the interfaces, operating at the phase level along with other operations" (p. 43). The 'categoryless' nature of categories and the idea that SOs are *defined contextually* (Pesetsky & Torrego 2004), receiving their label at the interfaces, outside narrow syntax proper, is precisely the message that seems to transpire when one considers the data from Riau Indonesian discussed in section 3.2. The discussion of complementizers and prepositions in the next two sections is concordant with Chomsky's claim that Merge yields unlabeled SOs. It is precisely because of this constraint-free (i.e. understood here as 'anti-identity condition'-free) application of Merge that adjacent SOs of the [XX] type may at times arise.

2.2. *Complementizers*

In relation to C-within-C it has been argued that "C never embeds in C, directly. A sequence in which Cs occur in Cs really is a [C–*v*…[C–*v*… [C–*v*]]] sequence, as seen in [(4a)], or even a [C–*v*–D… [C–*v*–D… [C–*v*–D…]]] sequence, as seen in [(4b)]" (Arsenijević & Hinzen 2012: 425):

(4) a. [$_{CP}$ Allegedly, [$_{TP}$ John will [$_{vP}$ deny
[$_{CP}$ that [$_{TP}$ Bill has ever [$_{vP}$ said [$_{CP}$ that …]]]]]]]
b. [$_{CP}$ Allegedly, [$_{TP}$ John will [$_{vP}$ deny [$_{DP}$ the very possibility
[$_{CP}$ that [$_{TP}$ Bill has ever [$_{vP}$ defended [$_{DP}$ the claim [$_{CP}$ that …]]]]]]]]]
(Arsenijević & Hinzen 2012: 425)

These examples provide the basis for assuming that any [C C] sequence should be mediated by sequences of other categories, such as [*v*–V], to be licit. (5), however, shows a [C C] sequence (unmediated by [*v*–V], yet licit), which is available with the so-called 'way of asking/speaking verbs'. Although some analyses (e.g., Brucart 1993) put the non-interrogative C in [Spec,CP], other analyses argue in favor of a 'doubly filled Comp' in Spanish (Plann 1982, Suñer 1992).

(5) Me preguntó que qué quería. [Spanish]
CL ask.3SG that what want.1/3SG
'He/she asked me (*that) what did I/he/she want.'
(adapted from Demonte & Fernández-Soriano 2009: 30)

According to Suñer (1992), (5) shows a recursive C. Suñer's argument was questioned in Demonte & Fernández-Soriano (2009), who argued against a recursive C and in favor of an analysis along the lines of the *wh*-element being merged in FocP and the declarative *que* 'that' occupying the head position in ForceP.

Since the analysis adopted seems important in deciding whether or not (5) shows a recursive C, it is useful to discuss the two main counterarguments that Demonte & Fernández-Soriano bring up in their critique of Suñer's proposal. First, they argue that if C was really recursive, we should see verbs like that in (5) taking a bare declarative C. They correctly note the impossibility of having this combination with *preguntar* 'to ask', but (6) shows it to be possible with *decir* 'to say' in the context of a bare declarative and (7a) in the context of recursive C headed by a declarative that selects an embedded interrogative.

(6) Me dijo que Juan es listo. [Spanish]
CL say.3SG that John is smart
'He/she told me that John is smart.'

(7) a. Me dijo **que qué** quería. [Spanish]
CL say.3SG that what want.1/3SG
'He/she told me what did I/he/she want.'

b. Me dijo qué quería.
CL say.3SG what want.3SG
'He/she told me what he/she wanted.'

Thus, the incompatibility they notice might boil down to lexical selection rather than the nature of C as being (non-)recursive.

The second argument Demonte & Fernández-Soriano (2009) offer relates to the observation that interrogative sentences in 'doubly filled C' constructions cannot be infinitival (8), which cannot be justified under the recursive C hypothesis.

(8) a. *Preguntó/dijo que adónde ir. [Spanish]
ask/say.3SG that where go
'He/she asked/said where to go.'
b. Preguntó/dijo adónde ir.
ask/say.3SG where go
'He/she asked/said where to go.'
(Demonte & Fernández-Soriano 2009: 31)

Demonte & Fernández-Soriano (2009) explain the contrast in (8) by arguing that "[w]ithin the same Complementizer system a ForceP node is included, headed by a declarative *que* [cf. (7a) above]. One can argue that this element only appears in finite clauses […] therefore excluding infinitival interrogatives" (p. 31). However, the same assumption (namely, that declarative *que* only appears in finite clauses) can also be held under a recursive C hypothesis, where the higher C position is filled by declarative *que*.

Leaving aside the nuances of their analysis, Demonte & Fernández-Soriano (2009) are right in placing these two elements in different functional positions: It can be argued that the two adjacent instances of *que* in (7a) belong to two different categories because they come with different flavors; one is clearly declarative (*que*), while the other is interrogative (*qué*). Another counterargument to the claim of a doubly filled C in (7a) is that the interrogative element could be analyzed as a specifier instead of another head. Both these counterarguments can be legitimately voiced; hence, my goal is to demonstrate the existence of X-within-X in the complementizer system with elements of the *same (non-interrogative) flavor*. (9) seems an instantiation of recursive C, not mediated by [*v*–V] and not involving any interrogative element.

(9) Acho **que** amanhã **que** a Ana **que** vai conseguir
think.1SG that tomorrow that the Ana that will manage
acabar o trabalho. [European Portuguese]
finish the assignment
'I think tomorrow Ana will manage to finish the assignment.'
(Mascarenhas 2007: 10)

As Mascarenhas correctly observes when presenting (9):

> In general, it seems clear that an analysis under the cartographic hypothesis that argues that the identical complementizers delimit the C-domain is bound to fail for E[uropean] P[ortuguese]. If one were to commit to that analysis, one would be forced to either assume that three 'que's in a Portuguese triple-C construction occupy three different functional positions, or that the whole domain is recursive. Both options are very undesirable.
>
> (Mascarenhas 2007: 9)

When comparing the different options for explaining (9), he implicitly deals with the following question: Why would one treat C_1, C_2, and C_3 in (9) as [XXX] and not as [XYZ]? The answer is the following: In the absence of any argument for a distinct semantic import or syntactic function that would set apart C_1 from C_2 and C_3, the three "identical complementizers" (to borrow Mascarenhas's term) in (9) should be treated as such. All else being equal, a theory that assumes an unconstrained Merge that permits recursive C constructions has to do less explaining (and as a result needs less defending) than any richer theory that duplicates the entire domain or assumes that phonologically and semantically identical elements are assigned distinct labels in terms of their syntactic status.

Complementizer doubling has also been noted in Icelandic with two phonologically distinct C elements.

(10) Þetta er bókin sem (að) ég keypti. [Icelandic]
this is book that that I bought.1SG
'This is the book that I bought.'
(Larsson 2014: 447, from Thráinsson 2007: 450)

Under the claims of the present analysis, [CC] in European Portuguese and Icelandic is similar to the [DD] pattern that one finds in Greek: They are examples of X-within-X structures which involve functional heads; that is, the sort of structures one would not expect to find if one believed that Merge came with restrictions that preclude merging an element of a category C with another element of the category C without the mediation of [*v*–V]. However, since Merge comes with no such restrictions, these patterns arise. Under this assumption, it is not necessary to resort to either doubling or tripling the entire left periphery, or to assuming different functional positions for the different C elements. If these posited C elements do not have different functions or distinct semantic import, they should be analyzed uniformly, as elements of the same category.

The existence of data like (9)–(10) is not a problem under a recursive C hypothesis, where multiple instances of C are possible. The most important point at stake is that this analysis is more economical because it neither forces us to seek different functional positions for elements that do not have different functions, nor duplicates the entire left periphery to accommodate the data. Put differently, from a syntactic point of view, the recursive C analysis should be preferred over alternatives, by virtue of the most widely accepted minimalist guiding principle of methodological economy (as Hornstein 2001 puts it): Occam's razor.

2.3. *Prepositions*

The X-within-X patterns below are intriguing because they are immune to any form of criticism that may suggest that, instead of X-within-X, they amount to X-within-Y. In other words, (11)–(12) do not seem amenable to cartographic, articulated functional structure analyses of the P domain such as den Dikken's (2009), which discusses cases of P recursion of locative and directional PPs. Indeed, in his examples, two different flavors can be assumed: PP_{DIR} and PP_{LOC}. However, the prepositions in (11)–(12) do not contribute different kinds of (spatial) information, hence labeling them in a different way is unmotivated—and, therefore, uneconomical and undesirable.

(11) amb sense mobles [Colloquial Catalan]
with without furniture
'without furniture'

(12) me ðixos onira [Greek]
with without dreams
'without dreams'

Even more convincing from a syntactic point of view are data that show preposition doubling in Dutch. The reason for this is that even if one tried to analyze (11)–(12) as involving two different types of prepositions (e.g., [$_{PP\text{-}X}$ amb [$_{PP\text{-}Y}$ sense [$_{DP}$ mobles]]]),[3] an analysis along these lines would probably not work for Dutch because, in parallel to what has been observed above for complementizers in European Portuguese, in the prepositional domain too, it is possible to find doubling of the exact same element in an [XX] configuration.

(13) Hij heft zijn t-shirt verkeerd om (om) aan. [Dutch]
he has his t-shirt wrong around around on
'He is wearing his t-shirt inside out.' (Aelbrecht & den Dikken 2013: 41)

The two *om* elements couldn't possibly be taken to fall under different categories; as Aelbrecht & den Dikken (2013: 41) clarify "[t]hat the second *om* in [(13)] is a double of the first, and not an independent particle, is clear from the fact that there is already a particle present (*aan* 'on') in the sentence". In this context, they describe these two elements as "two *immediately adjacent identical* P-elements" (p. 41, emphasis added).

Similar to (3), (13) shows two adjacent identical elements ([DD] and [PP] in (3) and (13), respectively). There are two empirical arguments that I put forth for the examples in this section—and particularly those examples which involve [XX] structures that feature two occurrences of the exact same element. The first one is that, to the best of my knowledge, *there is no analysis* of (3) or (13) that suggests that the two elements in the [XX] configuration belong to two different categories. My classification of (3) as X-within-X then is not due to an underlying burden-of-

[3] To the best of my knowledge, such a claim has not been pursued in the literature.

proof argument—although I do agree with Hornstein's (2001, 2009) claim that, methodologically speaking, the burden of proof is on those that postulate the richer theory—rather, it boils down to an empirical argument. One cannot show why an [XX] analysis of (3) should be preferred over an alternative [XY] analysis because the latter analysis simply does not exist, probably due to the fact that there are no grounds to support it (e.g. distinct semantic import, different syntactic function). In a similar vein, (13) shows an [XX] structure, and nowhere do Aelbrecht & den Dikken (2013) imply that this might be an [XY] structure. On the contrary, they are explicit about the fact that their example shows two adjacent tokens of the same preposition. In this case, too, there is no [XY] proposal to be contested; [XX] is the only analysis available.[4]

The second argument is that, even if an alternative analysis of (3) or (13) existed, according to which the first of the two identical elements belongs to a category X that embeds a category Y (so that these examples would actually show an element X embedding YP rather than X embedding XP), this analysis would make the 'no X-within-X' claim vacuous if one adopted the earlier mentioned DM idea that a 'category' feature is not intrinsic to mergeable items. Assuming that syntactic categories are bundles of features, but that a categorial feature is not among them, one would need to endow one of the two elements with a different feature, which would be a property of either element but not of both. In the absence of any such feature, and any distinctness between the two elements across levels of linguistic analysis, and pending a better understanding of syntactic categories, $[_{XP}\ X\ [_{XP}\ X\ \ldots]]$ appears to be a possible configuration.

One reviewer suggests that if X_1 and X_2 are identical, this would seem to suggest doubling at work: If doubling is an instance of a general PF reduplication process, then there is no evidence for additional syntactic structure for X_1 at all. Following standard DM assumptions, I take PF to not function as a generative system than can derive nodes/words. In the words of Embick & Noyer (2007: 293):

> While PF processes may be possible for certain aspects of word formation broadly construed, the important point is that such PF processes do not constitute a separate generative system for deriving words. Rather, PF processes effect modifications to the structures generated by the syntax, modifications that are limited to minor operations that manipulate nodes in a sharply constrained fashion.

Data from Catalan, Greek and Dutch suggest that same-category embedding is found in the P domain, too, similar to what happens in the other functional categories discussed above. It seems a safe claim to make that same-category embedding is not a marginal phenomenon restricted to one category of functional heads or one language. It is not even restricted to functional categories. Data from sign languages can be particularly telling in relation to instances of X-within-X in lexical categories.

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[4] Of course, the fact that one finds an analysis of type A in the literature, but not of type B, does not mean that analysis A is right and analysis B is wrong. However, this absence may be an indication of the (un)controversial status of the data at hand. I am grateful to an anonymous reviewer for comments on this point.

Recall that Arsenijević & Hinzen (2012) discuss noun–noun compounds like [$_N$ [$_N$ [$_N$ [$_N$ war$_N$ film$_N$] studio$_N$] committee$_N$] session$_N$] and stress the counter-cyclic, 'anti-recursive' derivation of such compounds. According to the analysis of Roeper & Snyder (2005), recursive applications of Merge in this example expand the syntactic structure downward—in a violation of the Extension Condition—and target the bottom position of an already projected structure. Under Roeper & Snyder's (2005) analysis, a compound like [$_N$ [$_N$ [$_N$ [$_N$ war$_N$ film$_N$] studio$_N$] committee$_N$] session$_N$] involves base-generation of [war] as an abstract clitic complement of the head [film]. Upon movement and left-adjunction of the complement to the head, the complement position is rendered available again and [studio] can be merged there. In relation to noun–noun compounds, it is important to notice that it is not direct X-within-X embedding that is at stake anymore. The possibility of having an unbounded, direct X-within-X pattern is now straightforwardly assumed and, as Arsenijević (2012: 5) notes, this sort of "N–N compounds are known exactly for their structurally unbounded recursive nature". The issue at stake is the syntactic derivation of these compounds which, according to some analyses, proceeds in a way best described as 'anti-recursive'. Crucially, the above analysis relies on assuming an *endocentric* compound that involves a head–complement relation between [film] and [war] and then between [war film] and [studio], and so on. Consider, however, some of the compounds from Al-Sayyid Bedouin Sign Language (ABSL) that Meir *et al*. (2010) list.

(14) a. gun+police [ABSL]
'soldier'
b. sweat+sun
'summer'
c. tap-on+strong
'iron' (Meir *et al*. 2010: 319)

It is hard to trace a head–complement relation in the above compounds; hence, it is equally difficult to assume a syntactic analysis that relies on such a configuration. Compounds like the ones given in (14) are by no means restricted to ABSL, but still observing patterns in a language that is only a few decades old can be very insightful when the discussion revolves around how certain constructions emerge and how they are linked with the operations that give rise to them. With respect to syntactic configurations in compounding, *dvandva* compounds (i.e. reduplication of Sanskrit *dva* 'two', literally 'pair') can be quite informative (15).

(15) a. car+plane+train [American Sign Language]
'vehicle'
b. clarinet+piano+guitar
'musical instrument'
c. mother+father+brother+sister
'family' (Klima & Bellugi 1979: 234–235)

The X-within-X patterns observed in the *dvandva* compounds in (15) show no head–complement relation and no counter-cyclic derivation. Therefore, they cannot be linked to arguments about 'anti-recursion'. Needless to say, exocentric compounds are also found in spoken languages, though I do not deem it necessary for the present discussion to provide an exhaustive list of compound patterns across languages. The above examples sufficiently illustrate the existence of X-within-X structures with functional and lexical categories across different languages in different modalities.

At this point, one may wonder about how exactly the data presented in this section would deal with all the evidence that exists in the literature in support of the anti-identity condition (summarized recently in Alexiadou 2014a; see also Hoekstra 1984, Pesetsky & Torrego 2006, Heck 2010). The answer is that the present discussion does not cast any doubt on the mechanics of Distinctness through which anti-identity is achieved in a number of cases (e.g., by adding structure in nominalizations: *the destruction of the city* vs. **the destruction the city*; Alexiadou 2014a). Many examples can be cited here as illustrating the existence of an anti-identity thesis: structure that is added or deleted in a way that splits [X(P)X(P)] into [X(P)Y(P)X(P)] or an extra feature on one of the elements that participate in an apparent [XX] construction would call for an [XY] analysis (e.g., see Heck 2010 on a fine-grained analysis of double complementizers in Polish, recursive prepositions in English, and other phenomena). It is important to note at this point that both the conceptualization of Merge presented here and the above listed examples complement such analyses rather than not question or even replace them.

To explain this further, I have proposed that unmediated [X(P)X(P)] structures do surface cross-linguistically. Of course, this does not alter the fact that in *many* other cases the mechanics of Distinctness mediate same-category embedding, and some analyses correctly point this fact out. In terms of the conceptualization of Merge, I argued in this section that (i) [XX] structures surface cross-linguistically because any possible restrictions to same-category embedding do not arise in narrow syntax but at the level of the interaction of syntax with the interfaces, (ii) Merge is unconstrained in this respect, and (iii) categories/labels do not boil down to a property (i.e. a categorial feature) to be inherently found in the SOs they characterize. Crucially, these are precisely the conclusions individually hinted at in much recent work on Distinctness. For example, Alexiadou (2011, 2014a) notes that well-formed examples that violate Distinctness exist and that the insertion of morphological material as a means to mediate [XX] is unexpected if one accepts that morphology merely interprets syntax (point (i)). Heck (2010: 18), after reviewing a broad range of phenomena across languages, reaches the conclusion that "[o]ne might […] suspect that categorial features do not exist to begin with" (point (iii)). For van Riemsdijk (2008), the anti-identity condition (what he refers to as Identity Avoidance) is neither a syntactic ban nor a property of narrow syntax, but rather a property that holds at the interfaces (points (i) and (ii)).

As a matter of fact, van Riemsdijk (2008) makes an even broader and far more interesting claim (especially from a biolinguistic point of view) when he suggests that Identity Avoidance might not even be specific to language but rather be "a general principle of biological organization" (p. 242). The discussion in Walter (2007) points to the same direction. She argues that an anti-repetition bias is due to the fact that repetition poses a problem to the perceptual system in a way that can affect linguistic processes but also other cognitive domains such as vision. In my view, talking about a general cognitive bias on repetition avoidance instead of a hard linguistic constraint that bans same-category embedding explains why examples like the ones presented in this section are possible, while at the same time Distinctness is achieved through many ways in countless other examples.

Naturally, defining what counts as a category is of key importance in the present discussion. Put differently, the next section asks whether the examples of the present section could be analyzed as X-within-Y.

3. Restrictions to Same-Category Embedding and the Nature of Categories

Any discussion of same-category embedding advocates the necessity of having a theory about what counts as a category. In the present discussion, I hold a mainstream view of categories. More specifically, I follow Chomsky's (2001) clausal skeleton in assuming that we can reliably talk about 'Core Functional Categories' in terms of their syntactic configuration (e.g., complementizers vs. determiners). Arguments for X-within-X are constructed on the basis of the criteria upon which elements A and B (e.g., demonstratives and determiners) are claimed to be sufficiently different to be considered falling into two categories; and defining 'same' when one talks about *same*-category embedding might turn out to be a very slippery turn. In other words, one should make sure that what is described as X-within-X is not reducible to an analysis of X-within-Y, where X and Y are instances of elements of similar or (closely) related categories but *not* of the same category.

References to cartography are recurrently found in different approaches to (same-category) embedding mentioned in previous sections. For instance, the existence of adjacent PPs where the Ps come with different flavors (e.g., directional, locative, etc.) has its roots in cartography (den Dikken 2009). This resort to cartography is vital and it is no accident that Demonte & Fernández-Soriano (2009) argue against a recursive C in Spanish, acknowledging Rizzi (1997) as their point of departure. Apparently, the cartographic enterprise seems to provide a set of theoretical assumptions that works nicely for postulating restrictions on recursion, on the basis of arguing that functional elements that look alike eventually belong to different syntactic categories.

Consider, for example, the existence of D-within-D in Greek. I have earlier argued for adjacent D heads in (1), repeated in (16) below. (1) involves a demonstrative and a determiner, hence it is subject to different analyses that affect its status as an X-within-X pattern by possibly arguing that the adjacent Ds belong to distinct categories.

(16) afto to telos [Greek]
this the ending
'this ending'

It is likely that different analyses would place the demonstrative in a functional projection other than D; for example, a Dem^0 preceding an AP (e.g., Leu 2008).[5] If one assumes that the demonstrative and the determiner are different types of elements, (1) is not an instance of X-within-X. The question is whether there really are instances of X-within-X [XX] that *cannot* be described as X-within-Y [XY]. The answer is positive. (3) reproduced as (17) below is one such case; not only are they both indefinite articles, they are two instantiations of the same article too.

(17) (Kapçi) kapçi kala θa kanun na kitun ti ðulia tus. [Greek]
some some well FUT do.3PL SUBJ see.3PL the work POSS
'Some people would do well to mind their own business.'

Returning to the discussion of categories, Rizzi (1997) puts together a variety of constructions from a number of languages to describe the nuances of 'the fine structure of the left periphery'. Within such a cartographic approach that puts forth a highly articulated structure of functional and lexical heads—but also morphemes, certain types of adverbs and adjectives, quantifiers, classifiers, numerals, and many others—a number of instances of X-within-X could be accounted for the basis of one and only one argument: What counts as X is not the same category in both cases. Put differently, even the most minimal morphosyntactic difference could be treated as a sufficient basis for extending one's non-exhaustive inventory of projections by adding yet another element to it. Recent studies in cartography roughly estimate the number of such projections at up to 400 (Cinque & Rizzi 2008: 47).

Given that different heads attract different features and that the inventory of the latter is also a bottomless pit,[6] both postulating more primitives for these

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5 Despite the fact that I discuss the possibility that, under this approach, the demonstrative would fall under a functional projection other than D, it is important to emphasize the fact that a syntactic analysis along the lines of Leu's $[_{DP}\ [_{DemP}\ Dem^0\ [_{AP}\ A^0]]\ D^0\ [NP]]$ would yield a linear order that does not work for instances of demonstrative doubling in Greek (optionally combined with determiner spreading in (ii)), as (i) shows; see also Alexiadou (2014b) for the structure of DP in Greek.

(i) *afto kokino to vivlio [Greek]
this.DEM red the.D book
'this red book'

(ii) afto *(to) kokino (to) vivlio
this.DEM the.D red the.D book
'this red book'

6 In effect, these two inventories feed one another, at the end giving rise to a highly stipulative, open-ended array of linguistic primitives. Boeckx (2011) comments on the pervasiveness of features in all domains of linguistic inquiry by arguing that it is dubious whether they can go beyond language-specific particularities and approach language as a biological organ instead of languages through descriptive grammars. In this context, his suggestion to

inventories and dismissing X-within-X patterns on the basis of these primitives is anything but minimalistic; and it certainly does not abide by principles of economy and parsimony. In other words, if one is willing to pursue such an argument, the effortless way to deal with almost every instance of X-within-X that I showed above is that—paraphrasing the old lady's triumphant assertion that "it's turtles all the way down" (Ross 1967: v)—it will always be categories all the way down.

3.1. Categories All the Way Down

If one is determined enough to stretch the argument about different categories, almost all [XX] adjacencies could be explained away by claiming that what looks like [XX] is in reality [XY]. Alternatively, the direct nature of the embedding might be questioned, so that [XX] would be [XYX]. The question is whether we wish to insert more pluralism and complexity into an already over-articulated conceptualization of categories in exchange for approaching issues like X-within-X structures and the way these are manifested across languages and modalities. If we do so, by extension, we insert complexity into our conceptualization of innateness as well, since cartographers argue that their inventory of projections is part of Universal Grammar. In my examples of recursive C, cartography might easily facilitate alternative analyses that show the different C heads as falling to different categories. However, cases like (11)–(13) and (17) are harder to dismiss.

The response to analyses that assume categories all the way down requires shifting the object of inquiry from languages to the language faculty. The course of evolution would never have endowed our species with the exuberance of having a UG that encapsulates a humongous array of parameters (Newmeyer 2005: 53)—or projections, features, and whatever other name one may employ—all of which UG-encoded. And in the case of projections, these would even come along with their own possible permutations, since not all languages manifest the same surface linear order for all these projections. We do not capture linguistic primitives this way; we create them. This process lacks explanatory adequacy, since the relevant observations are not derived in any way if they are just taken for granted by means of being relegated to the 'innateness' factor, that is, treated as UG-encoded. When describing linguistic data, one should be clear about what the goal is. Illuminating, in this sense, is the late Sascha Felix's view about the orientation of current work carried out in the field of linguistics:

> In some sense I feel that much (but obviously not all) of current linguistic work displays a relapse to the spirit prevailing in pre-Chomskyan times. *Linguistics is about describing language data. Period. Beyond this there is no deeper epistemological goal. Of course, those who became linguists because they like to play around with language data could not care less, because they can pursue their interests under any development of the field, nowadays possibly with less pressure and stress.* Personally I felt that much of what I was offered to read in recent

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"recover from our severe case of featuritis" (p. 224) finds application also in the case of the endless array of projections that one assumes, usually on the basis of different features/functional categories.

> years was intolerably boring and that the field of linguistics was becoming increasingly uninteresting and trivialized. (Felix 2010: 71, emphasis added)

One of the goals of the biolinguistic enterprise is to approach the language faculty in a way that facilitates the creation of interdisciplinary bridges between linguistics, biology, neuroscience, and other allied disciplines. Establishing these bridges is easier once the right level of granularity across the primitives of the different disciplines is achieved. By not taking highly articulated syntactic structures for granted in our theory of innateness, we effectively narrow down UG to a *few* computational principles (as Di Sciullo *et al*. 2010 suggest we should do). As a result, we obtain a picture of our endowment for language that is easier to work with from a biological point of view. If anything, it is this picture that has the potential to overcome the 'granularity mismatch problem' (Poeppel & Embick 2005). The anti-cartographic approach pursued in the present work is in line with the view that Bouchard (2012) expresses in his discussion of the nature of UG: When we abide by a false stipulation, "we are not capturing a generalization but creating it, at a cost" (p. 12).

Juxtapose Bouchard's view with Shlonsky's (2010) thesis that cartography attributes a cardinal role to features and that "[t]he study of the feature inventory of UG requires a massive database compiled on the basis of detailed studies of particular grammars" (p. 424). This approach is problematic in three ways: First, it suffers from inserting unnecessary complexity into UG, stemming from the urge to invent ever more features. This is the exact opposite of what Chomsky (2007) had in mind when he suggested "approaching UG from below". Second, precisely because this complexity translates into an evolutionarily implausible theory of UG (cf. Newmeyer's view reported above), interdisciplinary bridges between linguistics and biology remain elusive. Third, this approach seeks to inform the primitives of UG based on the patterns observed in language data. However, observing the environment can be utterly uninformative for understanding UG: Cartography reflects hierarchies that one finds across different languages. But how can one be sure that language diversity does not fail to project the whole possible range of them? More importantly, even if it does project them, why should one translate this diversity into features and projections in UG?

3.2. *Categoryless Categories: The Case of Riau Indonesian*

One possible way to go about understanding the nature of categories is to discuss their status in languages that show an idiosyncratic level of complexity due to environmental triggers. Complexity is understood in this context as the development of grammatical markers in a language.[7] Through grammaticali-

[7] Language is a complex adaptive system, and discussion of the emergence of complexity in language can be linked to other formal systems. A number of recent proposals have grounded generative operations in mathematical approaches to nesting and complexity: Fortuny-Andreu & Corominas-Murtra (2009), for example, present a theory of nesting through an algorithm that generates hierarchically organized linguistic expressions. As they correctly note, "nests are a useful representative tool in other domains besides language where either some recursive algorithm or evolutionary process is at work, which suggests the unifying force of the mathematical abstraction

zation, lexical items lose some of their phonological substance and/or semantic function and develop morphological or syntactic functions. In a nutshell, the process of grammaticalization entails a gradual progression from something being a semantically contentful item (i.e. a lexical element) to a grammatical marker (i.e. a functional element). The link between complexity and environmental needs has been explicitly established in Wray & Grace (2007), who argue that esoteric, intra-group communication allows for grammatical/semantic complexity, whereas exoteric, inter-group communication leads to the development of rule-based regularity and semantic transparency in language. Gil (2009) suggests that the level of grammatical complexity that is needed for some contemporary cultures is no greater than that of an Isolating-Monocategorial-Associational (IMA) language. The second characteristic of such a language, which according to Gil characterizes an early stage in the phylogenetic development of human language, refers to the absence of distinct syntactic categories. According to the description of the IMA prototype in terms of complexity provided by Gil (2009), no contemporary language absolutely satisfies this prototype, but there are some examples of Relative IMA languages. One such language is Riau Indonesian described in Gil (1994 *et seq.*).

Basic sentence structure in Riau Indonesian might consist entirely of items that reflect the underspecified, monocategorial character of the language. Gil (2009) describes (18) as underspecified in terms of thematic roles and indeterminate with relation to ontological categories. As such, the possible interpretations of (18) go beyond mere ambiguity.

(18) ayam makan [Riau Indonesian]
chicken eat
(an association of CHICKEN and EAT) (Gil 2009: 23)

(18) might mean, depending on the context, that 'the chicken is/was/will be eating' or 'the chickens that were eaten' or 'the reason chickens eat'. It seems that lexical categories such as V and N or functional categories such as T are underspecified in Riau Indonesian (Gil 2009). The classification of the categorial status of *ayam* or *makan* is not intrinsic to syntax but arises post-syntactically (Leivada 2015). In Yoder's (2010) re-analysis of Gil's examples, the argument is made that

[their proposal] is based on" (p. 99). With respect to complexity, Lupyan & Dale (2010) present a statistical analysis of over 2,000 languages which shows that language structure and complexity are determined in part by social structure. They present a linguistic learnability landscape for languages and they formalize this landscape as a mathematical model. According to their 'Linguistic Niche Hypothesis', a relationship exists between linguistic structure and social structure in the sense that "the level of morphological specification is a product of languages adapting to the learning constraints and the unique communicative needs of the speaker population" and "the surface complexity of languages arose as an adaptation to the esoteric niche" (p. 7). As Boeckx *et al.* (2013) note, apart from reflecting statistical correlations, the predictions of Lupyan & Dale (2010) map nicely onto the findings elicited by a task that examines interpretations of spatially modulated verbs in Nicaraguan Sign Language. More concretely, Senghas (2003) reports a mismatch in form that can be observed from one age cohort to the cohort that follows and suggests "that each age cohort […] transforms the language environment for the next, enabling each new cohort of learners to develop further than its predecessors" (p. 511). Complexity evolves gradually in natural languages, and thanks to this gradual development, certain insights about the nature of categories can be obtained from languages that are still in their early stages of development.

Riau Indonesian has the absolute basic categories (V, N, A); but still the examples Yoder analyzes are indicative of the reduced grammatical complexity and the underspecified nature of lexical items, for instance, when an adjective functions as an argument of the predicate (19). Even if one accepts that Riau Indonesian possesses basic functional categories, it is clear that these are not as elaborate as cartographic approaches portray them.

(19) masok putih, masok putih, masok putih [Riau Indonesian]
enter white enter white enter white
'The white one is going in, the white one is going in, the white one is going in.' (Gil 1994: 182)

Muysken (2009) addresses the origin of functional categories and proposes a co-evolution model: As lexical systems became richer and more complex, functional categories emerged at the syntax–lexicon interface. This gradual co-emergence suggests that it is possible that in the early stages of language development or in cases of languages that come alongside a special 'environment' factor, salient morphosyntactic distinctions of categories might not be present. This possibility is already acknowledged in Chomsky (1980), when he argues that the development of some complex structures is subject to the degree of stimulation received from the environment (see also Boeckx *et al*. 2013). Pursuing this line of reasoning implies that syntactic categories might materialize underspecified in the early stages of language development and then gradually become more complex depending on environmental triggers. In other words, the status of X in X-within-X might be underspecified enough that it is not possible to reconstruct X as Y and argue against same-category embedding.

Given that cartography has long been the point of departure for a plethora of studies, it is probably not easy to dismiss it without proposing alternatives. Therefore, attempts have been made to bring together some of these alternatives such as the collection of papers in van Craenenbroeck (2009). Although different contributions to this volume reached different conclusions (some claiming that there is no alternative to cartography, others recognizing the shortcomings of the cartographic enterprise), it seems that the answer with respect to why direct self-embedding manifests with certain constraints in the majority of modern languages relates both to one's understanding of categories (i.e. what counts as X instead of Y in X-within-X) and to interface filters. Put differently, interface filters constrain the existence of X-within-X patterns once a sufficient degree of complexity has been attained in a given language. However, given the unconstrained nature of Merge, these patterns can still arise. Similarly, the cross-linguistic hierarchy of functional projections that cartographers observe should be approached as the by-product of (UG-externally) imposed conditions on well-formedness and computational efficiency derivable from the syntax–semantics interaction (and possibly other principles involved in language processing) and not merely taken for granted under the designation 'UG'.

One such recent attempt to derive the functional hierarchy is Ramchand & Svenonius's (2014): The clausal tripartition into C > T > V is proposed to have its grounds on conceptual primitives such as events (i.e. VPs), situations (i.e. TPs), and propositions (i.e. CPs). This tripartition is reminiscent of Grohmann's (2000, 2003) suggestion to build phrase structure around three domains (CP, TP, VP), which may themselves consist of finer structure and in this sense be 'prolific'. Drawing a distinction between Rich Functional Hierarchies and Core Functional Hierarchy, Ramchand & Svenonius (2014) suggest that these two have distinct origins with the former being language-specific and developed individually across languages and the latter universal. Of course this effort to derive hierarchies should expanded to other domains (e.g., adverbs or prepositions), but the details of these analyses remain to be worked out.

4. Outlook

This paper focused on X-within-X structures across different languages. I showed that restrictions on same-category embedding are not as robust as portrayed in the literature. In this context, restrictions on category distribution should better be approached as by-products of UG-externally imposed conditions on well-formedness and computational efficiency. X-within-X patterns come 'for free' in such a system and need only be constrained by output conditions. In order to ensure efficiency also in terms of the theory of language put forth, we need to cast a limit to the ever-growing inventories of structural primitives—be it features or functional projections. These inventories are nurtured by an approach to language in the following sense: Language is a descriptive set of rules that aims to present a range of grammatical phenomena observed in languages. These presentations of grammatical phenomena play a significant role when one's goal is to describe the grammar of a given language in detail, but may not always be equally informative when one aims to approach the language faculty itself as well as its primitives from a biologically plausible point of view.

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Locality in Language and Locality in Brain Oscillatory Structures

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From the perspective of brain oscillations, an explanation is offered as to why external systems of language cannot deal with identical categorial elements in certain local domains. An equivalent locality effect in brain structure is argued for which causes a (cognitively problematic and ambiguous) synchronization of rhythms in the *gamma*, *beta1*, and *beta2* bands. These rhythms can be related to different categories, and their limited patterns and interactions may explain syntactic constraints on phrases, phases, and Internal Merge.

Keywords: brain oscillations; constraints; dynomics; labeling; locality

1 Introduction and Background

One of the most extensively investigated issues in linguistics is locality: Operations take place within concrete domains or chunks of structure, which is manifested in turn by the cyclicity of the derivation. At first glance, two kinds of locality constraints are discernible in language: within domains, or short-distance, and across domains, or long-distance. However, the cyclicity of operations like Internal Merge (IM, traditionally known as movement) makes it possible to reduce constraints across domains, keeping them within. These domains could be phrases or phases; in fact, there are recent studies which argue that phrases and phases are very close to each other (Epstein & Seely 2002, Müller 2004) and strongly correlated to projection or labeling (Narita 2012, Boeckx 2014a).

Inspired by Boeckx (2008) and his unification of the products of External Merge (EM) and IM, where projection is treated equally in phrases and chains, I offer an explanation about combinatorial or interpretative constraints within (and apparently across) locality domains, in a phase-like fashion, from a single logic. Concretely, the constraints will be couched in terms of brain oscillations (Buzsáki 2006), roughly: They arise from the limited oscillatory patterns that

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certain local brain structures can sustain. This is a first expansion of the research first and originally presented in Ramírez (2014).

Brain oscillations are the emergent mechanism by which brain activity is self-organized (Buzsáki 2006). Biophysical properties of brain components and their interactions locally and globally submit brain activity to rhythmic patterns, as reflected by electroencephalographies and magnetoencephalographies (see chapter 4 of Buzsáki 2006 for an overview about recording methods). At different spatial scales, periods of high activity resulting from the synchrony of neural excitation—within miliseconds time windows—alternate with periods of low activity produced by coordinated inhibition. Such phases enable, respectively, the integration and segregation of information, forming assemblies (Hebb 1949) both at the level of coherent representations (Gray & Singer 1989, Engel & Singer 2001) and transient networks (Fries 2005). There is not a unique brain oscillation but a huge amount at multiple frequencies ranging from .05Hz to 500Hz (Buzsáki & Draguhn 2004). The most popular bands are *delta* (1-4Hz), *theta* (4-7Hz), *alpha* (8-14Hz), *beta* (15-29Hz), and *gamma* (30-90Hz).

The principles governing these oscillations can be explanatory regarding locality. From a cognitive perspective, the constraints are roughly reflected by a conflict at the 'external systems' interpreting *XX-like constructions. We will name this phenomenon 'anti-identity', based on the work of Richards (2010) and Boeckx (2014a). Intra-phasally, these authors note that phase complements cannot contain two identical categorial elements (2). However, this can also be reflected by selection constraints within phrases (1).[1]

(1) a. * John *v* [eat [apples] [oranges]].
b. * [. . . [John] [Mary] *v* [eat apples]].
c. * V X X / X X V

(2) a. *sono [queste foto del muro] [la causa della rivolta]. *Italian*
are these pictures of-the wall the cause of-the riot
'These pictures of the wall are the cause of the riot'.
(adapted from Moro 2000)
b. *Describieron [a un maestro de zen] [al papa]. *Spanish*
described to a master of zen to-the pope
'They described a Zen master to the pope'.
(adapted from Boeckx 2008)
c. * V X X

Similarly, Grohmann (2000, 2011) points to an "anti-locality" constraint (3), which, very roughly, bans (movement) dependencies in local chunks of structure that are transferred to the external systems (unless repaired later by a sort of spell-out mechanism). Despite their similarity to phases at first glance, Grohmann (2011) restricts these chunks to 'Prolific Domains' where thematic, agreement and discourse relationships are established (which would correspond to *v*P, TP, and CP).

[1] For expository purposes, I keep the structural details to a minimum, especially because the assumption of an exoskeletal labeling mechanism (Boeckx 2014a) would introduce structures that generative tradition is not very familiar with. The main interest lies in identifying the coexistence of similar elements.

(3) a. *... T ... [John$_i$ likes t$_i$].[2]

(adapted from Grohmann 2011)

b. * [X V X]

To put it in broad terms, depending on whether the local domain (3) is considered VP or *v*P (see Larson 1988, Hale & Keyser 1993), at least two kinds of anti-locality can be defended. As discussed by Grohmann (2011), a more classical approach to anti-locality takes the relevant domain to be XP, so intra-phrasally there is a maximum of one occurrence of each element. In contrast, a more recent approach, consistent with Grohmann's particular perspective, takes the relevant domain to be larger, so there is a maximum of an occurrence of each element within each prolific domain.

A third approach would consist of defending that the relevant domain in (3) is a phase complement, which would bring anti-locality and anti-identity very close to each other. However, a more elegant idea, a truly attractive one, is to unify these three possibilities, arguing that the relevant domain in (3) is at the same time a phrase and a phase, which in turn corresponds to the interpretatively coherent units at the interfaces that Grohmann's Prolific Domains define.

In this respect, Grohmann (2011) remarks the appeal of fusing 'standard' locality with 'anti-locality' domains, although this would be "by no means necessary". Nevertheless, to deepen our understanding of these strikingly similar locality phenomena, such unification may be "by all means necessary". For space reasons, I will not go into details, but it should be noted that the arguments against such unification are rather weak. It has been argued that intra-phrasal anti-locality is redundantly barred by Last Resort considerations or a ban on vacuous operations; however, given the present assumption that Merge is free and the abuse of feature-driven explanations (Boeckx 2014a), these counterarguments lose their strength. Besides, it has been argued that Prolific Domains differ from phases in that the first are three, whereas the latter only two (*v*P and CP); in spite of that, phases begin to be related to deeper requisites for syntactic derivations (Boeckx 2014a), providing a category-neutral definition of them rather than recasting old concepts of barrier nodes (Boeckx & Grohmann 2007b).[3] Finally, Müller (2004, 2011) argued for an unification of phrases and phases, and for a reformulation of the Phase Impenetrability Condition (Chomsky 2000, 2001), in phrasal terms. This approach is based, among other reasons, on the lack of correspondence between spell-out domains and classical Chomskyan phases, and the successive cyclic movement through every phrase edge (Boeckx 2003), as reflected by morphological side-effects, reconstruction operations, etc. Even though one may think that Müller's (2004, 2011) approach suffers from 'featuritis' (Boeckx 2010) and that constraints such as Ph(r)ase Balance (Heck & Müller 2000, Müller 2004) may not be so persuasive, I think that defining (anti-)local domains as phrasal provides us at least with a theoretically superior alternative (see Boeckx 2007). In sum, it is not illogical to analyze anti-locality in the same terms that anti-identity is approached in the present article.

[2] Classical movement traces are adopted only for expository purposes.

[3] As a matter of fact, Grohmann (2011) discusses the possibility that some verbal Prolific Domains may be nominalized. Such expansion is similar to the one suffered by phases once a more category-neutral definition of them is defended.

Inter-phasally, minimality effects have been noted since Katz's (1964) A-over-A principle (for a review, see Lahne 2012), where elements higher in the structure, but below the probe (Chomsky 2001), act as interveners for or blockers to the movement of lower elements of the same type (Rizzi 1990). This is illustrated in (4).

(4) a. How do you think [he *v* [behaved (how)]]?
(adapted from Rizzi 2011)
b. *How do [who *v* [behaved (how)]]?
c. V [...X... [...X...]]

The aim of this paper is to explain all these locality constraints from a single principle couched in brain oscillatory terms: The brain tends to synchronize its activity (Buzsáki 2006), which in turn conditions the patterns that local peripheral language regions can sustain. Regarding the 'periphery of language regions', we will follow Boeckx's (2014b) suggestion to reverse the mainstream neurolinguistic assumption that language relies on a *core* set of specific regions, that is transiently assisted by a structural *periphery*, responsible for domain general operations (Fedorenko & Thompson-Schill 2014). On the contrary, once language is decomposed (Poeppel 2012), the best candidate for its mechanisms could be provided by the temporal dynamics of domain general regions (Boeckx & Benítez-Burraco 2014, Ramírez *et al.* 2015). The latter would form the actual language *core*, while the periphery would be limited to subareas of the classical Broca—Wernicke model (see Friederici 2011 for a review), where more specific computations would take place.

To explain all the apparently distinct constraints above from a single principle in classical language subareas, it is needed to equate what seems to be locality across domains to locality within domains. For cases like (4), the cyclic nature of IM allows us to extend the *XX constraint we see in phase complements (2) to IM (3), as Chomsky (2013) points towards. Thus, as (5) shows, both elements involved in the apparent inter-phasal restriction are *not* in different domains when the constraint actually applies.

(5) a. *How do you wonder [who (how) *v* [behaved (how)]]?
(adapted from Rizzi 2011)
b. *V [...X X... [...]]

Both intra- and apparent inter-domain constraints can be considered, then, as local limitations of merge or intolerance for ambiguity (Boeckx 2014a) in external systems. Next, on the premise that (linguistic) cognition is constructed over brain rhythms (Buzsáki 2006, Boeckx & Benítez-Burraco 2014), it makes sense to argue that locality effects in language arise from locality effects in brain structures over brain oscillations. Developing Boeckx's (2013: 10) idea that anti-identity constraints "may result from constraints imposed by how many distinct rhythms the brain can couple in particular activities", I argue that an ambiguous synchronization of rhythms in the *gamma-beta* bands, due to a locality effect of the sub-regions in classical language areas (at the periphery), leads to anti-identity effects on a cognitive level. However, this effect may not hold at the large-scale core language network, where the hippocampus, the thalamus, and the basal ganglia, with their canonical *theta*, *alpha*, and *beta* rhythms, respectively, may enable neural syntax (Buzsáki 2010, Buzsáki & Watson 2012) of a higher complexity (Ramírez *et al.* 2015).

A review of the cognitive neuroscience literature will suggest that non-Phase Heads (PHs) may oscillate at *gamma*, that transitive PHs—those with complex complements Boeckx (2014a)—would do so at *beta2*, and that intransitive PHs—those with singleton complements—oscillate at *beta1*. In fact, this triangle represents the three elements of phrases/phases: complement, head, and specifier/edge. In like manner, a review of the theoretical linguistics literature will suggest that intransitive PH, specifiers and internally merged elements might be unified, in the present model, under the same *beta1* categorial rhythm. This will allow us to explain constraints on ph(r)ase structure and movement across domains in the same terms used intra-ph(r)asally. Finally, some space will be devoted to constraints on adjunction and alternatives to the proposed implementation.

To explain the constraints that cause these linguistic properties, I will develop the idea that regions of peripheral systems that interpret the three kinds of items listed above can only sustain a maximum of one rhythm for each of their respective bands. This is so because the structure would be too small to sustain them separately if multiple rhythms were to be initially generated. Since synchrony in the same band becomes mandatory and each band identifies an elementary category, multiple elements of the same type can neither be differentiated nor properly computed in each domain/cycle of the derivation.

Thus, locality effects in language emerge from locality effects on brain activity in relatively small populations of neurons, and ambiguity in linguistic structure is the result of ambiguity in the sustainment of brain oscillations which are responsible for, among other things, identifying elements. Pursuing this kind of research in what has been named by Kopell *et al.* (2014) the 'dynomics' framework, we are getting closer to reaching the implementational level of Marr (1982). In this sense, we might be able to explain why cognition has certain properties and not others from the physiological constraints of the circuits that generate it. This might be an adequate response to the kind of why question advocated in the minimalist program (Chomsky 1995): Why do we have the above-listed properties of cognition? Our answer is that we have them because we have those possible patterns of interactions in neural syntax, and not others.

Furthermore, as will be discussed, such an explanation reflects a lax interpretation of Chomsky's (1986) distinction between I-language in the core and E-language in the periphery, and discusses the computational potential in terms of whether or not subcortical sources of slower rhythms are recruited. Finally, this same constraint explained at the meso-scale brain activity level is applicable to other cognitive domains. Along this line, it offers an alternative view to the limitations of cognitive neuroscience in studies of working memory, consciousness, or attention. If our hypotheses are on the right track, the core rather than the periphery of regions constrains the capacities of the system as well.

2 Labeling Elements by Oscillatory Bands

I assume Boeckx's (2014a) elementary categorization and develop his suggestion that elements are identified as a function of the concrete rhythm that forms and sustains their neural assemblies (Boeckx 2013). A first division can be done by claiming that non-PHs are sustained by *gamma* oscillations and PHs by *beta* oscillations. Furthermore, among PHs, two *beta* sub-bands may implement transitive and intransitive PHs: *beta2* and *beta1*, respectively. As discussed below, intransitive PHs bear appealing resemblances to specifiers and internally merged elements, which prompts one to consider them as a single elementary category. The latter unification, in turn, will extend the explanatory reach considerably, but at the expense of some empirical reach.

These three rhythms are those sustained canonically by the cortex (Roopun *et al.* 2006, 2008). With layer 4 acting as a frontier (Maier *et al.* 2010), *gamma* is mainly registered in supragranular layers, *beta2* in infragranular layers, and *beta1* emerges from the interaction of both infragranular and supragranular layers in certain circumstances as described below. Furthermore, the layered dynamics distinction is reinforced by anatomic connections and the direction of the flow of information from these areas: Supragranular layers connect primarily with higher areas in a feedforward manner, whereas infragranular layers send feedback to the first and connect with subcortical structures such as the thalamus (Douglas & Martin 2004, Bastos *et al.* 2012, Miller & Buschman 2013). Finally, different bands are discretized by different precise scales: A logarithmic scale around 2.16 that differentiates bands (Buzsáki 2006) allows us to potentially differentiate two categories in the *low beta-gamma* range, which would correspond to PHs and non-PHs, whereas a further discretizing golden mean of around 1.6 (Roopun *et al.* 2008) offers us a new categorial distinction within the *beta* range: transitive PHs for high and intransitive PHs for low *beta*. In short, the cortex offers anatomical, dynamic, information-dealing and "mathematical" reflections of that ternary categorial distinction.

2.1 Unifying Intransitive PHs, Specifiers, and IMed elements

Before moving to specific labeling using oscillations, the extension of the categorial rhythm *beta1* of intransitive PHs to specifiers and internally merged elements must be justified to some degree. First,an attempt will be done to unify specifiers and internally merged elements. Afterwards, they will be unified with intransitive PHs.

From a more cognitive point of view, one interpretation of Uriagereka's (1999) multiple Spell-Out model is that specifiers are always derived in parallel to the clausal spine. Although their fusion to the spine, cannot be strictly considered a case of IM, because the structure generated as a specifier is not contained in the spine, I argue that such a combination bears significant resemblances to the (sub)processes of IM. In this respect, IM has been theorized as being decomposed in copy and remerging (Corver & Nunes 2007).[4] If we envisage the copy mecha-

[4] One reviewer points to "more recent formulations under which IM is simply Merge and Label". However, the theoretical option of copy+remerge adopted here is more akin to the model under development. In my opinion, the hypothesis most prone to an interdisciplinary approach should be favored. At any rate, I only sketch a guideline subject to reformulations, not aiming to be conclusive.

nism as a more substantial availability in memory, with certain independence from its derivational history, this may be a feature required both by specifiers and internally merged elements. Being derived in parallel as a specifier may require that elements be available longer than usual before being coupled to the clausal spine; this is also the case for internally merged elements which are susceptible to merging operations longer. Independence from the derivational history may be reflected by the opacity of specifiers and moved elements for sub-extraction.

That copy-like mechanism, consisting of a longer holding in working memory, is a defining feature of the genesis and sustainment of *beta1* oscillations (see below). Thus, at least to a certain point, we can abstract significant commonalities between specifiers and internally merged elements, converging on the attributes of the *beta1* rhythm. This hypothesis makes further sense if we consider the position where internally merged elements land: It is always a specifier-like position, which is also the case for the edge in phases.

The next step to confer plausibility to the intended unification is to find significant commonalities between specifiers/internally merged elements and intransitive phases. In this respect, Boeckx (2014a) argues that specifiers are always intransitive phases, which is what prevents external systems suffering an anti-identity violation, where the combination of the PH of the current derivational stage and the PH merged to its edge takes place. As a matter of fact, in Boeckx's (2014a) theory, there is only one intransitive PH in each derivation apart from the specifiers, which is the one precisely at the bottom of the structure (consider how problematic that derivational point has always been in terms of labeling, etc.).

This not only equates intransitive PH and specifiers to a certain degree (and, by extension, internally merged elements), but also discards the other structures (adjuncts) derived in parallel that could be related to *beta1* as well. Boeckx (2014a) argues that adjuncts are transitive PHs that lead to anti-identity violations in their merge and from which islands, due to a forced transfer, could arise. Furthermore, the aforementioned fact that intransitive heads are those that initiate the derivation significantly connects with how that *beta1* rhythm is evoked: to be exact, by novel/unfamiliar elements, which can be interpreted as the beginning of each (parallel) derivation.

2.2 *Rhythmic Gamma, Beta2, and Beta1 Labels*

The purpose of this section is to justify why these specific rhythms are assigned to their concrete categories. Regarding non-PHs, the essential idea is that they are objects with less combinatorial potential or are simpler. I attribute *gamma* to non-PHs by considering the formation of neural words from Buzsáki's (2010) theory of neural syntax and the role *gamma* plays in binding features into coherent objects, which could be concepts lexicalized later (Buzsáki 2006, Bosman *et al.* 2014, Honkanen *et al.* 2014).

The observation that there is an increase of *gamma* activity as more items are represented or held (Roux & Uhlhaas 2014) suggests that *gamma* represents the contents of working memory (Honkanen *et al.* 2014) as well as items in language (see discussion below on the disambiguation of *gamma* and *beta* roles). Furthermore, *gamma* oscillations are supposed to sustain more local operations due to conduction delays (von Stein & Sarnthein 2000, Buzsáki 2006), all of which connects to their simplified nature with respect to *beta* PHs.[5]

The main property of PHs is that, due to a longer syntactic life, they are more complex than non-PHs in the sense that they act as linkers between themselves and among derivational stages. Such a complexity difference between *gamma* and *beta* has already been shown by Honkanen *et al.* (2014) in the representation of objects in visual working memory, with the more complex being represented by *beta* as opposed to *gamma*. Furthermore, in implementational terms, further complexity may also require the recruitment of a larger neural population, which is in line with the slowing down of *gamma* rhythms until they reach *beta* once PHs are labeled.

I attribute *beta* to PHs by taking into account the functional role of sustaining the *status quo* attributed to the rhythm in working memory (Engel & Fries 2010). Although there is ambiguity in the literature about *gamma* and *beta* in the function of holding items (Dipoppa & Gutkin 2013), there is accumulating evidence to ultimately disambiguate it, and to claim that only *beta* is the rhythm responsible for holding objects (Tallon-Baudry *et al.* 2004, Deiber *et al.* 2007, Engel & Fries 2010, Parnaudeau *et al.* 2013, Salazar *et al.* 2012, Martin & Ravel 2014), whereas *gamma* forms them initially. What defines PHs is that they have a longer merging life, which particularly enables them to be related to more elements. This is visible in the construction of the clausal spine over PHs embedding complements, where PHs function as links between different derivational phases and local domains—they remain at the edge, which enables them to be in at least two phases. Thus, it makes sense that PHs must be cognitive sets held longer in working memory by means of *beta*, which might be the capacity that allows us to transcend the computational complexity of finite state machines (Chomsky 1957).

In addition, the phase-edge presents a computational requirement that has also been noted in goal-directed behavior. One of the functions of PHs remaining at the edge when transfer takes place is to integrate the results of different derivational stages, as heads in phrases integrate complements and specifiers, which, as defended, might be the same case. Equally, Duncan (2013) argues that complex tasks are divided into hierarchically organized sub-goals, which he considers attentional episodes and which must be executed in the proper sequence to accomplish the final objective. Thus, both in the phase-edge and in the succession of attentional episodes, the results of one sub-process must be communicated to the next. Crucially, it is assumed that these kinds of processes require top-down control, which is a mechanism usually attributed to *beta* bands (Bastos *et al.* 2015).

The latter argument in favor of *beta*-holding PHs has to do with basal ganglia implications. Namely, that *beta* holding in memory mechanism can be understood as a selection mechanism provided by the basal ganglia, typically explained as one of its loops being disinhibited, thus favoring one (motor) representation at the expense of others (Koziol *et al.* 2009). Cannon *et al.* (2014) and Antzoulatos & Miller (2014) point to basal ganglia precisely as a cortical *beta* generator which, when added to evolutionary considerations (Buzsáki *et al.* 2013), suggests embrac-

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5 The consistencies with cognitive neuroscience are stronger in Ramírez *et al.*'s (2015) model. Following Boeckx's (2014a) labeling by phase theory, it is argued that all items are born as *gamma* assemblies, and only some of them later become more complicated and are identified by *beta* as PHs. This is due to their derivational history, which is a longer coupling to a merging *alpha* rhythm and a delayed synchronization to a transfer *theta* rhythm.

ing Maier *et al.*'s (2010) hypothesis that subcortical rhythmogenesis of slow rhythms are detected in infragranular cortical layers. Thus, a coherent picture emerges with respect to PHs where basal ganglia, infragranular cortex, *beta*, and holding elements converge on a single mechanism.

Now, a further distinction among PHs can be made: *beta2* for transitive PHs and *beta1* for intransitive PHs (specifiers/internally merged elements). This could also help to disambiguate, in Cannon *et al.*'s (2014: 714) terms, the "mystery of multiple *beta* rhythms". The strongest arguments I can provide are threefold: (i) related to the fact that intransitive PHs are the beginning of each derivation and *beta1* is caused by unfamiliar or novel elements, (ii) related to what has been described as one sub-process of movement, *beta1* genesis occurs from previous *beta2* and *gamma*, in a copy-like fashion, and (iii) related to the longer syntactic availability of intransitive PHs, moved elements and specifiers, *beta1* presents the capacity to sustain representations in absence of inputs. Now, each of these points will be developed.

First, Kopell *et al.* (2010) note that *beta1* rhythm is unchained by unfamiliar, as opposed to familiar, elements. The first can be reinterpreted as novel elements and extended to those initiating a new process. Similarly, Boeckx (2014a) argues that intransitive PHs are those that always initiate the derivation. This strikes me as highly similar, that is to say, the novelty of both PHs and unfamiliar elements. Considering the discussion above, if phase heads are sustained by *beta*, it makes sense to think that the more novel process has to do with the initiation of the derivation and, therefore, unchains *beta1*.[6]

Second, with regard to the copy sub-process of movement, *beta1* genesis may shed light on the issue. *Beta1* rhythm emerges when a period of high excitation decays, and *gamma* in supragranular layers and *beta2* in infragranular layers begin to interact and reset each other (Kramer *et al.* 2008). The result of that interaction, which implies a transient 'fusion' of supragranular and infragranular layers, is that their phases are added up: "[T]he period [of *beta1*] (65 ms) is the sum of the natural periods (25 ms [of *gamma*] and 40 ms [of *beta2*]) of the excitable oscillators" (Kramer *et al.* 2008: 2) (see also Roopun *et al.* 2008). For that *beta1* rhythm to arise, then, the "initial interval of coexistent gamma and beta2" preceding the new oscillation is crucial (Kramer *et al.* 2008: 2).

If labeling PHs is obtained by slowing down the rhythm that initially sustained the assembly of the item from *gamma* to *beta*, what *beta1* genesis suggests would be a second labeling after the first one, a sort of dual process. In the first step, the initial part of the process would be when the assembly that represents the externally merged PH oscillates at *beta2* in the period of high excitation, and the supragranular layers at *gamma* receive a kind of feedback or top-down signal as *beta2* in the infragranular cortex. Later, when excitation decays, a second labeling-like operation would take place. Thus, the second part of the process would be a slowing down of the assembly and the fusing of the infragranular and supragranular layers into a single *beta1* rhythm. That latter sub-process as a sort of copy/second labeling is strikingly similar to the decomposition of movement operation in early minimalism (Chomsky 1993): Merge and label initially the PH using *beta2* and then copy it using *beta1*.

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6 Furthermore, this makes sense when taking into account that *beta1* arises from the decay of a strong excitation, which might be caused by the awaking of the network by way of bursts.

As discussed above, intransitive PHs, moved elements, and specifiers are closely related: If specifiers are derived in parallel, then maybe what is actually embedded in the clausal spine is really a copy of their PH which, as discussed, is certainly the case for intransitives. This copy-like mechanism not only resembles explanations given in linguistics at a computational level; it also leads to another property that crucially defines internally merged elements: their longer availability to syntax, which leads us to the next argument.

Third, when considering the idea that the internally merged element must be available longer, *beta1* again provides an appealing parallelism. In this respect, Kopell *et al.* (2010, 2011) provide crucial insights by looking at the physiological properties of the rhythm. They note that *beta1* dependence on inhibitory rebound allows it "to continue in the absence of continuing input" (Kopell *et al.* 2010: 3), providing memory for the objects. That extra memory might be just what enables internally merged intransitive PHs to be available longer to syntax. Furthermore, Kopell *et al.* (2011) note that within *beta1*, different assemblies (at *gamma*) can coexist without so much competition between them, which creates a context for simultaneously coding the past and present and relating temporally segregated objects. It is hard for a linguist not to read these different past and current elements as being the different relationships that the occurrences or copies of moved elements establish. So, *beta1* is perfect for comparing new and old information and putting together information from different modalities because of its wider temporal windows rather than faster rhythms (see Senkowski *et al.* 2008 and in particular Dean *et al.* 2012). The same hypothesis of an extended window of integration without further input and without much competition between elements is consistent with sustaining the *status quo* as suggested by Engel & Fries (2010) discussed above.

Furthermore, there are two additional considerations that may reinforce labeling by *beta1*: That rhythm stops when the excitation decays too much, or when it is reactivated and replaced by *beta2* and *gamma* (Cannon *et al.* 2014). From both versions of finishing the rhythm, two properties of movement could be inferred. When it decays too much, movement could be barred because of memory limitations. When the cortex is reactivated and *beta1* is replaced by *beta2*, the two *beta2* rhythms could be related to the maximum of two interpretative positions in chains (Boeckx 2012), which is usually attributed to a complete valuation of unvalued features (Chomsky 2000, 2001), barring third interpretative positions for internally moved elements.

Last but not least, when there is *beta1* in the cortex, there is less competition among assemblies (Kopell *et al.* 2011). This could be correlated to why PHs (at *beta2*) can be connected to both complements (at *gamma*) and specifiers (at *beta1*), despite the strong prohibition against *XX oscillations and categories, and to why adjuncts, which might be assemblies oscillating at *beta2*, show stronger constraints on movement or are more opaque.

To close the discussion, further support is needed for the idea that (externally merged) transitive PHs are sustained precisely by *beta2*. In this respect, the strongest argument comes simply from the complementarity of rhythms and the anatomical, dynamic, processing, and mathematic distinctions on the cortical level (as discussed above). If the hypothesis about the 'novelty' or 'familiarity' properties of objects related to *beta1* is on the right track, it makes sense to consider transitive

PHs as more familiar/less newer elements (Kopell *et al.* 2011); therefore, they do not cause *beta1*. Furthermore, if the remainder of the discussion is also on the right track, there is no space for assigning transitive PHs in the oscillatory spectral range I circumscribed the operations to. The ideal scenario would be a strong disambiguation of the functional roles of *beta1* and *beta2* rhythms, but in this respect the literature has yet to make this point clear. Thus, although we should expect further theoretical refinement, there is no significant counterargument for considering *beta2* as being the rhythm responsible for transitive PHs.

3 Ambiguous Synchrony and Short (and Apparently Long) Anti-locality

In the previous section, a way to categorially distinguish three elements as a function of the rhythms that sustain them has been developed. Non-PHs would oscillate at *gamma*, transitive PHs at *beta2* and intransitive PHs at *beta1*. As summarized in Section 1, in certain domains external systems cannot interpret *XX-like constructions. This, translated in oscillatory terms, means that certain brain structures are unable to sustain more than one of those rhythms in each band. So, the constraint can be formulated in the following way: What kind of system cannot sustain multiple rhythms in the same band?

I hypothesize that this is the case of a system that is too local or too small. The brain tends to synchronize its activity in the form of coupled oscillations (Buzsáki 2006). That coupling depends, among other factors, on the distance that separates the neurons. In other words, if the distance is long then there are only certain rhythms that are slow and powerful enough to synchronize cells. In contrast, when the structure is local/small, even fast rhythms are able to acquire the population in-phase. The latter is potentially the case for the language-specific sub-regions in external systems. Within these sub-regions, neurons are so close that their natural tendency towards synchrony forces them to be coupled even in fast rhythms. Thus, if there is a rhythm in the *beta-gamma* band, it will recruit the whole population at the specific oscillatory band regardless of whether independent neurons begin to oscillate independently. In fact, they will be synchronized far too early for multiple assemblies to be differentiated within the same band. Since labeling depends on how many of these rhythms can be sustained, only one category can be identified in each band.

If external systems, like sub-regions of Broca's areas, are local, they cannot sustain multiple rhythms in the same band without synchronizing the whole population and treating the rhythm, and consequently the neural assembly, as a single element. So, external systems cannot simultaneously sustain more than one *gamma*, more than one *beta2*, or more than one *beta1*, which, linguistically speaking, means that there cannot be more than one non-PH, more than one transitive-PH, or more than one intransitive-PH(/specifier/moved element) in certain derivational stages.

What this hypothesis implies is that: (i) the intolerable ambiguity in external systems is equal to an unavoidable and ambiguous synchronization of rhythms in brain structures and (ii) this might only happen in local/small brain regions due to conduction delays, so the locality in language can also be understood as locality in brain activity terms. Thus, we can capture one of the main language constraints or conditions, the anti-identity (Boeckx 2014a), in an implementational fashion.

This enables us to explain the linguistic manifestation of those rhythmic limited patterns, and the structural constraints of phrases and phases. It has been argued that phrases contain a maximum of one head, one complement, and one specifier (see Boeckx 2008 for a feature-valuation approach and Kayne 1994 for one in terms of linearization). My proposal about labeling and anti-identity can explain that ternary structure, as (6) makes evident. There, a maximum of three distinct elements can co-exist in a local domain: one *gamma*/complement item, one *beta2*/head item, and one *beta1*/specifier item.[7]

(6) a. [... head [complement specifier head [...]]]
    b. [... *beta2* [*gamma beta1 beta2* [...]]]

Thanks to the unification of phrases and phases, this ternary structure above can be extended to phase-structure (7). Furthermore, the distinction between transitive and intransitive PHs into sub-bands of *beta* enables us to explain why two-phase heads can co-exist without violating the anti-identity constraint.

(7) a. [... C [T (EA) *v* [...]]]
    b. [... transitive-PH [non-PH (intransitive-PH/edge) transitive-PH [...]]]
    c. [... *beta2* [*gamma* (*beta1*) *beta2* [...]]]

Following the logic of the present model, if we assume that there is only a maximum of one non-PH and one transitive-PH in transferred phase complements (Boeckx 2014a) (with optional specifiers as intransitive-PH embedded in the latter), it is also possible to explain the rhythmic nature of the derivation of the clausal spine, with PHs and non-PHs alternating (Richards 2010) (8). The fact is that more than one *gamma* cannot be sustained and more than one *beta2* cannot either at certain derivational stages, which forces the clausal spine to be formed in that rhythmic fashion.

(8) a. ...[C [T *v* [V *n* [N]]]]
    b. ...[PH [non-PH PH [non-PH PH [non-PH]]]]
    c. ...[*beta* [*gamma beta* [*gamma beta* [*gamma*]]]]

Once we differentiate further between the types of PHs (transitive and intransitive), the pattern above, which is limited to a maximum of one element of a particular category in a phase complement, can explain more typical anti-identity violations. In (9a), two *beta1*/intransitive-PHs cannot coexist in that local domain. On the contrary, as (9d) shows, when we extract one of these conflicting oscillations, the rhythmic patterns become sustainable, since one of the *beta1s* then co-exists with one *beta2* in the next domain/derivational cycle.

(9) a. *sono [queste foto del muro] [la causa della rivolta]. *Italian*
    are these pictures of-the wall the cause of-the riot
    'These pictures of the wall are the cause of the riot'
    (Moro 2000)

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[7] Given an exoskeletal labeling operation (Boeckx 2014a), the constraint arises when the phase complement is transferred. In any case, the limit of elements inside the phrase is the same.

b. *transitive-PH [non-PH(V, be) intransitive-PH intransitive-PH]
c. **beta2* [(*gamma*) *beta1 beta1*]
d. [Queste foto del muro sono [la causa della rivolta]]. *Italian*
these pictures of-the wall are the cause of-the riot
'These pictures of the wall are the cause of the riot'
e. *[intransitive-PH transitive-PH [gamma(V, ser) intransitive-PH]]
f. *[*beta1 beta* 2 [(gamma) *beta1*]]

The same logic can be extended to the ambiguous ungrammatical co-existence of two transitive PHs, which prohibits constructions like (10a). However, turning one of these transitive PHs into one intransitive PH solves the impossible sustainment of two *beta2* rhythms (10d). Furthermore, (10a) impossible sustainment of two *beta2* rhythms in locality might represent the case of adjuncts and islands, generally speaking, if we follow Boeckx's (2014a) idea that adjuncts, in contrast to specifiers, are often structurally equivalent to the transitive-PH they adjoin to. The latter could then explain the less opaque nature of specifiers to sub-extraction, for example, since *beta1* and *beta2* co-existence is possible; this is not the case with two *beta2* in adjunction.

(10) a. *Describieron [a un maestro de Zen] [al papa]. *Spanish*
described to a master of zen to-the pope
'They described a Zen master to the pope'.
(Richards 2010)
b. *transitive-PH [non-PH transitive-PH transitive-PH]
c. **beta2* [*gamma* (V, *describir*) *beta2 beta2*]
d. Describieron [un maestro de Zen] [al papa]. *Spanish*
described a master of zen to-the pope
'They described a Zen master to the pope'.
e. transitive-PH [non-PH intransitive-PH transitive-PH]
f. *beta2* [*gamma* (V, *describir*) *beta1 beta2*]

Given the potential equivalence between anti-identity and anti-locality discussed in Section 1, the same explanation given for (9)—(10) can account for *XX conflicts in structures like (11). If such an hypothesis is on the right track, it could offer "the kind of 'deeper' explanation on independent grounds" that Grohmann (2011: 271) pursues.[8]

[8] However, the repair strategy of spelling out the lower occurrence of the conflicting element is not transparent.

... [John$_i$ likes himself$_i$]. (adapted from Grohmann 2011)
... [intransitive-PH non-PH ??]
... [*beta1 gamma* ??]

I leave this issue open to future research. Speculatively, it must have to do with the insertion of an adjunct element oscillating at *beta2*. At first glance, this would cause a conflict with the ph(r)ase head within the domain, which would be labeled by the same band. Nevertheless, a forced transfer like in the case of adjunction discussed in the context of (10a) may be resorted to.

(11) a. *... [$John_i$ likes t_i].

(adapted from Grohmann 2011)

b. * ... [intransitive-PH non-PH intransitive-PH]

c. * ... [*beta1 gamma beta1*]

Finally, if we continue to assume that internally merged elements are held by *beta1*, we can also explain the *XX intervention effect of movement discussed in Section 1. Chomsky (2013) notes that labeling problems with XP—XP structures can be extended to constraints on movement over intervening elements. All we have to do is reduce locality across domains to locality within domains, represented by the cyclic nature of IM. As (12) makes clear, when one *beta1* internally merged element coexists in a derivational stage with another one, *XX constraints arise in the form of two *beta1*. As detailed earlier, both oscillations are impossible to sustain, because we are again trying to sustain two rhythms in the same (*beta1*) band, and a minimality effect arises.

(12) a. *How do you wonder [who (how) *v* [behaved (how)]]?

(Rizzi 2011)

b. * PH [... intransitive-PH intransitive-PH transitive-PH [non-PH...]]

c. * *beta2* [... *beta1 beta1 beta2* [*gamma*..]]

Despite its empirical and explanatory reach, my model faces a problem: There is no anti-identity violation when, due to cyclic IM, one moved element co-exists with one specifier (13).

(13) a. How do you think ... [he (how) *v* [behaved (how)]]?

b. PH [... intransitive-PH (intransitive-PH) transitive-PH [non-PH ...]]

c. *beta2* [... *beta1 beta1 beta2* [*gamma*..]]

My model predicts an anti-identity effect between *he* and the second occurrence of *how* in (13), given that both are sustained by *beta1* and there is a moment in the derivation when they co-exist. However, the intervention between *he*, the presumed probe of C, and the goal *how*, in Chomsky's (2001) terms, does not cause a minimality effect. What might be the solution then? In Ramírez (2014), I offer an alternative account for specifiers. I argue that they are synchronized with the PH to which they are merged under a single *beta* rhythm. This possible synchronization would explain (i) why binarity is respected in the transfer of elements, contrary to the triplets we observe, (ii) why specifiers are embedded in their heads, and (iii) why they are more integrated than adjuncts, which in contrast would be impossible to be coupled to the PH (speculatively, due to conduction delays). Although that view would still account for constraints on internally merged elements, as long as we sustain their equivalence to *beta1*, differentiating them from specifiers and intransitive PHs would imply that we lose the explanatory power regarding the ternary structure of phrases and phases. Another option would be to increase the number of bands that label items, for instance, resorting to the range from slow to fast *gamma*. That theoretical possibility would be arbitrary without detailed justification and reduce the explanatory power that the above commonalities offer.

However, the problem my proposal faces here is not exclusive to it. One of the lessons in generative linguistics is that the more linguistic data are anayzed in detail, the more exceptions to explanatory theories there are. The phenomena of adjuncts, specifiers, islands, and so on are not fully satisfied nor in Kayne (1994) nor in Uriagereka (1999). Nevertheless, it does not prevent these theories from being some of the most elegant and inspirational ones to have led investigations in the field. Relatively speaking, we should not simply discard the present model because of some counterarguments from data coming from isolated linguistic debates (but see Leivada 2015 for discussion from a biolinguistic approach, and see Section 5 about experimental data). In this respect, inspiration may come from cognitive neuroscience: Conflicting evidence about the functional role of *alpha* oscillations did not discourage pursuing good intuitions; on the contrary, they have led to fruitful debates and a much deeper understanding of rhythm (see Palva & Palva 2007, 2011, and references therein).

It may be better to leave conflicting data to further inquiry, since it has been shown that the ambiguous synchronization of oscillations in local brain structures can potentially account for the ternary structure of phrases (6) and phases (7), the rhythmic nature of clausal linguistic structures (8), anti-identity constraints (9)—(10) extendable to adjuncts in the case of transitive PHs, anti-locality (11), and intervention effects in IM (12). Crucially, just a single common principle can explain the main linguistic properties: The brain synchronizes its activity (locally). The degree of plausibility in that kind of explanation allows it worth pursuing, even if this paper turns out to be completely wrong in its implementation.

4 From Global to Local: Periphery Constraining the Core

The present model also represents, in a seductive way, a lax interpretation of the distinction between I-language and E-language (Chomsky 1986). I-language should be understood here as a global, domain-general computational system, while E-language would be a more local and specific system or an interface which the former connects to. Before transfer to external systems, a domain-general large-scale core set of regions would be used. Thus, we work on a global scale that governs the freer syntax of I-language. However, after transfer, when rhythms are circumscribed to the cortical limitations of external systems, we move to local structures and to sub-regions of the Broca—Wernicke network. We are, in fact, moving from global to local in that transition to what can be represented in E-language. That E-language domain would impose constraints which cannot be expected in the network of I-language because the latter has more computational power thanks to the subcortical sources of slower rhythms.

The neural syntax in both language core and periphery is then governed by the same principles. It is only because of the locality of the structures involved in E-language that we cannot exploit the full potential that large-scale structures offer. As a result, ambiguities arise. That might reflect one of the main advantages of recruiting subcortical sources of slow rhythms: They offer the potential to govern the syntax of language of thought by means of *alpha* and *theta* (and *beta*) oscillations as well (whose presence in the cortex may need subcortical collaboration), which enables a neural syntax exempt from the limits of small regions and, consequently, of a higher complexity.

This kind of constraint at certain local and peripheral structures of a system can be exploited by other theories which deal with locality conditions and capacity limits. As mentioned earlier, these kinds of cognitive limitations have been approached from different perspectives, for instance: (1) regarding the limits of global workspace for consciousness, Min (2010) argues that the limited capacity of attention and consciousness is due to the mechanical limitations of the thalamic reticular nucleus synchronizing processes;[9] (2) regarding attention, Miller & Buschman (2013) speak broadly about a limited 'bandwidth'; (3) regarding working memory limits, Lisman (2010) argues for a maximum of 7±2 *gamma* cycles representing items embedded in a single *theta* cycle, which is exportable to the limitation to 4 items in the interaction of *gamma* and *alpha* (Roux & Uhlhaas 2014); or (4), alternatively, Palva *et al.* (2010) attribute visual working memory limits to a bottleneck effect of oscillations in a hub like the intraparietal sulcus.

Nevertheless, there is, as far as I know, no explanation like the one offered here, where the constraint does not come from the core of the system itself but rather from the periphery to which it connects. This, furthermore, is reinforced by a solid theoretical background in generative grammar that, in this respect, has not changed in recent times. The distinction between I-language and E-language has already been related to the observation that external systems impose constraints that would not otherwise be expected, such as pronouncing only one copy of internally merged elements. In sum, a complementary explanation of capacity limitations may come from peripheral structures.

5 Conclusions: The Unexplored Dimension of Broca's Problem

Joining Kopell *et al.*'s (2014) framework of 'dynomics', it seems possible to explain why cognition has certain properties and not others from the physiological constraints of its brain circuits, understood as dynamic structures with activity at a real time scale rather than as more or less static maps that are mainstream in neurolinguistics (Poeppel 2012).

This approach has drawn our attention to the temporal dimension on multiple scales of brain activity (Buzsáki 2010), which allows a mechanistic explanation of what bars certain syntactic derivations and, consequently, what defines some crucial properties of linguistic structures.

That sort of answer suits the minimalist *why* question, namely why language has certain properties and not others (Chomsky 1995). It does so from an interdisciplinary perspective, fusing neuroscience and linguistics. Such a methodology not only offers a more solid, deeper, and less falsifiable answer, but also provides bridges to other levels of research, from genome to phenome (Boeckx & Theofanopoulou 2014), since, as Siegel *et al.* (2012) suggest, brain rhythms lie just in the middle across various levels of research.

Thus, this article not only contributes to biolinguistics in the *strong sense* (Boeckx & Grohmann 2007a) along the lines of Giraud & Poeppel (2012) in the realm of phonology, but also confers theoretical plausibility and pursues Boeckx

[9] Similarly to the present model, limits are stronger in mono-modality (language periphery) than in cross-modality (language core)

& Benítez-Burraco's (2014) invitation to explore a new dimension of Broca's problem: Brain oscillations.

It seems possible to explain that some principles can be reduced to a physical and general restriction over brain/language/mind structure: Peripheral (language) systems are far too local to sustain multiple rhythms in the same band by which elements are identified.

Of course, the present work is mainly theoretical, so hopefully some empirical testing will, in the future, shed more light on the issues at hand. As a first and specific example, electroencephalographies should not register more than one *gamma*, one *beta1* and one *beta2* oscillation in cortical sub-regions of the Broca—Wernicke network. If they were registered, they should be coupled as single oscillations in very few cycles. Alternatively, due to the interdisciplinary approach of my model, a similar locality effect could be registered in regions usually associated with other cognitive domains and even involving other (fast) rhythms. Then, support may come from neuroscience studies beyond the field of linguistics. Time will tell.

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Linguistics and Some Aspects of Its Underlying Dynamics

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In recent years, central components of a new approach to linguistics, the Minimalist Program, have come closer to physics. In this paper, an interesting and productive isomorphism is established between minimalist structure, algebraic structures, and many-body field theory opening new avenues of inquiry on the dynamics underlying some central aspects of linguistics. Features such as the unconstrained nature of recursive Merge, the difference between pronounced and un-pronounced copies of elements in a sentence, and the Fibonacci sequence in the syntactic derivation of sentence structures, are shown to be accessible to representation in terms of algebraic formalism.

Keywords: algebraic methods; coherent states; deformed Hopf algebra; Fibonacci progression; self-similarity

1. Introduction

The linguistic component of the present work is based on 'generative grammar' (GG; Chomsky 1955 et seq.). Our work deals with a relatively recent version of the theory called the 'Minimalist Program' (MP; Chomsky 1995) and more particularly with a very recent further development over the past few years that has brought linguistics even closer to physics. We will not go into the debate pro and con GG, embodied in a vast literature, out of which we indicate only some basic references.[1]

We show how some MP features are quite well suited to a mathematical representation in terms of algebraic methods and tools. This goes beyond a pure, although difficult, formal exercise, since it reveals the dynamics underlying aspects of

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[1] A use-based explanation of language is offered in Tomasello (2003) and Bybee (2007); for counters, see Wexler (2002), Crain et al. (2009), and Pietroski (2003). For a statistical approach to syntax and language learning, see Reali & Christiansen (2005), Perfors et al. (2011), and Christiansen & Chater (2015); for counters, see Berwick et al. (2011) and Berwick et al. (2013a).

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the MP, which thus appears much richer than one might had suspected. Especially, it uncovers many contact points of the linguistic structure with concrete properties of nonlinear algebraic formalism commonly used in the description of physical systems. Although in our scheme linguistic structures are classical ones, we find that an isomorphism can be established between the MP linguistic structure and many-body field theory. In our opinion, a very rewarding result, no matter from which standpoint one looks at, e.g. recognizing the deep dynamical processes underlying the MP linguistic structures, or, vice versa, the linguistic content of the many-body formalism. The plan of the paper is the following. In section 2, the most relevant aspects of the MP are presented, including a re-analysis of X-bar trees, their self-similarity properties, and their formalization under our schema. In section 3 and its subsections, the interfaces, the manifold of concepts, and the copies of lexical elements are discussed. Section 4 is devoted to final remarks where comments on the entropy and the arrow of time are presented. Finally, in the Appendices A–C, some details of the mathematical formalism are reported. Some properties of the Fibonacci matrix are discussed in Appendix D.

2. The Relevant Components of the Minimalist Program

In the MP, accrued emphasis is put on "third factors of language design,"[2] that is, principles that are not specific to language, nor specific to biological systems; basically, minimal (strictly local) search, minimal computation. In other words, the physics and the mathematics of language. For a broader approach to language and language evolution, see Perlovsky & Sakai (2014) and Perlovsky (2013) and references therein. The most basic and simplest operation now is binary Merge.

The binary, unordered set created by Merge is then Merged with a third element from the lexicon. This binary Merge is recursively repeated until the whole sentence is terminated. The syntactic process, called 'derivation' is similar to a proof ending when the sentence is terminated. In more complex sentences, with subordinates, relatives, or embeddings, the process goes on until the derivation finally stops (Chomsky 2001).

There are intermediate cyclic points of derivational (computational) closure, called Phases.[3] The syntactic derivation (the specific mental computation) stops when a Phase is reached, and then a higher Phase is opened. The process continues inside-out, building higher and higher components in the syntactic hierarchy. All these recursive operations are binary and leave the items being merged unaltered.

There are few components overall: External Merge, Internal Merge, Agree, and the Labeling Algorithm—to which we will return later. There is nothing else in syntax; it is therefore called Narrow Syntax.

In the previous theory of Government and Binding (Chomsky 1981; Haegeman 1991), there where more components and entities. These are now, in Minimalism, subsumed under more basic operations, under a constraint of strict locality. The head gives the name to the constituent it generates (nouns to Noun Phrases, verbs

[2] The other two factors are: genetic predispositions and peculiarities of the local language that the child has to learn (Chomsky 2005).

[3] For greater clarity, we will use upper case P for Phases in syntax and lower case p for phases in physics.

to Verb Phrases, and so on). More generally, we have an {H, XP} construction, a Head and a Phrase. The X in XP is a generalization, meaning that it can be any one of a great variety of phrasal categories.

What were previously (in the theory of Government and Binding) called 'empty categories' (because they are not pronounced or written) are now simplified in terms of copies. Copies come for free, so to speak, because they are elements already present in previous steps of the derivation, for instance, items extracted from the lexicon.[4] The replacement of empty categories with copies of lexical elements (pronounced or un-pronounced—a distinction to which we return below) is a step in simplification and has proved to be a legitimate move in many cases.

In GG the condition of 'strict locality' applies to the structure of the sentence, not necessarily to what is, or is not, 'close' on the surface of the sentence. It has been emphasized that one cannot just count the number of words separating the affected elements in the sentence. What counts are the number and kind of nodes separating the affected elements in the syntactic tree. In GG, long before the MP, this central property of syntax had been called 'structure dependence'. It constitutes a sharp departure from many old and new anti-generativist approaches to language based on statistics or conventions of use.

The generative theory of grammar has allowed a deep analysis of many languages and dialects. It also turned out that the vast majority of all Phrases had the same structure, X-bar structure, which is recursive: An element of the structure (a node of the X-bar tree) can contain another X-bar structure, and so on; recursively, indefinitely.[5]

Perhaps, this is a good point where to insert in our presentation a first part of our algebraic formalization. In fact, we will see that we obtain in a straightforward way the recursivity, or self-similarity, of the X-bar structures.

2.1. *X-Bar Structures, Their Self-Similarity, and the Breakdown of Time Reversal Symmetry*

It has become standard in GG to construct syntactic trees that have only two branches departing from each node. This is referred to as 'binary branching' (Kayne 1984). In fact, we have a collection of binary entities. Lexical items are represented, by useful convention, as $(+,\ -)$, which is written in the matrix formalism by using the standard vector notation $\begin{pmatrix} 1 \\ 0 \end{pmatrix}$ for '$+$' and $\begin{pmatrix} 0 \\ 1 \end{pmatrix}$ for '$-$'. The notation for $(+a, -b)$ is then $a\begin{pmatrix} 1 \\ 0 \end{pmatrix} + b\begin{pmatrix} 0 \\ 1 \end{pmatrix} = \begin{pmatrix} a \\ b \end{pmatrix}$. Thus, Nouns are $(+N, -V)$, Verbs as $(+V, -N)$. This notation can be usefully extended to Phrasal Heads $(+H, -C)$ and Complements $(+C, -H)$. In the syntactic derivation, we have Terminal nodes $(+T)$ and nonterminal nodes $(-T)$. Copies of lexical items, or of larger structures, in a sentence can be pronounced $(+Pr)$ or not-pronounced $(-Pr)$. Recursive applications of Merge may produce a Phase $(+Ph)$ or not $(-Ph)$. The most basic

4 Different languages treat the copies differently. In most languages only the higher copy is pronounced, but there are languages in which the lower copy is pronounced and also languages in which all copies are pronounced. In the latter case, this applies to 'short' elements (equivalent to the English 'who', 'which', and similar), never to whole Noun Phrases.

5 The X is a portmanteau symbol, covering most kinds of Phrases.

syntactic operation, Merge, generates a binary set. This suggests to us to formalize the binary branching in terms of standard formalism of vector or state spaces and matrix multiplications. In the following we will also use the shorthand notation $|0\rangle = \begin{pmatrix} 0 \\ 1 \end{pmatrix}$ and $|1\rangle = \begin{pmatrix} 1 \\ 0 \end{pmatrix}$.

In general, we may consider a collection of N objects ('particles' or 'lexical elements'), which in a standard fashion can be labeled by $i = 1, 2, \dots N$ as $|0\rangle_i$ and $|1\rangle_i$.

In Appendix A we introduce so-called Pauli matrices and the matrices σ^+ and σ^-. The interest in the matrices $\sigma^\pm$ is due to the fact that they generate the transitions between the two states $|0\rangle$ and $|1\rangle$:

(1) $$\sigma^-|1\rangle = |0\rangle \, , \quad \sigma^+|0\rangle = |1\rangle \, , \quad \sigma^-|0\rangle = 0 \, , \quad \sigma^+|1\rangle = 0$$

In order to see how 'binary Merge' between two states is generated, consider these two states $|0\rangle$ and $|1\rangle$. They may represent two lexical elements or two levels of the same lexical element. In the following we will consider generalization to the collection of N elements and restore the index i, which now for simplicity we omit.

In physics, $|1\rangle$ is said to be the excited state with respect to $|0\rangle$ which is called the 'vacuum' or the ground state. The process leading from $|0\rangle$ to $|1\rangle$ is called the excitation process and the one leading from $|1\rangle$ to $|0\rangle$ is called the decay process of the $|1\rangle$ state. We thus start with $|0\rangle$. Of course, we want to move on from the state $|0\rangle$. Here and in the following we do not consider the (trivial) possibility to remain in the initial state $|0\rangle$, which is equivalent to "nothing happens."[6] The interesting possibility is the one offered by the process leading from $|0\rangle$ to $|1\rangle$. According to (1), this process is obtained by applying σ^+ to $|0\rangle$:

(2) $$|0\rangle \quad \rightarrow \quad \sigma^+|0\rangle = |1\rangle$$

Thus, as a first single step the state $|1\rangle$ has been singled out. By 'single step' we mean that we have multiplied $|1\rangle$ by one single matrix, the σ^+, not by a product of σ's. In this connection, consider that $\sigma^+\,\sigma^+ = 0 = \sigma^-\,\sigma^-$. Therefore, the only possibilities to step forward of a single step is given in (2), and from there, one more single step is obtained as:

(3) $$\nearrow \; \sigma^-|1\rangle = |0\rangle$$
$$|0\rangle \quad \rightarrow \quad \sigma^+|0\rangle = |1\rangle \quad \rightarrow$$
(4) $$\searrow \; \sigma^+\sigma^-|1\rangle = |1\rangle$$

Note that application of $\sigma^+\sigma^-$ is considered to produce a single step, since it is equivalent to the application of the unit matrix I to $|1\rangle$. In general, for any integer n, $(\sigma^+\sigma^-)^n\,|1\rangle = 1 \times |1\rangle$. Note also that (3) describes the decay process of the excited state $|1\rangle$ to $|0\rangle$. The equation (4) describes the 'persistence' in the excited

[6] The physical meaning of this is that we neglect fluctuations in the ground state, which can be described by $\sigma^-\,\sigma^+|0\rangle = 1|0\rangle$, i.e. $|0\rangle \rightarrow |0\rangle$. In the quantum formalism, this is achieved by considering the so-called 'normal ordering' or 'Wick product' of the operators. But here we do not need to insist further on such an issue.

state, which represents a dynamically non-trivial possibility and thus we have to consider it. One more step forward leads us to (5)–(7) and so on:

(5) $\qquad\qquad\qquad\qquad\qquad\qquad\qquad \nearrow\ \sigma^-|1\rangle = |0\rangle \ \rightarrow\ \sigma^+|0\rangle \ = \ |1\rangle$

$\qquad\qquad |0\rangle \ \rightarrow\ \sigma^+|0\rangle = |1\rangle \ \rightarrow$

(6) $\qquad\qquad\qquad\qquad\qquad\qquad\qquad \searrow\ \sigma^+\sigma^-|1\rangle = |1\rangle \ \begin{smallmatrix} \nearrow \sigma^-|1\rangle \ = \ |0\rangle \\ \searrow \sigma^+\sigma^-|1\rangle \ = |1\rangle \end{smallmatrix}$

(7) 1 1 2 3

At each step, new branching points $\begin{smallmatrix}\nearrow\\ \searrow\end{smallmatrix}$ (new nodes of the X-bar tree) are obtained and the X-bar tree is generated by recursive σ 'operations', i.e. by multiplying $|0\rangle$ and $|1\rangle$ by the σ matrices, which we also call σ 'operators'. The set of these operations constitute what is named, in technical terms, the "SU(2) transformation group" (Perelomov 1986; see also Appendix A).

The conclusion at this point is that we have the 'number of the states' in these first steps in the sequence: $1\ 1\ 2\ 3$, starting with $|0\rangle$, [one state] , then in equations (2) [one state], (3) and (4) [2 states], and (5) and (6) [3 states], respectively, (cf. (7)).

From here, from the two $|1\rangle$'s, we will have in the next step two $|0\rangle$'s and two $|1\rangle$'s, and from the $|0\rangle$ we will get one single $|1\rangle$—in total 5 states: $1\ 1\ 2\ 3\ 5$. We will get thus, in the subsequent steps, other states, and their numbers obtained at each step are in the Fibonacci progression ($\{F_n\}, F_0 \equiv 0$) with the ones obtained in previous steps. In general, suppose that at the step F_{p+q}, one has p states $|0\rangle$ and q states $|1\rangle$; in the next step we will have: $(p+q)\,|1\rangle$ and $q\,|0\rangle$, $F_{q+(p+q)}$. In the subsequent step: $(p+2q)\,|1\rangle$ and $(p+q)\,|0\rangle$, a total of states $2p+3q = (q+p+q)+(p+q)$, i.e. the sum of the states in the previous two steps, which agrees with the rule of the Fibonacci progression construction.[7]

It is interesting to remark that $(\sigma^+\sigma^-)^n|1\rangle = 1 \times |1\rangle$, for any integer n, can be thought of as a 'fluctuating' process: The σ^- brings $|1\rangle$ down to $|0\rangle$, and σ^+ again up to $|1\rangle$, and so on for any integer n: $\sigma^+\sigma^-$ induces fluctuations $|1\rangle \ \leftrightarrows\ |0\rangle \ \leftrightarrows\ |1\rangle$ (through the 'virtual' state $|0\rangle$); this is the meaning of the fact above observed that $\sigma^+\sigma^-$ is equivalent to 1 at any integer power n when operating on $|1\rangle$. This 'fluctuating activity' corresponds, in the syntactic derivation, to successive applications of Merge. Simplifying a bit, when recursive Merge reaches the topmost node of a Phase, that is, a point of computational closure, everything underneath, in the tree, becomes off limit. The condition called 'Phase Impenetrability Condition' (PIC) (Chomsky 2000, 2001; Richards 2007; Gallego 2012) specifies that nothing in a lower Phase is accessible to the syntactic operations that create the immediately higher Phase. The syntactic objects of the lower Phase and the lower Phase itself are dynamically 'demoted' to a $|0\rangle$ state. The 'fluctuating activity' is also much suggestive when one thinks of the processes (of milliseconds or so) in the selection of lexical items and the recursive Merge of these into syntactic objects.

[7] There are many ways to capture how natural phenomena generate the Fibonacci, or F progression, both in inorganic and organic systems (especially in botanic structures). The present approach is, we think, particularly elegant and especially close to how the F progression is generated in syntactic structures.

Summarizing, we have described the 'action' on the state $|0\rangle$ and $|1\rangle$ by application (multiplication) of the sigma matrices. In the physics jargon, one says that the 'dynamics' of a system is defined once the rule of 'how to go' from one step to the next one in the system evolution is found. Accordingly, in the present case, we can say that the X-bar tree (or F tree) has been obtained as a result of the $SU(2)$ dynamics (namely the set of operations induced by products of σ matrices), with the additional result that its multiplicity of states, its recursivity or self-similarity properties turn out to be described by the Fibonacci progression.

We also observe that the full set of σ^+ and σ^- products compatible with the $SU(2)$ algebra (the products used above and leading, as we have seen, to the F progression) generates what is called the Jaynes-Cummings-like dynamics, which has a wide range of physical applications (see e.g. Gerry & Knight 2005; Blasone et al. 2011). Thus our construction presents features which certainly deserve much attention, since we now have that the X-bar tree, which plays so a crucial role in the MP, arises as a result of a *dynamical* model in linguistic, its recursive property being related to the self-similarity property of the Fibonacci progression. The paramount importance of the Fibonacci progression in language has been stressed by Medeiros (2008), Idsardi & Uriagereka (2009), Piattelli-Palmarini & Uriagereka (2004, 2008), and in Medeiros & Piattelli-Palmarini (in press). References therein cover a variety of instantiations of Fibonacci structures in natural systems ranging from binary stars to ferromagnetic droplets, from botanic forms to brain waves and beyond.

We close this subsection by observing that at any given step of the X-bar tree (the F tree), the simple knowledge of the state $|0\rangle$ or $|1\rangle$ is not sufficient in order to know its parent state in the previous step; we should also know which one is the branch we are on. This in part corresponds to the PIC mentioned above and to one of the major problems in all of contemporary linguistic theory. In speaking and reading we proceed left to right, from the 'outside' (the main sentence), to the 'inside' (subordinate sentence), but the syntactic derivation proceeds from right to left, from inside out. This creates a conflict, namely that presumably the construction of Phases—that is, of periodic points of closure—solves (Piattelli-Palmarini & Uriagereka 2004, 2005, 2008).

While the tree construction (the 'way forwards') is fully determined by the σ's operations, the 'way backwards', as said, is not uniquely determined solely by the knowledge of the state $|0\rangle$ or $|1\rangle$. On the other hand, suppose one goes backwards of, say, q steps starting from a given, say, $|1\rangle$ (or $|0\rangle$). Then returning to such a specific state is no more guaranteed since at each branching point one has to chose which way to go (unless one keeps memory of its previous path, the Ariadne's thread…). In the syntactic derivation, 'forward' consists in building further structure from the inside out, from right to left, proceeding upwards in the syntactic tree. The opposite, 'backwards', consists in the derivation 'looking down' to lower levels. The PIC, as we have just seen, constrains this operation to a strict minimum. Omitting details, only the leftmost (and topmost) 'edge' of the lower Phase is (quite briefly) still accessible to the operations building the next higher Phase.

The lesson is that, parameterizing by time the moving over the X-bar tree, time-reversal symmetry is broken. In other words, as seen above, the 'way forwards' and the 'way backwards' cannot be trivially exchanged, which means that on the axis of the coordinate representing the time (the time axis), the origin—say the time t_0—

is not a symmetric point under exchange of the forward and backward direction, indeed, which in turn forbids that one can choose it or move it on the time axis arbitrarily. In such a case, according to the Noether theorem, the system energy is not conserved. The system may exchange, release or receive, energy with its environment. It is an open or dissipative system. We therefore need to deal with the formalism specially devised for dissipative systems. We will consider such a problem in the following. Before that we need to comment briefly in the following section on the 'interfaces', namely the conceptual intentional (semantic) system (CI) and the sensory-motor system (SM), to which Narrow Syntax has to make contact.

3. The Interfaces

Narrow Syntax has to make contact (has to interface) with two distinct systems: the conceptual intentional (semantic) system (CI) and the sensory-motor (articulation, auditory, or visual perception) system (SM). Language, for centuries, has been correctly conceived as sounds with meanings.[8] But it is better now conceptualized as meanings with sounds, because Narrow Syntax is optimized to interface with the CI system, not so much with the SM system. CI 'sees' all copies, and interprets them, but at the SM interface only one copy is pronounced (usually the higher copy; see footnote 4), while the other copy (or copies) remain(s) silent (deleted at SM):

(8) Which books did you read [books]?

The rightmost (hierarchically lower) copy in English and in many other languages is not pronounced. We see that 'copies' now become important objects in the linguistic structure. We will show how this can be accounted for in our modeling. Until 2012, the 'optimality' of Narrow Syntax with regard to the CI system was supposed to operate as follows: There are features that are 'meaningful', called interpretable features, which CI can understand, and other features that are uninterpretable, meaningless. From 2012 on, the bold hypothesis is that Merge does not form sets that have a category, not any more. It works freely and without constraints (a bit like Feynman's sum of all histories, before amplitudes give the wave function). It is 'only' at the interface with CI that categories are needed (CI needs labeled heads: which one is a verb, which one a noun, an adjective etc.).[9] A minimal search process called the Labeling Algorithm is what does this job (Chomsky 2013, in press). In this framework, categorization and non-commutativity are only necessary at the CI interface. Order is important, obviously, at the SM interface (what to pronounce first, second etc., and what not to pronounce at all—deleted copies), but there is strong evidence that order does not appear at the CI interface. Order

[8] Sound is the traditional expression, but we now know that it is unduly too restrictive: This should extend to gestures in sign languages (see the classic analysis of American Sign Language by Klima & Bellugi 1979 and many studies ever since) to touch in deaf-and-blind subjects (C. Chomsky 1969, 1986)

[9] It needs more than this: If XP is a VP at CI (the highest node, a Complementizer Phrase), then the mapping from Narrow Syntax to the SM system (externalization) must also know that it is a VP . Therefore labeling must be done at Transfer, so that the information goes to both interfaces.

is probably a reflex of the SM system, not feeding Narrow Syntax or CI.[10] And categorization has to be the same at CI for interpretation at SM and for externalization. Today, some syntacticians try to shoehorn the previous analysis into this more stringent picture. Not everyone is persuaded that it can be done completely. But interesting explanations with elegant simplifications have been obtained already (see, among many, Berwick et al. 2013b; Cecchetto & Donati 2010; van Gelderen 2014; Hornstein 1999; Hornstein et al. 2005). In essence: Explain and unify in terms of unconstrained Merge and the Labeling Algorithm many (ideally, all the) special properties of syntax. In many linguistic expressions, nothing is invoked beyond the simplest computational operation Merge and reasonable interpretations of general principles of strict locality and Minimal Computation (MC). It's third factors (physics) all the way.

3.1. The Manifold of Concepts

We are now ready to resume the discussion of the algebraic formalism. Our first task is to consider the whole set of N elements introduced in the subsection 2.1 and thus restore the subscript i labeling each element in the set of N elements.

One may regard the collection of the associated states as the one at a given step of high multiplicity in the Fibonacci tree. Since N can be as large as one wants, we may always have a state which is the direct product of a large number (in principle, an infinite number, hence one needs field theories) of factor states, $\Pi_{i=1,N}|s_i\rangle \equiv |s_1\rangle \otimes |s_2\rangle \otimes \ldots |s_i\rangle \otimes \ldots \equiv |s_1, s_2, \ldots s_i \ldots\rangle$, with $s_i = 0$ or 1 for each $i = 1, 2, \ldots, N$. The most general state. denote it by $|l\rangle$, is then a superposition of all states with l elements in $|1\rangle$ and $N - l$ elements in $|0\rangle$. Its explicit form is given in Appendix B. The difference between the number of elements in $|1\rangle$ and the one of the elements in $|0\rangle$ is measured by σ_3 and is given by $\langle l|\sigma_3|l\rangle = l - \frac{1}{2}N$. This quantity is called the order parameter. Its being non-zero signals that the $SU(2)$ symmetry is broken.[11]

In Appendix B (see also Beige et al. 2005; De Concini & Vitiello 1976), it is shown that in the large N limit the su(2) algebra of the σ matrices, represented in the space of the $|l\rangle$ states, for any l, and written in terms of $S^{\pm}$ and $S_3 \equiv \sigma_3$, where $S^{\pm} = \sigma^{\pm}/\sqrt{N}$, transforms (rearranges) into the algebra in (9) .

(9) $$[S_3, S^{\pm}] = \pm S^{\pm}\,, \quad [S^-, S^+] = 1$$

The result (9) is a central result. Its physical meaning is that, as a consequence of the spontaneous breakdown of symmetry, long range correlation modes (the Nambu-Goldstone modes) are *dynamically* generated (the Goldstone theorem; Goldstone et al. 1962). These Nambu-Goldstone modes represent collective waves spanning the whole system and are here represented by the ladder $S^{\pm}$ operators. They are the carrier of the *ordering* information through the system volume (Shah et al. 1974; De Concini & Vitiello 1976; Umezawa 1993; Blasone et al. 2011).

10 There are two notions of order to be taken into account: ordering of the syntactic operations and ordering of the items in the externalized linguistic expression. Here we have dealt with the latter, while a treatment of the first comes in what follows.

11 The phenomenon of spontaneous symmetry breakdown is throughly studied in many-body physics. For example, in the case of the electrical or magnetic dipoles, the order parameter provides the measure of the polarization or magnetization, respectively.

Order thus appears as a *collective* dynamical property of the system. The order parameter provides indeed a measure of the system ordering. Different degrees of ordering correspond to different values, in a continuous range of variability, of the order parameter, thus denoting different, i.e. physically inequivalent *phases* of the system.

When spontaneous symmetry occurs, the system may be found therefore in different dynamical regimes or physical phases. These are described by different spaces of the states of the system each one labeled by a specific value assumed by the order parameter. Such a process of dynamical generation of physically different phases, each one characterized by collective, coherent waves, represented by the ladder operators $S^{\pm}$, is called *foliation* in the jargon of quantum field theory (Celeghini et al. 1992; Vitiello 1995; see also subsection 3.2 and Appendix C).

In GG, the phenomenon of symmetry breaking—the anti-symmetry of syntax and the dynamic anti-symmetry of syntax—have been cogently argued for by Kayne (1994) and Moro (2000), respectively, for example. This is in part why issues about the status of X-bar (as part of Narrow Syntax or as an emergent configuration of recursive binary Merge) have been recently debated (Chomsky 2013, in press; see also Medeiros & Piattelli-Palmarini, in press). In essence, if Merge is unconstrained and does not, in itself, produce ordered sets, we have an initial symmetry (i.e. before the interfaces with CI and SM). Labeling and ordering at the interfaces break this symmetry and create order. This process does not involve any material transfer, something that is obviously excluded in the case of language.[12]

We thus realize that, due to the spontaneous symmetry breakdown, our system has undergone a formidable dynamical transition, moving from the regime of being a collection of elementary components (lexical elements) to the regime of collective, coherent $S^{\pm}$ fields. Our main assumption at this point is to identify a specific *conceptual, meaningful* linguistic content (a Logical Form, LF)[13] with the collective coherent phase associated to a specific value of the order parameter. The semantic level, characterized by a *continuum* of concepts or meanings (the 'manifold of concepts'), thus emerges as a dynamical process out of the syntactic background of lexical elements, in a way much similar (mathematically isomorph) to the one by which macroscopic system properties emerge as a coherent physical phase out of a collection of elementary components at a microscopic (atomistic) level in many-body physics (Umezawa 1993; Blasone et al. 2011).

In conclusion, we can now give a quantitative characterization of the 'interfaces' where the Narrow Syntax has to make contact with the CI system: When interfaces are met we have the spontaneous breakdown of symmetry in the large N limit. It is there that a specific meaning or 'concept' arises from a 'continuous' context of possible concepts by selecting out one representation of the algebra from many of them

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12 A comparison due to the Oxford particle physicist Frank Close is the following (Close 2011): Imagine several guests sitting at a very large circular dinner table. Each has a napkin on his or her right and one on the left. They are uncertain about which one to pick up. Until a more daring guest decides to pick up the one on (say) the right. Everyone else follows and we have a 'wave' of napkin pickups. The underlying symmetry is broken. No movement of matter, no forces applied. In analogous situations in physics, a Nambu-Goldstone boson (a mass-less particle) is thus generated.

13 The notion of LF as the last syntactic input to full meaning is well consolidated in GG and has been since the pioneering work of Higginbotham & May (1981) and May (1985).

'unitarily inequivalent' among themselves (each corresponding to a different concept) (Vitiello 1995). The concept appears at that point as a collective mode, not a result of associative process pulling together bits and little lexical pieces, words etc. The collectiveness comes from the 'phase coherence', whose carriers are the collective Nambu-Goldstone $S^{\pm}$ fields. We also understand why "only at the interfaces the issues of ordering become relevant" (cf. previous subsection). Order indeed is lack of symmetry and it can only appear when this is spontaneously broken.

For the same reason, categorization and non-commutativity (and order) are only necessary at the CI interface. Indeed, only at the large N limit CI needs labeled heads: which one is a verb, which one a noun, an adjective etc. We have seen that the formal construction of the binary Merge does not require labeled structures (Noun, Verb, Adjective, Preposition etc.). The necessity of labeling (through the Labeling Algorithm) only arises at the interface with meaning. Interpreting the different constituents (Phrases) is a necessity for the CI system, with the formal label of a syntactic object triggering different intentional landscapes. Once the Narrow Syntax has made contact with the CI system, through the action-perception cycle (Vitiello 1995) of the cortex dynamics, the SM system gets also involved and therefore the linguistic structures can be externalized, allowing to communicate to other speakers all the required subtleties of meaning.

The formalism here presented thus endorses Chomsky's thesis that Merge is unconstrained, and that issues of labeling (headedness, categorization of lexical items) and ordering only arise at the interfaces of Narrow Syntax with the CI and the SM systems.

3.2. *Copies of Lexical Elements*

We now consider the feature of the copies of lexical elements in the MP. At the end of subsection 2.1, we have observed that time-reversal symmetry is broken moving along the X-bar tree. We saw that when the breakdown of time-reversal symmetry occurs, one cannot treat the system as a closed system. It is a dissipative system and from the standpoint of the algebraic formalism, this means that one has to set up a proper mathematical scheme, which is achieved by doubling the system degrees of freedom (Celeghini et al. 1992). This goes as follows.

Consider a dissipative system, say A. It is an open system interacting with the environment in which it is embedded, denote it with $\tilde{A}$. In order to carry on the analysis of the system properties one cannot avoid to consider the fluxes of energy, matter, information, etc. exchanged between the system A and its environment $\tilde{A}$. This implies that the study of the dissipative system cannot ignore the study also of the properties and features of the environment. Thus one needs to consider both, the system and its environment. This means that, instead of considering the system A separated from the environment, one is brought to consider algebraic forms including both of them, $\{A, \tilde{A}\}$, namely $A \to \{A, \tilde{A}\}$. However, one must pay attention in treating the system elements and the environment element, since in general, the system elements cannot be exchanged or confused with the elements of the bath or environment in which the system is embedded. They need to be considered on a different footing. This is obtained by introducing a 'weight factor', or 'deformation parameter', say θ, with different values for A and $\tilde{A}$ (Celeghini et al. 1998; Blasone et al. 2011). Such a procedure may be formulated in a precise manner and goes under the name of 'deformed Hopf algebra structure', which is a noncommutative algebra. See Appendix C for introductory details. The conclusion is that one has now to deal with a 'doubled' system: A and its double or 'copy' $\tilde{A}$. As a matter of fact, since the fluxes between A and $\tilde{A}$ must be balanced, one may think indeed of $\tilde{A}$ as a 'copy' of A, in the sense that $\tilde{A}$ represents the sink where, say, the energy from the source A goes, and vice versa, A also represents the sink where the energy from the source $\tilde{A}$ goes. The 'tilde' operators $\tilde{A}$ thus denote the doubled operators in the doubling of the algebra $\mathcal{A} \to \mathcal{A} \times \mathcal{A}$ (see also Appendix C).

Note that, when considering the elements of A (and $\tilde{A}$), one should use subscripts, say $\mathbf{k}$, denoting characterizing properties of the A (and $\tilde{A}$) modes, e.g. $A_{\mathbf{k}}$. For simplicity we omit such subscripts as far as no misunderstanding occurs.

Simplifying a bit, the doubling of the space and of the operators creates a strict correspondence between each operator and its 'double' (the tilde operator). This two-way interaction is quite specific. In the case of language, each copy interacts with the initial (in a sense, the 'original') element and meaning is accordingly extracted at CI. As CI well 'understands', the interpretation is determined by this dual correspondence.

Denote now by $|0\rangle \equiv |0\rangle \times |0\rangle$ the state annihilated by A and $\tilde{A}$: $A|0\rangle = 0 = \tilde{A}|0\rangle$ (the vacuum state). By proper algebraic operations (see Blasone et al. (2011) and Celeghini et al. (1998) for the technical details) one may show that starting from the operators A and $\tilde{A}$, the operators $A(\theta)$ and $\tilde{A}(\theta)$ may be obtained, such that they do not annihilate $|0\rangle$. Let us denote the state annihilated by these operators by $|0(\theta)\rangle_{\mathcal{N}}$. Its explicit form is given in Appendix C.

The vacuum state $|0(\theta)\rangle_{\mathcal{N}}$ is a well normalized state: ${}_{\mathcal{N}}\langle 0(\theta)|0(\theta)\rangle_{\mathcal{N}} = 1$. The meaning of the subscript $\mathcal{N}$ is clarified below (see the comments after equation (10)). We remark that the vacuum state $|0(\theta)\rangle_{\mathcal{N}}$ turns out to be a generalized $SU(1,1)$ coherent state of condensed couples of A and $\tilde{A}$ modes (Perolomov 1986; Celeghini et al. 1992), which are entangled modes in the infinite volume limit. The vacuum $|0(\theta)\rangle_{\mathcal{N}}$ is therefore a state densely filled with couples of A and $\tilde{A}$: It is a *coherent condensate* of the couples $A\,\tilde{A}$.[14]

For notational simplicity from now on we will denote by A and $A^\dagger$ the operators S^- and S^+ in (9), respectively. Thus, the doubling process implies that correspondingly we also have $\tilde{S}^-$ and $\tilde{S}^+$, which will be denoted as $\tilde{A}$ and $\tilde{A}^\dagger$, respectively.

One can show that ${}_{\mathcal{N}}\langle 0|0(\theta)\rangle_{\mathcal{N}} \to 0$ and ${}_{\mathcal{N}}\langle 0(\theta')|0(\theta)\rangle_{\mathcal{N}} \to 0$, $\forall \theta \neq \theta'$, in the infinite volume limit $V \to \infty$ (Celeghini et al. 1992, 1998). Thus we conclude that the state space splits in infinitely many physically inequivalent representations in such a limit, each representation labeled by a θ-set $\{\theta_{\mathbf{k}}, \forall \mathbf{k}\}$. This is the *$\theta$-foliation*

[14] In language, in first approximation, the vacuum state is silence. Just like in the present algebraic formalism, there are many kinds of silence. Not only how a silence gap is interpreted in the unfolding of a conversation, but in a more specific and more technical sense. There is, literally, a syntax of silence (Merchant 2001) in linguistic constructions called ellipsis (*Mary bought a book and Bill did ____ too*) and sluicing (*Ann danced with someone but I do not know who ____*). Jason Merchant and other syntacticians and semanticists have persuasively shown that what can be omitted is never just an arbitrary 'bunch of words', but an entire syntactic constituent (an entire Verb Phrase, most frequently). The syntax and semantics of several well defined but unpronounced elements has been part of the theory since the beginning of GG (Chomsky 1955/1985).

process of the state space (already mentioned in subsection 3.1). In the present case of linguistics this represents the process of generation of the manifold of concepts. It is a dynamical process since the generator $G_{\mathbf{k}}$ (see Appendix C for its definition) is essential part of the system Hamiltonian (Celeghini et al. 1992) . Thus in linguistics the 'manifold of concepts' is made of 'distinct', different spaces (the 'physically inequivalent' representations), each one representing a different 'concept' (in language we have the LFs composing the global LF of the entire sentence), here described as the coherent collective mode generated through the X-bar tree as illustrated in subsection 2.1.

These spaces (concepts) are protected against reciprocal interferences since the spaces are 'unitarily inequivalent', i.e. there is no unitary operator able to transform one space in another space (Vitiello 1995, 2001), which corresponds to the fact that syntactic Phases cannot be commingled, nor 'reduced' one into the other. Phases are, as we said above, mutually impenetrable. In practice, however, the unitary inequivalence is smooted out by realistic limitations, such as, for example, the impossibility to reach in a strict mathematical sense the $V \to \infty$ limit (i.e. the 'infinite number' of lexical elements or the theoretically infinite number of choices for the co-referentiality indices in the logical form of even the simplest sentences).[15] Thus, realistically, we may also move from concept to concept in a chain or trajectory going through the manifold of concepts (Vitiello 1995, 2004a, 2004b; Freeman & Vitiello 2006, 2008; Capolupo et al. 2013). These trajectories may be thought as producing 'association of concepts' in their evolving through the manifold of concepts. Remarkably, one may have a multiplicity of such 'associations', each one produced by a specific trajectory, among the many possible ones. One may thus follow different, distinct, non-interfering paths in the space of the concepts. Such features are indeed implied by the fact that the trajectories, although deterministically evolving, are found to be chaotic trajectories (Vitiello 2004b). This corresponds to the compositionality of meanings, when the syntactic derivation proceeds 'upwards' (that is, forward) from the lower Phases to the higher Phases, from local LFs to the composition of more inclusive LFs.

In order to better understand the role played by the 'tilde copies', $\tilde{A}$, it is interesting to compute $N_{A_{\mathbf{k}}} = A^{\dagger}_{\mathbf{k}} A_{\mathbf{k}}$ in the state $|0(\theta)\rangle_{\mathcal{N}}$:

(10) $\mathcal{N}_{A_{\mathbf{k}}}(\theta) \equiv {}_{\mathcal{N}}\langle 0(\theta)|A^{\dagger}_{\mathbf{k}} A_{\mathbf{k}}|0(\theta)\rangle_{\mathcal{N}} = {}_{\mathcal{N}}\langle 0(\theta)|\tilde{A}_{\mathbf{k}}(\theta)\tilde{A}^{\dagger}_{\mathbf{k}}(\theta)|0(\theta)\rangle_{\mathcal{N}} = \sinh^2 \theta_{\mathbf{k}}$

From this we see that for any $\mathbf{k}$ the only non-vanishing contribution to the number of non-tilde modes $\mathcal{N}_{A_{\mathbf{k}}}(\theta)$ comes from the tilde operators, which can be expressed by saying that these last ones constitute the dynamic *address* for the non-tilde modes (the reverse is also true, the only non-zero contribution to $\mathcal{N}_{\tilde{A}_{\mathbf{k}}}(\theta)$ comes from the non-tilde operators). In the case of language, this 'address' corresponds to the link between the two copies, or among a chain of cyclic copies in more complex sentences.

In conclusion, the physical content of $|0(\theta)\rangle_{\mathcal{N}}$ is specified by the $\mathcal{N}$-set $\equiv \{\mathcal{N}_{A_{\mathbf{k}}}(\theta), \mathcal{N}_{A_{\mathbf{k}}}(\theta) = \mathcal{N}_{\tilde{A}_{\mathbf{k}}}(\theta), \forall \mathbf{k}\}$, which is called the *order parameter*. It is a characterizing

[15] One of the leaders in the semantics of natural languages wrote (Heim 1983: 232): "We just focused on a particular logical form that grammar provides for the sentence 'She hit it' [...] But there are infinitely many others, since the choice of indices is supposed to be free. So [the simple logical form there reported] represents really only one of many readings that the sentence may be uttered with."

parameter for the vacuum $|0(\theta)\rangle_{\mathcal{N}}$ and explains the meaning of the $\mathcal{N}$ subscript introduced above.

All of this, therefore, sheds some light on the relevance of 'copies' in the MP. In some sense they are crucial in determining (indeed providing the address of) the whole conceptual content of the considered linguistic structure. They provide the *dynamic reference* for the non-tilde modes. Unpronounced copies, being silent, do not reach the SM system, but they are crucially interpreted by the CI system. They are necessary to the understanding of the meaning of what is actually pronounced. Remarkably, they are 'built in' in the scheme here proposed; they are not imposed by hand by use of some constraint 'external' to the linguistic system. It is in this specific sense that we speak of 'self-consistency': Our formal scheme is computationally (logically) self-contained. Perhaps the real power of the linguistic tool available to humans consists in such a specific feature.

4. Concluding Remarks

The essence of the contribution we propose in this paper for the understanding and the physical modeling of the Minimalist Program consists in having pointed out the *dynamical* nature of the transition from a numeration of lexical items to syntax and from syntax to the logical form (LF) of the sentence and from LF to meaning. This has brought us to the identification of the manifold of concepts, to the self-similar properties of the X-bar trees and of their dissipative character (breakdown of the time-reversal symmetry), to the role of the copies in the conceptual intentional system CI. The Hopf algebra structure has shown that the doubled tilde operators, which we have seen to play the role of the copies in the CI system, are 'built in' in the computationally self-contained algebraic scheme. These copies or tilde modes have been recognized to provide the *dynamical reference* (the 'address') of the non-tilde modes. The result is the logical self-consistency (inclusion of the *reference* terms) of languages.

We have also pointed out the mechanism of the foliation of the space of the states, out of which the great richness of the conceptual content, the 'multiplicity' of inequivalent meanings (nested LFs) emerges (see the comments following (10) and the remark by Heim in footnote 15). In this connection, we would like to call the attention of the reader on a further aspect of the scheme we propose in order to model some features of the MP, namely on its intrinsic thermodynamic nature. It is indeed well known (Umezawa 1993) that within the scheme one can consistently define thermodynamic quantities (operators) such as the entropy and the free energy. Let us consider here the entropy.

Thinking of the entropy as an 'index' or a measure of the degree of ordering present in the state of the system (lower entropy corresponding to higher degree of order), one can show that the state $|0(\theta)\rangle_{\mathcal{N}}$ can be constructed by the use of the entropy operator S (Celeghini et al. 1992; Umezawa 1993; Blasone et al. 2011). Its expectation value in $|0(\theta)\rangle_{\mathcal{N}}$ is given by the familiar form, where $W_n \equiv W_n(\theta)$ is some quantity, which here we do not need to specify:

(11) $$ {}_{\mathcal{N}}\langle 0(\theta)|S|0(\theta)\rangle_{\mathcal{N}} = \sum_{n=0}^{+\infty} W_n \log W_n $$

Remarkably and consistently with the breakdown of time-reversal symmetry in dissipative systems (the appearance of the *arrow of time*), time evolution can be shown to be controlled by the entropy variations (De Filippo & Vitiello 1977; Celeghini et al. 1992). These indeed control the variations in the $A - \tilde{A}$ content of $|0(\theta)\rangle_{\mathcal{N}}$, thus controlling the time evolution (the trajectories) in the manifold of concepts (the space of the *infinitely many* LF, see Heim 1983 in footnote 15). Entropy is thus related with the semantic level of the LF, *meanings*, which are dynamically arising as collective modes out of the syntactic (atomistic) level of the basic lexical elements.

In conclusion, we have uncovered the isomorphism between the physics of many-body systems and the linguistic strategy of the Minimalist Program. Although we have exploited the algebraic properties of the many-body formalism, in our scheme the linguistic structures are 'classical' ones. It is known, on the other hand, that the many-body formalism is well suited to describe not only the world of elementary particle physics and condensed matter physics, but also macroscopically behaving systems characterized by ordered patterns (Umezawa 1993; Blasone et al. 2011). Our discussion seems to imply that the crucial mechanism of the foliation of the space of the states has to do with the basic dynamics underlying the linguistic phenomena observed at a macroscopic level. It is an interesting question whether the basic dynamics underlying the richness of the biochemical phenomenology of the brain behavior (Vitiello 1995, 2001; Freeman & Vitiello 2006, 2008; Capolupo et al. 2013; Freeman et al. 2015) also provides the basic mechanisms of linguistics.

Appendix A: On Pauli Matrices and Their Algebra

Consider the 2×2 matrices σ_1, σ_2, σ_3 and the unit matrix I:

$$\sigma_1 = \frac{1}{2}\begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}, \quad \sigma_2 = \frac{1}{2}\begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}, \quad \sigma_3 = \frac{1}{2}\begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}, \quad I = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}.$$

The σ_1, σ_2, σ_3 are the Pauli matrices. They were introduced as an elegant device in the treatment of magnetic spin. The formalism, however, is directly applicable to any system that has two possible states (Perelomov 1986). The space of states on which the matrices operate is built indeed on the basis vectors $\begin{pmatrix} 0 \\ 1 \end{pmatrix}$ and $\begin{pmatrix} 1 \\ 0 \end{pmatrix}$, which we will denote by $|0\rangle$ and $|1\rangle$, respectively. The scalar product is denoted by $\langle i|j\rangle = \delta_{ij},\ i, j = 0, 1$. The Pauli matrices satisfy the $su(2)$ algebra, which, in terms of the matrices $\sigma^{\pm} = \sigma_1 \pm i\,\sigma_2$, $\sigma^{+} = \begin{pmatrix} 0 & 1 \\ 0 & 0 \end{pmatrix}$ and $\sigma^{-} = \begin{pmatrix} 0 & 0 \\ 1 & 0 \end{pmatrix}$, is given by the commutation relations

$$[\sigma_3, \sigma^{\pm}] = \pm\sigma^{\pm}, \quad [\sigma^{-}, \sigma^{+}] = -2\sigma_3 .$$

When we have a collection of N objects ('particles' or 'lexical elements'), which are represented for each i by the 'ground states' $|0\rangle_i$ and 'excited states' $|1\rangle_i$, $i = 1, 2, 3, \ldots N$, we have $\sigma^{\pm} = \sum_{i=1}^{N} \sigma_i^{\pm}$ and $\sigma_3 = \sum_{i=1}^{N} \sigma_{3i}$.

We also write $\sigma_{3i} = \frac{1}{2}(|1\rangle_{ii}\langle 1| - |0\rangle_{ii}\langle 0|)$, with eigenvalues $\pm\frac{1}{2}$, $\sigma_i^{+} = |1\rangle_{ii}\langle 0|$ and $\sigma_i^{-} = |0\rangle_{ii}\langle 1|$.

Appendix B: Dynamical Rearrangement of the $SU(2)$ Symmetry

Consider the state $|l\rangle$ introduced at the beginning of subsection 3.1, namely the state which is a superposition of all states with l elements in $|1\rangle$ and $N-l$ elements in $|0\rangle$. Its explicit form is given by:

$$\begin{aligned}|l\rangle \;\; &\equiv \;\; [\,|0_1 0_2 \ldots 0_{N-l}\, 1_{N-l+1} 1_{N-l+2} \ldots 1_N\rangle + \ldots \\ &\quad + |1_1 1_2 \ldots 1_l\, 0_{l+1} 0_{l+2} \ldots 0_N\rangle\,]/\sqrt{\tbinom{N}{l}}\end{aligned}$$

For any l we have (Beige et al. 2005; Blasone et al. 2011):

$$\begin{aligned}\sigma^+|l\rangle &= \sqrt{l+1}\,\sqrt{N-l}\,|l+1\rangle\,, \\ \sigma^-|l\rangle &= \sqrt{N-(l-1)}\,\sqrt{l}\,|l-1\rangle\end{aligned}$$

This shows that $\sigma^\pm$ and σ_3 acting on $|l\rangle$ may be represented as (the so-called Holstein-Primakoff non-linear realization; Holstein & Primakoff 1940; Shah et al. 1974; De Concini & Vitiello 1976; Blasone et al. 2011)

$$\sigma^+ = \sqrt{N} S^+ A_S\,, \;\; \sigma^- = \sqrt{N} A_S S^-\,, \;\; \sigma_3 = S^+ S^- - \tfrac{1}{2}N,$$

with $A_S = \sqrt{1 - S^+ S^-/N}$, $S^+|l\rangle = \sqrt{l+1}\,|l+1\rangle$ and $S^-|l\rangle = \sqrt{l}\,|l-1\rangle$, for any l. The σ's still satisfy the su(2) algebra (cf. Appendix A). However, in the large N limit, we have:

$$\sigma^\pm\,|l\rangle = \sqrt{N}\,S^\pm\,|l\rangle$$

and thus $S^\pm = \sigma^\pm/\sqrt{N}$ for large N: The phenomenon of the contraction of the algebra occurs (Inönü & Wigner 1953; De Concini & Vitiello 1976; Beige et al. 2005). This means that in the large N limit the su(2) algebra written in the space of the $|l\rangle$ states, for any l, in terms of $S^\pm$ and $S_3 \equiv \sigma_3$, contracts to the so-called (projective) e(2) algebra:

$$[S_3, S^\pm] = \pm S^\pm\,, \quad [S^-, S^+] = 1\,,$$

which is the equation (9) in the subsection 3.1. This is a centra result. It expresses the 'rearrangement' of the su(2) algebra in the e(2) algebra, which is isomorph to the Heisenberg-Weyl algebra (Perelomov 1986), with S_3 playing the role of the number operator and $S^\pm$ the role of ladder operators. The *rearrangement of symmetry* is a well known *dynamical* process (De Concini & Vitiello 1976; Umezawa 1993), which occurs when there is spontaneous breakdown of symmetry characterized by a non-vanishing classical field called order parameter. In the present case, the order parameter is given by $\langle l|\sigma_3|l\rangle = l - \frac{1}{2}N \neq 0$.

Appendix C: Doubling of the Degrees of Freedom for Dissipative Systems

Let us denote by $\mathcal{A}$ the operator algebra of a given system. The algebra mapping $\mathcal{A} \to \mathcal{A} \times \mathcal{A}$ defines the doubling of the degrees of freedom of the system. It is a natural requirement to be satisfied when one has to consider, for example, the total energy of a system of two identical particles, $\mathcal{E}_{tot} = \mathcal{E}_1 + \mathcal{E}_2$, or their total angular momentum $L_{tot} = L_1 + L_2$. These sums are defined in the algebra $\mathcal{A} \times \mathcal{A}$ and denote the Hopf coproducts $\mathcal{E}_{tot} = \mathcal{E} \times 1 + 1 \times \mathcal{E}$ and $L_{tot} = L \times 1 + 1 \times L$, respectively, which are commutative under the exchange of the two considered particles.

As said in the subsection 3.2, most interesting is the case of two elements which cannot be treated on the same footing, as it happens when dealing, for example, with open or dissipative systems (e.g. finite temperature systems), where the system elements cannot be exchanged with the elements of the bath or environment in which the system is embedded, or as in the case of linguistics where, at the syntactic and semantic levels, lexical elements, as well as conceptual contents, cannot be simply interchanged. In these cases, we need to consider *q-deformed* Hopf algebras with *noncommutative* Hopf coproducts $\Delta A_q = A \times q + q^{-1} \times A \equiv A\, q \,+\, q^{-1}\, \tilde{A}$ (Celeghini et al. 1998), with the operator (matrix) $A \in \mathcal{A}$ and q a number chosen on the basis of some mathematical constraint on which we do not need to comment here. The doubled operators in the doubling of the algebra $\mathcal{A} \to \mathcal{A} \times \mathcal{A}$ is denoted by the 'tilde' operators $\tilde{A}$.

For simplicity we are omitting subscripts $\mathbf{k}$ denoting properties of the A (and $\tilde{A}$) modes, e.g. $A_{\mathbf{k}}$, as far as no misunderstanding occurs.

In conclusion, we have the 'copies' $\tilde{A}$ of the operators A, the Hopf doubling of the algebra $A \to \{A,\, \tilde{A}\}$ and of the state space $\mathcal{F} \to \mathcal{F} \times \tilde{\mathcal{F}}$. The operators A and $\tilde{A}$ act on $\mathcal{F}$ and $\tilde{\mathcal{F}}$, respectively, and commute among themselves.

By using the so-called deformation paramenter $q(\theta)$, with $q(\theta) = e^{\pm\,\theta}$, one obtains (Celeghini et al. 1998; Blasone et al. 2011) the operators $A(\theta),\ \tilde{A}(\theta)$ and the so-called Bogoliubov transformations:

$$\begin{aligned} A(\theta) &= A \cosh\,\theta - \tilde{A}^\dagger \sinh\,\theta, \\ \tilde{A}(\theta) &= \tilde{A} \cosh\,\theta - A^\dagger \sinh\,\theta. \end{aligned}$$

The canonical commutation relations (CCR) are

$$[A(\theta), A(\theta)^\dagger] = 1, \qquad [\tilde{A}(\theta), \tilde{A}(\theta)^\dagger] = 1.$$

All other commutators equal to zero. The Bogoliubov transformations provide an explicit realization of the doubling or 'copy' process discussed above.

The state annihilated by A and $\tilde{A}$ is denoted by $|0\rangle \equiv |0\rangle \times |0\rangle : A|0\rangle = 0 = \tilde{A}|0\rangle$ (the vacuum state). $A(\theta)$ and $\tilde{A}(\theta)$ do not annihilate $|0\rangle$. They annihilate the state $|0(\theta)\rangle_{\mathcal{N}}$ (Celeghini et al. 1992; Umezawa 1993; Blasone et al. 2011) given by

$$|0(\theta)\rangle_{\mathcal{N}} = e^{i\sum_{\mathbf{k}} \theta_\kappa G_{\mathbf{k}}}|0\rangle = \prod_{\mathbf{k}} \frac{1}{\cosh\,\theta_\kappa} e^{(\tanh\theta_\kappa A_\kappa^\dagger \tilde{A}_\kappa^\dagger)}\,|0\rangle,$$

where θ denotes the set $\{\theta_\kappa, \forall\mathbf{k}\}$. As usual, the symbol † in $A^\dagger$ denotes the hermitian conjugate matrix, namely the transpose and complex conjugate of the matrix representation of A. In the operator $e^{(i\sum_{\mathbf{k}} \theta_\kappa G_{\mathbf{k}})}$, $G_{\mathbf{k}} \equiv -i\,(A_{\mathbf{k}}^\dagger \tilde{A}_{\mathbf{k}}^\dagger \,-\, A_{\mathbf{k}} \tilde{A}_{\mathbf{k}})$. $G_{\mathbf{k}}$ is called the generator of the Bogoliubov transformations and of the state $|0(\theta)\rangle_{\mathcal{N}}$.

We have ${}_{\mathcal{N}}\langle 0|0(\theta)\rangle_{\mathcal{N}} \to 0$ and ${}_{\mathcal{N}}\langle 0(\theta')|0(\theta)\rangle_{\mathcal{N}} \to 0, \forall\theta \neq \theta'$, in the infinite volume limit $V \to \infty$. As already observed in subsection 3.2, this shows that the state space splits in infinitely many physically inequivalent representations in such a limit, each representation labeled by a θ-set $\{\theta_{\mathbf{k}} = \ln\, q_{\mathbf{k}},\, \forall\mathbf{k}\}$. This is the $q(\theta)$-*foliation* dynamical process of the state space.

Appendix D: Some Useful Formulas on Fibonacci Matrix

The matrix

$$F \equiv \frac{1}{2}\, I + \sigma_3 + 2\,\sigma_1 = \frac{1}{2}\begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} + \frac{1}{2}\begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} + \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix} = \begin{pmatrix} 1 & 1 \\ 1 & 0 \end{pmatrix}$$

is called the Fibonacci matrix. For the n-powers F^n of the F matrix, with $n \neq 0$, we have

$$F^n \;=\; \begin{pmatrix} F_{n+1} & F_n \\ F_n & F_{n-1} \end{pmatrix} = F_{n-1}\, I + F_n\, F, \qquad \mathrm{n} \neq 0$$

where the matrix elements $F_{n+1},\, F_n,\, F_n,\, F_{n-1}$, with $F_0 \equiv 0$, for any $n \neq 0$, are the numbers in the Fibonacci progression $F_0 = 0,\, F_1 = 1,\, F_2 = 1,\, F_3 = 2,\, F_4 = 3,\, F_5 = 5,\, F_6 = 8,\, F_7 = 13, \ldots$ Moreover, also the coefficients of the matrices I and F in the last member on the r.h.s. of the above relation are the Fibonacci numbers F_{n-1} and F_n. We can indeed verify that

$$\begin{aligned}
F^1 &= \begin{pmatrix} 1 & 1 \\ 1 & 0 \end{pmatrix} = F, \\
F^2 &= \begin{pmatrix} 2 & 1 \\ 1 & 1 \end{pmatrix} = I + F, \\
F^3 &= \begin{pmatrix} 3 & 2 \\ 2 & 1 \end{pmatrix} = I + 2\,F, \\
F^4 &= \begin{pmatrix} 5 & 3 \\ 3 & 2 \end{pmatrix} = 2\,I + 3\,F, \\
F^5 &= \begin{pmatrix} 8 & 5 \\ 5 & 3 \end{pmatrix} = 3\,I + 5\,F, \\
F^6 &= \begin{pmatrix} 13 & 8 \\ 8 & 5 \end{pmatrix} = 5\,I + 8\,F, \\
\ldots & \; etc. \; \ldots,
\end{aligned}$$

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Why the Left Hemisphere Is Dominant for Speech Production: Connecting the Dots

Harvey M. Sussman

Evidence from seemingly disparate areas of speech/language research is reviewed to form a unified theoretical account for why the left hemisphere is specialized for speech production. Research findings from studies investigating hemispheric lateralization of infant babbling, the primacy of the syllable in phonological structure, rhyming performance in split-brain patients, rhyming ability and phonetic categorization in children diagnosed with developmental apraxia of speech, rules governing exchange errors in spoonerisms, organizational principles of neocortical control of learned motor behaviors, and multi-electrode recordings of human neuronal responses to speech sounds are described and common threads highlighted. It is suggested that the emergence, in developmental neurogenesis, of a hard-wired, syllabically-organized, neural substrate representing the phonemic sound elements of one's language, particularly the vocalic nucleus, is the crucial factor underlying the left hemisphere's dominance for speech production.

Keywords: left hemisphere specialization; phonological representation; rhyming; syllable frame; vocalic nucleus

1. Introduction

When the right hemisphere of a bisected brain is presented with a spoken word the input signal is semantically processed; however, when instructed to say the word it just heard, the split-brain subject is silent (Gazzaniga 1970, 1983). When sodium amytal is selectively administered to right-handed patients prior to brain surgery muteness is experienced (in approximately 96% of cases) when the left hemisphere is anesthetized, while right hemisphere anesthesia affects only 4% of the population (Rasmussen *et al.* 1977). Despite this robust hemispheric asymmetry for speech production in the human brain, no specific, micro-level neural account has been posited to account for this behavioral dominance. Two macro-level accounts of left hemispheric asymmetry for speech output have been put forth. One classic view holds that the left hemisphere selectively inhibits the right hemisphere from participating in language output (e.g., Kinsbourne 1974, Kinsbourne *et al.* 1978, Chiarello *et al.* 1996, Liégeois *et al.* 2004). An inhibitory-based explanation for left hemisphere dominance suggests that to avert 'equipotentiality', the left hemisphere must take on an active preventative role.

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A second hypothesis, formulated from an evolutionary perspective, claims a selective advantage for having separate hemispheres for mediating the well known antagonistic modes of neural processing—analytical symbol translation in the left hemisphere versus spatial, gestalt-like synthesis in the right hemisphere (Levy 1969). Since neural substrates underlying these opposing processing modes cannot easily co-exist (i.e. seeing both 'trees' and 'forests' in the same hemisphere), selective evolutionary pressures housed them in separate hemispheres to minimize processing conflicts and maximize what each hemisphere is best structured to do.

Interestingly, there is no lack of specificity in accounting for hemispheric asymmetries underlying speech processing/perception, despite the fact that speech processing involves far more bilateral interactions than speech production (Hickok *et al.* 2000, Hickok *et al.* 2007, Peelle 2012). One long held view proposes that the left hemisphere is specialized to process the rapid temporal changes (e.g., F2 transitions) characterizing speech (e.g., Tallal *et al.* 1973, 1974, Tallal *et al.* 1981, Zatorre *et al.* 2001, Zatorre *et al.* 2002). An alternative, but somewhat related, view claims that prelexical speech perception is actually processed bilaterally, but different tuning properties of temporal integration windows (40 Hz gamma and 4–10 Hz theta-range) underlie hemispheric-specific differences, with the left hemisphere being specialized to process acoustic signals spanning short temporal windows (appropriate for phonemes) and the right hemisphere specialized for longer temporal windows mediating prosodic cues such as intonation (Poeppel 2003).

We often hear the expression, "We simply didn't connect the dots". To avoid such an oversight dots will be connected from the following areas of language study: (1) infant babbling, (2) the phonological primacy of the syllable, (3) split-brain studies, (4) developmental apraxia of speech, (5) speech errors, (6) a perspective on neocortical operations as learned auto-associative memories, and (7) electrophysiological recordings from human left posterior superior temporal gyrus (pSTG) during presentation of (a) a stop place continuum and (b) an extensive phonetic inventory contained within 500 sentences spoken by 400 speakers. It will be shown that the collective findings from the above studies strongly suggest that the left hemisphere forms, and thus has exclusive access to, neural substrates tasked to represent/map phonemic sound segments that are the prerequisites to both initiate and drive speech motor output.

2. Dot #1: Lateralization of Infant Babbling

Infant babbling provides insights into the prelinguistic beginnings of sound generation in a developing infant. Before canonical babbling (CVs) starts, infants progress from squeals, squeaks, and various forms of yells to produce cooing noises. Importantly, infant coos can be considered precursors to vowel-like sounds, the first speech-like sounds (Locke 1989, Oller 2000). More pertinent to the argument to be made is the intriguing possibility that early infant babbling might also be asymmetrically controlled and monitored by the left hemisphere. Graves *et al.* (1990) observed that when normal adult subjects are speaking, there is a measurable difference in the mouth opening extents in the two sides of the mouth, with the right side opening being greater during generation and recall of word lists.

Adapting the metric of 'right mouth asymmetry', Holowka *et al.* (2002) videotaped 10 babies between the ages of five and 12 months, equally distributed across an English and French home environment. Independent scorers, unaware of the purpose of the study, analyzed randomly selected portions of the videos (N = 150 segments) during three different types of mouth activity: babbles (CV repetitions), non-babbles (vocalizations without a consonant-vowel structure), and smiles. A laterality index was generated to assess the three oral activities. All 10 babies showed a right mouth asymmetry when babbling (+0.88), equal mouth openings for non-babbling (–0.08), and left mouth asymmetry for smiles (–0.82). The greater right-than-left asymmetry in mouth openings was interpreted as reflecting greater involvement of the left hemisphere during babbling utterances. The authors state: "We thus conclude that babbling represents the onset of the productive language capacity in humans, rather than an exclusively oral-motor development" (Holowka *et al*. 2002: 1515).

So the first 'dot' is pre-linguistic sound generation—initially vocalic-like and then, from approximately 7 to 18 months, CV-like sequences, envisioned as being initially and preferentially encoded in an emerging neural substrate in the left hemisphere. These earliest speech-like sounds can be conceptualized as the instantiation of the 'speech sound map' (possibly) forming in left ventral premotor cortex (BA 6, 44) as described in the DIVA computational model (Guenther *et al.* 2006, 2012). If these babbling results are replicated in future studies, then one might say the neural precursors of the eventual phonological primitives of one's language have asymmetrically taken root in the left hemisphere.

To ground this neurogenesis assumption to a neural model of language function (e.g., Hickok *et al.*, 2004), the initial 'dot' is envisioned as the earliest neural 'seeds' of dorsal stream projections (left dominant 'sensori-motor interface' in parietal-temporal Spt area) to the left frontal *'articulatory network'*. Admittedly, this hypothesis does not account for why the hypothesized left hemisphere laterality for babbling exists in the first place. The 'usual suspect,' genetic predisposition, might have to suffice at the moment.

3. Dot #2: Phonological Primacy of the Syllable

The second 'dot' serves to connect the emergence of early infant vocalizations, organized around duplicated and variegated babbling (a CV 'syllable' structure), to well known first principles of phonological language structure. The syllable, while long resisting an unambiguous definition (see Bell & Hooper 1978), nevertheless has properties strongly supporting its primacy in the phonological structure of the world's languages. The following attributes of syllables provide support for this claim: (i) the syllable-bound nature of prosodic events such as stress, rhythm, juncture; (ii) reduplication and deletion processes in a child's phonological development (Fudge 1969, Moskowitz 1970, 1971, Hooper 1972, 1976); (iii) native language syllable constraints that play a key role in pronunciation errors in second language acquisition (Broselow 1983, 1984); and lastly, (iv) the finding that the most prevalent and permutable unit in sub-lexical transfers during language play is unequivocally the syllable (Sherzer 1976). The language play data also corroborates the finding that young, pre-reading, children possess an intrinsic ability to recognize and respond to the syllable structure of words when asked to tap their hand in cadence to the audio sounds of spoken words (Liberman 1973). Additional examples of the primacy of the syllable can be observed in apraxic and dysarthric speakers whose output patterns are described as staccato, sing-song concatenations of dissociated syllable-by-syllable strings (Kent *et al.* 1982, 1979).

To summarize up to this point, the first two dots can be taken to support the contention that the earliest speech sound networks in the neurogenesis of language structure, and hence, spoken output, in frontal and temporal areas of the left hemisphere, are organized around segmental-like entities, initially grouped in a prototype sequential structure resembling CV syllable forms. Leaving left handedness issues aside, it is postulated that no such neural substrates, tasked to encode a language's sound segments-to-speech motor neural networks, exist in the right hemisphere of right-handed speakers.

4. Dot # 3: The Right Hemisphere of Split-Brain Subjects Cannot Rhyme

The development of the split-brain paradigm by Sperry and colleagues provided, for the first time, an elegant experimental method to direct sensory information to isolated hemispheres of the human brain and independently assess their relative processing capabilities for various types of language-related input signals (Sperry 1961). A visual tachistoscopic projection system (T-scope) was used to present various words/symbols onto visual half-fields for very brief time periods (usually 150 msec) to avoid a stimulus confound due to saccadic eye movements. A stimulus input to the right visual field (RVF) projected the image exclusively to the left visual cortex, and a left visual field (LVF) stimulus was exclusively projected to the right visual cortex.

In split-brain subjects, due to their complete cerebral commissurotomy, there is no inter-hemispheric transfer of information, and hence each hemisphere "has its independent mental sphere or cognitive system-that is, its own independent perceptual, learning, memory, and other mental processes" (Sperry 1961: 1). In preliminary studies, it became obvious that only the left hemisphere was capable of speaking, and the right hemisphere could only manually respond by directing the individual's left hand to write or select seen objects from behind the T-scope screen.

One of the most creative adaptations of this paradigm was developed by Eran Zaidel in a series of elegant studies exploring the information processing capacity of the right hemisphere (Zaidel 1978). Zaidal realized that, to fully analyze the capabilities of the right hemisphere across a varied set of language tasks, it would require a longer stimulus exposure interval than 150 msec. To enable longer scrutiny intervals Zaidel devised a projection system that was yoked to the saccadic movements of the subject's eye. Each split-brain subject was fitted with a customized contact lens. Stimuli (e.g., groups of four words, or four pictures of common objects) were projected to separate visual half fields, and as the subject's eyes moved for each saccade, the projection system compensated by moving the exact distance to keep the image stabilized in the same visual half-field. This allowed subjects to take as long as needed to visually process what was being asked of them, e.g., "point to the two pictures of objects that rhyme" when shown four pictures, two of which were a baseball bat and a man's hat.

Zaidel ran a series of inter-related experiments that explored information transfer from one modality form to another: sound-to-meaning (via a picture), sound-to-spelling (orthography), spelling-to-picture, picture-to-sound, spelling-to-sound, meaning-to-sound, and orthography-to-sound. While the left hemisphere of the split-brain subjects had no trouble successfully performing all the tasks, the right hemisphere revealed a striking inability to evoke the *sound image* of a seen object or letter string (that they knew the meaning of), and, of most importance to the argument being put forth here, *a striking inability to assess rhyme*. Whenever the task required a transfer from either semantics (pictures of objects), or letter strings (e.g., B-I-R-D, C-A-T, H-O-U-S-E) for judging a rhyme (e.g., "Which word rhymes with *hat*?"), the right hemisphere was incapable of performing the meta-linguistic conversion of a seen picture or letter string into an internalized sound equivalent.

Another test to assess rhyming ability presented a slide having four pictures, two of which, when pronounced, rhymed, and two did not. The subject was told to point to the two pictures that sound the same, but have different meanings (e.g. rose/toes, mail/male). They would use their left hand to point to their answers. Presented by themselves for comprehension (e.g. hear word '*mail*' or see letters *M A I L*, and asked to point to the correct picture), the right hemisphere knew what the stimulus word meant, but when asked to judge a rhyme (even with similar orthography as in 'nail'), the right hemisphere was clueless. If the orthographic pairings differed in spelling (e.g., *pea/key)*, or presented idiosyncrasies of English pronunciation (e.g., *lint/pint*), performance was considerably worse.

The take-away message from the third 'dot' is the following: To be able to generate a rhyme or judge whether a word pair contains a rhyme, the neural processing substrate must be able to internally generate the sound equivalent of the orthographic word or picture of the object—primarily the vowel/coda of a lexical string. It's very quiet inside your brain, but the left hemisphere is uniquely adept at internally generating sound equivalencies of input letter strings or seen objects. These encoded segmental-based network representations have a dual function: They (i) inherently possess the sound equivalencies of the phonemic units making up the word and (ii) serve as the neural source for generating speech production, or said another way, the phonological intent that drives and initiates the motor programming to elicit a speech output signal.

These critical properties—internal generation of sound equivalencies of phonemes and an ability to go from 'intent-to-motor activation'—are hypothesized to be present, in the overwhelming majority of right-handed adults, only in the left hemisphere of the brain. The inescapable truth is that if rhyming ability can only be performed by the left hemisphere, then the neural equivalent of vocalic nuclei of syllable codas is only present in the sound processing regions of the left hemisphere.

5. Dot #4: Rhyming and Phonetic Category Deficiencies in Children with Developmental Apraxia of Speech

What happens if and when such (hypothesized) lateralized neural sound substrates fail to develop in neurogenesis? The answer might lie in the childhood speech deficit known as Development Apraxia of Speech (DAS). DAS is customarily defined as a neurologically based disorder in the ability to carry out coordinative movements of the speech articulators in the absence of impaired neuromuscular functioning (Shriberg *et al.* 1997). The behavioral symptomatology of DAS presents with a wide array of speech/language deficits encompassing input, organizational, and output processing. However, output processing deficits have had a disproportionate influence in diagnosis and treatment of this childhood language disorder. The primary production-based deficits include: a restricted phonemic repertoire, predominance of omission errors, *frequent vowel errors*, inconsistency of errors, restricted use of word shapes (they produce mostly CVs), and better receptive than expressive test scores (Marquardt *et al.* 1998).

Studies in our lab focused on the representational and perceptual abilities of children with DAS—specifically, their ability to generate and assess rhymes (Marion *et al.* 1993) and categorical perception of speech (Sussman *et al.* 2000, 2002). The theoretical impetus for these studies was the hypothesis that the underlying etiological cause of DAS was a neural dysmorphology in left hemisphere areas mediating the phoneme-sized phonological representations necessary to both form sound equivalencies and to initiate and control on-line articulatory programming of those sound strings. A child with DAS was perhaps operating with an impoverished phonological neural representation network that severely precluded both selection and access to the neural correlates of the phonological forms guiding speech motor performance. In effect, a DAS child trying to speak would be analogous to an adult playing scrabble with hard to read letter tiles because they were blurry or malformed.

A strong test of the hypothesis that DAS is based on a left hemisphere developmental dysmorphology in the neurogenesis of brain tissue that mediates phonological representations is to assess the rhyming abilities of DAS children (matched to typically developing controls). The essence of rhyming ability is the internal generation of vowel sounds, holding them in short term working memory, and meta-linguistically judging (dis)similarities across word pairs.

Marion *et al.* (1993) devised three rhyming tasks. (i) Rhyme production: Following presentation of a target word (N = 12), the child had to produce as many rhyming words as possible in 30 seconds. (ii) Assessing rhyming word pairs: Using a target word, which of two words rhymes best with the target word? (iii) Rhyme perception: For each target word, 10 words were presented and the child indicated which words rhymed with the target item. The results were very revealing—the DAS children (N = 4) could not generate rhymes, or even recognize rhyming words, while the four control children exhibited signifycantly higher scores on every task. For example, in the rhyme production study the DAS children produced a score of <2.0 correctly rhyming words compared to over 30 for the control children. In the rhyming pairs test, which was much easier, the DAS children scored between 40–50% correct matches, while the control children scored close to 100%. On the rhyme perception test, the DAS children produced an over-abundance of false rhymes while generally failing to recognize correct rhymes.

The striking inability to form and recognize rhymes in DAS closely resembles the right hemisphere's rhyming deficiencies documented in split-brain subjects (Rayman *et al.* 1991). The main difference is that with split-brain subjects, their right hemisphere is innately incapable of rhyming, whereas in DAS children, it is hypothesized that their phonologically impoverished left hemisphere substrates were attempting to perform the mental operations required for rhyming, but falling short. Once again, to be able to rhyme, brain regions must possess the internalized neuronal equivalent of the sound evoked by the vowel-dominant coda cluster of a word. This seems to be the exclusive provenance of the speaking left hemisphere. If, as hypothesized, DAS is caused by a dysmorphology of left hemisphere neural substrates that normally process sound elements, that in a normally developing brain, map/represent the finite set of phonetic segments comprising the sound inventory of a language, then normal left hemisphere dominance in speaking may well be attributable to the exclusive presence of such substrates as the requisite 'start' button initiating and controlling the serial ordering of speech. DAS children might very well lack this 'start' button initiation in going from phonological representation to phonetic/ articulatory output.

Another way to probe the integrity of neural-based phonological categories is to perform labeling studies as part of a categorical perception procedure. Using an identification task with a 14-item stimulus continuum ([ba-da-ga]), Sussman *et al.* (2002) showed poor categorization skills in all five DAS children tested relative to five typically developing controls. The DAS group showed equivocation in labeling within-category allophonic stimuli and an absence of quantal shifts in identification percentage scores at expected phonetic boundaries. The perceptual sensitivity of the two groups to F2 changes in adjacent CV stimuli was also assessed by using a cumulative d' statistic. The less steep slope of the d' function in the DAS group revealed a considerably diminished perceptual sensitivity to systematic changes in the acoustic stimuli. Simply put, the DAS children exhibited a very fragile control of categorical entities and their internalized phonologically-based structure.

There are two basic requirements needed to establish well-formed contrastive phonetic categories: (i) sensitivity at phonetic boundaries, combined with (ii) the ability to ignore or generalize across (within category) allophonic variations. The second element is not often discussed, but there needs to be a basic neuronal mechanism that maintains categorical consistency in the face of non-phonemic signal variation. Tolerating and generalizing across subtle, within-category, allophonic variations is crucial in establishing well-formed categorical representations. A recent MMN study (Miglietta *et al.* 2013) successfully partitioned allophonic-based ERPs from phonemic-based ERPs across vowel pairings in a dialect of Italian. Thus, neural computations exist for within-category phonetic distribution patterns. Non-contrastive auditory differences must therefore require a learned inhibitory-based computation to allow for faster unfettered access to higher perceptual phonemic representations.

The collective findings from these DAS studies adds another crucial dot—if the neural networks that encode basic phonological units, the building blocks of language, fail to develop in a normal fashion, the resulting outcome is what we see in the highly unintelligible and very limited speech/language capabilities of children diagnosed with DAS.

6. Dot #5: Speech Errors and the Slot-Segment Hypothesis

One of the many unknowns about speech production is the answer to the question: "What phonological entity is most closely related to the neuro-motor commands underlying speech production?" Possible candidates for the 'phonological primitive' are the phoneme, the extrinsic allophone, the syllable, the word, the phase, etc. The existence of linguistic abstractions, unfortunately, cannot be empirically validated by brain imaging techniques. The phoneme, however, as one possible candidate for this elusive unit, possesses a high degree of psychological reality based on its overwhelming prevalence in speech error corpora. For example, considering only exchange errors, e.g., *'guinea pig cage' — 'guinea kig page'*, Shattuck-Hufnagel (1983) reported that 138 of 210 errors (66%) occurred as phonemic segments in the 1981 MIT corpus. No other sound structure unit was even close. What is considerably more important, however, than proclaiming what linguistic entity best corresponds to the neural correlate of phonological structure is what can be learned from studying speech errors:

> The interest is rather in how particular errors shed light on the underlying units of linguistic performance, and the production of speech. What is apparent, in the analyses and conclusions of all linguists and psychologists dealing with errors in speech, is that, despite the semi-continuous nature of the speech signal, there are discrete units at some level of performance which can be substituted, omitted, transposed, or added. (Fromkin 1971: 29)

Behavioral data from sound exchanges provide a window into the premotor planning stage of an utterance before actual production of that utterance. The displaced phoneme-sized exchanges characterizing speech errors have contributed to several theoretical insights into the neural events taking place prior to overt motor programming. One such insight was the suggestion by Shattuck-Hufnagel (1975, 1979) that there are two separate but interactive neural network structures underlying the representation of phonologically organized sound units. She postulated a neural framework for syllable structure ('serially ordered slots'), and an independent, but synaptically inter-connected representational network for the phonetic segments. Such a two-tiered interactive neural substrate helped to conceptualize the various rules that Fromkin (1971) earlier formulated governing the nature of segmental-based sound exchanges. Rule #1 was that consonants always exchange with consonants and vowels only exchange with vowels. Rule #2 stated that sound exchanges always occur within the same syllable position. So in the error '*the nipper is zarrow*' (for the 'zipper is narrow') the migrating 'n' in '*narrow*' erroneously fills the C1 slot of word 1, instead of the intended occupant /z/; the displaced 'z' doesn't disappear in a brain 'cloud', but fills in the now vacated C1 slot in word 2, left empty by the transposed 'n'. Thus, the empty slot awaits a new segmental occupant, acting as a place-holder for the displaced phoneme. The sound-based units are very real in a neural sense. Synaptic connections between re-arranged segment-based networks and canonical syllable-shape networks still manage to produce fluent output containing the speech error.

Rule #1 is inviolate in speech error analyses and can speak to the primacy of the vowel in a syllable (i.e., there is no syllable without it). Vocalic-like sounds in early infant vocalizations (dot #1) can be viewed as the earliest input signal in developmental neurogenesis to fill this integral slot of the emerging syllable-based neural scaffolding. In essence the vowel can be conceptualized as being 'prepackaged' and anchored into the nucleus slot of any future syllable form (CV, CVC, CCV, CCVC, etc.) that develops over time with increasing phonological complexity (Sussman 1984). Each language forms a neural slot framework structure driven by its own syllable shape(s), for example CV in Japanese and Hawaiian, (CCC)V(CCCC) in English.

Dot #5 (speech errors) serves to consolidate several previous dots. If the left hemisphere exclusively houses the neural substrates forming syllable frames, with their synaptic network linkages to auditory-encoded segmental entities of a given language, with primacy of the vocalic nucleus, then it is no mystery that speech output programming is under the exclusive control of the left hemisphere. A hemisphere devoid of a segmental-sound-based encoding infrastructure does not possess the 'neural-sparkplug' that, in effect, serves as the 'intent' to initiate and control the serial ordering of sound units underlying speech motor programming.

7. Dot #6: The Neocortex—Computational or Serially Ordered Memory System?

In his book *On Intelligence,* Hawkins (2004) puts forth several insights regarding the operational properties of the neocortex. A basic postulate is that "the neocortex uses stored memories to […] produce behaviors" (p. 69). So rather than computing unique solutions to perform motor behaviors, the brain possesses stored memories, learned across development through repeated experiences. Moreover, these motor memories sequentially operate in an auto-associative manner. We activate memories, whether motor, visual, or sound, the way you learned them, and each temporally ordered memory elicits the next. Common everyday examples show the validity of this simple, but largely ignored feature of neural operations within our 'connectome'—e.g., one cannot (easily) sing a song, recite a well known passage, or the alphabet, backwards; hearing the start of a familiar tune sequentially elicits the next portions, in the temporal order in which it was learned. Spoken language, like all serially ordered motor skills, unfolds in sequential fashion, each set of articulatory movements, organized around sequential syllabic frames, automatically triggers the next. If, as strongly suggested by the preceding 'dots', the left hemisphere's auditory/speech motor areas are the exclusive repository of the neural networks instantiating production of segmental-based units, with their inherent sound and articulatory motor equivalencies, organized around syllable-by-syllable concatenations, then speech output should only be possible in the left hemisphere. The connectome of the right hemisphere is generally regarded as a synthesis specialist, processing holistically (faces, not noses), not analytically. A gestalt-based neural structure is not conducive to motorically producing a serially-ordered, symbol-based, syllabically organized, set of learned articulatory behaviors inherently linked to sound equivalents.

An interesting addendum to this hypothesized scenario is the added concept of a hierarchically-organized invariance in the way the neocortex is organized for processing input signals and also executing motor behavior (Hawkins 2004). Our brains, unlike artificial intelligence systems, can recognize faces from any angle or position; we can recognize familiar tunes regardless of the instrument playing them—e.g., the *Stars Spangled Banner* is easily recognized if played by a harmonica, tuba, piano, or whistled. A computer can only store information the way it was presented, there is no tolerance for variability. Speech, whether in input or output mode, is highly adaptable.

The widely used bite block paradigm (e.g. Kelso & Tuller 1983) illustrates this concept: When acrylic bite blocks are placed between a speaker's back molars, thus precluding jaw movements in articulation, a speaker can immediately, on the first trial, compensate for the lack of jaw movement by using new/novel tongue configurations that create equivalent vocal tract resonance properties to arrive at the auditory target of the speech sounds produced. Similarly, a pipe smoker can produce intelligible speech whilst biting down on the pipe stem. The invariance that characterizes both speech perception (e.g., different F2 transitions in /dV/ utterances can all be heard as the same /d/), and speech production (e.g., myriad of ways the same sound can be produced by varying articulatory motor contributions) serves to point out that the 'sound plan' neural infrastructure, as envisioned in this account, is linked to highly flexible and synergistic speech motor net-works.

8. Dot #7: Recording from Intracranial Electrode Arrays in Human Left pSTG

A major premise of this paper is that speech sounds exist as stored representations in auditory neural substrates of the left hemisphere. For scientists outside the field of experimental phonetics this might sound a bit silly: "How could speech sounds not be represented in the human brain?" However, the longstanding theoretical division in the field of experimental phonetics between auditory vs. gestural views of underlying neural correlates of speech units has prevented a unified theoretical position to emerge, even after six decades of experimental research (e.g., Studdert-Kennedy 1998, 2005, Studdert-Kennedy *et al.* 2003).

Recent game changing studies by Chang and his colleagues at UCSF have served to strongly substantiate an auditory-based position. Chang *et al.* (2010) synthesized 14 uniquely different stop consonant-vowel syllables by systematically altering the onset frequencies of the F2 transition to create a [ba-da-ga] continuum as used in categorical perception studies. They were presented in random order to four subjects, post craniotomy and prior to surgery for epilepsy. Evoked potentials were obtained for each stimulus presentation via a customized 64-electrode microarray placed on left pSTG. The specific question addressed was whether pSTG neural activity patterns would correspond to the precise spectro-temporal changes in the external acoustic signal (i.e., veridical representation, and hence 14 different ERPs), or to a higher order linguistic extraction of phonetic categories (only three unique ERP patterns)? The analysis was based on the degree to which a multivariate pattern classifier was able to distinguish single-trial response patterns of the evoked cortical potentials. Response amplitude and across-stimuli dissimilarities peaked at 110ms after stimulus onset, and the topography of the most discriminative cortical sites clearly revealed only three discrete activation patterns, not 14. The local and transient response properties revealed distributed, but non-overlapping, spatial representations for stop place category-based patterns. Thus, it is no longer necessary to only *postulate* the existence of auditory representations of the sounds of human language in the brain— they indeed have neurophysiological reality.

The abstract from Chang *et al.* (2010) succinctly captures the essence of their findings and the implications for understanding the neural underpinnings of speech and language phonological structure:

> Speech perception requires the rapid and effortless extraction of meaningful phonetic information from a highly variable acoustic signal. A powerful example of this phenomenon is categorical perception, in which a continuum of acoustically varying sounds is transformed into perceptually distinct phoneme categories. We found that the neural representation of speech sounds is categorically organized in the human posterior superior temporal gyrus. Using intracranial high-density cortical surface arrays, we found that listening to synthesized speech stimuli varying in small and acoustically equal steps evoked distinct and invariant cortical population response patterns that were organized by their sensitivities to critical acoustic features. Phonetic category boundaries were similar between neurometric and psychometric functions. Although speech sound responses were distributed, spatially discrete cortical loci were found to underlie specific phonetic discrimination. Our results provide direct evidence for acoustic-to-higher order phonetic level encoding of speech sounds in human language receptive cortex. (Chang *et al*. 2010: 1428)

The electrophysiological recordings of Chang *et al.* (2010), limited to only three stop consonants (/bdg/) and one vowel (/a/), have been expanded more recently to include the entire English phonetic inventory (Mesgarani *et al.* 2014). Using the same high-density multi-electrode arrays placed over the left STG in six subjects undergoing craniotomies, they reported high selectivity at numerous single electrode sites responding to the unique spectrotemporal acoustic properties of speech sounds.

Phoneme groups (stops, fricatives, nasals, semi-vowels, vowels) were organized into highly differentiated clusters based on shared phonetic features, primarily distinguished by manner of articulation, and secondarily by place of articulation distinctions. A needed control to fully comprehend the significance of these findings is to perform the same analysis on patients undergoing a right craniotomy and placing the recording electrode array on right pSTG. The absence of fine tuning for spectrotemporal acoustic cues defining phonetic structure groupings in right hemisphere superior temporal cortex would further support the views being hypothesized in this paper.

9. Summary and Conclusions

Several inter-related areas of research and theory were described: (1) lateralization of infant babbling; (2) phonological primacy of the syllable; (3) the inability of the right hemisphere of split-brain subjects to generate/assess rhymes; (4) the inability of children diagnosed with a left hemisphere-based language disorder (DAS) to generate/assess rhymes and behaviorally evidence well formed speech sound categories; (5) analyses of speech exchange errors supporting an underlying, tiered, syllable slot-segment neural structure; (6) a view of cortical organization and processing as memory networks characterized by being experientially learned, activated in serial temporal order, with auto-associative triggering, and hierarchically organized to achieve invariant representations; and (7) recent evidence from intra-cranial electrode arrays on human left pSTG showing distributed neural foci invariantly encoding phonetically structured categories.

A connecting theoretical thread was sewn across these seven research areas suggesting that the asymmetrical dominance of the left hemisphere to control speech output might be due to the exclusive existence of specialized neural substrates encoding the phonological elements of language, organized in canonical syllable-sized representational networks. This left hemisphere network initially develops during early infant vocalizations, from coos to canonical CV babbling, to early first words. Of most importance is that this emergent neural substrate can serve as the exclusive neural 'start button' to bring about articulatory motor programming. It is maintained that the right hemisphere does not possess such sound unit-based neural networks, as primarily holistic processing has no use for serial processing of symbolic units that are integrally connected to speech motor pathways. This account focused only on underlying structural properties of left hemisphere neural tissue to account for asymmetry in speech motor output. What remains to be explained is why and how this hemispheric specialization began.

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★ FORUM ★

Notice

I would like to use this opportunity to briefly thank all those involved in creating the ninth volume of *Biolinguistics*. It is quite clear that *Biolinguistics* already has undergone some changes. A major change was to move from the traditional issue-based volume to a single volume per year with continuous publication in 2013. I believe the move was necessary and welcome, from editor's and authors' perspectives alike, as pieces can now be published as they get accepted, revised, and copy-edited. However, it has also meant a drop in size, with 329 pages in volume 7 (2013) to 258 in volume 8 (2014), to the all-time low of 132 published pages in the current volume 9 (2015). One aspect is surely the increased workload for the editor with the move from two joint editors who shared publishing duties to a single editor; this is something I am working on. In fact, there will be some more changes to the journal, which I hope to be settled in early 2016 with the anniversary volume 10. The reshuffled and rejuvenated Task Team will play a pivotal role as will the slightly rearranged Editorial Board.

However, another aspect, I would like to believe, is the growth of the field of biolinguistics itself (see also my editorial from the beginning of the year, '*Biolinguistics* for Biolinguistics', pp. 1–7 of volume 9, 2015). There are now more output venues for biolinguistic research—and perhaps, one could argue, there is now more fierce competition getting accepted in *Biolinguistics*. The rejection rates seem to suggest so but it is also clear that the journal could do with more high-caliber submissions across all categories (articles, briefs, reviews, forum pieces).

My special gratitude goes to the reviewers that have served us throughout 2015, who are listed below. Many thanks also to the section editors that assisted me on several submissions. For everything else, I thank all supporters as well as the members of the *Biolinguistics* Advisory Board, the Editorial Board, and the now reinforced Task Team that are not specifically mentioned by name for their active participation and constructive feedback.

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