biolinguistics

volume 11 — special issue — 2017

ISSN 1450-3417

50 Years Later: A Tribute to Eric Lenneberg's *Biological Foundations of Language*

Patrick C. Trettenbrein

Introduction

"The study of language is pertinent to many fields of inquiry," reads the first sentence of the preface to *Biological Foundations of Language*. The serious scientific study of the biological foundations of the human capacity for language as one of the youngest branches of linguistic inquiry, nowadays frequently referred to using the label "biolinguistics," began roughly half a century ago and was, in part, fuelled by the so-called "cognitive revolution" (Miller 2003) of the 1950s. Eric Lenneberg's book *Biological Foundations of Language*, one of the field's founding documents, was first published in 1967, that is exactly 50 years ago. Today, though not as universally known as it should be, Lenneberg's book is regarded as a classic by most people in the field. Consequently, this year's anniversary provides an excellent occasion for revisiting Lenneberg's by now classic work and reassessing the scope, validity, and foresight of the evidence presented and arguments put forward.

The purpose of this special issue thus is to reconsider and reflect on Eric Lenneberg's ideas and how they influenced (or actually didn't influence, because they were quickly forgotten) today's field of biology of language. In his *Biological Foundations of Language*, amongst other things, Lenneberg already outlined the possibility of a genetics of language and wrote about language and the brain long before any of the multitude and major technological advancement in both, genetics and neuroimaging, that we have seen in the past decades were even looming on the horizon. A whole lot has been learned since *Biological Foundations of Language* was first published and there can be little doubt that Lenneberg would be amazed by

---

I would like to seize this opportunity to thank all those who contributed to this special issue of *Biolinguistics* in one way or another, be it by submitting papers, reviewing manuscripts, or assisting with various steps of the editorial process. A special thank-you is due to Kleanthes K. Grohmann, editor of *Biolinguistics*, for providing me with the opportunity to organise and guest-edit this special issue, as well as for his enduring support, advice, and assistance throughout the editorial process. Similarly, I would like to thank Katie Gough, Lisa Miklauschitsch, Valeriia Perepelytsia, Corina M. Sturm, and Michael C. Trettenbrein for volunteering their time as editorial assistants. I am grateful to Koji Fujita for bringing the Japanese translation of *Biological Foundations of Language* to my attention as well as for generously providing pictures and scans. On a related note, thanks is due to Maximilian Gorfer for translating the preface of the Japanese edition. The production and publication of this special issue was subsidised by the Referat für Wissenschaft & Forschung der Steiermärkischen Landesregierung.

biolinguistics

ISSN 1450–3417

the technological and methodological progress in neuroimaging, neuroscience at large, and genetics. The contributions collected in this special issue discuss various aspects of these developments insofar as they revisit and/or update Lenneberg's interpretation of the data and especially their theoretical implications from a contemporary point of view.

About Eric Lenneberg

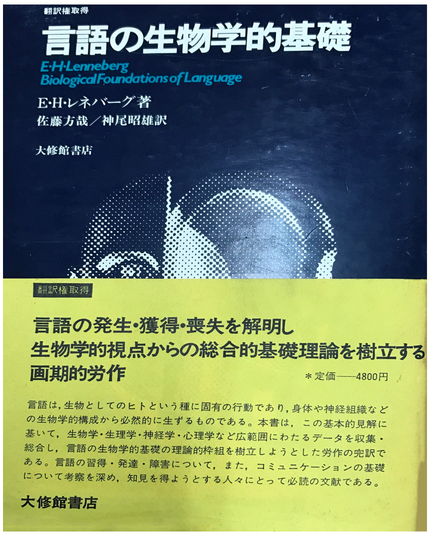
Figure 1: The late Eric H. Lenneberg as portrayed in Lenneberg & Lenneberg (1975). © Elsevier Inc.

Eric Heinz Lenneberg (* 19th September 1921 – † 31st May 1975) was born in Germany in 1921 and attended grammar school in Düsseldorf before his family, being Jewish, had to flee from the Nazis to Brazil. He lived in Brazil until 1945, at which point he left for the United States in order to study at the University of Chicago. After obtaining his bachelor's degree, Lenneberg continued his university education by studying linguistics and, in 1956, received his Ph.D. in linguistics and psychology from Harvard. However, Lenneberg was not done yet and went on to study neuroscience at Harvard Medical School. He was interested and tried to keep up with the literature and latest developments in embryology, neuroanatomy, motor control, and evolution, to name just a few of his spheres of interest. Throughout his life Lenneberg held appointments at a number of major universities in the United States and lectured all over the world. He organised workshops in cooperation with the Neuroscience Research Program (Arbib, this issue), UNESCO (Lenneberg & Lenneberg 1975), and the Max Planck Society in Germany and published a number of volumes on a variety of issues in the study of mind, brain, and language.

This brief sketch of Lenneberg's life and education already reveals why he was probably uniquely suited to co-found what would later become biolinguistics together with two other young students who were at Harvard at the same time, the graduate student Morris Halle and the then even younger Harvard junior fellow Noam Chomsky. Their shared scepticism about the radical behaviourism that dominated psychology in Cambridge at the time led them to read a lot of the now classical ethological literature coming from Europe (e.g., Konrad Lorenz, Nikolaas Tinbergen, Otto Koehler, etc.). Lenneberg and Halle could speak German and would frequently read ethological publications in their original language. This reading matter provided a basis for considering the human language capacity as beeing rooted in the biological nature of the species, rather than a cultural or technological achievement (for a brief recap of these early days see the interview with Noam Chomsky in this issue). Together, Lenneberg, Halle, and Chomsky spearhead the cognitive revolution in linguistics and psychology by adopting a biological approach to the study of the human language capacity. While Chomsky's own work focused on the formal analysis of natural language (also reflected in the appendix he contributed to *Biological Foundations of Language*), Lenneberg was interested in the biological facts about language and its development—ontogenetically as well as phylogenetically—his ultimate goal being to develop a biological theory of language (Lenneberg 1964a). These efforts culminated in *Biological Foundations of Language*.

About the Book

Biological Foundations of Language was first published in 1967 and, somewhat surprisingly, has never been reissued, neither in its original nor as an updated version. However, a German translation was published as *Biologische Grundlagen der Sprache* only in 1972 and, as Koji Fujita brought to my attention, a Japanese translation of the book also exists (see figure 2). Interestingly, Lenneberg used the foreword for the Japanese translation of the book to clarify what he meant by discontinuity in the evolution of language, emphasising the qualitative difference between human language and animal communication systems and pointing out that such a discontinuous take on language evolution does not imply that natural history itself is discontinous—a topic that is also taken up in two contributions to this special issue by Callum Hackett as well as Sergio Balari and Guillermo Lorenzo.



翻訳権取得
言語の生物学的基礎
E·H·Lenneberg
Biological Foundations of Language
E·H·レネバーグ著
佐藤方哉／神尾昭雄訳
大修館書店

翻訳権取得
言語の発生·獲得·喪失を解明し
生物学的視点からの総合的基礎理論を樹立する
画期的労作
＊定価——4800円

言語は，生物としてのヒトという種に固有の行動であり，身体や神経組織などの生物学的構成から必然的に生ずるものである。本書は，この基本的見解に基いて，生物学・生理学・神経学・心理学など広範囲にわたるデータを収集・総合し，言語の生物学的基礎の理論的枠組を樹立しようとした労作の完訳である。言語の習得・発達・障害について，また，コミュニケーションの基礎について考察を深め，知見を得ようとする人々にとって必読の文献である。

大修館書店

Figure 2: The cover of the Japanese translation of *Biological Foundations of Language*.

The scope and depth of Lenneberg's book is intimidating, even upon rereading 50 years after it was first published: Chapter after chapter, the contemporary reader will find ideas that they might think of as particularly "modern" or "new" already discussed in varying depth but always with persuasive clarity. For example, Lenneberg already noted that "speech and language are not confined to the cerebral cortex" and warned us that there is no single brain region to which the language capacity is confined, while there clearly are specific regions and networks that are crucially involved in language processing. Despite this, *Biological Foundations of Language* was by no means intended to serve as a textbook or survey of the literature at the time; instead Lenneberg's vision had been to write what he himself called a "theoretical treatise."

The body of issues in which Lenneberg was interested and on which he would elaborate in his 1967 book is foreshadowed in a vast number of publications preceding *Biological Foundations of Language*. For example, in Lenneberg (1964) we read that

> [...] all these considerations serve to establish an hypothesis and to stimulate new directions for research on the nature of man. However, the facts presented [in this paper] do not constitute a theory. Let us hope they will lead to one in the future. (Lenneberg 1964: 85)

Biological Foundations of Language then was Eric Lenneberg's attempt to get at least a step closer to such a theory, as is evident from the concluding chapter, in which he provides his attempt at such a biological theory of language. This explains why, in many respects, Lenneberg's book was kind of a latecomer to the party: Skinner's take on language had famously been debunked by Chomsky (1959) and many a behavioural psychologist had been "converted" and was already advocating for a more nativist take on the study of language solely on the basis of arguments and analyses stemming from theoretical linguistics.

Therefore, Lenneberg's *Biological Foundations of Language* was, at least in part, also supposed

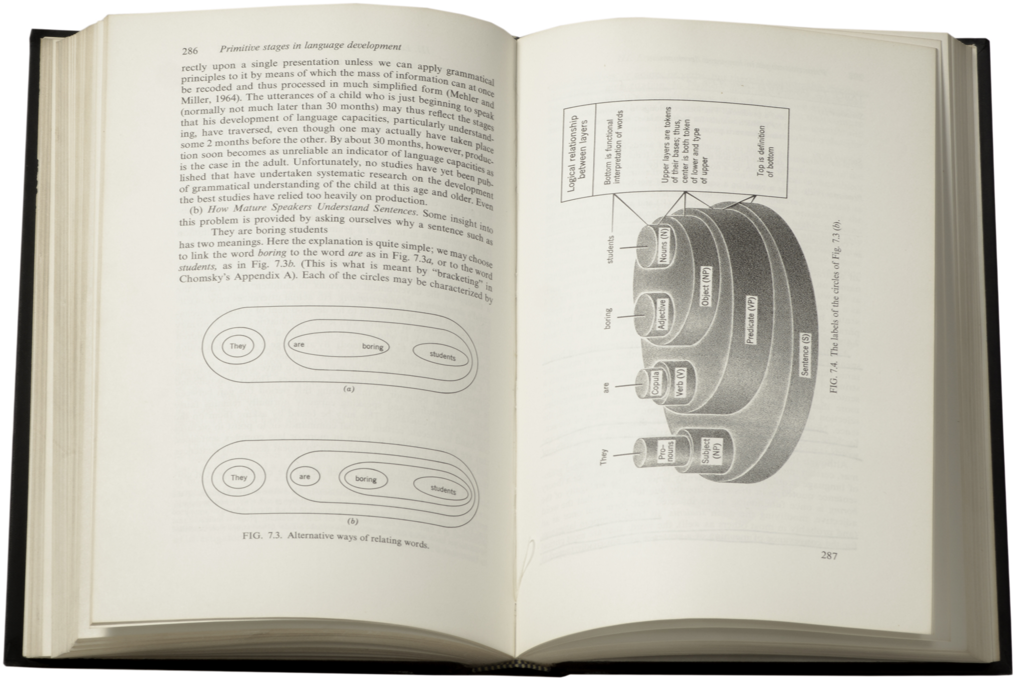
> [...] to provide a palpable biological plausibility for conclusions to which a number of uncomfortable Empiricists [...] [had] committed themselves on the basis of formal argument alone.
>
> (Bem & Bem 1968: 498–499)

Interestingly, as Bem & Bem also say in their review, Lenneberg's line of argument and the evidence on which he relies, more often than not, is indirect or negative. Thus, it is not so clear as to whether Lenneberg succeeded in identifying the biological basis of linguistic competence in his book because this, of course, is still an ongoing endeavour. The connection between theoretical linguistics and biology remains indirect, even in neurolinguistics as the currently most promising point of contact with recent interesting results (e.g., Ding et al. 2016, Nelson et al. 2017; for a brief review see Zaccarella & Friederici, 2016).

In the famous debate on "bio-linguistics" in 1974, organised by Massimo Piattelli-Palmarini, in which Lenneberg unfortunately did not participate, the psychologist Hans-Lukas Teuber already pointed out that there are two different ways of looking for a possible physiology of language: A crude and a fine one. Back then Teuber was optimistic about both, whereas it now seems appropriate to say that only the crude one has seen significant progress in the past decades (reviewed in detail in Friederici 2017; with a focus on syntax in this issue), especially due to advent of neuroimaging. Yet, some very interesting results have also come from the "fine" approach (e.g., Zilles et al. 2015), while (many) long-standing problems remain (see the contributions to this issue by Sussman and Piattelli-Palmarini; also, more generally, see Gallistel & King 2009). In this context, it is interesting to see that Lenneberg's thinking was so modern in many respects as to indirectly anticipate this development: In what basically amounts to a side note in the book's chapter on "Neurological aspects of speech and language," Lenneberg reaches a conclusion similar to the one reached by Gallistel & King (2009) on computational grounds:

> [...] we may conceive of molecular changes taking place within cells that are located over wide areas in the brain and causing changes in the interaction between cells. [...] We are now merely postulating that the restructuring may take place within cells instead of between them.
>
> (Lenneberg 1967: 215)

This point of view has now received some tentative empirical support (e.g., Jirenhed et al. 2017; see Trettenbrein 2016 for a review), with far-reaching implications.



286 *Primitive stages in language development*

rectly upon a single presentation unless we can apply grammatical principles to it by means of which the mass of information can at once be recoded and thus processed in much simplified form (Mehler and Miller, 1964). The utterances of a child who is just beginning to speak (normally not much later than 30 months) may thus reflect the stages that his development of language capacities, particularly understanding, have traversed, even though one may actually have taken place some 2 months before the other. By about 30 months, however, production soon becomes as unreliable an indicator of language capacities as is the case in the adult. Unfortunately, no studies have yet been published that have undertaken systematic research on the development of grammatical understanding of the child at this age and older. Even the best studies have relied too heavily on production.

(b) *How Mature Speakers Understand Sentences.* Some insight into this problem is provided by asking ourselves why a sentence such as

They are boring students

has two meanings. Here the explanation is quite simple; we may choose to link the word *boring* to the word *are* as in Fig. 7.3*a*, or to the word *students,* as in Fig. 7.3*b*. (This is what is meant by "bracketing" in Chomsky's Appendix A). Each of the circles may be characterized by

FIG. 7.3. Alternative ways of relating words.

FIG. 7.4. The labels of the circles of Fig. 7.3 (*b*).

287

Figure 3: Eric Lenneberg's work and especially his *Biological Foundations of Language* stand out in many respects. One is the illustration of "bracketing" in order to introduce the concept of a phrase-marker (right), discussed in detail in an appendix by Noam Chomsky.

Writing in 2003, George Miller noted that he preferred to speak of the cognitive sciences instead of cognitive science, because there still is no "unified science that would discover the representational and computational capacities of the human mind and their structural and functional realization in the human brain" (Miller 2003: 144). In this greater context, the study of the biology of language is just a select set of questions singled out from the whole of cognitive science so that the same reservations apply. Similarly to the situation in the cognitive sciences in general, there still is no unified science of language that would discover the representational and computational capacities of the human language faculty and their structural and functional realisation in the human brain. Otherwise put, despite all technological advancements and progress in the study of language it is still not clear whether the prospect of such a unified science, though incredibly attractive, is realistic even in the very long run—be it for biolinguistics or cognitive science in general.

What's in this Issue?

Against this background, the contributions collected in this special issue come from a variety of sub-fields that all ask and try to answer biolinguistic questions, each attempting to provide a new piece in the puzzle and relying on a variety of different methods—ranging from neuroimaging to grammaticality judgements.

Keeping with Lenneberg's developmentalist spirit, a vast number of contributions discuss various aspects of language development in health and disease. Insofar as necessary the contributions expand upon or (slightly) modify and revise Lenneberg's original theoretical considerations on the basis of the contemporary abundance of developmental data that just was not available when Lenneberg was writing. Famously, Lenneberg already recognised that

> [e]ven if the maturational scale as a whole is distorted through retarding disease, the order of developmental milestones, including onset of speech, remains invariable. Onset and accomplishment of language learning do not seem to be affected by cultural or linguistic variations.
>
> (Lenneberg 1964b: 66–67)

Accordingly, Koji Hoshi evaluates Lenneberg's legacy to the field of (child) aphasiology, with a special focus on Landau-Kleffner syndrome, a child aphasia of epileptic origin. Karin Stromsworld and Aliza Lichtenstein contrast the relationship between phoneme production and perception in speech-impaired and typically-developing children, and their conclusion offers support for Lenneberg's observation that children's ability to perceive speech does not dependent on their ability to produce speech.

Keeping with Lenneberg's focus on case studies that have the potential to shed light on the biological basis of the human language capacity, Maria Kambanaros and Kleanthes K. Grohmann present a cast study of a child with 22q11 deletion syndrome and argue for a language profile associated with the syndrome that is distinct from specific language impairment, thereby relying on pathologies in order to advance a comparative biolinguistic approach to uncovering the genetic basis of language (i.e. Universal Grammar). Keeping with the genetic theme, Ken Wexler reviews his and others' work on language development and the genetic basis of human grammatical abilities, culminating in an outline of what he refers to as "a program for the genetics of grammar".

Very much in Lenneberg's spirit of attempting to construct a biological theory of language, a number of contributions seek to make theoretical advances in many respects: Evelina Leivada takes Lenneberg's conjecture that categories such as noun phrase, noun, and verb are not absolute constructs but flexible and contextually defined (Lenneberg 1967, 1975) as a point of departure for her interdisciplinary approach towards labels, Label, and the study of categories more generally. Also on a theoretical note, Veno Volenec and Charles Reiss draw upon some of Lenneberg's ideas to outline their theory of how phonology and phonetics interface which they have termed 'Cognitive Phonetics'. From the perspective of theoretical syntax, Misha Becker reviews the question of how human children figure out that a string of words was generated by displacement operations due to the transformational nature of natural language syntax. Maria Garraffa reconsiders Lenneberg's notion of language as a maturational controlled behaviour, using the emergence of non-local syntactic dependencies and their impairment as an example.

Coming from the perspective of the cognitive neuroscience of language, Barbara Lust and colleagues revisit the regression hypothesis of language development, attempting to link language acquisition to language deterioration against the background of a lot of independent work in neuroimaging and the cognitive neuroscience of language more generally by linking behavioural findings to data from structural Magnetic Resonance Imaging (MRI). Amongst ample other things, Eric Lenneberg already recognised that a "general 'grammatizing' ability" is a constitutional propensity that is "conspicuously absent in the parrot, and only develops in crude rudiments in several retarded children" (Lenneberg 1960: 882). Against this background, Angela Friederici presents an succinct overview of recent neuroimaging studies on the neural syntactic network and discusses these against the background of non-human primates' syntactic capabilities.

Any volume honouring Eric Lenneberg and his pioneering work would seem incomplete if it did not mention his idea of a critical period for first language acquisition in one way or another. Harvey Sussman approaches this question from the micro-perspective of neural morphology (Teuber's above-mentioned "fine" look), discussing whether an anatomical feature of a neuron's dendritic arborisation could be a potential neural correlate for this time-sensitive development. Taking a more macro or "cruder" look (in Teuber's sense), Elissa Newport and colleagues revisit Lenneberg's ideas about early developmental plasticity by discussing data from children after left-hemisphere perinatal stroke. Their data and review of the literature suggest "that there are very limited and patterned ways in which language develops in the human brain." Then, taking a lifespan-perspective, Ellen Bialystok and Judith Kroll revisit the concept of a critical period for first language acquisition against the background of studies with bi- and multilingual subjects, showing that "there is variation in how constrained or plastic different aspects of language acquisition may be."

As already mentioned above, Lenneberg was interested in many different biological subfields and tried to keep track of all of them. Needless to say, this is a next to impossible undertaking (even 50 years ago), which is why he ended up sending off graduate students to work on problems and in subfields that he found interesting. One example is the study of motor control that repeatedly resurfaces in workshops he organised and his edited volumes. The graduate student sent off by Lennberg to focus on the study of motor control was Avis Cohen, who generously contributed a short personal piece in which she revisits her career and interaction with her advisor. The connection of motor control to writing and language is also discussed in the contribution of Maria Teresa Guasti, Elena Pagliarini, and Natale Stucchi who draw upon Lenneberg's biological notion of language and related ideas like rhythmicity and temporal structural regularities to argue that individuals with developmental dyslexia are less efficient than control individuals in using structural regularities during handwriting and some language activities.

Given that Lenneberg was amongst the first who saw language as a biological object, he was of course also profoundly interested in how the human language faculty had evolved. Callum Hackett reviews justifications for a discontinuity theory of language evolution—now a somewhat prominent position in biolinguistics (Berwick & Chomsky 2016) that was first formulated by Lenneberg. Interestingly, as mentioned above, Lenneberg sought it necessary to clarify the sense in which he considered the evolution of language to be discontinuous in the foreword to the Japanese translation of *Biological Foundations of Language*. Similarly, also building upon Lenneberg's ideas about the evolution of the language faculty, Sergio Balari and Guillermo Lorenzo propose a homological program for the study of language phylogeny that rest upon a non-functional homology concept.

In the Forum section, Tecumseh Fitch invites us to ask the question "What would Lenneberg think?" and, luckily, also attempts to provide an answer to said question by discussing the advances in comparative study of cognition, neuroimaging, and genetics in the past 50 years since *Biological Foundations of Language* was first published, and especially in recent years. In a very similar spirit, Massimo Piattelli-Palmarini also revisits *Biological Foundations of Language* from a contemporary point of view, thereby reevaluating Lenneberg's thought and original insights, while updating and/or building on Lenneberg's original ideas.

Lastly, it should be recalled that Lenneberg was an avid organiser of (international) workshops all over the world. Unfortunately, the reports of these workshops have frequently been forgotten by now because they are hardly (or not at all) accessible online. Luckily, Michael Arbib has managed to rescue from oblivion one of these reports, more precisely, a report entitled "Language and Brain: Developmental Aspects" from a Neurosciences Research Program work session organised by Lenneberg in 1972. Arbib's reappraisal of the report as well as scans of the original report are now available online as part of this special issue. Finally, in conversation with myself, Noam Chomsky as one of Eric Lenneberg's contemporaries back in Harvard and one of the co-founders of the field has answered a number of questions about the early days, the importance of Lenneberg's work, and the current state of the field.

I end by thanking everyone who has made this special issue of *Biolinguistics* possible, be it by submitting papers, reviewing manuscripts, or assisting with various steps of the editorial process and hope that you will enjoy reading it as much as we have enjoyed putting it together. It seems to me that Tecumseh Fitch in his contribution to this issue is on the right track when he concludes that, were he alive today, Eric Lenneberg would be pleased to see the progress the field has made since *Biological Foundations of Language* was published. The biolinguistic approach to the study of language has aged well and Lenneberg would certainly be eager to find out what the future may hold.

References

Bem, Daryl J. & Sandra L. Bem. 1968. Nativism revisited: A review of Eric H. Lenneberg's Biological Foundations of Language. Journal of the Experimental Analysis of Behavior 11(4), 497–501.

Berwick, Robert C. & Noam Chomsky. 2016. *Why Only Us? Language and Evolution*. Cambridge, MA: MIT Press.

Chomsky, Noam. 1959. A review of B. F. Skinner's *Verbal Behavior*. *Language* 35(1), 26–58.

Ding, Nai, Lucia Melloni, Hang Zhang, Xing Tian, & David Poeppel. 2016. Cortical tracking of hierarchical linguistic structures in connected speech. *Nature Neuroscience* 19(1), 158–164. doi:10.1038/nn.4186.

Friederici, Angela D. 2017. *Language in our Brain: The Origins of a Uniquely Human Capacity*. Cambridge, MA: MIT Press.

Gallistel, Charles R. & Adam P. King. 2009. *Memory and the Computational Brain: Why Cognitive Science Will Transform Neuroscience*. Chichester, England: Wiley-Blackwell.

Jirenhed, Dan-Anders, Anders Rasmussen, Fredrik Johansson, & Germund Hesslow. 2017. Learned response sequences in cerebellar Purkinje cells. *Proceedings of the National Academy of Sciences of the United States of America* 114(23), 6127–6132. doi:10.1073/pnas.1621132114.

Lenneberg, Eric H. 1960. Language, evolution, and purposive behavior. In Stanley Diamond (ed.), *Culture in history: Essays in honor of Paul Radin*, 869–893. New York, NY: Columbia UP.

Lenneberg, Eric H. 1964a. A biological perspective of language. In Eric H. Lenneberg (ed.), *New Directions in the Study of Language*, 65–88. Cambridge, MA: MIT Press.

Lenneberg, Eric H. 1964b. The capacity for language acquisition. In Jerry A. Fodor & Jerrold J. Katz (eds.), *The Structure of Language: Readings in the Philosophy of Language*, 579–603. Englewood Cliffs, NJ: Prentice-Hall.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Lenneberg, Eric H. 1972. Language and brain: Developmental aspects. A report based on an NRP Work Session held November 19–21, 1972, and updated by participants. *Neuroscience Research Programme Bulletin* 12(4), 511–656.

Lenneberg, Eric H. & Elizabeth Lenneberg (eds.). 1975. *Foundations of Language Development: A Multidisciplinary Approach*. New York, NY: Academic Press.

Miller, George A. 2003. The cognitive revolution: A historical perspective. *Trends in Cognitive Sciences* 7(3), 141–144. doi:10.1016/S1364-6613(03)00029-9.

Neisser, Ulric, Daniel Tapper, & Eleanor J. Gibson. 1975. *Eric H. Lenneberg: September 19, 1921—May 31, 1975*. Office of the Dean of the University Faculty of Cornell University.

Nelson, Matthew J., Imen El Karoui, Kristof Giber, Xiaofang Yang, Laurent Cohen, . . . , & Stanislas Dehaene. 2017. Neurophysiological dynamics of phrase-structure building during sentence processing. *Proceedings of the National Academy of Sciences of the United States of America* 201701590. doi:10.1073/pnas.1701590114.

The New York Times. 1975, June 7. *Eric Lenneberg, Professor, dies*, 30.

Trettenbrein, Patrick C. 2016. The demise of the synapse as the locus of memory: A looming paradigm shift? *Frontiers in Systems Neuroscience* 10(88). doi:10.3389/fnsys.2016.00088.

Piattelli-Palmarini, Massimo. 1974. *A Debate on Bio-Linguistics: Conference Held at Endicott House, Dedham, Massachusetts, 20-21 May, 1974*. Centre Royaumont pour une science de l'homme report.

Zaccarella, Emiliano & Angela D. Friederici. 2016. The neurobiological nature of syntactic hierarchies. *Neuroscience & Biobehavioral Reviews* 81, 205–212. doi:10.1016/j.neubiorev.2016.07.038.

Zilles, Karl, Maraike Bacha-Trams, Nicola Palomero-Gallagher, Katrin Amunts, & Angela D. Friederici. 2015. Common molecular basis of the sentence comprehension network revealed by neurotransmitter receptor fingerprints. *Cortex* 63, 79–89. doi:10.1016/j.cortex.2014.07.007.

Patrick C. Trettenbrein
University of Graz
Department of Linguistics
Merangasse 70/III
8010 Graz-St. Leonhard
Austria

patrick.trettenbrein@gmail.com

The Relationship between Phoneme Production and Perception in Speech-Impaired and Typically-Developing Children

Karin Stromswold & Aliza Lichtenstein

One of the central questions that Eric Lenneberg raised in his seminal book, *Biological Foundations of Language* is: What is the relationship between language comprehension and language production? This paper reviews Lenneberg's case study of a child with congenital anarthria and then presents the results of two studies that investigate the relationship between phoneme perception and production. The first study investigates the phoneme identification skills of a child with developmental apraxia who, like the anarthric child studied by Lenneberg, had essentially no speech yet had no difficulty understanding speech. The second study investigates the extent to which 28 typically-developing children's ability to identify phonemes is related to their ability to produce phonemes. The results of both studies support Lenneberg's conclusion that children's ability to perceive speech is not dependent on their ability to produce speech. Thus, Lenneberg's original case study and the two studies presented in this paper argue against gestural theories of speech perception such as the Motor Theory.

Keywords: developmental apraxia; phoneme; production; perception; Motor Theory

1. Introduction

One of the hallmarks of much of Lenneberg's work and especially his seminal book *Biological Foundations of Language* is the importance he placed on the study of language acquisition by special populations and the insights that such populations can provide about the biological bases of language and language acquisition. A case in point is the production and perception of speech. It is well established that the articulatory gestures used to produce phonemes vary depending on the speaker, the situation in which speech occurs, and the phonological environment in which the phonemes appear. The result is that the acoustic realization of phonemes

---

We are grateful to all of the children who participated in the studies, and the parents, daycare providers and teachers who facilitated us in testing them. We thank Paul deLacy and Shigeto Kawahara for their advice on the design and implementation of the second experiment, and anonymous reviewers for their helpful comments and suggestions. These studies could not have been conducted without support from the Merck Foundation and the National Science Foundation (BCS-9875168; BCS-0446850).

ISSN 1450–3417

varies tremendously both within and across speakers. By studying children with Down Syndrome, deafness and speech impairments, Lenneberg sought answers to three related questions that continue to haunt developmental psycholinguists. The first is how children learn to produce the articulatory gestures needed to produce phonemes. The second is how children (and by extension adults) perceive speech despite the acoustic variability associated with phonemes. The third question is, what is the relationship between the development of speech production and speech comprehension.

At the time Lenneberg was writing, behaviorist theories of language development held sway, with many researchers positing that children learned to talk by listening to their own babble and successively modulated their speech to match that of the people around them. According to gestural theories of speech perception such as the Analysis by Synthesis theory (Halle & Stevens 1962) and the Motor Theory (e.g., Liberman & Mattingly 1985, 1989), speech perception is to a greater or lesser extent parasitic on speech production. Proponents of the Motor Theory, for example, argue that there is a set of invariant motor commands (gestural scores) that underlie phoneme production and perception, and identification of the gestural scores associated with phonemes form the basis of speech perception (e.g., Liberman & Mattingly 1985, Liberman & Mattingly 1989, Liberman & Whalen 2000, Galantucci, Fowler & Turvey 2006).

As Lenneberg (1967) succinctly put it:

> It is a fundamental assumption [of such theories] that responding is prior, in a sense, to understanding. However, there is a type of childhood abnormality that contradicts this assumption. These are children with inborn disability to coordinate their muscles of the vocal tract sufficiently to produce intelligible speech. The disturbance is seen in varying degree ranging from mild impediment to congenital anarthria.
>
> (Lenneberg 1967: 305)

Over the course of the 1960s, Lenneberg conducted an in-depth study of a child with congenital anarthria who had no intelligible speech, yet had no difficulty understanding language. The child was born at 38 weeks gestation and small for gestational age. In addition to his expressive language disorder, the child had dysmorphic features (bilateral club feet, a hair lip, bilateral simian palmar creases, strabismus), "soft" neurological signs (e.g., difficulty distinguishing left from right), and mildly depressed IQ (between 70–85), all suggestive of a syndromic disorder. As an infant, the child reportedly cried normally, but never babbled. Throughout childhood, his vocal productions were extremely limited with occasional grunts that accompanied the gestures he used to communicate, and vocalizations that sounded like "Swiss yodeling" when he played.

Despite having profoundly impaired speech, Lenneberg reported that the child had no difficulty understanding what was said to him in either normal social settings or in experimental contexts. Lenneberg argued that this child's intact comprehension of language argued against theories that posited that children "learn" to speak by listening to their own babble and modulating their speech. Lenneberg further argued that,

> since his own sounds are demonstrated to be objectively very different from those of the adults, the child must have some peculiar way of determining or recognizing similarities in the presence of diversifications.
>
> (Lenneberg 1962: 126)

Since Lenneberg's landmark case study, a number of studies have investigated the relationship between speech perception and production in children with speech disorders. A study of children with cerebral palsy who were anarthric or dysarthric (a less severe form of anarthria) revealed that such children were just as good as control subjects at detecting whether the name of a picture was spoken correctly or altered by a single phoneme, indicating that they had no difficulty discriminating between phonemic contrasts that they could not produce (Bishop, Brown & Robson 1990). Researchers have also investigated the speech perception of children with developmental verbal dyspraxia (which is also referred to as childhood apraxia of speech, congenital apraxia or simply dyspraxia). As is the case with anathria and dysarthria, dyspraxia affects all aspects of speech, with the most severely dyspraxic children having no intelligible speech. However, whereas dysarthria is a neuromotor disorder, dyspraxia is believed to be a motor-speech planning disorder, the hallmark of which is difficulty coordinating and executing the purposeful articulatory movements necessary for speech (see Hall, Jordan & Robin 1993).

Some studies suggest that dyspraxic children have intact phoneme perception despite their profound speech impairments. For instance, Hoit-Dalgaard et al. (1983) found no significant relationship between phoneme perception and production of voice onset time (VOT) in dyspraxic children. Groenen et al. (1996) conducted a study of Dutch-speaking dyspraxic children's ability to perceive and produce synthetically-produced minimal pairs of words that differed in place of articulation. In an identification task, dyspraxic children's identification function was equally as sharp as typically-developing children's, indicating that the phonetic processing of the two groups was equally consistent. However, in a discrimination task, the dyspraxic children had lower scores than the typically-developing children. Furthermore, the frequency with which the dyspraxic children made place of articulation errors was correlated with their scores on a place of articulation discrimination task (Groenen et al. 1996).

In contrast with Groenen at al.'s findings that dyspraxic children performed poorly on a phoneme discrimination task but not on a phoneme identification task, Sussman, Marquardt, Doyle & Knapp (2002) reported that all of the three dyspraxic children in their study performed aberrantly on a phoneme identification task. Marion, Sussman & Marquardt (1993) assessed four dyspraxic children's phonological awareness through a series of tasks that assessed the children's ability to produce and identify rhyming words. In striking contrast to age- and sex-matched controls who performed at or near ceiling on all tasks, the dyspraxic children were not only incapable of producing rhyming words, but they were at or near chance level on all the identification tasks including a simple rhyme recognition task. However, rather than attributing the dyspraxic children's poor performance on the perceptual tasks as being caused by their inability to produce rhymes as a motor theorist might, Marion et al. (1993) attributed the dyspraxic children's poor performance on both types of tasks as reflecting an underlying deficit in phoneme representation.

In summary, although the results of studies of the speech perception abilities of dyspraxic children are mixed, probably reflecting differences among studies in the criteria used to diagnose dypraxia and the tests used to assess speech perception (Hall et al. 1993), at least some dyspraxic children appear to perceive speech normally. Furthermore, it is possible that abstract phonological deficits underlie both the production and the perceptual impairments exhibited by some dyspraxic children (Marion et al. 1993), or that perceptual deficits are the cause of dyspraxic children's impaired speech production.

Other studies have investigated the speech perception abilities of children with more circumspect articulatory impairments that affect the ability to produce particular phonemes. Some of these studies have failed to find a relationship between the ability to produce phonemes and the ability to perceive them. For example, Rvachew & Grawberg (2006) found that preschool children' s articulatory accuracy was not related to their phonological awareness. In another study of preschool children with phonological impairments, Bird & Bishop (1992) found that all 14 children were able to discriminate between phonemic contrasts that that they could not produce, with 7 of the 14 children performing near perfectly on the discrimination task. Similarly, Thyer & Dodd (1996) found no differences in auditory processing in children with impaired speech. In a study comparing the categorical perception abilities of children who did and did not have speech sound disorders, Johnson et al. (2011) found no difference between the groups in the sharpness or location of categorical boundary for synthetic stop-vowel (da/ta) syllables that varied in voice onset time (da/ta), but they did find marginally significant group differences for synthetic fricative-vowel (su/ʃu) syllables that varied in frequency of the friction noise.

In contrast with the studies mentioned above, some studies have found a correlation between phoneme perception and production in children with speech sound disorders. For example, Marquart & Saxman (1972) found a significant correlation between how often children with speech sound disorders misarticulated words and how often they misperceived words. Rvachew et al. (2003) found that misarticulating children have poorer phonemic perception of both correctly articulated words (e.g., *lake*) and incorrectly articulated words (e.g., *lake* as *wake*). In categorical speech perception studies with synthetically produced /r/ and /w/ tokens, children who frequently mispronounced /r/ as /w/ (e.g., saying *rabbit* as *wabbit*) had less clear categorical boundaries for /r/ and /w/ than children who did not mispronounce /r/ (Monnin & Huntington 1974, Hoffman et al. 1985, Ohde & Sharfe 1988). Less sharp categorical boundaries during perception tasks have also been found for other contrasts, such as /s-ts/ contrast in coda position (Raaymakers & Crul 1988) and fricatives (Rvachew & Jamieson 1989), a finding that Rvachew & Jamieson attributed to some misarticulating children having an underlying deficit in speech perception.

Given that impairments in speech perception are likely to result in impairments in speech production (e.g., as is evident in the impaired speech of most children with substantial hearing impairments), the mere correlation of speech production and speech perception abilities does not provide evidence *for* gestural theories of speech perception that posit that speech perception is parasitic on speech production. On the other hand, if even some children with impaired speech production nonetheless have normal speech perception abilities, this argues *against* the primacy of speech production. We sought to further elucidate the relationship between speech perception and speech production in two studies.

2. Study 1—Case Study of Phoneme Identification in a Profoundly Dyspraxic Child

The first study investigated the speech perception abilities of a profoundly dyspraxic child who – like Lenneberg's anarthric child – had no intelligible speech, yet appeared to understand everything that was said to him. In a phoneme identification task, we found that despite being unable to speak, the child had no difficulty understanding and discriminating among words that differed in phonemically minimal ways (e.g., *wake*, *lake* and *rake*), even when these words were said out of context.

2.1. Case History

2.1.1. Medical History

Review of the child's medical records revealed that his prenatal course was unremarkable except for a mild case of polyhydramnios (a condition sometimes seen with oral motor problems) and a cesarean section delivery for breech presentation at 41.5 weeks gestation. Notably, he had no history of seizures, head injury, anoxic insult, or otitis media. All developmental milestones were reportedly achieved at the normal age, with the exception of an expressive language disorder first noted by his parents at 12 months of age and his pediatrician at 18 months of age. In contrast to the child Lenneberg studied, the child had no other delays or abnormal findings aside from his expressive language disorder. Specifically, he had no dysmorphic features, exhibited none of the "soft" neurological signs frequently observed in children with mild developmental disabilities, had no sign of cranial nerve damage, and had no difficulty producing simple rapid voluntary movements of the mouth or hands. He also had no history of excessive drooling or the sorts of feeding problems often associated with oral motor problems. Brainstem auditory evoked response potentials and audiometric examination revealed normal hearing bilaterally. Electroencephalography (EEG) and computed tomography (CT) scans (performed without contrast agent) were also reportedly normal.

2.1.2. Psychological Testing

At age 2;4 (years;months), the child's performance on the Bayley Scales of Infant Development (Bayley 1969) was reportedly age-appropriate for all areas except for delays noted in language and fine motor skills. At 2;8, his performance on the Stanford-Binet Scale IV (Terman & Merrill 1960) and the Merrill-Palmer Psychomotor Scale (Stutsman 1981) were age appropriate and his performance on concrete problem solving were at the late 4-year-old level, suggesting average or above average intelligence and normal fine motor skills. The clinical psychologist who evaluated him at that time described him as a "pleasant, well-organized and independent little boy."

2.1.3. Language History

The child's mother reported that his speech and expressive language development was markedly different from that of her five older children. He never babbled or cooed, but began to use points and gestures to communicate at or before a year of age. Despite having no expressive speech, his parents, therapists and doctors reported that he had no difficulty understanding what was said to him.

The child's language was formally evaluated for the first time when he was 2;4. According to the speech pathologist's report, he made no linguistic sounds, and communicated through points and gestures with an occasional grunt and high pitch squeal. His receptive language was at the early 2-year level and his expressive language was at the 6- to 12-month level on the Reynell Developmental Language Scales (RDLS, Reynell & Huntley 1971). Based on his history, vocalizations (or lack thereof) and RDLS scores, he was given the clinical diagnosis of developmental verbal dyspraxia.

The RLDS was repeated when the child was 2;8 at which time he scored at the 2;3 level on receptive section and at the 12-month level on the expressive section. At 2;8, AS phonological development was formally evaluated. According to the speech pathologist's report, his speech was grossly impaired at both the segmental and suprasegmental level: his vocal repertoire consisted of three sounds that were "consonant-like" (most closely resembling [d], [r] and [m]) and 2 or 3 sounds that were "vowel-like" ([u], [o] and possibly [i]), and these sounds were only used as isolated vowels and in simple consonant-vowel combinations. During the course of the evaluation, he produced only a handful of linguistic or nonlinguistic vocalizations, and he did not produce any vocalizations more complex than a single syllable, nor did he produce any intelligible words.

2.2. Phoneme Identification Task

2.2.1. Stimuli

At age 3;5, the child's ability to identify phonemes was assessed by having him point to pictures that depicted words that differed from one another in phonemically minimal ways (e.g., *van* and *fan*; *coat* and *goat*; *deer* and *tear*). Forty-four words were chosen because they were easy to depict, frequent, and were phonologically minimally distinct from other words on the list (see Appendix A). Of the 44 words, 9 had one phonological foil (e.g., *van* only had the foil *fan*), 15 had two phonological foils (e.g., *wake* had *lake* and *rake*), 9 had three phonological foils (e.g., *door* had *four, sore* and *shore),* 8 had four phonological foils (e.g., *wrap* had *cap, lap, map,* and *rat),* two words had 5 phonological foils (e.g., *mat* had *bat*, *cat*, *hat*, *map* and *rat*), and one word had 6 phonological foils (*cat* had *bat*, *cap*, *coat*, *hat*, *mat*, and *rat*). All of the words had at least one phonological foil that differed only in onset position (e.g., *fan* and *van*), 5 had at least one phonological foil that differed only in the vowel (e.g., *coat* and *cat*), and 8 had at least one phonological foil that differed only in coda position (e.g., *map* and *mat*). Some words differed from their phonological foils by only a single articulatory feature. For example, *goat* and *coat* differed only in voicing, *feet*, *seat* and *sheet* differed only in place of articulation, and *sea* and *tea* differed only in manner of articulation, whereas other words differed from one another in more than one phonetic feature (e.g., *cat*, *hat*, *rat*, *mat*), or by the addition of a phoneme (e.g., *sea* and *seat*, *sore* and *store, ear* and *tear*).

2.2.2. Procedure

Forty-four colored pictures were placed in random order in front of the child. As each picture was laid out, the experimenter said the word depicted by the picture. Once all of the pictures were displayed, the child was told. "See these cards. We're going to play a game – I'm going to say a word and I want you to look very carefully and find the picture that matches what I say." The words were then read in random order. Words were said live, and if the child did not respond, the word was repeated up to two times. Each trial took approximately 1 minute, and the entire task took approximately 1 hour to complete. During the task, the child gestured, but made no attempt to say any of the words.

2.2.3. Results

For 42 of the 44 trials (93%), the child correctly chose the picture that matched the word. Even if we assume that, for each trial, the child selected randomly from the target word and a single phonological foil word (i.e., p = .5 for each trial), it is extremely unlikely that the child did this well by chance alone (cumulative binomial $p < .000001$).[1] Successful performance on a phoneme identification task requires not just the ability to perceive relatively subtle phonemic differences, but also knowledge of the meanings of words being tested and the ability to interpret the pictures correctly. Consider the child's two mistakes: for *hall* he chose the "door" picture (of a partially opened door) and for *sore* he pointed to the "tear" picture (of an eye with a tear). The semantic similarity – and the lack of phonological similarity – between the target words and the words he chose suggest that these errors reflect limitations in his picture identification skills or vocabulary, rather than his phoneme perception skills.

3. Study 2—Phoneme Identification and Production in Typically-Developing Children

Lenneberg's original study of an anarthric child and the case study of a dyspraxic child presented in the first study demonstrate that normal phoneme comprehension is possible even when phoneme production is profoundly impaired. In a second study, we investigated the relationship between phoneme comprehension and phoneme production in preschool-aged children who were typically developing. In a phoneme identification task, the children chose the picture that matched target words from among four pictures, and in a phoneme production task, the same group of children said the target words used in the phoneme identification task.

---

[1] Arguably, a more realistic hypothesis is that, the child selected randomly from among the target word and the foil words that differed for the target word by a single phoneme. Because each word had on average 2.6 phonologically minimal foils, on average, the probability of guessing correctly on a trial is .278 (1/3.6). Thus, selecting the correct picture for even 19 target words by chance alone is unlikely (binomial cumulative $p < .05$).

3.1. *Methods*

3.1.1. Participants

Twenty-eight (16 males and 12 females) monolingual, English-speaking children (mean age = 4.15; range 3.0–5.25) participated. All children were typically developing, with no history of speech, hearing, language or other impairment that might influence language development or interfere with their ability to perform the tasks. In addition, all of the children performed at age-appropriate levels on the Denver Articulation Screening Examination (DASE; Drumwright 1971, Drumwright et al. 1973).

3.1.2. Stimuli

There were 45 target words in the phoneme identification task (see Appendix B). Each target word was grouped with three distractor words that differed minimally from the target word to form a phonological minimal quartet. Quartets were designed to assess consonants in both onset and coda position because the acoustic features that distinguish between phonemes often differ depending on whether the consonants are onsets or codas (e.g., voice onset time affects perception of voicing for oral stops in onset position, whereas the duration of the preceding vowel affects perception of voicing for oral stops in coda position), and because children sometimes mispronounce the same phoneme differently in onset and coda position. For example, children tend to voice unvoiced consonants in onset position (e.g., mispronouncing *park* as *bark*) and de-voice consonants in coda position (e.g. mispronouncing *pig* as *pick*). Quartets were also designed to assess consonants in both consonant clusters and non-consonant clusters because the acoustics and articulation of consonants differ in clusters and non-clusters.

Of the 45 quartets, 30 assessed phoneme perception in onset position (e.g., target *rip* and distractors *lip, whip, zip*) and 15 assessed phoneme perception in coda position (e.g., target *pig* and distractors *pick*, *pin*, *pit*). In 30 quartets, the target word's onset or coda was a consonant cluster (target *snail* and distractors *sail, nail,* and *mail*) and in 15 quartets the target word was a non-cluster (e.g., target *buzz* and distractors *bug, bus, bud).* All target and distractor words were depictable, high frequency, monosyllabic words that are acquired at a young age.

Taken as a group, the 45 target words and their distractors assessed children's perception and production of consonants that differed in voicing, manner of articulation and place of articulation. We selected and grouped target and distractor words to maximize our ability to assess children's phoneme production and perception for common childhood speech errors (Sanders, 1972; Grunwell 1987) such as consonant cluster reduction (saying *snail* as *sail*), fronting errors in which the place of articulaton of a phoneme is substituted (e.g., saying *crash* as *trash*), voicing errors (e.g., saying *park* as *bark*), stopping errors in which a fricative is said as an oral stop (saying *toe* for *sew),* gliding errors in which liquids are produced or perceived as glides (e.g., saying *wake* for *rake*) and /r/–/l/ substitutions (e.g., saying *rip* for *lip* or vice versa).

Phonological quartets were designed so that a single quartet targeted multiple aspects of phoneme perception and production. Consider the phonological quartet *tree, tea, dee* (the letter "D"), and *three*. *Tree* and *tea* differ in that *tree* has a complex onset, *tea* and *dee* differ in that *tea* has an unvoiced onset, and *tree* and *three* differ in that *tree* begins with a stop consonant rather than a fricative. Thus, this one quartet potentially provides information about the production and perception of consonant clusters, voicing, and stopping. Because we used phonological quartets rather than phonological pairs in the phoneme identification task, there are 3 target-distractor word pairs and 2 distractor-distractor pairs for each trial, for a total of 225 word pairs (45 items × 5 word pairs per item). Sixty-five word pairs differed in a way that targeted consonant cluster reduction, 28 word pairs targeted fronting errors, 25 word pairs targeted voicing errors, 16 word pairs targeted stopping errors,7 word pairs targeted gliding, and 7 word pairs targeted /r/–/l/ substitution (see Appendix C).

3.1.3. Recordings

A native monolingual English-speaking woman who was naïve to the nature of the experiment and received no guidance regarding the pronunciation of the words said the instructions and target words used in the phoneme identification and perception tasks and in the DASE. Two experimental phonologists deemed that she had a typical New Jersey accent with no evidence of articulatory problems, and that she spoke clearly, but did not hyper-articulate. Words were recorded in a sound attenuated booth using a head-mounted *Shure Microphone* attached to a *Roland Edirol R09 Solid State Recorder* that recorded stimuli in 16 bit, 44.1kHz.wav format.

To avoid list intonation, each target word was inserted in the carrier sentence *say the word [___], twice*. The carrier sentence ended with the word *twice* in order to avoid phrase-final lengthening or creakiness in the target word. Target words were extracted from the carrier sentences using Praat (version 5.0.4, Boersma & Weenink, 2008). Each target word was recorded 9 times, and the best example of each target word was chosen using the criteria of naturalness, clarity, least background noise, and least aspiration. The amplitude of each word was then adjusted to a mean of 70 dB. Five monolingual English speakers with no background in linguistics and no knowledge of the experiment judged the target words to be natural sounding, clear, and similar to one another.

3.1.4. Experimental Apparatus

Visual stimuli were presented on a 15" Macbook Pro computer screen and audio stimuli were presented via *Sennheiser HD 202* headphones. Psyscope was used to control presentation of stimuli and record data, with the experimenter initiating trials and marking selections via an external keypad attached to the laptop. Children's eye movements were recorded with the laptop's built-in camera and the entire session was video-recorded.

3.1.5. Phoneme Identification Task

In a phoneme identification task, children listened to target words and selected the picture that matched each target word from among four pictures that depicted the target word and its phonological foils. For example, in one experimental trial, children heard the target word *snail* and viewed pictures of a snail, a nail, a sail and mail (see figure 1). Prior to doing the experimental trials, children were given practice trials with phonologically distinct words (e.g., *star*, *bird*, *fork*, and *cheese*). If a child got a practice trial wrong or pointed to more than one picture, s/he was corrected. For the experimental trials, no feedback was given and, if a child pointed to more than one picture, his or her first selection was counted.

Figure 1: Sample Trial in Phoneme Identification Task. (Written words are presented for explanatory purposes only and did not appear in the task presentation.)

All trials began with a cartoon character appearing at the center of the screen for 2500 msec. After the cartoon fixation target disappeared, the screen went blank and the experimenter asked the child if s/he was ready. When the child was focused on the screen, the experimenter initiated a trial and the target word was played simultaneously with the four pictures. Stimuli were presented in pseudo-random order. The target picture did not appear in same quadrant more than 2 times in a row, the target segment was in onset position no more than 4 times in a row, and the target segment was a consonant cluster no more than 4 times in a row. In addition, onset and coda target segments and consonant cluster and non-cluster target segments occurred equally often in the first and second half of the list. Half of the participants received the items in the original order, and half received the items in the reverse order.

3.1.6. Standardized Articulation Assessment

After the children finished the phoneme identification task, we assessed their articulatory abilities using the DASE (Drumwright 1971; Drumwright et al. 1973), a test in which children repeat 22 words that contain 30 target phonemes. We used the same equipment and procedures for the DASE as we did for the phoneme production task.

3.1.7. Phoneme Production Task

Once children had completed the DASE and taken a short break, they repeated the 45 target words from the phoneme identification task. For each of the 45 target words, children viewed a picture of the target word while listening to the instruction "say the word [___]." (In order to ensure that the target words were acoustically identical in the identification and production tasks, the production task audio instructions were extracted from the same recordings that were used in the phoneme identification task.) Using pictures in the production task helped ensure that children's errors were true mispronunciations and not the result of misunderstanding the target word (e.g., mistakenly saying *wake* rather than *lake* because they misperceived the target word as *wake*), and using a repetition task rather than a picture-labeling task ensured that children said the same words that were used in the identification task (e.g., they didn't call a lake a pond).

For each trial, the audio instruction began at the same time that the matching picture appeared in the center of the computer screen. The picture remained on the screen until the child said the word. When the child finished saying the word, the experimenter pressed a key and the screen went blank. When the child indicated s/he was ready for the next trial, the experimenter pressed a key and the next trial began. Items were presented in the same order in the phoneme production task as they were in the phoneme identification task, and the apparatus that was used in the phoneme production task was the same as in the phoneme identification task except that, in addition to wearing headphones, children wore a head-mounted microphone that was attached to an Edirol recorder.

3.2. *Results*

3.2.1. Phoneme Identification

Because each target word had three phonological distractors, chance performance rate is 25%. Overall, children correctly identified the target word 63% of the time, and all children performed at significantly better than chance level. As expected, children's performance was significantly correlated with their age (r = .52, p = .005). Accuracy and reaction times were analyzed using ANOVAs with subject as a random variable. There was no significant main effect of sex on either accuracy or reaction time (RT) regardless of whether incorrect trials were included or excluded (both F's < 1) and, thus, sex was excluded from all subsequent analyses.

Because there were no liquids in coda position, in this and subsequent onset/coda analyses, items in which the target segment contained a liquid were eliminated. When these items were excluded, children correctly identified significantly more onset target words than coda target words (73% and 54% respectively) by both non-parametric tests (χ^2 = 34.57, $p < .00005$) and parametric tests with subject as a random variable ($F(1, 27)$ = 28.96, $p < .0005$, η_p^2 = .518). Children were also significantly more accurate for target words without consonant clusters than target words with consonant clusters (67% and 61%, respectively) by both non-parametric tests (χ^2 = 5.52, p = .019) and parametric tests with subject as a random variable ($F(1, 27)$ = 8.355, p = 0.008, η_p^2 = .236).

Consistent with onset/coda accuracy results, when all trials were included, children were faster on onset target words than coda target words (2680 ms and 2991 ms respectively, $F(1, 27) = 8.52$, $p = 0.007$, $\eta_p^2 = .240$), but this difference was not significant when incorrect trials were eliminated (2489 and 2634 respectively, $F(1, 27) = 2.795$, $p = 0.11$). Also consistent with accuracy results, children were faster for target words that lacked consonant clusters compared to those that had consonant clusters, regardless of whether incorrect trials were included (2655 ms and 2985 ms respectively, $F(1, 27) = 25.89$, $p < .0005$, $\eta_p^2 = .490$) or excluded (2409 ms and 2766 ms respectively, $F(1, 27) = 18.64$, $p = .003$, $\eta_p^2 = .408$) from the analyses.

3.2.2. Phoneme Production

For the production task, if children said the target segment correctly, the item was scored correct even if they mispronounced other parts of the target word (e.g., if a child said *late* for *lake* and the onset was the target segment, the child received credit for having said the item correctly). Overall, children said the target segment correctly 85% of the time. As was the case for the phoneme identification task, children's accuracy rates on the phoneme production task were significantly correlated with their age ($r = .47$, $p = .01$). There was no significant difference in accuracy rate for boys and girls (83% and 88% correct respectively, $F < 1$), and sex was eliminated from all subsequent analyses. When trials with target words containing a liquid were excluded, children were not significantly worse at producing onset versus coda targets (87% accuracy for both) by either non-parametric or parametric tests ($\chi^2 = 0.43$; $F(1, 27) < 1$). Children were, however, significantly less accurate at producing consonant cluster targets than non-cluster targets (82% and 91%, respectively) by both non-parametric tests ($\chi^2 = 15.15$, $p < .00005$) and parametric tests ($F(1, 27) = 7.35$, $p = .01$, $\eta_p^2 = .214$).

3.2.3. The Relationship between Phoneme Identification and Phoneme Production

A multiple regression analysis of children's phoneme identification accuracy rates was conducted with age and phoneme production accuracy as predictors. This analysis revealed that children's age was a significant predictor of identification accuracy ($\beta = .428$, $t(25) = 2.25$, $p = .033$), but their phoneme production accuracy rate was not ($\beta = .185$, $t(25) = 0.97$, $p = .340$), with the overall fit of the model being fairly good ($R^2 = .292$). We next analyzed each child's data separately to determine whether individual children tended to misidentify and mispronounce the same target items. These analyses revealed Spearman's r of between –.16 and .31 (mean $r = .04$), with only one child's correlation coefficient being significant at the $p = .05$ level ($r = .31$, $p = .038$).

The above analyses simply address the question of whether children misidentify and mispronounce the same target items. A more precise question is whether children misperceived items in the same *way* that they mispronounce them. Collapsing across children, of the 1,260 trials (28 participants × 45 target words), there were only 87 cases in which a child misidentifying and mispronounced the same target word. Of these 87 instances, in 78 cases children misidentified and mispronounced a target word in different ways (e.g., misidentifying the target word *flight* as being *light* and mispronouncing *flight* as *fight*). There were only 9 cases in which a child misidentified and mispronounced a target word in the same way (e.g., misidentifying and mispronouncing the target word *robe* as *rope*).

Even though the children very rarely mispronounced and misidentified target words in the same way, it is possible that the same types of phonological processes underlie both their misidentification and mispronunciation errors. For example, a child who reduces fricative consonant clusters might misidentify *skis* as *keys* and mispronounce *spark* as *park*, yet correctly pronounce *skis* and correctly identify *spark*. To investigate whether the same phonological processes underlie children's misidentification and mispronunciation errors, we divided target segment phonemes into three groups based on their manner of articulation (oral stops, approximates and fricatives).[2]

For every phoneme in target onsets and codas that children either misidentified or mispronounced, we compared the place of articulation (POA), manner of articulation (MOA) and voicing of that target phoneme with the child's erroneous phoneme. In addition to tallying the children's POA, MOA and voicing errors, we also tallied their phoneme deletions and additions. Additions were very rare, with most cases of epenthesis involving the insertion of a vowel within a consonant cluster.[3] In most cases, target phonemes and erroneous phonemes differed by a single feature (e.g., POA /s/ ⇒ /f/, MOA /s/ ⇒ /t/, voicing /s/ ⇒ /z/), but occasionally target and erroneous phonemes differed by more than one feature (e.g., /s/ and /d/ differ in both MOA and voicing). In all cases we assumed that children's errors differed minimally from the target segment. For consonant cluster targets, we considered each consonant separately. So, for example, if a child said /tr/ as /w/, we assumed that the child deleted the stop /t/ and said the approximate with the wrong POA. Table 2 provides an example of how errors were coded.

Using this coding procedure, we tallied the types of errors each child made on approximates, fricatives and stops in the identification and production tasks (see Appendix D). Inspection of these tallies suggests that the pattern of errors differed from child to child and that individual children had different patterns of errors on identification and pronunciation tasks. To test statistically whether children misidentified and mispronounced the same types of phonemes, for each child we determined the type of phoneme (approximate, fricative or stop) the child got wrong most often on the identification task and on the production task. Of the 28 children, two children were eliminated from the analysis because

Stop Errors	**Approximate Errors**
POA: tr ⇒ pr	POA: tr ⇒ tw, tl
MOA: tr ⇒ sr	MOA: tr ⇒ ts
Voicing error: tr ⇒ dr	Deletion: tr ⇒ t
POA + voicing: tr ⇒ br	
POA + MOA: tr ⇒ fr	
MOA + voicing: tr ⇒ zr	
Deletion: tr ⇒ r	
Addition: tr ⇒ tər	

Table 1: Sample error classification for the target cluster /tr/.

---

[2] For the purposes of the error analyses, affricates were treated as being composed of a stop followed by a fricative (tʃ = t+ʃ, dʒ = d+ʒ). Although there were 3 target segments that contained nasals (*snail*, *smell*, *crunch*), because children only made a handful of mistakes involving nasals, we chose to exclude them from our error analyses.

[3] For the purposes of the analyses, epenthetic phonemes were classified with the phoneme that preceded them.

they had no production errors and 6 children were eliminated because, for one or both of the tasks, two phoneme types were tied for most common. Of the remaining 20 children, for 6 children the phoneme type that was most frequently mispronounced was also the phoneme type that was most often misidentified. Given that there were 3 types of phonemes, the probability that a child would make the most errors on the same phoneme type in both tasks is .33 by chance alone. Thus, we cannot reject the hypothesis that it was a chance occurrence that, for 6 of 20 children, the same phoneme type was the most commonly misidentified and mispronounced (cumulative binomial $p = .69$).

We next investigated whether children made the same types of errors on the identification and production tasks. The vast majority of children's errors were POA, MOA, voicing or deletion errors, so, for each child, we determined which of these 4 error processes was the most common for each of the two tasks. Two children were eliminated from the analysis because they had no production errors and 4 children were eliminated because for one or both of the tasks, two error processes were tied for most common. Of the remaining 22 children, for 8 children the most common error process on the identification task was the same as the most common error process on the production task. Given that there were 4 possible error processes, the probability by chance alone that the same error process would be the most common on both tasks is 0.25 (1 in 4) for each child. Thus, we cannot reject the hypothesis that it was a chance occurrence that the most common error process in the identification task was the same as the most common error process in the production task for 8 of 22 children (cumulative binomial $p = .16$).

4. Discussion

In 1962, Lenneberg wrote:

> Our understanding of human behavior is often greatly enlightened by careful investigations of clinical aberrations and in many instances disease or congenital abnormalities provide conditions that may replace the crucial experiments on children that our superego forbids us to plan and perform. (Lenneberg 1962: 419)

Five years later, in *Biological Foundations of Language*, Lenneberg (1967) went one step further, arguing not just that special populations can provide important insights about language and language acquisition, but that

> to ignore or overlook [such cases] is inexcusable as it may result in theories that are flatly contradicted by pertinent facts in pathology.
>
> (Lenneberg 1967: 304)

The intact language comprehension abilities of Lenneberg's anarthric child and the dyspraxic child reported in this paper underscore the importance of studying how special populations use and acquire language. As Lenneberg so elegantly wrote:

> The theoretical importance of extreme dissociation between perceptive and productive ability lies in the demonstration that the particular ability which we may properly call "having knowledge of a language" is

> not identical with speaking. Since knowledge of a language may be established in the absence of speaking skills, the former must be prior, and, in a sense, simpler than the latter. (Lenneberg 1967: 308)

Contrary to Lenneberg's position, proponents of gestural theories of speech perception such as the Motor Theory argue that speech production is primary, that

> the phonetic elements of speech, the true primitives that underlie linguistic communication, are not sounds but rather the articulatory gestures that generate those sounds. (Liberman & Whalen 2000: 188)

The discovery of mirror neurons in nonhuman primates has led to a resurgence of interest in gestural theories of speech perception such as the Motor Theory. Indeed, the results of some neuroimaging studies that show activation of motor areas during speech perception tasks appear to provide support for the Motor Theory. (For a critical review of such studies, see Hickok 2010). However, the fact that adults are able to perceive speech normally despite temporarily-induced impairments of speech production (e.g., Hickok et al. 2008) or acquired neurological insults that permanently impair speech production (e.g., Hickok et al. 2011) puts into question the Motor Theory's claim that articulatory gestures form the basis of adults speech perception (for a review see Stasenko, Garcea & Mahon 2013).[4]

It is logically possible that articulatory gestures/motor areas play a critical role in the development of speech perception, even if they no longer play such a role in speech perception in adults. Lenneberg (1964) argued that the linguistic abilities of children who are profoundly deaf, have Down Syndrome or have anarthria show that

> motor skills are neither necessary nor sufficient prerequisites for the development of those psychological skills which seem to be an essential substrate for mature language. (Lenneberg 1964: 127)

Indeed Lenneberg (1962: 423) argued that, for children, "the vocal production of language is dependent upon the understanding of language but not vice versa." Furthermore, the fact that developmental impairments in speech perception almost always result in impairments in speech production means that studies that show children with speech impairments often have impaired speech production are not evidence for gestural theories of speech perception. In contrast, the existence of even a handful of children like Lenneberg's anarthric child and the dyspraxic child presented in the first study who have grossly impaired speech yet intact speech perception argues strongly against gestural theories of speech perception. Consistent with the primacy of speech perception abilities, Rvachew, Nowak & Cloutier (2004) found that training children with phonological expressive delays to attend to phonemic contrasts (by providing them feedback on a task very much like the experimental task used in the second experiment) improved the children's ability to produce these contrasts.

---

[4] One might argue, however, that some weaker version of the Motor Theory could accommodate these findings. Perhaps, for example, the gestural scores could be encoded in an abstract manner that does not involve the motor regions per se. However, as noted by Hickok and others, this leaves open the possibility that the underlying encoding is fundamentally perceptual rather than motoric.

According to Liberman & Whalen (2000),

> co-articulation creates a complex relationship between the acoustic signal and the phonetic structure [. . .]. Unraveling that complex relationship between signal and message is the business of the same phonetic module that produced it, for that module incorporates the constraints necessary to process the signal so as to recover the very gestures that were, by their co-articulation, responsible for its apparent complications.
>
> (Liberman & Whalen 2000: 189)

If this were true, not only would we fail to find cases like Lenneberg's anarthric child and the dyspraxic child presented in the first study, we would predict a causal link between speech production and speech perception in typically-developing children. Contrary to this prediction, in the second study, we failed to find any evidence of a relationship between typically-developing children's ability to identify and produce phonemes. A multiple regression analysis revealed that children's accuracy on the phoneme production task was not a significant predictor of their accuracy on the phoneme identification task independent of their age, and analyses of individual children's performance on the items in the identification and production tasks revealed a significant correlation for only one of the 28 children.

Consistent with the results of these regression analyses, error analyses revealed that the children in the second study rarely got the same items wrong on the identification and pronunciation tasks and, when they did, they almost always did so in different ways (e.g., misidentifying the target word *lake* as *rake* and pronouncing it as *wake*). Furthermore, when we analyzed each children's patterns of errors in the two tasks, we found they diverged considerably. First, we found no evidence that children typically made mistakes on the same class of phonemes (approximates, fricatives and stops) in the identification task and the production task. Second, we found no evidence that children made the same types of errors (e.g., voicing errors, POA errors, MOA errors, deletion errors etc.) in the two tasks.

Taken as a whole, many of the developmental studies reviewed in this paper and the results of the two studies presented provide strong evidence against the claim that speech production serves as the developmental backbone of speech perception and against gestural theories of speech perception more generally.

Appendix A: Study 1—Target Words and Phonological Foils

Target Word	Onset substitution	Vowel substitution	Coda substitution	Phoneme addition or deletion
ball	doll, hall, wall			
bat	cat, hat, mat, rat	boat		
boat	coat, goat			
cap	lap, map, wrap		cat	
cat	bat, hat, mat, rat	coat	cap	
cheese	knees			
coat	boat, goat	cat		
deer	tear			ear
dish	fish			
dog	log		doll	
doll	ball, hall, wall		dog	
door	four, shore, sore			
ear				deer, tear
fan	van			
feet	seat, sheet			
fish	dish			
floor				four
four	door, shore, sore			floor
goat	boat, coat			
hall	ball, doll, wall			
hat	bat, cat, mat, rat			
lake	rake, wake			
lap	cap, map, wrap			
log	dog			
mat	bat, cat, hat, rat		map	
map	cap, lap, wrap		mat	
knees	cheese	nose		
nose	toes	knees		
rake	lake, wake			
rat	bat, cat, hat, mat		wrap	
sea	tea			seat
seat	feet, sheet			sea
sheet	feet, seat			
shore	door, four, sore			
sore	door, four, shore			store
store				sore
tea	sea			tree
tear	dear			ear, tea
toes	nose			
tree	sea			tea
van	fan			
wake	lake, rake			
wall	ball, doll, hall			
wrap	cap, lap, map		rat	

Appendix B: Study 2—Phoneme Identification Stimuli Quartets

The target segment is in bold, followed by the three distractor words.

ONSETS	CODAS
SIMPLE C (N = 7)	**SIMPLE C (N = 8)**
Fricative	*Fricative*
s-eat sheet street eat	ri-**ce** write ride rise
s-ing string sting wing	bu-**zz** bug bus bud
f-at flat rat sat	*Stop*
Stop	pi-**g** pick pin pit
t-oe sew row throw	co-**ke** cone coat comb
g-as grass class glass	ba-**d** bat bag badge
Approximate	k-**id** kick kit king
l-ake rake wake ache	wro-**te** road rope robe
r-ip lip whip zip	ro-**be** wrote road rope
CC CLUSTER (N = 18)	**CC CLUSTER (N = 6)**
Fricative-Stop	*Fricative-Stop*
sp-ark bark park shark	bea-**st** bees beat beach
sk-is keys seas squeeze	cru-**st** crushed crunch crutch
Fricative-Nasal	fi-**st** fish fin fizz
sn-ail sail mail nail	*Nasal-Stop*
sm-ell shell sell bell	pa-**nt** pats pans pants
Fricative-Approximate	*Affricate (Stop-Fricative)*
sl-eep leap weep sweep	ca-**tch** cash cat cap
fr-y lie tie fly	ba-**dge** bad bat bag
fl-ight light white write	
Stop-Approximate	
bl-ed red bread bed	
gr-ab crab cab stab	
br-eak bake rake wake	
cr-ash trash cash rash	
tr-ail tail rail whale	
tr-ee tea dee three	
cl-ock block lock rock	
Affricate (Stop-Fricative)	
ch-ase face lace vase	
ch-ew shoe zoo two	
ch-ick sick tick trick	
ch-ip sip tip ship	
CCC CLUSTER (N = 5)	**CCC CLUSTER (N = 1)**
Fricative-Stop-Approximate	*Nasal-Stop-Fricative*
scr-eam cream steam stream	cru-**nch** crust crutch crush
str-ip trip rip tip	
spr-ing swing sing wing	

Appendix C: Study 2—Common Phonological Error Processes Assessed in Phoneme Identification Target-Distractor Word Pairs

Error Type	Onset Distractors	Coda Distractors	Distractors
Fronting	17 (e.g., **c**rash > **t**rash)	11 (e.g., co**k**e > coa**t**)	28
Voicing	12 (e.g., **g**rab > **cr**ab)	13 (e.g., ri**ce** > ri**se**)	25
Stopping	5 (e.g., **f**ry > **t**ie)	11 (e.g., ri**ce** > wri**te**)	16
Gliding	7 (e.g., **l**ake > **w**ake)	0	7
/r/–/l/ substitution	7 (e.g., rip > **l**ip)	0	7
Simple C Deletion	2 (e.g., **s**eat > eat)	0	2
Cluster Reduction	51 (e.g., **sn**ail > **n**ail)	14 (e.g., bea**st** > bea**t**)	65

Appendix D: Study 2—Classification of Identification and Pronunciation Errors

Participants are listed in order according to the combined number of errors they made in the two tasks, with the child who made the most errors given first.

Age	# Shared Errors (# identical)	Identification Errors[1]	Pronunciation Errors
3.33	12 shared errors (2 identical)	8 stops (4 delete, 2 voice, 1 MOA, 1 POA+voice) 8 fricative (5 delete, 1 POA, 1 voice, 1 POA+MOA) 5 approximates (4 POA, 1 POA+MOA)	23 fricatives (19 delete, 3 MOA, 1 POA) 8 approximates (8 POA) 8 stops (7 delete, 1 voice)
3.75	10 shared errors (1 identical)	13 stops (6 delete, 3 voice, 2 POA, 1 POA+voice, 1 other) 11 fricatives (5 delete, 3 POA, 1 POA+voice, 1 POA+MOA, 1 POA+MOA+voice 3 approximates (2 POA, 1 delete)	17 approximates (15 delete, 2 POA) 7 fricatives (3 POA+voice, 2 POA, 1 delete, 1 POA+MOA) 4 stops (3 delete, 1 MOA,
3.00	14 shared errors (1 identical)	10 stops (5 delete, 2 MOA, 2 voice, 1 POA+voice) 10 fricatives (4 POA, 4 delete, 2 voice) 6 approximates (5 delete, 1 POA)	9 approximates (8 POA, 1 delete) 8 stops (3 POA, 2 add, 2 voice, 1 delete) 7 fricatives (5 add, 2 delete)
3.33	6 shared errors (0 identical)	10 stops (4 delete, 2 POA+voice, 1 voice, 1 MOA, 1 POA+MOA) 8 fricatives (6 delete, 1 POA, 1 POA+MOA) 5 approximates (3 delete, 1 POA, 1 POA+MOA)	6 approximates (6 POA) 4 fricatives (3 POA, 1 add) 4 stops (2 voice, 1 add, 1 delete)
4.75	4 shared errors (0 identical)	13 stops (6 delete, 2 voice, 2 POA, 1 MOA, 1 POA+voice, 1 POA+MOA) 9 fricatives (6 delete, 2 POA+MOA, 1 POA) 7 approximates (5 POA, 2 delete)	4 fricatives (4 POA) 3 stops (1 delete, 1 MOA, 1 add)
3.42	6 shared errors (0 identical)	12 stops (6 delete, 3 voice, 2 POA, 1 add) 10 fricatives (7 delete, 2 POA, 1 POA+MOA, 1 add) 8 approximates (4 POA, 3 delete, 1 POA+MOA	7 fricatives (4 POA, 2 add, 1 delete) 2 stops (1 delete, 1 add)
3.50	3 shared errors (0 identical)	14 stops (7 delete, 2 POA, 1 MOA, 1 voice, 1 POA+MOA, 1 add, 1 POA+voice) 14 fricatives (9 delete, 4 POA, 1 voice) 8 approximates (7 POA, 1 POA+MOA)	3 fricative (3 add) 2 stops (2 add)
4.08	4 shared errors (0 identical)	11 stops (6 delete, 3 voice, 1 POA+voice, 1 add, 8 fricatives (3 delete, 2 voice, 2 POA, 1 POA+MOA) 6 approximates (3 delete, 3 POA)	3 fricatives (3 POA) 2 approximates (2 POA) 2 stops (1 delete, 1 add)
3.42	1 shared errors (0 identical)	12 fricatives (8 delete, 3 POA, 1 POA+MOA) 9 stops (6 delete, 1 voice, 1 POA, 1 POA+voice) 4 approximates (4 POA)	3 approximates (POA) 1 fricative (delete)
4.83	4 shared errors (1 identical)	10 stops (3 voice, 3 delete, 1 POA, 1 MOA, 1 POA+voice, 1 add) 7 fricatives (4 delete, 1 voice, 1 POA, 1 POA+MOA) 1 approximates (POA)	9 stops (4 add, 3 delete, 2 voice) 5 fricatives (3 POA, 2 add) 2 approximates (2 POA)
3.75	3 shared errors	11 stops (5 voice, 3 POA, 2 delete, 2 add)	4 fricatives (4 add)

	(0 identical)	10 fricatives (5 delete, 2 POA, 2 POA+MOA, 1 voice) 2 approximates (1 delete, 1 POA)	1 stops (1 add) 1 approximates (1 delete)
3.17	1 shared error (0 identical)	12 fricatives (8 delete, 2 POA+voice, 1 POA, 1 voice) 8 stops (5 delete, 2 POA, 1 POA+MOA, 1 add) 5 Approximate (4 POA, 1 delete)	2 stops (1 voice, 1 delete) 1 Approximate (1 delete)
3.42	3 shared errors (0 identical)	9 fricatives (6 delete, 2 POA, 1 POA+MOA) 7 stops (2 delete, 2 voice, 1 POA, 1 MOA, 1 POA+MOA) 2 approximates (1 delete, 1 POA)	5 fricatives (3 POA, 2 POA+voice) 4 approximates (4 POA) 2 stops (2 add)
4.92	3 shared errors (0 identical)	8 stops (4 delete, 2 voice, 2 POA) 7 fricatives (4 POA, 1 delete, 1 POA+MOA, 1 MOA+voice) 3 approximates (2 delete, 1 POA)	4 approximates (4 POA) 2 stops (add)
4.50	2 shared errors (1 identical)	13 fricatives (9 delete, 3 POA, 1 POA+MOA) 7 stops (6 delete, 1 POA) 6 approximates (3 POA, 1 delete, 1 POA+MOA, 1 POA+MOA+voice)	1 fricative (delete) 1 stops (voice)
3.17	1 shared error (0 identical)	7 stops (4 delete, 2 voice, 1 POA) 5 fricatives (2 delete, 2 POA, 1 POA+MOA) 2 approximates (2 POA)	6 Fricative (6 POA) 1 stops (add)
3.50	2 shared errors (1 identical)	12 stops (5 delete, 4 voice, 2 POA, 1 MOA) 5 fricatives (2 delete, 3 POA) 2 approximates (2 POA)	1 stops (voice) 1 fricative (POA)
5.17	2 shared errors (0 identical)	9 approximates (4 delete, 3 POA, 1 POA+MOA, 1 POA+MOA+voice 7 fricatives (4 delete, 1 POA, 1 voice, 1 MOA) 6 stops (3 delete, 1 voice 1 POA, other)	2 approximates (2 POA)
3.75	2 shared errors (1 identical)	11 stops (5 voice, 4 delete, 1 POA, 1 MOA) 8 fricatives (6 delete, 1 POA, 1 MOA) 2 Approximate (1 POA, 1 delete)	2 stops (2 delete, 1 voice) 1 fricative (POA) 1 approximates (delete)
4.83	1 shared error (0 identical)	8 stops (3 voice, 3 delete, 2 POA, 1 add) 4 fricatives (3 deleted, 1 POA+MOA) 3 approximates (3 POA)	1 stops (delete)
3.97	0 shared errors (0 identical)	9 stops (3 POA, 3 delete, 2 voice, 1 MOA) 6 fricatives (6 delete) 6 approximates 5 POA, 1 delete)	1 stops (delete)
4.00	2 shared errors (0 identical)	7 stops (3 delete, 2 voice, 2 MOA) 4 fricatives (2 reduc, 1 POA+MOA, 1 add 1 approximate (POA)	4 approximates (4 POA)
5.25	0 shared errors (0 identical)	7 fricatives (4 delete, 2 POA, 1 POA+MOA) 5 stops (2 delete, 2 voice, 1 MOA) 3 approximates (2 POA, 1 MOA)	1 stops (add)
4.25	0 shared errors (0 identical)	7 fricatives (4 delete, 1 voice, 1 POA, 1 POA+MOA)	2 stops (1 voice, 1 POA+MOA)

		4 stops (2 voice, 1 delete, 1 POA+MOA) 2 approximates (2 POA)	
4.42	1 shared error (1 identical)	6 fricative (4 delete, 1 POA, 1 MOA) 5 stops (2 delete, 2 voice, 1 POA+MOA) 1 approximate (1 POA)	1 stops (1 delete) 1 Fricative (1 delete) 1 Approximate (1 add)
5.17	0 shared errors (0 identical)	7 stops (4 delete, 2 voice, 1 add) 4 fricatives (3 delete, 1 POA) 3 approximates (2 delete, 1 POA)	No errors
4.83	0 shared errors (0 identical)	4 fricative (2 delete, 1 voice, 1 MOA) 4 stops (2 delete, 1 POA, 1 add) 1 approximate (1 POA)	1 stops (1 voice)
5.17	0 shared errors (0 identical)	5 stops (2 voice, 1 delete, 1 POA, 1 MOA) 1 fricative (1 POA) 1 approximate (1 POA)	No errors

---

[i] Error code:

Voice = voicing error (e.g., *robe => rope*; *pig=> big*)
POA = Place of Articulation error (e.g., *crash => trash*, *catch => cats*)
MOA = Manner of Articulation error (e.g., *sing => ting, trail => srail*)
Delete = deletion error (e.g., *seat => eat; break => rake; strip => rip*)
Add = epenthesis error (e.g., *skis => səkis*)

References

Bayley, Nancy. 1969. *Bayley Scales of Infant Development*. New York: Psychological Corporation.

Bird, Judith, Dorothy V. M. Bishop, & Norman H. Freeman. 1995. Phonological awareness and literacy development in children with expressive phonological impairments. *Journal of Speech and Hearing Research, 38(2)*, 446–462.

Bird, Judith & Dorothy V. M. Bishop. 1992. Perception and awareness of phonemes in phonologically impaired children. *European Journal of Language & Communication Disorders* 27(4), 289–311.

Bishop, Dorothy V. M., B. Byers Brown, & J. Robson. 1990. A relationship between phoneme discrimination, speech production, and language comprehension in cerebral palsied individuals. *Journal of Speech and Hearing Research* 33(2), 210–219.

Boersma, Paul & David Weenink. 2008. *Praat: Doing Phonetics by Computer*. Version 5.0.4. Available from http://www.praat.org.

Drumwright, Amelia. 1971. *Denver Articulation Screening Examination*. Denver, CO: University of Colorado Medical Center.

Drumwright, Amelia, Pearl Van Natta, Bonnie Camp, William Frankenburg, & Hazel Drexler. 1973. The Denver Articulation Screening Exam. *Journal of Speech and Hearing Disorders* 38(1), 3–14.

Galantucci, Bruno, Carol A. Fowler, M. T. Turvey. 2006. The motor theory of speech perception reviewed. *Psychonomic Bulletin & Review* 13(3), 361–377.

Groenen, Paul, Ben Maassen, Thom Crul, & Geert Thoonen 1996. The specific relation between perception and production errors for place of articulation in developmental apraxia of speech. *Journal of Speech and Hearing Research*, 39(3),468–482.

Grunwell, Pamela. 1987. *Clinical Phonology*. London, England: Croom Helm.

Hall, Penelope K., Linda S. Jordan, & Donald A. Robin. 1993. *Developmental Apraxia of Speech*. Austin, TX: Pro-Ed.

Halle, Morris & Kenneth N. Stevens. 1962. Speech recognition: A model and a program for research. *IRE Transactions on Information Theory* 8(2), 155–159.

Hickok, Gregory. 2010. The role of mirror neurons in speech perception and action word semantics. *Language and Cognitive Processes* 25(6), 749–77.

Hickok, Gregory, K. Okada, W. Barr, J. Pa, C. Rogalsky, . . . , & A. Grant. 2008. Bilateral capacity for speech sound processing in auditory comprehension: Evidence from Wada procedures. *Brain and Language*, 107(3), 179–184.

Hickok, Gregory, Maddalena Costanzo, Rita Capasso, Gabriele Miceli. 2011. The role of Broca's area in speech perception: Evidence from aphasia revisited. *Brain and Language* 119(3), 214–20.

Hoffman, Paul R., R. G. Daniloff, D. Bengoa, & Gordon Schuckers. 1985. Misarticulating and normally articulating children's identification and discrimination of synthetic [r] and [w]. *Journal of Speech & Hearing Disorders* 50(1), 46–53.

Hoit-Dalgaard, Jeannette, Thomas Murry, Harriet G.Kopp. 1983. Voice onset time production and perception in apraxic subjects. *Brain and Language* 20(2), 329–339.

Hustad, Katherine C. 2008. The relationship between listener comprehension and intelligibility scores for speakers with dysarthria. *Journal of Speech, Language, and Hearing Research* 51(3), 562–573.

Johnson, Erin P., Bruce F. Pennington, Joanna H. Lowenstein, & Susan Nittrouer. 2011. Sensitivity to structure in the speech signal by children with speech sound disorder and reading disability. *Journal of Communication Disorders*44(3), 294–314.

Kuijpers, Cecile T. L. 1996. Perception of the voicing contrast by Dutch children and adults. *Journal of Phonetics* 24 (3),367–382.

Lenneberg Eric H. 1962. Understanding language without ability to speak: A case report. *Journal of Abnormal and Social Psychology* 65(6), 419–425.

Lenneberg, Eric H. 1964. Speech as a motor skill with special reference to nonaphasic disorders. *Monographs of the Society for Research in Child Development* 29(1), 115–127.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York: Wiley.

Liberman, Alvin M. & Ignatius G. Mattingly. 1985. The motor theory of speech perception revised. *Cognition* 21(1), 1–36.

Liberman, Alvin M. & Ignatius G. Mattingly. 1989. A specialization for speech perception. *Science* 243(4890), 489–494.

Liberman, Alvin M. & Douglas H. Whalen. 2000. On the relationship of speech to language. *Trends in Cognitive Sciences* 4(5), 187–196.

Lotto, Andrew J., Gregory S. Hickok, & Lori L. Holt. 2009. Reflections on mirror neurons and speech perception. *Trends in Cognitive Sciences* 13(3), 110–114.

Marion, Michelle J., Harvey M. Sussman, & Thomas P. Marquardt. 1993. The perception and production of rhyme in normal and developmentally apraxic children. *Journal of Communication Disorders* 26(3), 129–160.

Marquardt, Thomas P. & John H. Saxman. 1972. Language comprehension and auditory discrimination in articulation deficient kindergarten children. *Journal of Speech, Language, and Hearing Research* 15(2), 382–389.

McAllister Byun, Tara, Mark Tiede. 2017. Perception-production relations in later development of American English rhotics. *PLoS ONE* 12(2), e0172022.

Monnin, Lorraine M. & Dorothy A. Huntington. 1974. Relationship of articulatory defects to speech-sound identification. *Journal of Speech and Hearing Research* 17(3), 352–366.

Morgan, Rosemarie A. 1984. Auditory discrimination in speech impaired children and normal children as related to age. *British Journal of Disorders of Communication* 18(1), 89–96.

Ohde, Ralph N. & Donald J. Sharf. 1998. Perceptual categorization and consistency of synthesized /r-w/ continua by adults, normal children and /r/- misarticulating 26 children. *Journal of Speech, and Hearing Research*, 31, 556–568.

Raaymakers, Emile M. & Thom A. Crul. 1988. Perception and production of the final /s-ts/ contrast in Dutch by misarticulating children. *Journal of Speech and Hearing Disorders* 53(3), 262–270.

Reynell, Joan, & R. M. C. Huntley. 1971. New scales for the assessment of language development in young children. *Journal of Learning Disabilities* 4(10), 10–18.

Rvachew, Susan, Michele Nowak, & Genevieve Cloutier. 2004. Effect of phonemic perception training on the speech production and phonological awareness skills of children with expressive phonological delay. *American Journal of Speech–Language Pathology*, 13, 250–263.

Rvachew, Susan, & M. Grawberg. 2006. Correlates of phonological awareness in preschoolers with speech sound disorders. *Journal of Speech, Language, and Hearing Research* 49, 74–87.

Rvachew, Susan, & Donald G. Jamieson. 1989. Perception of voiceless fricatives by children with a functional articulation disorder. *Journal of Speech and Hearing Disorders* 54(2), 193–208.

Rvachew, Susan, Alyssa Ohberg, Meghann Grawburg, & Joan Heyding. 2003. Phonological awareness and phonemic perception in 4-year-old children with delayed expressive phonology skills. *American Journal of Speech-Language Pathology* 12, 463–471.

Sander, Eric K. 1972. When are speech sounds learned? *Journal of Speech and Hearing Disorders* 37(1), 55–63.

Shuster, Linda I. 1998. The perception of correctly and incorrectly produced /r/. *Journal of Speech, Language, and Hearing Research* 41, 941–950.

Stasenko, Alena, Frank E. Garcea, & Bradford Z. Mahon. 2013. What happens to the motor theory of perception when the motor system is damaged? *Language and Cognition* 5(2-3), 225–238.

Sussman, Harvey M., Thomas P. Marquardt, J. Doyle, & H. Knapp. 2002. Phonemic integrity and contrastiveness in developmental apraxia of speech. In F. Windsor, N. Hewlett, & L. Kelly (eds.), *Themes in Clinical Linguistics and Phonetics*, 311–326. Mahwah, NJ: Lawrence Erlbaum.

Stutsman, Rachel. 1981. *Merrill Palmer Scale of Mental Tests*. East Aurora, NY: Slosson Educational Publications.

Terman, Lewis Madison & Maud Amanda Merrill. 1960. *Stanford-Binet Intelligence Scale: Manual for the Third Revision, Form L-M*. Boston, MA: Houghton-Mifflin.

Waterson, Natalie. 1987. *Prosodic Phonology: The Theory and Application to Language Acquisition and Speech Processing*. Newcastle upon Tyne: Grevatt & Grevatt.

Karin Stromswold
Rutgers University
Department of Psychology &
Center for Cognitive Science
152 Frelinghuysen Road
Piscataway, NJ 08854
United States of America

karin@ruccs.rutgers.edu

Aliza Lichtenstein
Rutgers University
Department of Psychology
152 Frelinghuysen Road
Piscataway, NJ 08854
United States of America

atbh22@gmail.com

Linguistic and Nonverbal Abilities over Time in a Child Case of 22q11 Deletion Syndrome

Maria Kambanaros & Kleanthes K. Grohmann

The aim of this study is to profile the cognitive–linguistic performance of a male child (P.I.) with 22q11 deletion syndrome (22q11DS). Specifically, receptive and expressive language performance and nonverbal IQ (NVIQ) are described at two different time points—when P.I. was 6 and 10 years of age, respectively. Using case-based methodology, P.I.'s NVIQ and performance on global and structured language tasks are compared to typically developing children of the same chronological age and school-aged children with specific language impairment (SLI). The results show no improvement in NVIQ or vocabulary, but his morphosyntactic abilities did improve over time. The findings are discussed in relation to two hypotheses, either that the profile of language impairment in children with 22q11DS is distinctive to the syndrome or that there is co-morbidity with SLI. This is particularly important for speech–language therapists who have a primary role in diagnosing communication deficits and providing treatment.

Keywords: 22q11 deletion syndrome; co-morbidity; cognitive–linguistic profile; complex syntax; faculty of language; language development; specific language impairment

1. Introduction

In chapter 6 of *Biological Foundations of Language*, Eric Lenneberg included a short section on types of "evidence for inheritance of language potential" (Lenneberg 1967: 248ff.). Since there was not much research available at the time, it stressed the importance of family histories for establishing a genetic base of language impairments and arguably laid the foundations for a long series of twin studies research on language (see e.g. Stromswold 1998, 2001, 2006, and the relevant literature cited). The present paper can be seen as a contribution to the endeavor of detailing language and cognition under special genetic circumstances (22q11 deletion syndrome), also providing further evidence that a "pathologically lowered IQ […] does not result in bizarre use of language" (Lenneberg 1967: 326).

We would like to thank Loukia Taxitari for statistical analysis, Eleni Theodorou for data collection, and Marina Varnava for further assistance. An earlier version of parts of this paper reporting the participant P.I.'s single-time performance at age 6 appeared with these researchers as co-authors (Kambanaros et al. 2018).

ISSN 1450–3417

Biolinguistics 11.SI: 57–81, 2017
http://www.biolinguistics.eu

The importance of genomics in speech pathology (or speech and language therapy) practice is highly recognized (ASHA 2015), yet cross-linguistic research describing language and communication abilities of children with genetically linked disorders is still in its infancy. This has a negative impact on speech pathologists' ability to provide diagnosis and guide interventions, which in turn influences educational, behavioral, and psychological outcomes for children with genetic syndromes. Likewise, there is yet a lot to be learned concerning the biological underpinnings of language informing multidisciplinary linguistics research.

This study reports on the nonverbal and language abilities of a school-aged boy genetically confirmed with 22q11 deletion syndrome (22q11DS), at two different time points in his life: at 6 and at 10 years of age. Historically 22q11DS, a neurogenetic condition, has been known by many names, including DiGeorge syndrome (DGS), Shprintzen syndrome, velocardiofacial syndrome (VCFS), conotruncalanomaly face syndrome, and CATCH22 (cardiac abnormality, abnormal face, T cell deficits, cleft palate, and hypocalcemia). The present research is the first to describe the linguistic manifestations of a language deficit associated with 22q11DS for Greek, a highly inflected, morphologically complex language. A core area of investigation will be our participant's abilities in structural language, that is, his morphosyntactic abilities and performance on more complex language tasks.

Our testing battery contains several measures for probing structural language, ranging from the comprehension of complex structures, morpho-syntactic properties, and other phenomena to the production of structurally complex clauses on a narrative retell task. But the entire testing battery goes well beyond structural language. As the first research on language abilities in 22q11DS for (Cypriot) Greek, we take a broader angle and also investigate global language and cognitive abilities, including receptive vocabulary, expressive vocabulary, and nonverbal intelligence.

Furthermore, to decipher the issue of co-morbidity with specific language impairment (SLI) reviewed below, our participant's performance on all measures is compared to two groups of children, namely a group of children with typical language development and a group with a clinical diagnosis of SLI. Taking the lead from Rice (2016), we will present the outcome of our detailed testing in a comparative conceptual framework, that is, 22q11DS vs. SLI.

2. Background on 22q11DS

22q11DS results from a submicroscopic hemizygous deletion at chromosome 22q11.2 specifically at the long arm (q) 11.2 band (Woodin et al. 2001). It is an increasingly common genetic disorder affecting at least 1 in 2,000–7,000 live births (Shprintzen 2008) and represents one of the most common known recurrent copy number condition variants (Squarcione et al., 2013). It follows an autosomal dominant inheritance pattern (a child only needs to get the abnormal gene from one parent in order to inherit the disease). However, only around 10% of cases are inherited; the majority of 22q11DS occurrences are due to random mutation (Shprintzen 2008). The phenotype is quite varied, with close to 200 clinical features identified so far as related to abnormalities of the heart, palate, velopharyngeal mechanism, immune system, central nervous system, and brain morphology (Woodin et al. 2001).

Being a highly variable disorder, much is still not known about the contributing factors to this variability. One speculation is that variation is related to how environmental factors interact with structural and sequence variation in the genome (Squarcione et al. 2013). Consequently, each child is affected differently and the symptoms vary widely, ranging from less severe to severely affected. Due to the many different body systems that can be involved in the phenotype, a multidisciplinary approach is recommended by best practice guidelines involving fetal medicine specialists, neonatologists, pediatricians, cardiologists or cardiothoracic surgeons, immunologists, cleft surgeons, speech and language therapists, endocrinologists, clinical geneticists, and general practitioners (Allgrove et al. 2012).

Furthermore, approximately 30% of individuals with 22q11DS develop schizophrenia during adolescence/early adulthood, making this syndrome a model for the disorder (Squarcione et al. 2013). Physically, children with 22q11DS tend to have similar facial features, which may include a long and narrow face, wide-set almond-shaped eyes, a broad nasal bridge and bulbous nose tip, a small mouth, small and low set ears that are folded over at the top, and an irregular skull shape (http://www.nhs.uk/conditions/digeorge-syndrome).

In contrast with the large body of literature on the behavioral and psychiatric profiles of individuals with 22q11DS (see Scandurra et al. 2013 and relevant references within), information on the pediatric population is limited. In fact, there is evidence that 22q11DS remains largely undiagnosed in many children as an isolated speech and language disorder, or a developmental delay, in the presence of few or no physical abnormalities because the large phenotypic variation renders diagnosis more difficult (Niklasson et al. 2001). Accordingly, the median age of diagnosis is reported to be as late as six and a half years (Solot et al. 2000).

The majority of individuals with 22q11DS shows relatively mild cognitive deficits, with verbal IQ often significantly higher than performance and/or nonverbal IQ. However, there are reports of individuals with low normal intelligence (IQ 71–85) and some with an IQ in excess of 85 (Niklasson et al. 2001). Individuals with 22q11DS show relative strengths in verbal ability, rote processing, verbal memory, reading, and spelling. In contrast, weaknesses have been reported in language abilities, attention, working memory, executive functions, visuospatial memory, and psychosocial functioning (see Woodin et al. 2001 for both points).

Most significantly, research on the manifestations of speech and language disorders in children with 22q11DS is not prominent, despite communication impairment hailed as the hallmark deficit, with an estimated 90% of children having some type of speech–language deficit (Persson et al. 2006). A detailed summary of available studies describing language impairment in children with 22q11DS is presented in the Appendix.

Preschool children with 22q11DS often show smaller vocabularies, word finding deficits, reduced length of sentences, delayed use of grammatical structures, and difficulties with discourse (Persson et al. 2006). Moreover, expressive language delays are often more severe than receptive language delays. By school age, there are persistent difficulties in syntax, word finding deficits, and problems with discourse organization (Solot et al. 2000, Persson et al. 2006).

Of clinical interest is the *co-morbidity of SLI* reported so far in a large 22q11DS cohort from the USA, where 36% of children were classified as having SLI on top of 22q11DS (Solot et al. 2000). A similar finding was reported in smaller case studies involving four Dutch children with 22q11DS and SLI (Goorhuis-Brouwer et al. 2003). SLI is a term applied to children whose speech and language is substantially below age level for no apparent reason, in the absence of neurological damage, impaired sensorimotor abilities, and so on (i.e., with normal intelligence levels, hearing, vision, etc.). The reader is referred to Bishop (2014) for a more recent definition of SLI.

Directly relevant to our research is the study from Sweden involving preschoolers and school-aged children with 22q11DS (Persson et al. 2006). Here, 19 children between 5 and 8 years of age genetically diagnosed with 22q11DS (10 girls) were assessed on receptive vocabulary knowledge using the Swedish version of the Peabody Picture Vocabulary Test (PPVT; Dunn & Dunn 1981), and on narrative retell performance using the Bus Story Test (BST; Renfrew 1997). The mean full-scale IQ of the group was 78 and six children had an additional autism spectrum disorder, attention deficit/hyperactivity disorder, or a combination of the two. For the PPVT, results revealed that the majority of children (n=14) scored moderately low on receptive vocabulary, revealing more difficulties with understanding single words/concepts beyond that expected for performance or nonverbal IQ.

With regard to the BST, the majority of the 22q11DS children (n=19) scored below the mean on the information score of the task. Of clinical interest was the finding of a negative correlation between age and the information score: The older the children were, the more difficulties they had recalling information on the BST. Furthermore, all but one participant had a shorter average sentence length than expected according to chronological age norms. In contrast, five participants produced subordinate clauses within normal limits, while 14 had a lower number of subordinate clauses according to chronological age norms, revealing low grammatical abilities. The type of qualitative errors analyzed from the 22q11DS group on the BST included grammatical errors (e.g., errors of prepositions, gender, definite articles) and incomplete utterances. Based on their findings, Persson et al. (2006) concluded that the majority of the 22q11DS participants had language impairments, and difficulties were found in all language areas investigated. The authors did not classify any member of the 22q11DS group as also having SLI due to the diverse non-linguistic characteristics of the group (e.g., behavioral difficulties, including both autism spectrum and attention deficit/hyperactivity disorders). The notion of 'specific' language impairment is commonly used in the context of otherwise normal development (see Bishop 2014).

The purpose of the present study is to profile the language abilities of one male child with 22q11DS (P.I.) and compare his performance to that reported for children with typical language development (TLD) and children with a history of SLI across a battery of linguistic measures. The aims of the study are four-fold:

1. to compare P.I.'s nonverbal and language performance across all measures with pre- and primary school-aged children with TLD for each time point;
2. to compare P.I.'s nonverbal and language performance across all measures with that of children diagnosed with SLI for each time point;
3. to investigate P.I.'s morphosyntactic abilities over time;
4. to shed light on the 22q11DS±SLI debate based on the findings.

3. Methodology

3.1. Participant

Our participant, P.I., was diagnosed with 22q11DS by the head geneticist of the Makarios Hospital Genetics Clinic in Nicosia, Cyprus, using the fluorescence in situ hybridization test (FISH) after his fifth birthday (though it was not reported to us how much genetic information was lost exactly). The FISH is specially designed to look for small groups of genes that are deleted and in the case of 22q11DS shows whether the region of chromosome 22 is present. If only one copy of chromosome 22 'lights up' with fluorescent DNA dye, rather than both copies, the test is positive for 22q11 deletion.

P.I. was born from healthy, unrelated parents who are both highly educated, with university degrees in an allied health profession (mother) and information technology (father). He also has a healthy brother who is older by three years. At diagnosis, P.I. presented with no cardiac malformation but with autoimmune disorder (thyroid disorder) and growth hormone deficiency. Hearing was tested by the Makarios Hospital Audiology Clinic and reported to be within normal limits. Also, the hospital reported no positive assessment of autism spectrum disorder symptoms or any other psychiatric condition.

When first assessed by a certified speech–language pathologist (first author) at the age of 6 years, he showed facial dysmorphia in line with 22q11DS characteristics (long narrow face, almond shaped eyes, bulbous nose, small mouth, and overfolded ear helix). The oral-peripheral motor examination revealed no structural abnormalities of the speech mechanism, including the palate, but P.I. required weekly speech therapy sessions for treatment of voice quality (e.g., hypernasality) and mild misarticulations because of velopharyngeal incompetence (VPI). He also had hypocalcaemia evidenced by poor dentition and several tooth cavities. In addition, P.I. presented with motor hypotonia and a delay in development of gross motor skills. Occupational therapy was recommended to the parents to assist with gross and fine motor skills. P.I. was enrolled in the preschool education program of a public school in Nicosia and was not receiving special education services.

At the age of 10 years, he was enrolled in grade 4 in a public school in Nicosia. He was receiving special education services predominantly for mathematics. He presented with hoarseness and reduced vocal volume, typical of VPI, but generally intelligible speech. He was also receiving regular dental care. His annual medical treatment at the Cyprus Institute of Neurology and Genetics involved full blood count for cytopaenias, serum calcium, and thyroid function. He was also being monitored yearly for autoimmune disease, height, and weight. At the time of the second testing, he was undergoing psychological evaluation for aggressive behavior.

Language testing across all measures was conducted over a three-month period at age 6 years and later at age 10 years. The reader is referred to Kambanaros et al. (2018) for a full description of P.I.'s language performance at 6 years of age.

3.2. *Comparative Groups*

A total of 38 Greek Cypriot bilectal pre- and primary school-aged children divided into two groups served as the comparative groups, one group with TLD and one group with a clinical diagnosis of SLI. Both groups are described in detail in the original research article on diagnosing SLI in the context of Cyprus (Theodorou et al. 2016). In line with Rowe & Grohmann (2013), we consider bilectal children those whose parents are both Greek Cypriots, who were born and raised in Cyprus, and who did not spend any large amount of time outside the country, including Greece. We did not control any more specifically for balanced input or age of exposure to Cypriot Greek and Standard Modern Greek but assumed the standard path of language development laid out in our previous research, summarized most recently in Grohmann & Kambanaros (2016).

Children in the comparative groups had (i) no known history of neurological, emotional, developmental, or behavioral problems; (ii) hearing and vision adequate for test purposes after school screening at the beginning of the school year; (iii) nonverbal performance in the broad range of normal; (iv) no gross motor difficulties; (v) normal articulation; and (vi) medium–high socio-economic status. All information was obtained from the speech–language pathologists and teachers or from the children's parents. The study was approved by the Cyprus Ministry of Education and Culture through the Pedagogical Institute.

3.2.1. *Typically Developing Children (5- to 6- and 7- to 8-Year-Old TLD Groups)*

Ten children (4 girls) with TLD aged between 4;5 and 6;6 with a mean age of 5;8 (SD 0.6) served as the younger TLD comparative group for time point 1, when P.I. was 6 years old, and 12 children (6 girls) aged between 6;7 and 8;7 with a mean age of 7;9 (SD 0.4) served as the older TLD comparative group for time point 2, when P.I. was 10 years old. According to the classroom teacher and parent reports, all participants in the control groups were typically developing in all respects. No child was previously referred to or had received treatment by a speech pathologist.

3.2.2. Language-Impaired Children (5- to 6- and 7- to 8-Year-Old SLI Groups)

Nine children with SLI (2 girls) aged between 4;11 and 5;11 with a mean age of 5;6 (SD 0.3) served as the language-impaired younger SLI comparative group for time point 1, when P.I. was 6 years old, and seven children with SLI (4 girls) aged between 6;2 and 8;6 with a mean age of 7;6 (SD 0.9) served as the language-impaired older SLI comparative group for time point 2, when P.I. was 10 years old. Children were diagnosed with SLI by certified speech–language pathologists based on case history information, informal testing of comprehension and production abilities, analysis of spontaneous language samples, and clinical observation. SLI diagnosis was confirmed by further testing on tools used for diagnostic purposes in Cyprus (Theodorou 2013, Theodorou et al. 2014, 2016). Children with SLI were receiving speech–language therapy services by practitioners in private settings.

3.3. Socio-Economic Status

All children came from families with a medium to high socio-economic status, as measured by mothers' education level using the European Social Survey (2010) database. We compared P.I.'s mother's education level (undergraduate university degree) to the education levels of the children's mothers in all control groups. No difference was observed in her education level compared to the younger TLD group mothers' ($t_{(9)} = 0.55$, $p = 0.60$), the older TLD group mothers' ($t_{(11)} = 1.73$, $p = 0.11$), or the older SLI groups mothers' ($t_{(6)} = 1.93$, $p = 0.10$), but the education level did differ from the younger SLI group mothers' ($t_{(8)} = 2.52$, $p < 0.05$), who had a lower mean than P.I.'s mother.

3.4. Materials and Procedures

3.4.1. Nonverbal IQ (NVIQ)

P.I. was tested on the Raven's Coloured Progressive Matrices (RCPM; Raven et al. 2000), following the Greek norms of Sideridis et al. (2015), at the age of 6 years and later when he was 10 years old. Children from both the TLD and the SLI groups were tested on the RCPM separately (Theodorou et al. 2016).

3.4.2. Language Measures

All language measures administered to P.I. and the comparative groups are described in Table 1. The reader is referred to Theodorou et al. (2016) for a detailed description of the overall testing aims, methodology, and results for the TLD and SLI groups serving as the comparative groups in this research.

Measure	**Domain**	**Source**
	Linguistic	
Diagnostic Verbal Intelligence Quotient (DVIQ)* [5 subtests]	• vocabulary (naming single concepts) • comprehension of morphosyntax • production of morphosyntax • sentence repetition • comprehension of metalinguistic concepts	Stavrakaki & Tsimpli (2000)
Peabody Picture Vocabulary Test (PPVT)*	receptive vocabulary (single words)	Simos et al. (2010)
Phonetic and Phonological Articulation Test*	articulation and phonological processing	Panhellenic Association of Logopedists (1995)
Expressive Vocabulary Test (EVT)*	expressive vocabulary (single words)	Vogindroukas et al. (2009)
Clitics-in-Islands Tool (CIT)	clitic production	Varlokosta et al. (2016)
Relative Clause Task	comprehension and production of relative clauses	Theodorou & Grohmann (2013), modified from Friedmann & Novogrodsky (2004) and Novogrodsky & Friedmann (2006)
Bus Story Test (BST) [4 measurements]	• Information score • Number of subordinate clauses produced • Number of t-units (sentences) produced • MLU–word	Theodorou & Grohmann (2010) for research purposes in Greek, based on Renfrew (1997)
	Cognition	
Raven's Coloured Progressive Matrices (RCPM)*	non-verbal performance	Raven et al. (2000)

* = the measure is norm-referenced for Greek

Table 1: A description of the cognitive and linguistic measures administered to P.I. and all comparative groups.

3.4.3. Structural Language Probes

Structural language probes are those considered to tap into morphosyntactic abilities and language complexity. For this study, the comprehension and production of morphosyntax subtests of the Diagnostic Verbal Intelligence Quotient (DVIQ), the sentence repetition subtest of the DVIQ, the number of subordinate clauses produced on the Bus Story Test (BST), performance on the Clitics-in-Islands Tool (CIT), and performance on the production and comprehension of restrictive relatives are reported in the Results section.

3.5. *Scoring and Analysis*

For all measures, an accuracy score was calculated by summing up the number of correct responses. For all sub-categories of the DVIQ (apart from sentence repetition), a single point was given for each correct response, and no point for each incorrect one. For sentence repetition, 3 points were given for each correct response, 2 points for each response with one error, 1 point for each response with 2 errors, and no points for responses with 3 or more errors. The main statistical analysis used was the Crawford–Howell t-test (Crawford & Howell 1998), a method developed in neuropsychology for the comparison of single cases with control groups (with small sample numbers). Using this method, P.I.'s accuracy scores on the different measures were compared to the TLD and SLI groups using a two-tailed t-test.

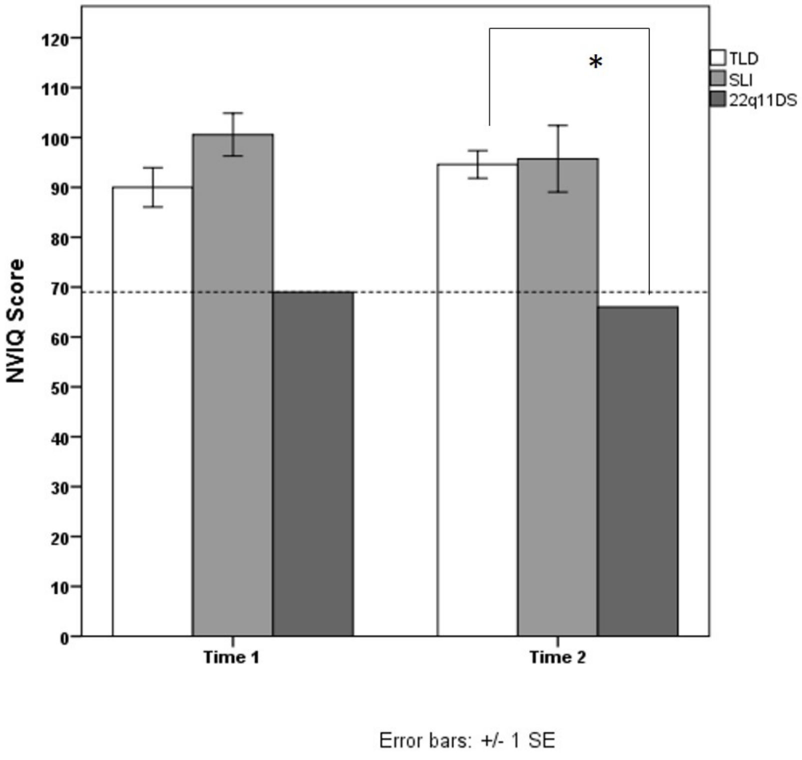
4. Results

The raw scores across all measures for P.I. and both comparative groups are reported in Table 2.

4.1. *Cognition*

At 6 years of age, no difference was observed between P.I.'s NVIQ and the TLD group's ($t_{(9)} = -1.61$, $p = 0.14$), and P.I. had marginally lower NVIQ than the SLI group ($t_{(8)} = -2.33$, $p = 0.05$). At 10 years of age, P.I. had lower NVIQ than the TLD ($t_{(11)} = -2.85$, $p < 0.05$) but not the SLI group ($t_{(6)} = -1.57$, $p = 0.17$). The results from the RCPM are presented in Figure 1.

Figure 1: Non-verbal IQ results (RCPM) separately for P.I. (22q11DS), TLD groups, and SLI groups at Time 1 and Time 2.



		Time 1				*Time 2*			
Measure	**Subtest (maximum score)**	**Score (SD)**			**Cut-off**	**Score (SD)**			**Cut-off**
		P.I.	*TLD–Y*	*SLI–Y*		*P.I.*	*TLD–O*	*SLI–O*	
Age		6	5;8 (0;6)	5;6 (0;3)		10	7;9 (0;6)	7;6 (0;3)	
DVIQ	Vocabulary (27)	19	22.9 (2.2)	16.8 (2.8)	19.90	20	24.7 (1.6)	20.6 (1.8)	22.35
	Production: Morphosyntax (24)	9	19.8 (2.1)	13.9 (2.7)	17.04	14	21.3 (1.4)	14.6 (1.9)	18.53
	Comprehension: Metalinguistic concepts (25)	17	19.9 (1.8)	18 (3.9)	18.73	20	22.6 (1.9)	19 (1.7)	20.14
	Comprehension: Morphosyntax (31)	16	25.4 (2.6)	24.6 (3.8)	23.85	26	28.6 (1.4)	26.4 (2.2)	26.84
	Sentence repetition (48)	35	45.5 (2.5)	40.9 (2.5)	43.18	46	47.3 (1.0)	42.3 (2.4)	44.91
	Total DVIQ Score (155)	96	133.5 (7.6)	114.1 (10.5)	124.5	126	144.5 (4.2)	122.9 (6.3)	135.0
PPVT	(212) (51)	34	63.8 (11.7)	54.8 (16.6)	56.74	23†	93.7 (25.9)	72.9 (16.7)	70.29
EVT	(50)	25	33.3 (5.1)	21.7 (2.7)	26.00	25	38.3 (3.7)	27.7 (4.8)	32.43
CIT	(12)	9	11.6 (1.3)	11 (1)	N/A	12	11.83 (0.4)	11.71 (0.5)	N/A
BST	Information	21	35.8 (11.8)	21.8 (8.9)	N/A	42	46.4 (8.9)	29.0 (8.2)	N/A
	No. of subordinate clauses produced	2	7.8 (4.1)	1.7 (1.5)	N/A	9	9 (3.0)	5.6 (1.9)	N/A
	No. of t-units (sentences) produced	10	20.6 (3.9)	15.6 (3.8)	N/A	26	20.5 (3.3)	20.1 (4.0)	N/A
	MLU–word	5.7	4.7 (1.2)	3.4 (0.7)	N/A	4.8	5.2 (1.3)	4.6 (1.1)	N/A

Key: DVIQ=Diagnostic Verbal IQ Test; PPVT=Peabody Picture Vocabulary Test; EVT=Expressive Vocabulary Test; CIT=Clitics-in-Islands Test; BST=Bus Story Test; No.=number; MLU–word=word-based mean length of utterance; 22q11DS=22q11 deletion syndrome; TLD=typical language development; Y=younger; O=older; SLI=specific language impairment; SD=standard deviation

†P.I. was tested on the shortened version of the PPVT (Simos et al., 2010) which has a maximum score of 51.

Table 2: Raw scores and standard deviations across language measures for P.I. and for all comparative groups (mean raw scores).

4.2. *Global Language*

4.2.1. Receptive Abilities

(i) Vocabulary (PPVT): At 6 years of age, P.I. performed similarly to both the TLD group ($t_{(9)}$ = –0.55, p = 0.59) and the SLI group ($t_{(8)}$ = –0.13, p = 0.90) for receptive vocabulary on the PPVT. P.I.'s performance at age 10 cannot be statistically compared to the comparative groups, as the shortened version of the PPVT was administered (Simos et al. 2010) and not the full battery that was used for the SLI and TLD children.

(ii) Comprehension of metalinguistic concepts (DVIQ): At 6 years of age, P.I. showed a similar performance to both the TLD group ($t_{(9)}$ = –1.54, p = 0.16) and the SLI group ($t_{(8)}$ = –0.24, p = 0.81) on the comprehension of metalinguistic concepts. At 10 years of age, P.I. again showed a similar performance to the TLD ($t_{(11)}$ = –1.32, p = 0.22) and the SLI groups ($t_{(6)}$ = 0.55, p = 0.60) on this subtest.

(iii) Information (BST): At age 6, there was a non-significant difference for this measure between P.I. and both the TLD ($t_{(9)}$ = –1.22, p = 0.25) and the SLI groups ($t_{(8)}$ = –0.08, p = 0.94). At 10 years of age, P.I. showed non-significant differences compared to the TLD ($t_{(11)}$ = –0.48, p = 0.64) and SLI groups ($t_{(6)}$ = 1.48, p = 0.19).

4.2.2. Expressive Abilities

(i) Vocabulary (EVT and DVIQ): At 6 years of age, P.I. showed a non-significant difference on the EVT compared to both groups of children, those with TLD ($t_{(9)}$ = –1.54, p = 0.16) and those with SLI ($t_{(8)}$ = 1.15, p = 0.28), and the same goes for the vocabulary production subtest of the DVIQ (TLD: $t_{(9)}$ = –1.71, p = 0.12; SLI: $t_{(8)}$ = 0.75, p = 0.48). At 10 years of age, P.I. performed significantly lower than the TLD children on the EVT ($t_{(11)}$ = –3.44, $p < 0.01$) and on the DVIQ ($t_{(11)}$ = –2.82, $p < 0.05$) but similarly to the SLI children for both the EVT ($t_{(6)}$ = –0.53, p = 0.62) and the DVIQ ($t_{(6)}$ = –0.31, p = 0.77).

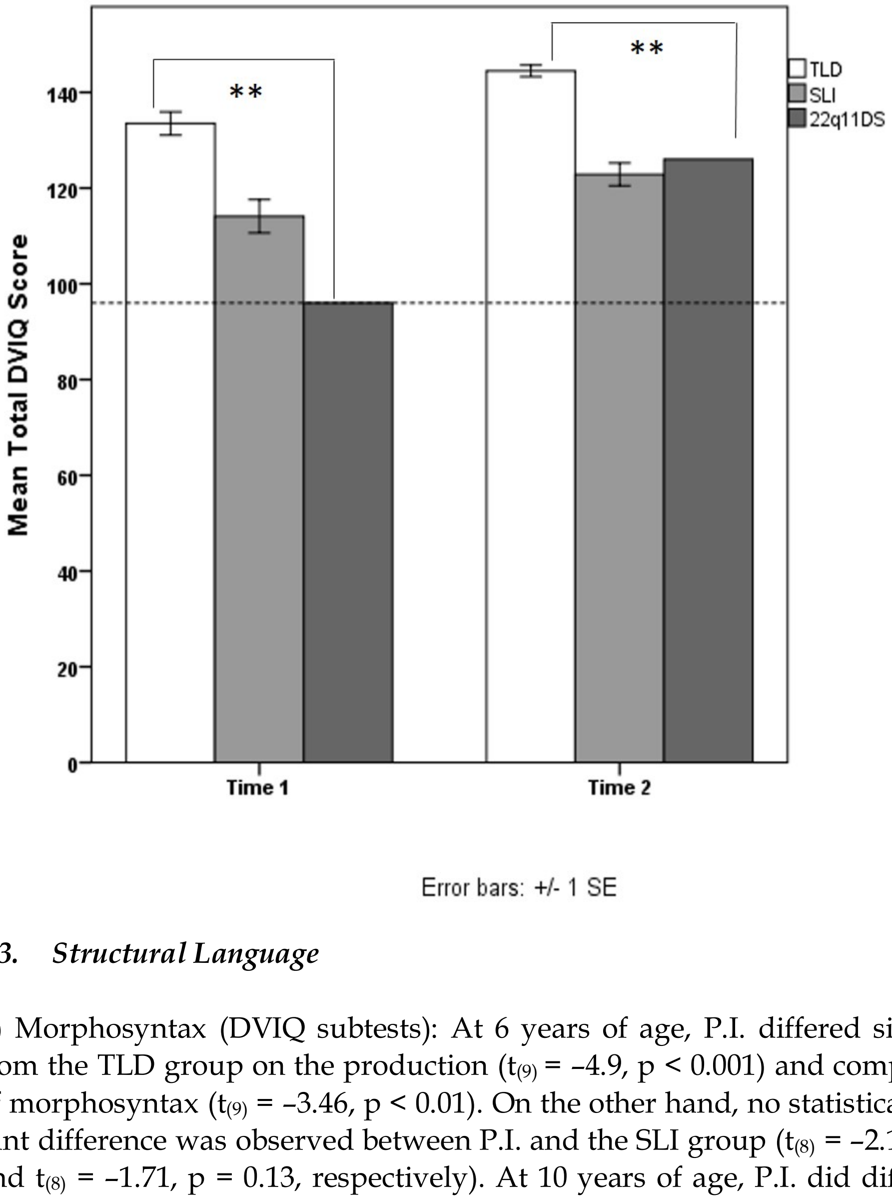
(ii) MLU–word (BST): At 6 years of age, there was a non-significant difference between P.I. and the TLD group for MLU–word ($t_{(9)}$ = –0.77, p = 0.46). When compared to the SLI group, P.I. showed higher performance ($t_{(8)}$ = 3.09, $p < 0.05$). At 10 years of age, however, no significant difference was observed for MLU–word between P.I. and both the TLD group ($t_{(11)}$ = –0.32, p = 0.75) and the SLI group ($t_{(6)}$ = 0.13, p = 0.90).

(iii) Number of sentences (BST): At 6 years of age, P.I. showed a significantly lower performance on the number of sentences produced measured in t-units compared to the TLD group ($t_{(9)}$ = –2.60, $p < 0.05$) but a non-significant performance to the SLI group ($t_{(8)}$ = –1.41, p = 0.20). At 10 years of age, P.I.'s performance was statistically similar to both the TLD group ($t_{(11)}$ = 1.59, p = 0.14) and the SLI group ($t_{(6)}$ = 1.36, p = 0.22).

4.2.3. Total DVIQ Language Score

P.I.'s total DVIQ language quotient score was significantly lower from the TLD groups at both ages (age 6: $t_{(9)} = -4.71$, $p < 0.01$; age 10: $t_{(11)} = -4.23$, $p < 0.01$). In contrast, his language performance did not differ from the SLI groups at either age (age 6: $t_{(8)} = -1.64$, $p = 0.14$; age 10: $t_{(6)} = 0.46$, $p = 0.66$). The results from the DVIQ battery are reported in Figure 2.

Figure 2: Total global language abilities (total DVIQ score) for P.I. (22q11DS), TLD groups, and SLI groups at Time 1 and Time 2.



4.3. *Structural Language*

(i) Morphosyntax (DVIQ subtests): At 6 years of age, P.I. differed significantly from the TLD group on the production ($t_{(9)} = -4.9$, $p < 0.001$) and comprehension of morphosyntax ($t_{(9)} = -3.46$, $p < 0.01$). On the other hand, no statistically significant difference was observed between P.I. and the SLI group ($t_{(8)} = -2.12$, $p = 0.07$ and $t_{(8)} = -1.71$, $p = 0.13$, respectively). At 10 years of age, P.I. did differ significantly from the TLD group on the production ($t_{(11)} = -5.14$, $p < 0.001$) but not on the comprehension of morphosyntax ($t_{(11)} = -1.76$, $p = 0.10$). Once more, no significant difference was observed between P.I. and the SLI group either ($t_{(6)} = -0.28$, $p = 0.79$ and $t_{(6)} = -0.18$, $p = 0.86$, respectively).

(ii) Sentence repetition (DVIQ subtest): At 6 years of age, P.I. differed significantly from the TLD ($t_{(9)} = -3.99$, $p < 0.01$) but only marginally from the SLI group ($t_{(8)} = -2.26$, $p = 0.05$). In contrast, at 10 years of age, he performed similarly to both the TLD group ($t_{(11)} = -1.29$, $p = 0.22$) and SLI group ($t_{(6)} = 1.47$, $p = 0.19$).

(iii) Number of subordinate clauses produced (BST): At 6 years of age, there was a non-significant difference for number of subordinate clauses produced between P.I. and the TLD group ($t_{(9)} = -1.35$, $p = 0.21$) and between P.I. and the SLI group ($t_{(8)} = -0.12$, $p = 0.84$). Also at 10 years of age, no significant difference could be observed for number of subordinate clauses produced between P.I. and the TLD group ($t_{(11)} = 0.00$, $p = 1.00$) or between P.I. and the SLI group ($t_{(6)} = 1.69$, $p = 0.14$).

(iv) Clitic production (CIT): At 6 years of age, P.I. showed similar performance on this task to both children with TLD ($t_{(9)} = -1.98$, $p = 0.08$) and those with SLI ($t_{(8)} = -1.90$, $p = 0.09$). At 10 years of age, P.I. showed similar performance on this task to both children with TLD ($t_{(11)} = 0.46$, $p = 0.66$) and those with SLI ($t_{(6)} = 0.08$, $p = 0.94$).

(v) Relative clauses: Comprehension and production of relative clauses was tested only when P.I. was 10 years old, given the known difficulty of the task with younger TLD children (for Cypriot Greek, using the same tool, see Theodorou & Grohmann 2013). For comprehension, P.I. performed significantly lower than both the TLD group ($t_{(11)} = -2.82$, $p < 0.05$) and the SLI group ($t_{(6)} = -4.28$, $p < 0.01$) on subject relative clauses, but similarly to both groups on object relatives (TLD: $t_{(11)} = -1.00$, $p = 0.34$; SLI: $t_{(6)} = -1.34$, $p = 0.21$). For production, no difference was observed for P.I. compared to both the TLD and the SLI groups, neither for subject (TLD: $t_{(11)} = 0.23$, $p = 0.24$; SLI: $t_{(6)} = 1.50$, $p = 0.19$) nor for object relative clauses (TLD: $t_{(11)} = 0.53$, $p = 0.61$; SLI: $t_{(6)} = 1.15$, $p = 0.29$).

4.4. *Performance on Morphosyntax over Time*

In total, P.I. was tested on five subtests tapping into morphosyntax (three DVIQ subtests, the CIT, and one measure from the BST narrative retell) at 6 years of age (Time 1) and at 10 years of age (Time 2). The results of the two different time points are presented in Table 3.

Structural language measure	**age 6**	**age 10**	**sign test**
DVIQ – Morphosyntax Production	9	15	+
DVIQ – Morphosyntax Comprehension	16	25	+
DVIQ – Sentence Repetition	35	46	+
CIT (production of object clitics)	9	12	+
Number of subordinate clauses produced (BST narrative retell)	2	9	+

Key: DVIQ=Diagnostic Verbal Intelligence Quotient; CIT=Clitics-in-Island-Test; BST=Bus Story Test; +=increase in scores between the two ages.

Table 3: P.I.'s performance on the structural language probes at 6 and 10 years of age, and significance reported by the sign test.

Calculations were based on the assumption that each subtest is independent of the other. The sign test, which is equivalent to the binomial test when the success probability equals 0.5, is used to explain the results. This test uses the binomial distribution to count the number of pairs (x_i, y_i) with the property $y_i–x_i>0$ (positive sign), where x_i denotes the score of P.I. for test i at Time 1 and y_i the corresponding score at Time 2. If for a given subtest the difference is positive, this indicates improvement; if it is negative, this shows a reduction in performance. The number of morphosyntactic subtests that showed improvement at Time 2 follows the binomial distribution with five trials and success probability of 0.5. The null hypothesis—that is, no improvement in morphosyntactic ability—was tested against the alternative hypothesis, that there is indeed improvement (i.e., success probability greater than 0.5). Overall, P.I. scored higher on all morphosyntactic subtests at 10 years of age. This reveals that morphosyntactic abilities significantly improved over time ($p = 0.031, < 0.05$).

5. Discussion

The purpose of the present study was to profile, for the first time, the longitudinal trajectory of language abilities of a Greek-speaking child with 22q11DS across a number of linguistic tools used for research purposes in Cyprus. This allowed a comparison to two groups of children: one group of children with typical language development (TLD) and another with clinically diagnosed specific language impairment (SLI). Of clinical significance was the finding that for P.I., nonverbal intelligence remained stable over time.

As such, and 50 years after Lenneberg's (1967) groundbreaking work, this study may pave the way for more linguistically informed research using a developmental approach in order to understand the connection between genetically-based immuno-deficiency and cognitive–linguistic performance in 22q11DS language. The larger picture this study may allows us to paint and thereby possibly extend concerns the direction suggested by Leivada (2015). The overarching idea, most recently expanded in Leivada et al. (2017), highlights the nature and limits of language variation across child and adult populations as well as pathologies, which we address briefly below. The ultimate question underlying this approach has to do with no less than possible variation or lack thereof in the human language faculty (Tsimpli et al. 2017), a concern already detectable in Lenneberg (1967).

5.1. P.I. Compared to TLD

P.I.'s global language performance was impaired compared to TLD peers at both 6 (Time 1) and 10 years of age (Time 2), respectively, as probed by the DVIQ battery (five subtests) used for language diagnostic practice in Cyprus (Theodorou 2013, Theodorou et al. 2014, 2016). This finding lends support to the claim that language impairment is evident during both the preschool and primary school years in 22q11DS (Persson et al. 2006). In contrast, receptive language abilities (PPVT, comprehension of metalinguistic concepts, and Information score on the BST) did not significantly differentiate P.I. from TLD groups.

One possible explanation is that receptive abilities are an area of strength for P.I. (as suggested for 22q11DS by Persson et al. 2006), but this is not a commonly reported finding in 22q11DS (Glaser et al. 2002). Similarly, expressive language abilities as measured for spoken vocabulary (EVT and DVIQ Vocabulary subtest) at Time 1 were comparable to TLD peers (6 years of age) but not at Time 2, where P.I. performed significantly lower than TLD (10 years of age). In fact, P.I.'s performance on expressive vocabulary remained identical over time revealing a plateau effect.

This finding is consistent with past reports that vocabulary is a vulnerable domain in 22q11DS, and school-aged children with 22q11DS continue to struggle with word learning (Solot et al. 2000, Persson et al. 2006). Moreover, on the BST only the number of sentences produced was significantly lower for P.I. compared to TLD peers, and only at 6 years of age (Time 1), while MLU–word remained on par with TLD peers. Regarding morphosyntactic abilities as measured by the number of subordinate clauses produced on the BST, there were no significant differences between P.I. and TLD children. The BST findings in this study are not in line with what was reported for the Swedish 22q11DS cohort (Persson et al. 2006): The majority of children showed a low information score, lower number of subordinate clauses, and shorter sentence length than expected according to the Swedish BST norms.

Furthermore, in relation to structural language as measured by abilities in morphosyntax, comprehension was significantly lower for P.I. compared to TLD children at 6 years of age (Time 1) but comparable by 10 years of age (Time 2). In contrast, compared to TLD peers, production of morphosyntax was significantly impaired at 6 years of age and remained so at 10 years of age. Finally, the only other morphosyntactic measures that differentiated P.I. from TLD children were the sentence repetition subtest of the DVIQ at 6 years of age only and the comprehension of subject relative clauses in which he performed significantly worse.

The above findings corroborate earlier clinical reports that impairments in morphosyntax and complex language are robust, non-language specific features of 22q11DS (Solot et al. 2000, Goorhuis-Brower et al. 2003, Persson et al. 2006).

5.2. P.I. Compared to SLI

By comparing our participant to a group of children with SLI, that is, children with known profiles of speech and language difficulties, allows us to decipher whether the profile of 22q11DS is distinctive to the syndrome or not. Global or core language abilities (total DVIQ score) did not differentiate P.I. from the SLI groups, neither at 6 (Time 1) nor at 10 years of age (Time 2). This was also the case for receptive language abilities and for all expressive language measures—with the exception of MLU–word, where P.I. produced significantly more words than his SLI peers only at 6 years of age. In a similar vein, for structural language, P.I. performed significantly worse compared to the SLI group on sentence repetition (DVIQ subtest)—but only at 6 years of age, and only on the comprehension of subject relative clauses.

5.3. *22q11DS Morphosyntax over Time*

Putting together the results of both comparative group (TLD and SLI), P.I. did perform significantly worse on two subtests from the DVIQ, production of morphosyntax and sentence repetition, and on comprehension of subject relative clauses. Unfortunately, we do not have a solid analytical knowledge base for the relevance of complex language stemming from the DVIQ subtests as markers of language difficulties. This is to say that we can describe the performance by individuals and groups, but we cannot yet pinpoint the source of deviations from the norm. Nevertheless, P.I.'s performance on the structural language probes revealed statistically significant differences over time (cf. Table 2), suggesting improvement in morphosyntactic abilities with increasing age.

This is a most encouraging finding. We can only speculate that the kind of language impairment found in 22q11DS is qualitatively different from SLI (see also right below). If so, parental language input coupled with regular schooling and specialist services do make a difference. The results suggest continued maturation of certain aspects of the language acquisition process with improvements in the 22q11DS child's language performance, even if language competence remains low.

22q11DS presents an interesting syndrome for further probing the biological underpinnings of language. One central issue concerns the invariance of the human language faculty (for recent discussion, see Tsimpli et al. 2017). According to the Locus Preservation Hypothesis (Leivada et al. 2017), operations in the computational system are not expected to be subject to impairment. This means that a 'small UG' in the sense of Hauser et al. (2002) is compatible with both invariance of the language faculty and the Locus Preservation Hypothesis, and it can address particular morphosyntactic problems in syndromes such as 22q11DS as well (see also below). This is very much work in progress, as can be witnessed from the very recent talks by Grohmann (2017) and Hinzen (2017).

5.4. *The Interesting Case of Early Clitic Production*

Qualitatively, there were differences between P.I., the TLD groups, and the SLI groups with clitic use probed by the CIT, a sentence completion task developed to prompt production of object clitics. The relevance of clitic production and their placement in the context of first language acquisition of Cypriot Greek has been highlighted in recent research (summarized in Grohmann 2014). P.I. behaved differently from both children with TLD and children with SLI by producing fewer clitics at 6 years of age, but this was statistically non-significant. However, he showed more omissions than either group, a phenomenon which is rare even for children with SLI (Theodorou & Grohmann 2015). Clitic production vis-à-vis omission has been taken as a clinical marker for SLI in other languages, though it is unlikely to be a clinical marker for SLI in Cypriot Greek (see Theodorou & Grohmann 2015)—and, if Leivada et al. (2017) are right, it does not tell us much about an underlying deficiency in children's language capacity either. However, at the age of 10 years, clitic production was within normal limits for P.I.

5.5. *22q11DS vs. SLI Debate*

Our final aim is to tentatively use our findings to shed light on the 22q11DS±SLI debate as reported in the 22q11DS literature and outlined in the introduction. One informative approach for a more general notion of language impairment is to compare NVIQ and the linguistic performance outcomes of children with SLI to our participant with 22q11DS.

Implementing the intriguing conceptualization suggested by Rice (2016), a first comparison can be summarized as a 2×2 design with four cells of interest identified as 'A', 'B', 'C', or 'D' (as in Table 4). If P.I. has concurrent SLI (cell A), he could be compared to children with SLI who do not have 22q11DS (cell B), to children with 22q11DS who do not have SLI (cell C), and to children with TLD without either condition (cell D).

		22q11DS	
		+	–
SLI	+	A	B
	–	C	D

Table 4: 2−2 design comparison for 22q11DS and SLI (22q11DS±SLI).

At this point, we can only speculate that if A=C and A≠B, it would suggest a distinctive linguistic profile contributing to 22q11DS but not SLI. In our view, P.I.'s language-specific symptoms suggest that it is the 22q11DS variant which is the common element and that this variant is not diagnostic of SLI (cell C). This would be in line with the conclusion reached by Persson et al. (2006). Within the larger context of Lenneberg (1967), it might also contribute to a better understanding of the above-mentioned connection between genetically-based immunodeficiency and cognitive–linguistic performance in 22q11DS language.

5.6. *Study Limitations*

This study was a preliminary investigation of the language profile of 22q11DS compared to children with SLI (as well as typically developing control groups). While the study presents data that support further research using a comparison group of children with SLI, several limitations were apparent based on the small number of participants. Furthermore, we had no SLI child in our database with P.I.'s chronological age and home background for Time 2. This precludes big generalizations for the different populations as a whole.

However, the results of this study indicate the potential benefits of research with larger numbers of children with 22q11DS and SLI in order to tease apart the cognitive and linguistic profiles of each group. Future work will also need to focus on investigating higher-order cognitive skills like executive functions, a proven area of relative weakness according to new research in 22q11DS (Maeder et al. 2016).

5.7. *Study Implications*

Seen from the perspective of a larger research agenda, further exploring the exact deficits in language and cognition presented by pathologies like 22q11DS contributes to the growing research interest in *comparative biolinguistics* (Wildgen 2008, Benítez-Burraco & Boeckx 2014, Kambanaros & Grohmann 2015, among others). This program of research investigates similarities and, especially, differences in specific tasks and abilities across different pathologies, from developmental language impairments and acquired language disorders to apparently non-linguistic pathologies, that is, those that are not primarily connected to language. By so doing, we may be able to shed light on the assumed invariance of the human language faculty (cf. Grohmann 2017, Hinzen 2017, Tsimpli et al. 2017), perhaps even "uncover the locus of variation (and its constraints) across genotypes, pathologies, or across species" (Leivada 2014: 54; see the more recent Leivada et al. 2017). The present research contributes to this endeavor.

What this means is that the question of how language pathologies may inform the human language faculty in the light of Universal Grammar (UG) and vice versa receives a new twist—and it gives rise to interesting new questions (Tsimpli et al. 2017). Regardless of the outcome of these developments, UG viewed from the perspective of language pathology may open new windows into the human faculty of language as conceived today, independently of whether we assume a full-fledged faculty of language in the traditional sense ('big UG'), a highly reduced one ('small UG'), or the distinction between the faculty of language in the broad vs. narrow sense (Hauser et al. 2002)—windows that may not have been available in earlier stages of theoretically informed language research. As Tsimpli et al. (2017) put it (see also Grohmann 2017), one primary aim would be to obtain distinctive linguistic profiles regarding, say, lexical and grammatical abilities and at the same time develop cognitive profiles across a range of genetically and non-genetically different populations who are monolingual, multilingual, or somewhere in between as well as populations with or without co-morbid linguistic and/or cognitive impairments as part of their genotype.

While individual variability is clinically crucial, population-based research can advance further (cognitive–)linguistic theorizing through behavioral testing that acknowledges the brain bases involved. This will offer a unique opportunity to researchers to collaborate in fields as different as (but not restricted to) genetic biology, neurobiology of the brain, cognitive neuroscience, cognitive and developmental psychology, speech–language pathology, psycho-, neuro-, and clinical linguistics, and language development—as well as theoretical linguistics.

In addition, it may inform better about the underlying faculty(s) involved, of particular concern, of course, the role of UG in pathology. Some recent work goes in this direction, if only partially, such as emergent perspectives on autism phenotypes (Bourguignon et al. 2012), the biological nature of human language and the underlying genetic programs (Di Sciullo et al. 2010), or the idea that syntactic networks may constitute an endo-phenotype of developmental language disorders (Barceló-Coblijn et al. 2015). And if the limited research on cognitive–linguistic performance in 22q11DS reported here is on the right track, this syndrome may be very fruitful for future insights as well.

Finally, there is clinical relevance for speech pathologists to recognize the communication and language symptoms of children with 22q11DS, and to be aware of differentiating characteristics between 22q11DS, SLI, and TLD. This will facilitate improved clinical guidelines for identification and treatment of children with 22q11DS. Given the limited research regarding language function in 22q11DS to date, this is not only a first case study for (Cypriot) Greek; it also addresses larger issues of language ability in 22q11DS with respect to adaptive functioning. Overall, the findings are relevant to clinical practice by demonstrating the value of language profiling in characterizing the pattern of language impairment, with the ultimate aim of developing appropriate treatment plans.

6. Conclusion

The purpose of the present study was to provide evidence for the language profile of 22q11DS. Based on the findings of a single case, we opt for a *distinctive language profile* of 22q11DS in comparison to specific language impairment. However, further research is needed to decide on the final outcome. In that respect, we do hope that our findings provide awareness of 22q11DS. They surely constitute a first contribution to the knowledge base of the behavioral language phenotype for (Cypriot) Greek, even if only based on a single case.

There is no doubt that care of children with 22q11DS is multidisciplinary and a lifelong requirement. Early recognition is of paramount importance to improve cognitive communication skills and ultimately quality of life. Beyond that, it is very well possible that the language-based multidisciplinary research activities suggested here for the future might shed more light on the underlying questions concerning the invariance of the human language faculty across populations and syndromes (Tsimpli et al. 2017), the purported preservation of the computational system (Leivada et al. 2017), and the biological underpinnings of language today, in the 50th anniversary year of the first concrete proposals (Lenneberg 1967).

Appendix: Summary of published research on language impairment in children with 22q11DS.

Study	**Solot et al. (2000)**	**Solot et al. (2001)**	**Gerdes et al. (2001)**	**Scherer et al. (2001)**
No. of participants	305	79	112	4
Language of investigation	English	English	English	English
Age range	≤ 5	0;7–16;7	0;4–6;0	2;0–4;6
IQ range	*not provided*	For preschool children: Bayley Scales of Infant Development (BSID; *mental scale score*) 68.6 ± 13.3 and WIPSI: 84.5 ± 10.4 For school-aged children: WISC-III, VIQ: 77.8 ± 13.6, PIQ: 71.7 ± 12.8, FSIQ: 73.0 ± 12.6	For children ≥4 WPPSI-R (mean Full-scale IQ: 78 ± 12, mean Performance IQ 78 ± 12, mean Verbal IQ 81 ± 13)	BSID–2 (*mental scale score)* for VCFS (range: 50–81) and DS (range: 45–62)
Language testing	a. Preschool Language Scales–3 (PLS–3) b. Clinical Evaluation of Language Fundamentals–Revised (CELF–R) c. Goldman–Fristoe Test of Articulation d. Peabody Picture Vocabulary Test–Revised e. Expressive One Word Vocabulary Test–Revised	a. PLS–3 b. CELF–R	PLS–3, two subtests: 1. Auditory Comprehension 2. Expressive Communication	a. Sequenced Inventory of Communicative Development–Revised b. 30-minute language samples (SALT) c. CDI completed by the parents d. Speech sound analyses (MBL calculated)
Linguistic deficit	1. severe delays in expressive language in 53% of the children 2. receptive language delays in 25% of the children 3. difficulties in a variety of linguistic domains (syntax, vocabulary, concepts, word-finding, discourse organization)	1. expressive language skills significantly worse than receptive language 2. SLI pattern in 36% of the school-aged children 3. speech abnormalities of varying kinds in 75.9 % of the school-aged children	1. delayed language emergence 2. voice quality disturbances 3. low facial tone 4. articulation errors 5. dysarthria	1. fewer different words and sounds used by children with VCFS than children with DS 2. a number of different sound classes used by children with DS vs. limited sound categories used by children with VCFS 3. smaller vocabulary size for VCFS vs. DS (CDI)
Conclusion	In children with the 22q11.2 microdeletion, the emergence of language is delayed until the age of 2–3 years of age. Some children present persistent developmental delays (into school-years): These delays cannot be explained by cognitive factors, but as the **presence of specific speech and language impairment**. Presence/absence of cardiac or palatal abnormalities: no effect on development outcome.	An **SLI pattern of disorder**: Children with 22q11.2 microdeletion syndrome present (a) delayed emergence of language and (b) persistent speech and language disorders.	Speech and language delays become obvious from the first year of life in almost all children with the deletion of the 22q11.2 chromosome. These children **present a very complex developmental disorder** (including, cognitive, and language delays, as well as behaviour abnormalities).	The communicative profiles of children with VCFS vs. age-matched children with DS are different. The overall performance of children with DS is analogous to their mental-matched peers, whereas overall children with VCFS **present severe deficits** in early vocabulary acquisition and speech sound production.

Study	**Glaser et al. (2002)**	**Goorhuis-Brouwer et al. (2003)**	**Persson et al. (2006)**
No. of participants	27	4	19
Language of investigation	English	Dutch	Swedish
Age range	6;0–19;0	5;0–6;8	4;11–8;5
IQ range	Mean full scale IQ of 69.4 (SD 16.4), range: 40–105	Nonverbal IQ (normal range: 90–112)	Mean full scale IQ of 78 (range: 57–102)
Language testing	a. Receptive Vocabulary (Concept and Directions, Word Classes, Sentence Structure (6–8y.o.) or Semantic Relationships (≥9y.o.)) b. Expressive Language (Formulated Sentences, Recalling Sentences, Word Structure (6–8y.o.) or Sentence Assembly (≥9y.o.)) c. Oral Test of Word Association (letter-naming and semantic trials)	a. Language Comprehension (Dutch version of the Reynell Test of Language Development) b. Language production (Test for Sentence Development) c. Spontaneous speech sample d. Articulation and DDK rates	a. Receptive vocabulary (Swedish version of the PPVT) b. Narrative retell abilities (Swedish version of the BST) c. Articulation
Linguistic deficit	1. receptive vocabulary scores significantly lower than expressive language scores in children with VCFS, the exact opposite in the DD group 2. similar performance in the WA test for VCFS, DD, and TD groups, with semantic sores lower than the letter-naming scores	1. long sentences produced *or* 2. two- and three-word utterances produced only *or* 3. primarily nonverbal communication	1. lower scores on receptive vocabulary than expected according to NVIQ 2. difficulties in retelling a narrative (information, sentence length, number of subordinate clauses produced)
Conclusion	**Unique Developmental Patterns:** As children with VCFS get older, their expressive language skills continue to improve. MRI supports the evidence for better receptive language than expressive language skills. The weakness of both VCFS and DD is interpreted as an outcome of the general cognitive impairment.	**Children were characterized as SLI:** phonological programming deficit syndrome (2/4) or verbal dyspraxia (2/4)	**Non-SLI:** Language impairment is neutral to the issue of delay vs. disorder. The 22q11DS group had a history of recurrent otitis media and hearing loss, behavioural difficulties including ASD and ADHD, a specific behavioural phenotype, and palatal/velopharyngeal anomalies.

References

Allgrove, J., Baker, K., Baxendale, H., Bu'Lock, F., de Waardt, D., Firth, H., Gennery, A., Habel, A., Herriot, R., Holland, A., Illingworth, C., Kumararatne, D., Mercer, N., Pannebakker, M., Parry, A., Roberts, A., and Tsai-Goodman, B. (2012). Consensus document on 22q11 deletion syndrome (22q11DS). Bristol: Max Appeal!.

American Speech-Language-Hearing Association (ASHA). (2015). Genetics in the practice of speech–language pathology and audiology: A Resource. [http://www.nchpeg.org/shla/index.php?option=com_content&view=article&id=19&itemid=27]

Barceló-Coblijn, L., Benítez-Burraco, A., and Irurtzun, A. (2015). Syntactic networks as an endophenotype of developmental language disorders: An evo-devo approach to clinical linguistics. *Biolinguistics*, 9, 43-49.

Benítez-Burraco, A., and Boeckx, C. (2014). Universal Grammar and biological variation: An EvoDevo agenda for comparative biolinguistics. *Biological Theory, 9*, 122-134.

Bishop, D. V. M. (2014). Ten questions about terminology for children with unexplained language problems. *International Journal of Language and Communication Disorders*, *49*(4), 381-415.

Bourguignon, N., Nadig, A., and Valois, D. (2012). The biolinguistics of autism: Emergent perspectives. *Biolinguistics*, 6, 124-165.

Crawford, J. R., and Howell, D. C. (1998). Comparing an individual's test score against norms derived from small samples. *The Clinical Neuropsychologist, 12*, 482-486.

De Decker, H. P., and Lawrenson, J. B. (2001). The 22q11.2 deletion: From diversity to a single gene theory. *Genetics in Medicine, 3*(1), 2-5.

Di Sciullo, A. M., Piattelli-Palmarini, M., Wexler, K., Berwick, R. C., Boeckx, C., Jenkins, L., Uriagereka, J., Stromswold, K., Cheng, L.L.-S., Harley, H., Wedel, A., McGilvray, J., van Gelderen E., and Bever, T. G. (2010). The biological nature of human language. *Biolinguistics*, 4, 4-34.

Dunn, L. M., and Dunn, L. M. (1981). *Peabody picture vocabulary test*. Circle Pines, MN: American Guidance Service.

European Social Survey (2010). *Round 5 source showcards*. London: Centre for Comparative Social Surveys, City University London.

Friedmann, N., and Novogrodsky, R. (2004). The acquisition of relative clause comprehension in Hebrew: A study of SLI and normal development. *Journal of Child Language*, *31*, 661-681.

Gerdes, M., Solot, C., Wang, P. P., McDonald-McGinn, D. M., and Zackai, E. H. (2001). Taking advantage of early diagnosis: Pre-school children with 22q11.2 deletion. *Genetics in Medicine, 3*(1), 40-44.

Glaser, B., Mumme, D. L., Blasey, C., Morris, M. A., Dahoun. S. P., Antonarakis, S. E., Reiss, A. L., and Eliez, S. (2002). Language skills in children with velocardiofacial syndrome (deletion 22q11.2). *Journal of Pediatrics, 140*(6), 753-758.

Goorhuis-Brouwer, S. M., Dikkers, F. G., Robinson, P. H., and Kerstjens-Frederikse, W. S. (2003). Specific language impairment in children with velocardiofacial syndrome: Four case studies. *Cleft Palate-Craniofacial Journal, 40*(2), 190-195.

Grohmann, K. K. (2014). CAT research on object clitic placement: Where we are now. In: K. K. Grohmann and T. Neokleous (Eds.), *Developments in the acquisition of clitics* (pp. 1-40). Newcastle-upon-Tyne: Cambridge Scholars Publishing.

Grohmann, K. K. (2017). 'Biolinguistic considerations of the language faculty across cognitive–linguistic phenotypes'. Keynote at the *2nd International Biolinguistics Conference*. Tsinghua University, Beijing. (December 8–10)

Grohmann, K. K., and Kambanaros, M. (2016). The gradience of multilingualism in language development: Positioning bilectalism within comparative bilingualism. *Frontiers in Psychology: Language Sciences, 7*: 37, doi:10.3389/ fpsyg.2016.00037.

Hauser, M. D., Chomsky, N., and Fitch, W. T. (2002). The faculty of language: What is it, who has it, and how did it evolve? *Science*, 298, 1569-1579.

Hinzen, W. (2017). 'Variation in language capacity across clinical populations'. Keynote address at the *2nd International Biolinguistics Conference*. Tsinghua University, Beijing. (December 8–10)

Kambanaros, M., and Grohmann, K. K. (2015). Grammatical class effects across impaired child and adult populations. *Frontiers in Psychology: Language Sciences, 6*: 1670, doi:10.3389/fpsyg.2015.01670.

Kambanaros, M., Taxitari, L., Theodorou, E., Varnava, M., and Grohmann, K. K. (2018). Language impairment in 22q11.2 deletion syndrome: A case study in Greek. In: E. Babatsouli, D. Ingram, and N. Müller (Eds.), *Crosslinguistic encounters with language acquisition: Typical and atypical development* (pp. 197–226). Bristol: Multilingual Matters.

Leivada, E. (2014). From comparative linguistics to comparative (bio)linguistics: Reflections on variation. *Biolinguistics, 8*, 53-66.

Leivada, E. (2015). The nature and limits of variation across languages and pathologies. Barcelona: University of Barcelona dissertation.

Leivada, E., Kambanaros, M., and Grohmann, K. K. (2017). The Locus Preservation Hypothesis: Shared linguistic profiles across developmental disorders and the resilient parts of the human language faculty. *Frontiers in Psychology* 8: 1765, doi: 10.3389/fpsyg.2017.01765.

Lenneberg, E. (1967). *Biological foundations of language*. New York: John Wiley.

Maeder, J., Schneider, M., Bostelmann, M., Debbane, M., Glaser, B., Menghetti, S., Schaer, M., and Eliez, S. (2016). Developmental trajectories of executive functions in 22q11.2 deletion syndrome. *Journal of Neurodevelopmental Disorders*, 8: 10, doi:10.1186/511689-016-9141-1.

Niklasson, L., Rasmussen, P., Óskarsdóttir, S., and Gillberg, C. (2001). Neuropsychiatric disorders in the 22q11 deletion syndrome. *Genetics in Medicine, 3*(1), 79-84.

Novogrodsky, R., and Friedmann, N. (2006). The production of relative clauses in syntactic SLI: A window to the nature of the impairment. *International Journal of Speech and Language Pathology, 8*, 364-375.

Panhellenic Association of Logopedists (1995). *Assessment of phonetic and phonological development*. Athens: PAL.

Persson, C., Niklasson, L., Óskarsdóttir, S., Johansson, S., Jönsson, R., and Söderpalm, E. (2006). Language skills in 5–8 year old children with 22q11 deletion syndrome. *International Journal of Language and Communication Disorders, 41*(3), 313-333.

Raven, J., Raven, J. C., and Court, J. H. (2000). *Manual for Raven's progressive matrices and vocabulary scales.* San Antonio, TX: Harcourt Assessment.

Renfrew, C. (1997). *The Renfrew language scales*, 4th edn. Milton Keynes: Speechmark.

Rice, M. (2016). Specific language impairment, nonverbal IQ, attention-deficit/hyperactivity disorder, autism spectrum disorder, cochlear implants, bilingualism and dialectal variants: Defining the boundaries, clarifying clinical conditions, and sorting out cases. *Journal of Speech, Language and Hearing Research, 59,* 122-132.

Rowe, C., and Grohmann, K. K. (2013). Discrete bilectalism: Towards co-overt prestige and diglossic shift in Cyprus. *International Journal of the Sociology of Language, 224*, 119-142.

Scandurra, V., Scordo, M. R., Canitano, R., and de Bruin, E. I. (2013). 22q11 deletion syndrome and multiple complex developmental disorder: A case report. *Journal of Clinical Medical Research, 5*(2), 135-139.

Scherer, N. J., D'Antonio, L. L., and Rodgers, J. R. (2001). Profiles of communication disorder in children with velocardiofacial syndrome: Comparison to children with Down syndrome. *Genetics in Medicine, 3*(1), 72-78.

Shprintzen, R. J. (2008). Velocardiofacial syndrome: 30 years of study. *Developmental Disability Research Reviews, 14*(1), 3-10.

Sideridis, G., Antoniou, F., Mouzaki, A., and Simos, P. (2015). *Raven's coloured progressive matrices and vocabulary* [in Greek]. Athina: Motivo.

Simos, P. G., Kasselimis, D., and Mouzaki, A. (2011). Age, gender, and education effects on vocabulary measures in Greek. *Aphasiology, 25*, 475-491.

Solot, C. B., Handler, S. D., Gerdes, M., McDonald-McGinn, D. M., Moss, E., Wang, P., Cohen, M., Randall, P., Larossa, D., and Driscoll, D. A. (2000). Communication disorders in the 22q11.2 microdeletion syndrome. *Journal of Communication Disorders, 33*, 187-204.

Solot, C. B., Gerdes, M., Kirschner, R. E., McDonald-McGinn, D. M., Moss, E., Woodin, M., Aleman, D., Zackai, E. H., and Wang, P. P. (2001). Communication issues in 22q11.2 deletion syndrome: Children at risk. *Genetics in Medicine, 3*(1), 67-71.

Squarcione, C., Torti, M. C., Di Fabio, F., and Biondi, M. (2013). 22q11 deletion syndrome: A review of the neuropsychiatric features and their neurobiological basis. *Neuropsychiatric Disease and Treatment, 9*, 1873-1884.

Stavrakaki, S., and Tsimpli, I. M. (2000). Diagnostic verbal IQ test for Greek preschool and school age children: Standardization, statistical analysis, psychometric properties [in Greek]. In: M. Glykas and G. Kalomiris (eds.), *Proceedings of the 8th Symposium of the Panhellenic Association of Logopedists* (pp. 95-106). Athens: Ellinika Grammata.

Stromswold, K. (1998). Genetics of spoken language disorders. *Human Biology, 70,* 297-324.

Stromswold, K. (2001). The heritability of language: A review and meta-analysis of twin, adoption and linkage studies. *Language, 77*, 647-723.

Stromswold, K. (2006). Why aren't identical twins linguistically identical? Genetic, prenatal and postnatal factors. *Cognition, 101,* 333-384.

Theodorou, E. (2013). Specific language impairment in Cypriot Greek: Diagnostic and experimental investigations. Nicosia: University of Cyprus dissertation.

Theodorou, E., and Grohmann, K. K. (2010). Narratives in Cypriot Greek mono- and bilingual children with SLI. In: A. Botinis (ed.), *Proceedings of ISCA Tutorial and Research Workshop on Experimental Linguistics 2010—25–27 August 2010, Athens, Greece* (pp. 185-188). Athens: ISCA & University of Athens.

Theodorou, E., and Grohmann, K. K. (2013). The acquisition of relative clauses in Cypriot Greek: Production and comprehension. *Diacrítica, 26*(1), 271-300.

Theodorou, E., and Grohmann, K. K. (2015). Object clitics in Cypriot Greek children with SLI. *Lingua, 161*, 144-158.

Theodorou, E., Kambanaros, M., and Grohmann, K. K. (2014). Specific language impairment in Cypriot Greek: Diagnostic issues. *Linguistic Variation, 13*(2) [2013], 217-236.

Theodorou, E., Kambanaros, M., and Grohmann, K. K. (2016). Diagnosing bilectal children with SLI: Determination of identification accuracy. *Clinical Linguistics & Phonetics*, doi:10.1080/02699206.2016.1182591.

Tsimpli, I. M., Kambanaros, M., and Grohmann, K. K. (2017). Language pathology. In: I. G. Roberts (Ed.), *The Oxford handbook of Universal Grammar* (pp. 486-508). Oxford: Oxford University Press.

Varlokosta, S., Belletti, A., Costa, J., Friedmann, N., Gavarró, A., Grohmann, K. K., Guasti, M. T., Tuller, L., et al. (2016). A cross-linguistic study of the acquisition of clitic and pronoun production. *Language Acquisition, 23*(1), 1-26.

Vogindroukas, I., Protopapas, A., and Sideris, G. (2009). *Word-finding test* [in Greek]. Chania: Glafki.

Wildgen, W. (2008). Sketch of an evolutionary grammar based on comparative biolinguistics. In: L. S. Röska-Hardy and E. M. Neumann-Held (Eds.), *Learning from animals? Examining the nature of human uniqueness* (pp. 45-59). Hove: Psychology Press.

Woodin, M., Wang, P. P., Aleman, D., McDonald-McGinn, D., Zackai, E., and Moss, E. (2001). Neuropsychological profile of children and adolescents with the 22q11.2 microdeletion. *Genetics in Medicine, 3*(1), 34-39.

Maria Kambanaros
Cyprus University of Technology
Department of Rehabilitation Sciences
15 Vragadinou
3041 Limassol
Cyprus
maria.kambanaros@cut.ac.cy

Kleanthes K. Grohmann
University of Cyprus & CAT
Department of English Studies
75 Kallipoleos, P.O. Box 20537
1678 Nicosia
Cyprus
kleanthi@ucy.ac.cy

Lenneberg's Contributions to the Biology of Language and Child Aphasiology: Resonation and Brain Rhythmicity as Key Mechanisms

Koji Hoshi

This paper aims to re-evaluate the legacy of Eric Lenneberg's monumental *Biological Foundations of Language*, with special reference to his biolinguistic framework and view on (child) aphasiology. The argument draws from the following concepts from Lenneberg's work: (i) language (latent structure vs. realized structure) as independent of externalization; (ii) resonance theory; (iii) brain rhythmicity; and (iv) aphasia as temporal dysfunction. Specifically, it will be demonstrated that Lenneberg's original version of the critical period hypothesis and his child aphasiology lend themselves to elucidating a child aphasia of epileptic origin called Landau-Kleffner syndrome (LKS), thereby opening a possible hope for recovery from the disease. Moreover, it will be claimed that, to the extent that the language disorder in LKS can be couched in these terms, it can serve as strong "living" evidence in support of Lenneberg's critical period hypothesis and his view on child aphasiology.

Keywords: (child) aphasiology; brain rhythmicity; critical period hypothesis; latent and realized structures; resonance theory

1. Introduction

Boeckx & Longa aptly and succinctly describe the value of Eric Lenneberg's pioneering and seminal work *Biological Foundations of Language* published in 1967 as being "regarded as a classic" and add:

> Like all classics, it deserves to be re-read at regular intervals, not only to appreciate the success (and limitations) of previous attempts at a synthesis among fields, but also to learn things that we all too often forget.
>
> (Boeckx & Longa 2011: 255)

I would like to express my sincere gratitude to two anonymous *Biolinguistics* reviewers for encouraging comments on the earlier version of this paper. I am also very grateful to Kyoko Miyazato for valuable discussion on the content and useful suggestions for the organization of the whole paper. Thanks are due to Michael Ainge for carefully proofreading the earlier version of this paper. Finally, I would like to thank Patrick C. Trettenbrein for editorial help and suggestions as the managing editor for this special issue. All remaining inadequacies are solely my own.

biolinguistics

ISSN 1450–3417
Biolinguistics 11.SI: 83–113, 2017
http://www.biolinguistics.eu

It is not an exaggeration to claim that Lenneberg's book set the highest standard of interdisciplinary biolinguistic investigation and has served as an unmatched exemplar in the field of biolinguistic research broadly construed. Even a cursory look at the table of contents of the book does not fail to strongly impress us with the fact that Lenneberg already covered virtually all the relevant topics that scholars in the current field of biolinguistics are still actively pursuing (see, e.g., Jenkins 2000, Boeckx & Grohmann 2013). The issues explored are the anatomy of the human body and brain, language and cognition, language evolution and genetics, the critical period for first language development, and language disorders—among many others.

One of Lenneberg's most significant contributions to the field of biolinguistics in particular and to the field of neurosciences in general is his insight into the significance of studying cognitively handicapped children including child aphasics as a means of uncovering the nature of human language and its brain mechanisms (see Lenneberg 1967: 304–326). Most concisely summarised in the following passage:

> It is often said that it is difficult enough to understand the development of behavior in the healthy individual and that we should, therefore, not complicate our task by trying to understand at the same time behavioral development in the presence of disease. Such a statement is based on the false assumption that disease results in more complicated behavior. However, we may consider it axiomatic that disease processes *do not usually add to the complexity of structure* of behavior.
>
> (Lenneberg 1967: 304–305; emphasis in original)

Accordingly, the main purpose of this paper is to re-visit Lenneberg's original version of the critical period hypothesis and his view on (child) aphasiology, which have been often forgotten, sometimes ignored or even misunderstood, in order to highlight their importance, relevance, and validity in exploring the nature of a certain child aphasia of epileptic origin called Landau-Kleffner syndrome (LKS; Landau & Kleffner 1957). It will be argued that LKS provides strong "living" evidence demonstrating the validity of Lenneberg's original version of the critical period hypothesis and his view on (child) aphasiology, to the extent that they are conducive to illuminating the very nature of the language disorder in LKS.

The structure of this paper is as follows: Section 2 deals with Lenneberg's view on human language, addressing the two fundamental underlying assumptions: brain-internal language as language capacity and its independence from externalization. I will also review some crucial concepts closely related to these assumptions: Lenneberg's first language development model, critical period hypothesis, and brain rhythmicity for speech production. Section 3 then is more specifically concerned with Lenneberg's view on child aphasia and its application to LKS. I will argue that Lenneberg's critical period hypothesis and child aphasiology can benefit our understanding the nature of the language disorder in LKS, opening up a novel possibility for an effective non-invasive medical intervention. Section 4 concludes.

2. Language Capacity and Externalization

2.1. Lenneberg's Critical Period Hypothesis and Related Concepts

In this subsection, I will lay out the two fundamental assumptions that underlie Lenneberg's (1967) version of the critical period hypothesis: (i) brain-internal language as language capacity; (ii) language capacity as independent from its externalization.

2.1.1. Brain-Internal Language as Language Capacity

Lenneberg conceives of human language as a brain-internal biological system and distinguishes between two biological levels of human language: latent structure and realized structure, explaining that

> the unfolding of language is a process of actualization in which latent structure is transformed into realized structure. The actualization of latent structure to realized structure is to give the underlying cognitively determined type a concrete form. (Lenneberg 1967: 376)

This process can be illustrated as in figure 1.[1]

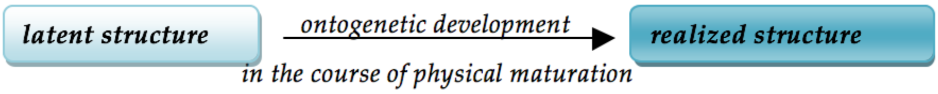


Figure 1: The actualization process from latent structure to realized structure in accordance with Lenneberg's (1967) formulation.

Crucially, he notes that

> [t]he actualization process is not the same as 'beginning to say things.' In fact, it may be independent from certain restraints that are attending upon the capacity for making given responses. Actualization may take place even if responses are peripherally blocked; in this case actualization is demonstrable only through signs of understanding language.
>
> (Lenneberg 1967: 376)

Let me emphasize at this juncture that this point is extremely important in correctly understanding a certain childhood aphasia of epileptic origin which I will be addressing in section 3.

---

[1] Lenneberg states the following in a footnote on the same page:

> This formulation might be regarded as the biological counterpart to what grammarians have for centuries called universal and particular grammar. Latent structure is responsible for the general type of all features of universal grammar; realized structure is responsible both for the peculiarities of any given statement as well as those aspects that are unique to the grammar of a given natural language. (Lenneberg 1967: 376)

It is remarkable that about five decades ago Lenneberg already conceived of human language as a biologically determined cognitive system in the brain. The underlying cognitively specified latent structure and the realized structure basically correspond to the initial state of I-language and the steady state of I-language in generative grammar, respectively. Indeed, Lenneberg touches upon the notion of "universal grammar" (i.e., latent structure; see also fn. 1) and claims that

> universal grammar is of a unique type, common to all men, and it is entirely the by-product of peculiar modes of cognition based upon the biological constitution of the individual. (Lenneberg 1967: 377)

More specifically, Lenneberg points out that the latent structure is determined by the following biological properties of the human form of cognition:

> The forms and modes of categorization, the capacity for extracting similarities from physical stimulus configuration or from classes of deeper structural schemata, and the operating characteristics of the data-processing machinery of the brain (for example, time-limitations on the rate of input, resolution-power for the analysis of intertwined patterns such as nested dependencies, limits of storage capacities for data that must be processed simultaneously, etc. (Lenneberg 1967: 375)

Lenneberg (1967: 375–376) maintains that maturation of a child will bring cognitive processes to what he calls "language-readiness" as a state of latent structure. He expounds on the claim by saying that

> it might be more fruitful to think of maturation, including growth and the development of behavior such as language as the traversing of highly unstable states; the disequilibrium of one leads to rearrangements that bring about new disequilibria, producing further rearrangements, and so on until relative stability, known as maturity, is reached.
>
> (Lenneberg 1967: 376)

Accordingly, on Lenneberg's conception of maturation, including the growth/development of human language, the child will go through various states of cognitive disequilibria until reaching a state of cognitive relative equilibrium/stability, as depicted in figure 2.

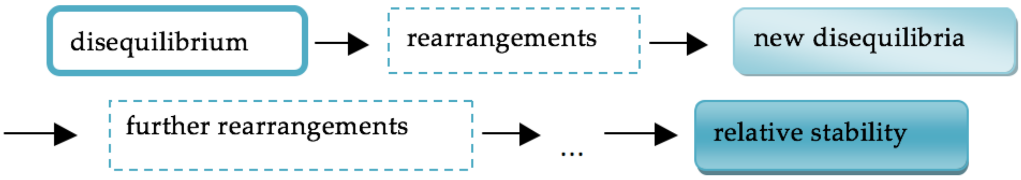


Figure 2: Schematic of the traversing of various cognitive states in maturation according to Lenneberg (1967).

As he remarks:

> Language-readiness is an example of such a state of disequilibrium during which the mind creates a place into which the building blocks of language may fit, (Lenneberg 1967: 376)

where the created place corresponds to what he refers to as a "biological matrix" (p. 394) of the latent structure, which virtually corresponds to Chomsky's (1965) language acquisition device (LAD) for first language acquisition in generative grammar. Lenneberg also adds:

> The realized structure or outer form of the language that surrounds the growing child serves as a mold upon which the form of the child's own realized structure is modeled. (Lenneberg 1967: 377)

Hence, Lenneberg's first language development model can be depicted as shown in figure 3.

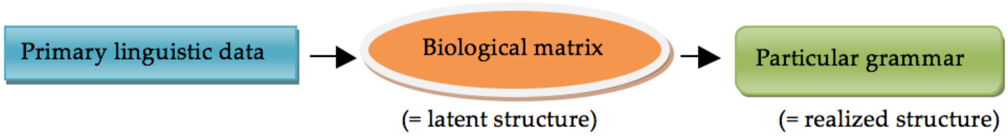


Figure 3: Schematic of Lenneberg's (1967) biological matrix for first language acquisition.

Furthermore, Lenneberg (1967: 383) clearly states that the biological capacity for utilizing the primary linguistic data as the language input is to be regarded as "breaking down of elements and resynthesizing them" to construct a particular grammar on the basis of the biological matrix of the latent structure. In fact, regarding the process of first language acquisition undertaken by the child by employing a biological matrix, Lenneberg furnishes the following explanation:

> Maturation brings cognitive processes to a state that we may call *language-readiness*. The organism now requires certain raw materials from which it can shape building blocks for his[/her] own language development. The situation is somewhat analogous to the relationship between nourishment and growth. The food that the growing individual takes in as architectural raw material must be chemically broken down and reconstituted before it may enter the synthesis that produces tissues and organs. The information on how the organs are to be structured does not come in the food but is latent in the individual's own cellular components. The raw material for the individual's language synthesis is the language spoken by the adults surrounding the child. The presence of the raw material seems to function like a releaser for the developmental language synthesizing process.
>
> (Lenneberg 1967: 375; emphasis in original)

Thus, Lenneberg's conception of the relation between primary linguistic data and language growth can be illustrated as in figure 4 which shows this relation in direct comparison with that between nourishment and physical growth.

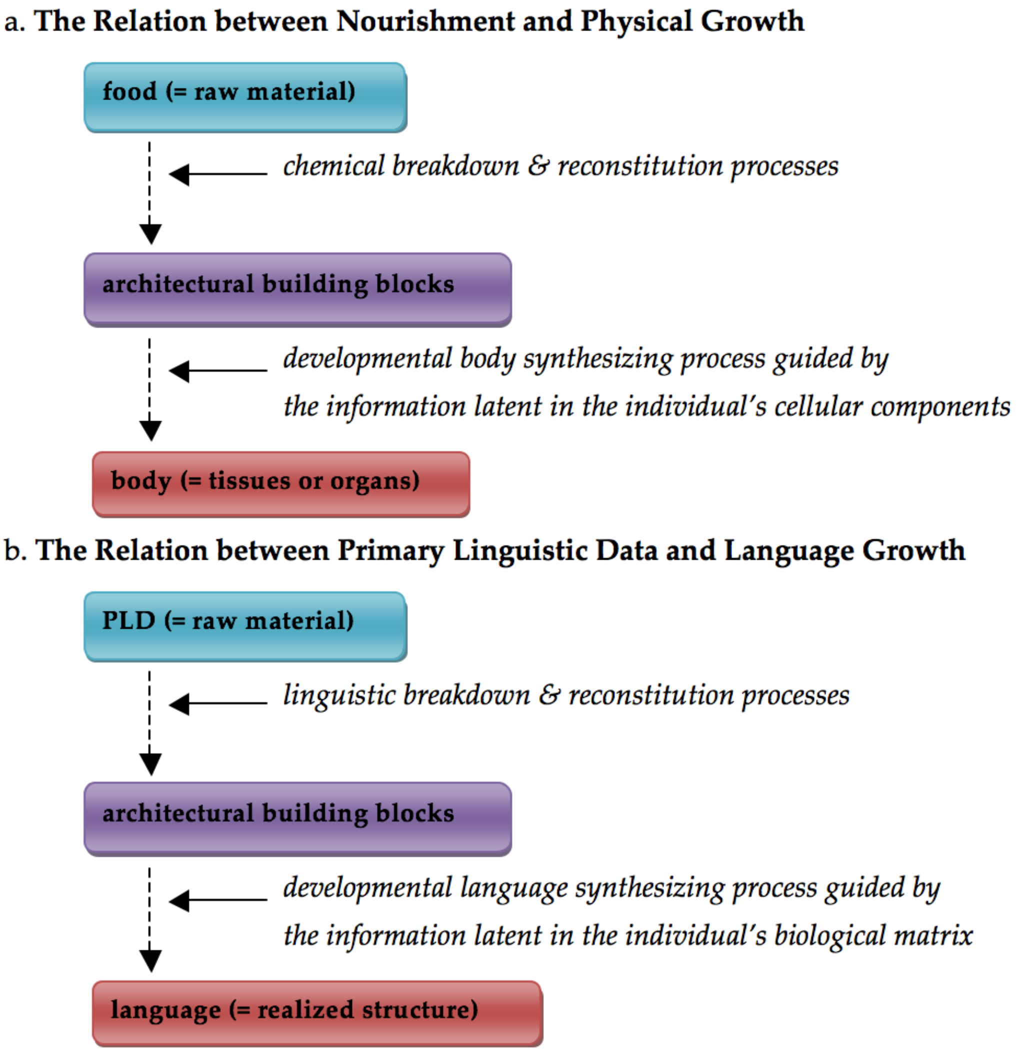


Figure 4: Comparison between food and language. The relation between nourishment and physical growth in (a), as well as primary linguistic data and language growth in (b).

While the architectural building blocks in the case of physical growth can be assumed to be proteins, the counterparts in the case of language growth can be regarded as lexical items LIs (rather than words) entering into structure-building in the sense standardly used in generative grammar. As investigation into this issue itself deserves a separate paper, I will not pursue it further here.

2.1.2. *Language Capacity as Independent From Its Externalization*

As summarized in Chomsky (2013, 2016, 2017a, 2017b), the generative enterprise of investigation into the nature of human language has reached the clear conclusion that we should regard "language as meaning with sound (or some other externalization, or none)" on the basis of empirical evidence concerning structure-dependence over linear order in syntax and semantics, reversing Aristotle's (1938)

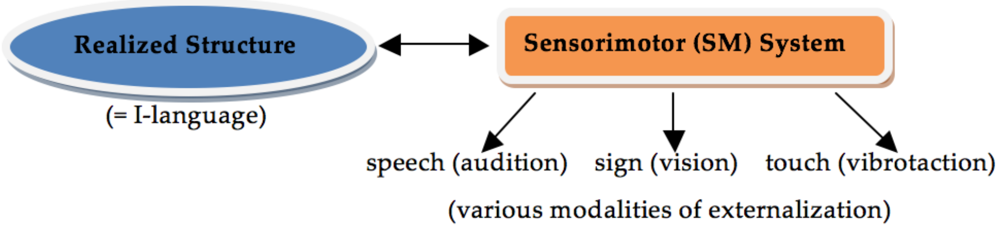


Figure 5: Architecture of human language according to Lenneberg (1967). Given that the realized structure can be actualized from the latent structure through the Tadoma method, vibrotaction is included as another modality for externalization.

classic dictum that "language is sound with meaning" (see also Berwick et al. 2013, Everaert et al. 2015).

In this connection, it is worth noticing that Lenneberg (1967) already held an idea similar to Chomsky's (1965, 1967) distinction between competence and performance (see Lenneberg 1967: 284 for reference to Chomsky 1967). Lenneberg considers the brain-internal language system as the capacity for language, which is independent from its externalization. He asserts that externalization, for instance, by speech, is "accessory" to the development of the language capacity in light of considerations of various cases of child language disorders (see also Lenneberg 1962, 1964 for more detailed discussion on child language disorders).[2] For instance, the following excerpt on a case of a child with congenital anarthria clearly illustrates this point:

> Congenital anarthria, as reported here, is a rare condition, but the case is by no means unique, and the discrepancy between speech skills and the capacity for understanding may, indeed, be observed in every child. The theoretical importance of the extreme dissociation between perceptive and productive ability lies in the demonstration that the particular ability which we may properly call *'having knowledge of a language'* is not identical with *speaking*. Since knowledge of a language may be established in the absence of speaking skills, the former must be prior, and, in a sense, simpler than the latter. Speaking appears to require additional capacities, but these are accessory rather than criterial for language development. (Lenneberg 1967: 308; emphasis in original)

With respect to the independence of the capacity for language from input modalities, Lenneberg is perceptive enough to point this out in chapter 8:

> Language acquisition is not dependent in man upon processing of acoustic patterns. There are many instances today of deaf-and-blind people who have built up language capacities on tactually perceived stimulus configurations. (Lenneberg 1967: 330–331)

(Also see C. Chomsky 1986 and Gleitman & Landau 2013 for intriguing discussion on language acquisition by deaf-blind children on the basis of a vibrotactile method

---

[2] Note that, as mentioned above, this view is virtually in line with the recent thesis in the minimalist program that externalization is altogether ancillary to I-language (see Chomsky 2013, 2016, 2017, Berwick & Chomsky 2016 and references therein).

of speech perception called the Tadoma method of speechreading.) Furthermore, the case of sign as a relevant modality is also now familiar to us due to remarkable progress in investigation of sign languages (see Petitto et al. 2016 and references therein *inter alia*).

In sum, Lenneberg's picture of human language architecture can be depicted as shown in figure 5 above.

2.2. *Lenneberg's First Language Development Model*

2.2.1. *Critical Period Hypothesis*

One of the integral components of Lenneberg's biolinguistic framework lies in the very notion of a "critical period" for first language development. Although the concept of the critical period for first language development was originally entertained by Penfield & Roberts (1959), it was clearly proposed in the context of biological foundations of language by Lenneberg (1967) for the first time.

The critical period for first language development corresponds to a time span during which an individual can automatically acquire his/her mother tongue by mere exposure to some samples of it without any conscious and labored effort, and, as Lenneberg depicts,

> the individual appears to be most sensitive to stimuli at this time and to preserve some innate flexibility for the organization of brain functions to carry out the complex integration of subprocesses necessary for the smooth elaboration of speech and language. (Lenneberg 1967: 158)

Here, "some innate flexibility for the organization of brain functions" in question refers to plasticity of the brain with respect to potentiality for physiological readjustment of either hemisphere to assume the language function in the case of brain lesions, which is operative before the end of the critical period (see Lenneberg 1967: 150–152).

With figure 2 above in mind, Lenneberg (1967) put forth the critical period hypothesis for first language acquisition, in which the onset of the critical period is the point of language-readiness as a state of disequilibrium and its end marks the point of relative stability (also see Lenneberg 1969). Taking stock of a variety of cases of child language acquisition (both normal and handicapped), he hypothesizes that the critical period at stake corresponds to the time span from around 2 years to around 12 or 13 years, as illustrated in figure 6 below.[3]

The rationale behind the presumed onset and end of the critical period at around 2 and 12–13 years of age, respectively, comes from the following facts,

---

[3] The critical period of the auditory system is known to be much earlier than two years (see also fn. 20 in the text). Shultz et al. (2014) examined neural responses to speech sounds compared with non-speech sounds in 1 to 4 months old infants using fMRI and observed neural specialization of left temporal cortex for speech in the first months of life. While the brain region in question continued to be responsive to speech sounds, it became less responsive to non-speech sounds. Although the exact formulation of the notion of critical period for first language development in other domains has been controversial in the literature ever since the proposal in Lenneberg (1967), I will not get into the debate here (see, e.g., Piekarski et al. 2017. Also see Hoshi & Miyazato 2016 and references cited therein for discussion and reflection on this debate).

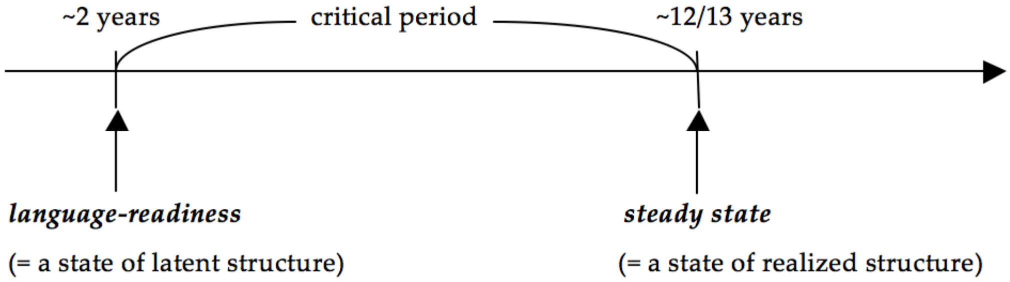


Figure 6: Schematic depiction of Lenneberg's original conception of the critical period for first language development.

among others:[4] First of all, even if brain lesions occur in either side of the brain in a child during the first two years of life, he/she could begin to speak at the usual time; otherwise, speech disturbances would result, though they are overcome in less than two years' time before the critical period ends around 12–13 years of age (Lenneberg 1967: 151). Furthermore, while the acquisition of language proceeds through the same fixed developmental stages in the retarded as well, such as children with Down's syndrome, their language development tends to come to a standstill after age 12–13 (Lenneberg 1967: 154–157).

During this period children could acquire their mother tongue on a biologically determined course of language development, given appropriate linguistic input from their environment. Lenneberg also remarks that

> it is interesting that the critical period coincides with the time at which the human brain attains its final state of maturity in terms of structure, function, and biochemistry (electroencephalographic patterns slightly lag behind, but become stabilized by about 16 years). Apparently the maturation of the brain marks the end of regulation and locks certain functions into place. (Lenneberg 1969: 639)

As amply documented and clearly demonstrated in Lenneberg (1967: Chs. 4 & 5), in ordinary child aphasia, brain lesions in the left hemisphere in a child do not prevent other parts (of the same hemisphere or) of the right hemisphere in the child from ontogenetically developing the language function by taking over the role and establishing a properly functioning language-related neural network, to the extent that he/she is still within the critical period thanks to plasticity of the brain (see also Hirsch et al. 2006). See also the discussion in section 3.

Although the fact that Lenneberg (1967) formulated the critical period hypothesis for first language acquisition is well-known among linguists, unfortunately, his original version of the critical period hypothesis does not seem to be understood correctly and appreciated fairly in the literature. First of all, Lenneberg's critical period is only concerned with first language acquisition and he does not say anything clear about second/foreign language acquisition. Furthermore, although

---

[4] Genie's case in Curtiss (1977) has also been known as a strong piece of evidence in support of Lenneberg's (1967) critical period hypothesis in the literature. See the short discussion of this in section 4.

the term has been commonly used in the broad notion of "first language acquisition" in the literature, it should be noted that Lenneberg's original version of the critical period hypothesis only applies to the ontogenetic development of language capacity and crucially it claims that linguistic output/externalization, for example, by articulation, is *not* subject to such a critical period (see Lenneberg 1967: 158). Hence, the development of the system for articulatory motor skills is free from the critical period at stake under Lenneberg's (1967) critical period hypothesis for first language development.

In sum, the essence of Lenneberg's (1967) original version of the critical period hypothesis for first language development can be stated as follows: Only the capacity for language is subject to the time constraint of the critical period (i.e. from approximately 2 years of age to 12–13 years of age) and thus externalization of the capacity for language does not respect the time constraint of the critical period.

2.2.2. Resonance Theory

Then, how do human children take in the primary linguistic data for developing their first language in the schematic in figure 3? Lenneberg rightly notes the need for "social settings as a trigger that sets off a reaction" (Lenneberg 1967: 378) for first language development. He appeals to the concept of "resonance" as a metaphor in his conceptualization of first language development model as follows:

> Perhaps a better metaphor still is the concept of resonance. In a given state of maturation, exposure to adult language behavior has an excitatory effect upon the actualization process much the way a certain object begins to vibrate in the presence of the sound. In the case of language onset, the energy required for the resonance is, in a sense, supplied by the individual him[/her]self. (Lenneberg 1967: 378)

Regarding the resonance analogy, he also adds that it vividly illustrates "how slight variations in the frequencies that impinge on the resonator may affect the quality or nature of the resonance" (Lenneberg 1967: 378). For that matter, he also discusses social aspects of behavior including language as follows:

> Certain social phenomena among animals come about by spontaneous adaptation of the behavior of the growing individual to the behavior of other individuals around him[/her]. ... In all types of developing social behavior, the growing individual begins to engage in behavior as if by resonance; he[/she] is maturationally ready but will not begin to perform unless properly stimulated. If exposed to the stimuli, he[/she] becomes socially "excited" as a resonator may become excited when exposed to a given range of sound frequencies [...].
>
> (Lenneberg 1967: 373–374)

Thus, under this view, the growing child is regarded as a biologically determined specific "linguistic resonator" that spontaneously resonates to the speech spoken by (an) adult(s) around him/her in social settings, as depicted in figure 7 below.

If you consider the spontaneous resonation in a child for first language acquisition in terms of the relation among the sensorimotor (SM) system, the latent

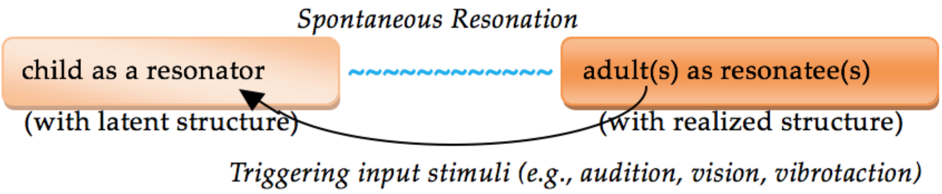


Figure 7: Schematic illustration of the child's spontaneous resonation for first language acquisition according to Lenneberg (1967).

structure, and the realized structure, you can regard audition, vision, or vibrotaction via the SM system as triggering spontaneous resonation in the child armed with the latent structure, and the latent structure of the resonating child will be led to his/her realized structure by gradually and spontaneously "synchronizing with" the comparable realized structure in the adult(s) interacting with him/her.

It seems that Lenneberg's metaphorical notion of "spontaneous resonance" in the child acquiring his/her first language goes well with his strong thesis in the beginning of his chapter 1 that says:

> A major objective of this monograph is [...] to show that reason, discovery, and intelligence are concepts that are as irrelevant for an explanation of the existence of language as for the existence of bird songs or the dance of bees.[5] (Lenneberg 1967: 1)

If the spontaneous linguistic resonation is one of our biologically determined instincts, the notion nicely captures our factual observation that a child is able to acquire his/her first language without any conscious effort in achieving that goal.

With regard to the relation between the critical period and the resonation for first language acquisition, Lenneberg states that

> [o]nce the critical period during which resonance may occur is outgrown, one language is firmly established, and exposure to new and different natural languages is no longer resonated to.
>
> (Lenneberg 1967: 378)

Thus, once the critical period has passed, the biologically determined autochthonous sensitivity of spontaneous resonance to the primary linguistic data will fade out.

Recall that the raw material in the primary linguistic data is processed and broken down into the linguistically-relevant building blocks for the child's own language, which must be resynthesized out of them while he/she is engaging in spontaneous resonation for first language acquisition, as illustrated in figure 4 above. Importantly, Lenneberg (1967: 376) clearly submits that this kind of developmental language synthesizing process on the basis of spontaneous linguistic resonation by the child is only operative during the critical period of biologically-determined, limited duration. In this connection, Lenneberg also notes:

---

[5] See Crain et al. (2016), Sugisaki (2016), and Yang et al. (2017) *inter alia* for an in-depth theoretical and empirical discussion of first language acquisition from a biolinguistic perspective.

> Resonance is linked to a postnatal state of relative immaturity and a concomitant lengthening of infancy and childhood, so that environmental influences (the molding after patterns available in the environment) can actually enter into the formative processes. (Lenneberg 1967: 392)

2.3. *Speech Production and Brain Rhythmicity*

Based on the view that the neural messages that are relevant to speech and language are "temporally coded signals" (Lenneberg 1967: 222), Lenneberg already entertained the following conjecture on brain oscillations:

> [s]pontaneous rhythmic activities of ganglia and even individual cells and fibers have been studied for many years now. It is entirely conceivable that these oscillations may serve a function that is analogous to the FM carrier frequency. (Lenneberg 1967: 216)

Furthermore, concerning brain rhythmicity, Lenneberg also notes that:

> It has long been known that the universally observed rhythmicity of the vertebrate brain [. . .] or central nervous tissue, in general [. . .] is the underlying motor for a vast variety of rhythmic movements found among vertebrates. If our hypothesis is correct, the motor mechanics of speech (and probably even syntax) is no exception to this generalization, and in this respect, then, speech is no different from many other types of animal behavior. In man, however, the rhythmic motor subserves a highly specialized activity, namely speech. (Lenneberg 1967: 119)

On the recognition that "rhythmic activity is a fundamental property of the vertebrate brain" (Lenneberg 1967: 116), Lenneberg also makes the following remark about a spontaneous dominant steady brain rhythm of approximately 7 Hz or faster over the temporo-parietal regions that is closely related to speech:[6]

> It is also interesting to note that children do not begin to develop speech until their brains have attained a certain degree of electro-physiological maturity, defined in terms of an increase with age in the frequency of the dominant rhythm. Only when this rhythm is about 7 cps [= Hz] or faster (at about age two years) are they ready for speech development.
>
> (Lenneberg 1967: 117)

In addition, Lenneberg proposes that there exists a physiological rhythm in speech which functions as an organizing principle/timing device for articulation with a duration of one-sixth of a second as the basic time unit in the programming of motor-speech patterns, virtually corresponding to the time unit for a syllable,

---

[6] The need for the emergence of this particular brain rhythmicity over the temporo-parietal regions for speech development in children seems to make sense, given that the dorsal-pathway for speech production crucially involves these brain regions, according to Hickok & Poeppel's (2007) dual-stream model of speech processing (see also Hickok 2009, 2012, Hickok et al. 2011). Also see Deonna & Roulet-Perez (2016) and Hoshi & Miyazato (2016) for some discussion on the dual-stream model of speech processing in connection with LKS, which will be taken up in section 3 in the text.

and that the timing mechanism renders the temporal ordering of speech events physically possible, using the metaphor that "[t]he rhythm is the grid, so to speak, into whose slots events may be intercalated" (Lenneberg 1967: 119).

The rationale behind Lenneberg's (1967) "basic timing mechanism" hypothesis is that neuromuscular automatisms for speech require an underlying physiological rhythm of periodic changes of "states" at a rate of approximately six cycles per second, i.e., 6+/–1 Hz. Thus, it is assumed that about one-sixth of a second is a fundamental temporal unit for articulatory programming in speech.

Interestingly, Lenneberg (1967) links this basic temporal frequency of 6+/–1 Hz for speech to the dominant steady brain rhythm of about 7 Hz over the temporoparietal regions mentioned above as an instance of neurological correlates in electroencephalogram (EEG).[7] Although the frequency band in the frontal lobe, especially (pre-)motor areas and Broca's area, necessary for speech development in a child is not reported in Lenneberg (1967), I will assume that a comparable or higher frequency range is required in these regions as well for speech development in a child.[8] This can be validated by Giraud et al. (2007), who incorporate MacNeilage & Davis's (2001) Frame/Content (F/C) theory for speech production and Poeppel's (2003) Asymmetric Sampling in Time (AST) theory for speech perception. Following MacNeilage & Davis (2001) and Poeppel (2003) in seeking to link speech processing to neural oscillations in the brain, Giraud et al. make the following observation on the basis of simultaneous EEG and fMRI recordings:

> spontaneous EEG power variations within the gamma range (phonemic rate) correlate best with left auditory cortical synaptic activity, while fluctuations within the theta range correlate best with that in the right. Power fluctuations in both ranges correlate with activity in the mouth premotor region, indicating coupling between temporal properties of speech perception and production. (Giraud et al. 2007: 1127)

Moreover, particularly interesting observations in Giraud et al. (2007) are the correlations between EEG and hemodynamic (fMRI) fluctuations in (pre)motor cortices. They found that, while 3–6 Hz EEG band is related to the motor region that controls movement of the mouth, 28–40 Hz EEG band is linked with the motor region that is responsible for controlling movement of the tongue. In light of this result, Lenneberg's (1967) observation on the relation between brain rhythmicity and speech development in a child seems to make sense. Given that speech production involves fine articulatory movements of the mouth and the tongue and that the 3–6 Hz EEG band and the 28–40 Hz EEG band are required to control movement of the mouth and the tongue in adults, respectively, it does not come as a surprise that brain oscillations with approximately 7 Hz or faster frequencies are needed to initiate speech development in a child, involving fine articulatory movements of both

---

[7] It is well-known that the most common brain rhythms have been classified by frequency, e.g., delta (δ) (0.5 – 4 Hz), theta (ϑ) (4 – 10 Hz), alpha (α) (8 – 12 Hz), beta (β) (10 – 30 Hz), and gamma (γ) (30 – 100 Hz), and those brain oscillations are reflections of synchronized neuronal activities in various cortical and subcortical structures (see Buzsáki 2006, Buzsáki & Watson 2012, Murphy 2015, 2016, Fernández 2015 *inter alia*). Delta /theta waves and beta/gamma waves are classified as slow waves and fast waves, respectively (see Bear et al. 2007).

[8] See Benasich et al. (2008) and Gou et al. (2011) for recent findings that the emergence of resting high-frequency gamma power neural synchrony in the frontal lobe across the first three years is crucial for early linguistic and cognitive development in a child.

the mouth and the tongue. I submit that these specific frequency bands should be taken into account in applying the non-invasive neuromodulation technology of transcortical direct current stimulation (tDCS; see Nitsche & Paulus 2000 *inter alia*) as a possible medical intervention for speech recovery in LKS, which will be addressed in the next section (see Hoshi & Miyazato 2016 for the proposal of using tDCS for speech and comprehension recovery in LKS).

In other words, Lenneberg (1967) makes the case that proper and stable development of particular brain rhythmicity as reflected in EEG patterns is required for emergence of speech in language development. In the final analysis, it is to be highly evaluated that, at the time when EEG-based investigations into language and cognition were not as sufficiently sophisticated as in our time, Lenneberg had already paid attention to the importance of scrutinizing patterns of brain oscillations in searching the biological nature of human language. In this sense, Lenneberg (1967) precedes the above-mentioned works in identifying the fundamental direction for theorizing on the relation between neuronal oscillatory patterns and linguistic performance of speech perception and speech production in natural language.[9]

3. Lenneberg's Child Aphasiology and its Application to LKS

3.1. Lenneberg's View on Aphasia in General

Lenneberg (1967) argues that aphasia in general has the following properties: (i) aphasic patients' capacity for language is not lost, but it is merely interfered with; (ii) aphasic symptoms are caused by disorders of timing/temporal integration mechanisms in charge of yielding proper orders of linguistic units.

Concerning (i), Lenneberg makes the following point on the basis of a survey of the clinical pictures of a variety of language and speech disorders, including receptive disorders, expressive disorders (subfluency, superfluency, semantic disturbances, difficulty in word finding, paraphasic disturbances), disorders of manner of production (errors of order, dysarthria, discoordinations):

> The most striking common denominator in aphasia is the ubiquitous evidence that the patient has not literally "lost" language; that is, he[/she] is not returned to a state of no language such as an animal or even a person who forgot everything he[/she] once knew in a foreign language. [. . .] In the literal sense of the word, the patient's language skills are merely interfered with; there are disturbances of cerebral function. Neither discrete words nor discrete grammatical rules are neatly eliminated from the store of skills. [. . .] [C]areful observation of the recovery process during the critical post-morbid period, makes it very plain that the patient does not start with specific lexical or grammatical lacunae, but that some basic physiological processes relating to activating, monitoring, or processing of speech are deranged. If there is clinical improve-

---

[9] It might be worth investigating whether or not the postulated ability/function for spontaneous resonation in a child acquiring his/her mother tongue in figure 7 would be significantly correlated with the emergence and development of a particular brain rhythmicity in him/her in the maturational course.

> ment, it is not due to the acquisition of new vocabulary or grammatical rules, but to release from inhibitory factors, to faster acting memory, to better controlled organization of elements, etc. Thus the distinction between loss of language and interference with skills leads to different approaches in rehabilitation and management of patients with aphasia.
>
> (Lenneberg 1967: 207)

Thus, Lenneberg regards aphasics as just having lost the ability of utilizing the language inside the brain for comprehension and production due to interference by brain lesions rather than having lost the language capacity itself. Accordingly, the realized structure (= capacity for language) established should remain there within the brain in aphasics.

With respect to (ii), in light of the tenet that time is the most significant dimension in language physiology, Lenneberg argues that aphasic symptoms in general can be characterized as disorders of timing/temporal integration mechanisms,[10] as can be appreciated by the following passage in the conclusion of chapter 5 of his book:

> Language is never totally and specifically lost except in combination with complete disruption of cognition. All disorders are aspects of interference with physiological processes prerequisite for the normal function of speech and language. Aphasic symptoms give no evidence of a fragmentation of behavior, that is, of dissolution of associatively linked 'simpler percepts.' Most of the symptomatology may be seen as disorder of temporal integration, of 'lack of availability at the right time.'
>
> (Lenneberg 1967: 222)

In addition, from Lenneberg's perspective of the architecture of human language as depicted in figure 5 above, (ii) can be interpreted as claiming that aphasia is generally caused by dysfunction of the sensorimotor (SM) system that is responsible for programing and implementing temporal sequencing of linguistic units for externalization, while the hierarchically structured expressions (associated with such linguistic units) generated by core syntax in the realized structure remain virtually intact.

3.2. *Lenneberg's View on Child Aphasia*

On a par with acquired adult aphasia, acquired ordinary child aphasia involves some sort of organical brain lesions due to traumas, tumors, or cerebrovascular damages. Interestingly, however, unlike aphasia incurred in adulthood, ordinary child aphasia will generally be overcome if it strikes the child early enough in life

---

[10] Lenneberg is also perceptive in suggesting that

> [c]ortical lesions primarily interfere with temporal integration of a higher order (words or grammatical category), whereas deeper lesions disrupt the necessary convergence of various afferent signals and the intimate coordination and integration for efferent impulses, thus producing disorders of production.
>
> (Lenneberg 1967: 222)

(Lenneberg 1967, 1969).[11] In fact, Lenneberg summarizes the relation between plasticity and lateralization of the brain in aphasia involving some brain lesions as follows:

> Aphasia is the result of direct, structural, and local interference with neurophysiological processes of language. In childhood such interference cannot be permanent because the two sides are not yet sufficiently specialized for function, even though the left hemisphere may already show signs of speech dominance. Damage to it will interfere with language; but the right hemisphere is still involved to some extent with language, and so there is a potential for language function that may be strengthened again. In the absence of pathology, a polarization of function between right and left takes place during childhood, displacing language entirely to the left and certain other functions predominantly to the right [. . .]. If, however, a lesion is placed in either hemisphere, this polarization cannot take place, and language function together with other functions persist in the unharmed hemisphere.
>
> (Lenneberg 1967: 153)

Thus, according to Lenneberg, the following generalization emerges for acquired ordinary child aphasia:

(1) ***Lenneberg's Generalization on Acquired Ordinary Child Aphasia***
The earlier the onset of the disorder is, the better the prognosis for recovery will be (see Lenneberg 1967: 153, 178. See also Lenneberg 1969, 1975 for further discussion).[12]

The pattern for ordinary child aphasia in (1) seems to be quite expected in the light of plasticity of the child brain in connection with Lenneberg's (1967) critical period hypothesis. If the onset of the language disorder is earlier, the relevant language function would be relocated or compensated for by the use of other parts of the (ipsilateral or contralateral) language-related brain regions to the extent that the child is still within the critical period.[13] This means that, in the case of ordinary

---

[11] See also Alajouanine & Lhermitte (1965) for a similar conclusion that the prognosis for acquired aphasia with lesions in childhood is definitely better than that in the adult.

[12] In fact, Lenneberg infers that "*language learning* can take place, at least in the right hemisphere, only between the age of two to about thirteen," (Lenneberg 1967: 153) namely, during the critical period for first language acquisition that Lenneberg postulated.

[13] There is a statement in Lenneberg (1967) that complicates the situation:

> If aphasia strikes the very young during or immediately after the age at which language is acquired (between 20 to 36 months of age), the recovery is yet different. Cerebral trauma to the two or three year old will render the patient totally unresponsive, sometimes for weeks at a time; when he[/she] becomes cognizant of his[/her] environment again, it becomes clear that whatever beginning he[/she] had made in language before the disease is totally lost, but soon he[/she] will start again on the road toward language acquisition, traversing all stages of infant vocalization, perhaps at a slightly faster pace, beginning with babbling, single words, primitive two-word phrases, etc., until perfect speech is achieved. In the very young, then, the primary process in recovery is acquisition, whereas the process of symptom-reduction is not in evidence.
>
> (Lenneberg 1967: 146, 150)

child aphasia, the child could overcome the aphasic state by appealing to plasticity of the neural network under development in the brain before the full maturity of the neural network is attained in accordance with the biologically determined critical period for first language acquisition.

Provided that Lenneberg's original version of the critical period hypothesis and his general view on aphasia were on the right track, it would yield a significant implication to the study of child aphasia. Even if a child is suffering from childhood aphasia, it is predicted that, in principle, there should be a case where *externalization* of "inner language," i.e., language capacity as either a partially actualized form or a fully actualized form of the latent structure, could happen even *after* the critical period ends, once the deficit in the neural system for articulatory motor skills for externalization is removed or disappears, on the condition that the core biologically determined system of the latent structure remains virtually intact in the child with aphasia and that acquisition of the mental lexicon along with language-particular morpho-phonology, syntax, and semantics should become possible by the end of the critical period. As Hoshi & Miyazato (2016) point out, this prediction is borne out by a certain childhood aphasia of epileptic origin, which I will be addressing in the next section.[14]

3.3. Landau-Kleffner Syndrome and Lenneberg's Critical Period Hypothesis

3.3.1. LKS: Epileptogenic Child Language Disorder

Landau-Kleffner syndrome (LKS) is a clinically rare language disorder of acquired childhood aphasia involving epilepsy (with or without clinical seizures), which was first reported by Landau & Kleffner (1957).[15] LKS emerges with epileptiform electroencephalographic (EEG) abnormalities typically involving continuous spike waves during slow sleep (CSWS) over the temporal regions, and unlike ordinary childhood aphasia, it does not implicate any particular brain lesions (Gordon 1990, Deonna 1991, 2000). While computed tomography (CT) and magnetic resonance

---

If this is the case in general, in acquired ordinary child aphasia with early onset, there is a possibility that partially or fully acquired realized structure will be lost by brain lesions. Nevertheless, it is not clear whether the partially or fully attained realized structure in such very young aphasics is truly lost altogether, or whether it is in fact still there in the brain but its externalization by speech is either merely interfered with and blocked or does not start yet (recall that a child's dominant brain rhythm over the temporo-parietal regions will reach 7 Hz or faster around 2 years of age), given Lenneberg's view on aphasia in general (see Lenneberg 1967: Ch. 5).

[14] Considering and comparing significant differences between LKS and other childhood aphasias/language disorders would go well beyond the scope of the current paper. See Bishop & Leonard (2000) *inter alia* for invaluable discussion and relevant references.

[15] See, for example, Deonna & Roulet-Perez (2016) for the most recent review and the most comprehensive discussion on LKS in the framework of epilepsy-aphasia spectrum. See also Steinlein (2009) for an in-depth description of LKS in the framework of epilepsy-aphasia syndromes. A *Biolinguistics* reviewer rightly remarked that LKS is not as clearly defined in the literature as one would probably expect, pointing out that Rapin et al. (1977) among others do not regard LKS as childhood aphasia but as verbal auditory agnosia. However, given that LKS patients, who suffer from verbal auditory agnosia, typically exhibit expressive language disorder as well at least during the acute period, I will include LKS under the general categorial term "childhood aphasia" in the text by broadly construing the notion of "aphasia" as referring to the state of either apparently sensory or expressive language disorders or both.

imaging (MRI) findings on patients with LKS are normal, single photon emission computed tomography (SPECT) and positron emission tomography (PET) studies on the patients show temporal lobe abnormalities in brain perfusion and glucose metabolism, i.e., decreased perfusion and hypometabolism, respectively (DaSilva et al. 1997, Pearl et al. 2001, and references therein; see Deonna & Roulet-Perez 2016 for other patterns.)

The child with LKS first undergoes a period of normal development of language, but usually after the onset of the disorder, the "language attained thus far" starts regressing. In LKS, it is quite common that both language comprehension and language production acutely or insidiously become extremely difficult or impossible, often leading to apparent deafness and mutism in the child suffering from it (see Gordon 1990, 1997, Tharpe & Olson 1994, Kaga 1999, 2011, Pearl et al. 2001 *inter alia* for more details). Moreover, in addition to the language disorder, the EEG abnormalities in LKS also cause behavioral and psychiatric disturbances such as hyperactivity, aggressive behavior, impulsivity, and attentional problems, which resemble autism spectrum disorders (ASD) (Stefanatos 2011, Mikati et al. 2010).

To be more specific about the language disorder in LKS, the epileptic discharges over the temporal regions, as reflected in the EEG abnormalities, will result in the child with LKS having extreme difficulty or impossibility of hearing linguistic sounds due to the dysfunction of the system of processing linguistic sounds in the non-primary auditory cortices during the active phase of LKS (Hirsch et al. 2006). Given that LKS patients could acquire a sign language even if they cannot restore their original spoken expressive language (see Deonna et al. 2009, Deonna & Roulet-Perez 2016 and references therein), it is clear that the childhood aphasia in LKS, which is caused by abnormal epileptic discharges over the temporal regions, only affects the SM system responsible for speech perception and speech production while the system of the language capacity remaining virtually intact.

This crucially means that virtually no linguistic input would become available during the active period and that no further primary linguistic data (PLD) would become accessible if the child with LKS were to be still in the process of first language development. I believe that the very notion of spontaneous resonance is also of service in considering LKS, because it is plausible to assume that spontaneous resonance illustrated in figure 7 is fatally blocked or disturbed in such a case of childhood aphasia, due to the epileptic discharges over the temporal regions affecting both hemispheres, which hampers brain plasticity, and the related difficulty of taking in verbal auditory input by the deficit of the system of spectrotemporal analysis in the superior temporal gyri (see Hoshi & Miyazato 2016 for details). This, in turn, suggests that the ability of processing linguistic sounds would, in principle, come back once the EEG abnormalities are either removed or disappear in LKS.

Moreover, the clinical seizures are generally infrequent and LKS-related epileptic clinical seizures themselves can be easily controlled by a single anti-epileptic medication: benzodiazepines such as clobazam (Pearl et al. 2001), valproate, and ethosuximide (Mikati et al. 2010). Although the epileptiform EEG abnormalities themselves cannot be easily suppressed by such anti-epileptic medication, the paroxysmal EEG abnormalities will improve gradually and usually disappear spontaneously by around 14 years of age in LKS (Massa et al. 2000, Robinson et al. 2001, Ramanathan et al. 2012, Deonna & Roulet-Perez 2010, 2016). Thus, it is quite natural to assume that verbal auditory input would become possible gradually well before 14 years of age and the quality of linguistic input would concurrently improve during the process of gradual amelioration of the EEG status in LKS.

Given that a child will acquire the core linguistic competence by around three years of age (Lenneberg 1967, Pinker 1994, O'Grady 2005) in a normal course of first language acquisition and that approximately 80 % of LKS has the onset ranging from three years old to eight years old (Kaga 2000), but that the earliest onset of LKS occurs around 18 months of age (Uldall et al. 2000), Hoshi & Miyazato (2016) divide LKS into two broad sub-types of what they call early LKS and ordinary LKS, defining them as follows:

(2) a. *Early LKS* has the onset before 3 years of age, when the affected child has not yet established the core linguistic competence sufficiently.
    b. *Ordinary LKS* refers to all other cases of LKS.

Interestingly enough, there are some children with early LKS who would be able to experience something like a linguistic big bang. Uldall et al. (2000) observe that their patient with early LKS (with onset at 18 months) speeded up language acquisition in his "catch-up periods" in such a way that he acquired vocabulary that would have normally taken one whole year to acquire was developed in just three months after the age of five years. They remark that

> the normal spurt of vocabulary usually seen at the age of 17–19 months seemed to have been blocked until it was 'released' by the prednisone course at the age of 5 years. (Uldall et al. 2000: 85)

As this case indicates, as long as the "inner language," or the realized structure in the sense of Lenneberg (1967) or I-language in the sense of Chomsky (1986), is established before the critical period ends, externalization of the inner-language would be possible even later in life, presumably producing the linguistic big bang.[16] Notice that, as long as externalization of the inner language is not subject to the critical period, as claimed in Lenneberg (1967), such a linguistic big bang, in principle, could occur even after the critical period ends in early LKS.

Thus, the prognosis patterns of LKS patients is that approximately 50 % of the patients recover fully and about 50 % of the remaining patients recover partially after a certain period of time (Mikati et al. 2010), which is remarkable in contrast with the case of autistic regression in autism spectrum disorders (ASD) (see Hoshi & Miyazato 2016 and references therein).[17] Furthermore, curiously enough, unlike ordinary child aphasia, for which Lenneberg's generalization in (1) holds, LKS tends to display just the opposite pattern. Namely, the earlier the onset of the disorder is, the worse the prognosis will be (see, e.g., Bishop 1985).[18] That is, a younger

---

[16] Note that Lenneberg's remark cited in the excerpt on page 96 also applies to the case of the linguistic big bang in early LKS in that the patient will not lose the acquired linguistic knowledge and will not re-start language acquisition from scratch.

[17] While the patient with ordinary LKS would be highly likely to recover from the state of aphasia in a relatively short period of time, the patient with early LKS would either recover from such a state after a relatively long period of time or not recover from it.

[18] This description does not express absolute correlations but just tendencies. In reality, much more complicated and varied patterns are clinically observed in specific cases in the literature

age of the onset of the language disorder is generally related to a dimmer prognosis for recovery from the state of aphasia in LKS.

Accordingly, the general tendency of childhood aphasia in LKS appears to be just the opposite of Lenneberg's generalization on the surface. However, notice that, due to the very nature of LKS as a form of non-lesional, epileptogenic aphasia in a child who is still under maturational development of linguistic and cognitive functions, unlike ordinary child aphasia, plasticity of the brain would not readily come into play in LKS, because epileptic discharges typically in the form of CSWS that originally emerge unilaterally would be bilateralized by propagating to the contra-lateral hemisphere, hampering the use of brain plasticity and impeding normal functioning of the language-related neural network (Hirsch et al. 2006, O'Hare 2008). Hence, Lenneberg's generalization in (1) cannot be applied to LKS on a fundamental ground in the first place.

This conjecture seems to fall into place, once Lenneberg's (1967) perspective and biolinguistic framework are taken into account. Recall from section 2.3. that Lenneberg fundamentally assumes that language is realized in the brain as temporally coded patterned activities with various oscillations (see Lenneberg 1967: Chs. 4 & 5 for details). Thus, if the language-related oscillations are disturbed, as reflected in EEG abnormalities, it is naturally expected that "language behavior" is also disturbed accordingly.

Nevertheless, it is important to recall that, as discussed above, such paroxysmal EEG abnormalities will gradually ameliorate and usually disappear spontaneously by around 14 years of age (Massa et al. 2000, Robinson et al. 2001, Ramanathan et al. 2012, Deonna & Roulet-Perez 2010, 2016). Thus, in accordance with the improvement of the EEG abnormalities, the quality of linguistic input would become better little by little as the child with LKS passes through the critical period. This should in turn facilitate at least the development of the realized structure in the sense of Lenneberg (1967) even in the case of early LKS.

3.3.2. *Some Implications to LKS*

First of all, if LKS falls under the category of child aphasia, Lenneberg's child aphasiology suggests that the capacity of language itself is not lost but should exist within the brain of the child with LKS, although it is interfered with somehow (but see fn. 13). As already discussed in sections 3.2. and 3.3.1., in the case of ordinary child aphasia, the interference is due to brain lesions; whereas, in the case of LKS, it is caused by epileptic discharges over the temporal regions that involve bilateralization of an originally unilateral focus. Given that the critical period spans from around 2 years of age to around 12/13 years of age, it is imperative that linguistic input become available again to the child with LKS before the critical period ends. This would guarantee that the latent structure be actualized into the full-fledged realized structure within the critical period in the sense of Lenneberg (1967), particularly in the case of early LKS. Recall that the epileptogenic EEG abnormalities

---

(see Deonna et al. 1977 for the varied prognosis of LKS depending on factors other than the onset of the disorder. See also Deonna & Roulet-Perez 2016 and references therein for detailed discussion). Moreover, the degree of recovery from LKS depends on not only the time of onset but the response to anti-epileptic medication and the severity of communication problems (see Pullens et al. 2015). Thus, this prognostic pattern is merely a general tendency in LKS.

in LKS will ameliorate gradually and disappear spontaneously by around 14 years of age. In other words, there exists a time lag between the end of the critical period and that of the EEG abnormalities in LKS. Thus, from a medical point of view, it is imperative to control the EEG abnormalities by the end of the critical period in order to obtain a better prognosis in LKS (see Hoshi & Miyazato 2016 for details).[19]

However, recall also that externalization of the language capacity, for example, by speech, is exempt from the time constraint of Lenneberg's critical period. As such, to the extent that the language capacity can develop within the critical period in the child with LKS, he/she will have a chance to (re-)start speaking the mother tongue even after the critical period ends.

To sum up, the relevant conjectures on LKS can be stated as follows:

(3) ***LKS Conjectures***

a. The language capacity is not lost but exits as either a partially realized structure (i.e. *early LKS*) or a fully realized structure (i.e. *ordinary LKS*).

b. Once linguistic input becomes available again, first language development could re-start in *early LKS* within the critical period and the use of language could be restored in *ordinary LKS*.

c. Externalization of the language capacity by speech, for instance, could take place without any influence of the time constraint of the critical period.

It seems to be quite plausible to guess that the three LKS conjectures in (3), which are based on Lenneberg's (1967) original version of the critical period hypothesis and his view on child aphasiology, are responsible for the fact that approximately 50 % of the LKS population recover fully and about 50 % of the remaining patients recover partially in the use of their mother tongue (Mikati et al. 2010).

LKS can be interpreted as a situation where the spontaneous resonating process for first language development has been hampered due to the disturbance of normal brain rhythmicity by epileptiform EEG abnormalities such as CSWS. As first language development will proceed via spontaneous resonance during the critical period, according to Lenneberg (1967), it is of vital importance to secure as much spontaneous resonance as possible during the critical period especially for the child with early LKS. If the spontaneous resonance can be secured, re-development of the language capacity on the basis of what the LKS patient achieved on the latent structure should be theoretically possible even in the child with early LKS. Recall that the dominant brain rhythm over the temporo-parietal regions has to reach about 7 Hz or faster in order for a child to be ready for speech development.[20] Thus, it is significant to get rid of the epileptiform EEG abnormalities typically with CSWS in order to secure steady spontaneous brain rhythms of

---

[19] See, for example, Faria et al. (2012) for an attempt to modulate epileptic activity focally in patients, including a patient with LKS by employing the non-invasive neuromodulation technology of tDCS successfully. See also Arle & Shils (2017) for an introduction to the latest development of innovative clinical neuromodulation, including tDCS.

[20] Lenneberg (1967: 117) states that the dominant brain rhythm of about 7 Hz or faster frequencies over the temporo-parietal regions is attained at around age 2 years in a child as a prerequisite for speech development. Given that the onset of the critical period is around 2 years of age in Lenneberg's critical period hypothesis and early LKS could start around 18 months, it is clear that the child with early LKS cannot start engaging in full development of speech.

that frequency range or a much higher frequency range over those regions and possibly over (pre)motor regions including Broca's area in the frontal lobe in the child with LKS.

In the history of aphasiology, aphasic symptoms have often been analyzed as "disconnection" syndromes along the lines of studies by prominent scholars such as Carl Wernicke, Ludwig Lichtheim, Hugo Liepmann, Jules Dejerine and Norman Geschwind (see Catani & Mesulam 2008a, b *inter alia* for a review). Lenneberg takes issue with this position, claiming that

> the ablation experiments reviewed earlier should be a warning to 'aphasiologists' not to interpret specific clinical symptoms of aphasic patients as disruption of associations or structural, cortical disconnections. There is no experimental evidence that any associative bonds may be disrupted by discrete cortical lesions. (Lenneberg 1967: 217)

(Also see Lenneberg 1975 for arguments in favor of such an anti-disconnection view on aphasia.[21])

If Lenneberg's (1967) biolinguistic framework and his view on child aphasia are fundamentally on the right track and are extendable to LKS as well, as argued above, it will open up a new possibility for rehabilitation/medical intervention on LKS patients who are still suffering from partial or no restoration of speech comprehension and/or production. Hoshi & Miyazato (2016) propose to use tDCS in facilitating speech production and comprehension in LKS patients on the basis of Hickok & Poeppel's (2007) dual-stream model of speech processing.

Since early LKS and autistic regression in autism spectrum disorders (ASD) present with similar "autistic" behaviors such as hyperactivity, aggressive behavior, impulsivity, and attentional problems as well as cognitive regression including language disorders (Stefanatos 2011, Mikati et al. 2010), the two cases are particularly confusing and easily susceptible to misdiagnosis (Deonna & Roulet-Perez 2010, Stefanatos 2011. See also Hoshi & Miyazato 2016 for details). Therefore, differentiating early LKS from autistic regression in ASD should be carried out as early as possible within the critical period in terms of the following risk markers: (i) whether or not the child in question has the LKS-characteristic EEG abnormalities typically with CSWS; (ii) whether or not the epileptic seizures in the child, if any, can be readily controlled by anti-epileptic medication such as benzodiazepines

---

On the other hand, as Kuhl (1993) and Kuhl et al. (2005) *inter alia.* show, the "critical period" for phonetic perception/discrimination in infancy comes well before the end of the 1st year of life. Note that this is not in variance with Lenneberg's critical period hypothesis, because such a task of phonetic perception/discrimination is handled by the sensorimotor (SM) system, which lies outside the domain of application of Lenneberg's original version of the critical period hypothesis as discussed in the text. If this and Lenneberg's view on aphasia are on the right track, it suggests that even the child with early LKS has already acquired the ability for phonetic perception/discrimination before the onset of the aphasia and still keeps the potential for use (but see fn. 13).

[21] Based on Hickok & Poeppel's (2007) dual-stream model of speech processing as a theoretical foundation, Hoshi & Miyazato (2016) propose to analyze the apparently disconnection outlook of LKS (see Tsuru & Hoeppner 2007) as an epiphenomenon, in line with Lenneberg's (1967) anti-disconnection view on aphasia. More specifically, they attribute the aphasic state in LKS to the result of a "domino effect" over the dorsal pathway and the ventral pathway, arising from the dysfunction of the system of spectrotemporal analysis in the dorsal superior temporal gyrus (STG).

including clobazam. Once it is found that the child is affected by LKS, proper antiepileptic medication should continue to be administered in order to consistently control epileptic seizures, if any, and hopefully to precipitate improvement of the EEG abnormalities (see Deonna & Roulet-Perez 2016) in order to make the quality of linguistic input better so that the language capacity would grow ontogenetically within the critical period in the LKS-affected child.[22]

4. Concluding Remarks

In this paper, I have revisited Lenneberg's (1967) biolinguistic framework and his view on (child) aphasiology in an attempt to re-evaluate his insights on biological aspects of human language and his pioneering contributions to the field of child aphasiology.[23]

As mentioned in fn. 4, one of the strong supports for Lenneberg's (1967) critical period hypothesis has come from a study on a language-isolated child called Genie, who was discovered isolated from language experience at age 13 and who was only able to develop a rudimentary syntax (Curtiss 1977). In this paper, I have aimed to show that children with LKS should be added as another type of language-isolated children that can provide further strong empirical evidence backing up Lenneberg's biolinguistic concepts. In the case of Genie, extreme deprivation of linguistic input during the critical period by her parents' intentional isolation and thus virtually no chance of spontaneous resonation to any adults surrounding her led to under-actualization of the realized structure out of the underlying latent structure in Lenneberg's (1967) framework.

On the other hand, LKS affords a rare opportunity to verify the validity of Lenneberg's critical period hypothesis from a different angle. In the case of LKS, while sudden deprivation of linguistic input occurs either before or after the onset of the critical period (i.e. around 2 years of age) and such a devastating condition will continue either for a relatively short period or for a relatively long period until presumably around 14 years old. Either way, it is highly likely that linguistic input will gradually come back, as discussed previously, before the end of the critical period (recall the dramatic recovery case of early LKS in Uldall et al. 2000). The fact that approximately 50 % of the LKS population recover fully and roughly 50 % of the remaining patients recover partially in the use of their first language (Mikati et al. 2010) clearly indicates that LKS can indeed serve as strong "living" evidence

---

[22] Even if the EEG abnormalities with epileptic discharges have gone by puberty/adolescence (Massa et al. 2000, Robinson et al. 2001, Deonna & Roulet-Perez 2010, 2016), there are still cases where restoration of speech comprehension and/or production would not likely occur fully or at all in LKS patients (see Hoshi & Miyazato 2016 for a detailed review and discussion on the various patterns of recovery in LKS patients), presumably because of an aftermath of the acquired dysfunction of those language-related regions that involved a long-lasting local impairment in the neural network in the regions, as reflected in residual glucose hypometabolism in the relevant areas that used to be hypermetabolic during the active phase of LKS (see Hirsch et al. 2006 for more discussion on the reason why some cases of LKS present with difficulty for language restoration).

[23] See, e.g., Benítez-Burraco (2016) for a concise but informative review of the current state of the field of clinical linguistics. See also Tsimpli et al. (to appear) for discussion on language disorders from the perspective of UG.

demonstrating the validity of both Lenneberg's (1967) original version of the critical period hypothesis and his fundamental view on (child) aphasia.

As already discussed as a consequence of medical import, it should become possible to differentiate early LKS and genuine ASD with autistic regression as a first step, on the basis of the fact that patients with LKS present with characteristic EEG abnormalities typically with CSWS and that epileptic seizures, if any, can be easily controlled by anti-epileptic medication (see Hoshi & Miyazato 2016 for more details). Therefore, it is expected that a much larger LKS-potential population within the ASD-diagnosed patients might turn out to fall under the category of early LKS and to become medically curable. In addition, although the EEG abnormalities in LKS will normally improve gradually and eventually disappear by adolescence, as mentioned before, amelioration of the EEG abnormalities could possibly be medically facilitated by the use of tDCS (see Faria et al. 2012) before the end of the critical period so that the child with LKS would have a better prognosis in both linguistic comprehension and linguistic production (see also Hoshi & Miyazato 2016 for a concrete suggestion on the use of tDCS for such a medical purpose). Taken altogether, I firmly believe that more attention should be paid to LKS in the context of (neuro)biolinguistics as well as medicine.

Two other major findings in Lenneberg's work are resonation and brain rhythmicity. As for resonation, it is expected that not only the language disorder but also the behavioral disturbances observed in children with LKS and a sub-population of children with AR in ASD might be related to malfunctioning of their proper resonation with relevant stimuli in their environment, including individuals interacting with them, due to brain rhythmicity disturbances (see Hoshi & Miyazato 2016 and references therein for some discussion on LKS and AR). Another speculation concerning brain rhythmicity is that application of tDCS may be instrumental to enhance the brain oscillations of the affected children up to the normal range of frequencies, viz., approximately 7 Hz or faster frequencies, which is reported by Lenneberg for speech development. Thus, I believe that these two groundbreaking concepts are likely to contribute to elucidating the mechanisms underlying child language and developmental disorders.

Before closing this section, I would like to refer to Lenneberg's (1967) research strategy in pursuing the biological aspects of human language. Recently, there is a burgeoning interest in looking into a variety of cognitive disorders and syndromes that affect language development such as autism spectrum disorders (ASD), specific language impairment (SLI), schizophrenia, Down's syndrome, and Williams syndrome among many others (see Bishop 1982, 2000, Bishop & Leonard 2000, Billard et al. 2009, Benítez-Burraco 2013, 2016, Benítez-Burraco & Boeckx 2014, 2015, Benítez-Burraco & Murphy 2016, Hinzen et al. 2015, Barceló-Coblijn et al. 2015, Hinzen & Rosselló 2015, Murphy & Benítez-Burraco 2016a, b, Tsimpli et al. to appear *inter alia*). Lenneberg (1967) also investigated not only (child/adult) aphasia but also Down's syndrome in an attempt to construct an integrative theory of the biological foundations of language. It should be recognized that it was Lenneberg (1967) that clearly demonstrated that such an enterprise requires the need for in-depth comparison between the nature of language behavior of those people with language-related cognitive disorders and syndromes and that of normally developed people.

In this connection, last but not least, let me draw the reader's attention to another legacy of *Biological Foundations of Language*, which is unlikely to be mentioned in academic settings: Lenneberg's dedicated sense of mission as a professional toward improving the fate of cognitively handicapped children and adults, as revealed by the following passage in the book:

> A particularly promising approach seems to be the systematic evaluation of patients with various deficits, especially [...] the mentally retarded. Modern advances in technology and methodology in behavior research are likely to lead to new knowledge about language function, and thus the patients whose misfortune serves as source material for new studies may, hopefully, eventually profit from the new advances in our understanding of language. (Lenneberg 1967: viii)

It should be kept in mind that *Biological Foundations of Language* is an immortal classic in the field of biolinguistics which not only set forth the fundamental tenet of the discipline but also encouraged a noble, humanistic attitude to engage ourselves to the biolinguistic enterprise, keeping in mind those people who are often forced to exist "on the periphery" in society, due to some chance misfortune. To the extent that what I suggested above holds some promise, it may well be possible to rescue a potentially large population of LKS-affected language-isolated children in the world with Lenneberg's "two legacies" in mind.

References

Alajouanine, Théophile & François Lhermitte. 1965. Acquired aphasia in children. *Brain 88, 653–662*.

Aristotle. 1938. *On Interpretation*, with Greek text translated by Harold P. Cook. Loeb Classical Library. Cambridge, MA: Harvard University Press.

Arle, Jeffrey & Jay Shils (eds.) 2017. *Innovative Neuromodulation*. London: Academic Press.

Bear, Mark F., Barry W. Connors & Michael A. Paradiso. 2007. *Neuroscience: Exploring the brain*. Lippincott Williams & Wilkins/Wolters Kluwer Health.

Benasich, April A., Zhenkun Gou, Naseem Choudhury & Kenneth D. Harris. 2008. Early cognitive and language skills are linked to resting frontal gamma power across the first 3 years. *Behavioural Brain Research* 195, 215–222.

Benítez-Burraco, Antonio. 2013. Genetics of language: Roots of specific language deficits. In Cedric Boeckx & Kleanthes K. Grohmann (eds.), *The Cambridge Handbook of Biolinguistics*, 375–412. Cambridge: Cambridge University Press.

Benítez-Burraco, Antonio. 2016. A biolinguistic approach to language disorders: towards a paradigm shift in clinical linguistics. In Koji Fujita & Cedric Boeckx (eds.), *Advances in Biolinguistics: The Human Language Faculty and Its Biological Basis*, 256–271. London: Routledge.

Benítez-Burraco, Antonio & Cedric Boeckx. 2014. Language disorders and language evolution: Constraints on hypotheses. *Biological Theory* 9, 269–274.

Benítez-Burraco, Antonio & Cedric Boeckx. 2015. Approaching motor and language deficits in autism from below: A biolinguistic perspective. *Frontiers in Integrative Neuroscience* 9, 1–4.

Benítez-Burraco, Antonio & Elliot Murphy. 2016. The oscillopathic nature of language deficits in autism: from genes to language evolution. *Frontiers in Human Neuroscience* 10: 120. doi:10.3389/fnhum.2016.00120.

Berwick, Robert C., Angela D. Friederici, Noam Chomsky & Johan J. Bolhuis. 2013. Evolution, brain, and the nature of language. *Trends in Cognitive Sciences* 17, 89–98.

Berwick, Robert C. & Noam Chomsky. 2016. *Why Only Us: Language and Evolution*. Cambridge, MA: MIT Press.

Billard, Catherine, Joel Fluss & Florence Pinton. 2009. Specific language impairment versus Landau-Kleffner syndrome. *Epilepsia* 50 (Suppl. 7), 21–24.

Bishop, Dorothy V.M. 1982. Comprehension of spoken, written and signed sentences in childhood language disorders. Journal of Child Psychology and Psychiatry 23, 1–20.

Bishop, Dorothy V. M. 1985. Age of onset and outcome in 'acquired aphasia with convulsive disorder' (Landau-Kleffner syndrome). *Developmental Medicine and Child Neurology* 27, 705–12.

Bishop, Dorothy V. M. 2000. Pragmatic language impairment: a correlate of SLI, a distinct subgroup, or part of the autistic continuum? In Dorothy V.M. Bishop & Laurence B. Leonard (eds.), *Speech and Language Impairments in Children: Causes, Characteristics, Intervention and Outcome*, 99–113. London/New York: Routledge.

Bishop, Dorothy V. M. & Laurence B. Leonard (eds.) 2000. *Speech and Language Impairments in Children: Causes, Characteristics, Intervention and Outcome*. London/New York: Routledge.

Boeckx, Cedric & Víctor M. Longa. 2011. Lenneberg's views on language development and evolution and their relevance for modern biolinguistics. *Biolinguistics* 5, 254–273.

Boeckx, Cedric & Kleanthes K. Grohmann (eds.). 2013. *The Cambridge Handbook of Biolinguistics*. Cambridge: Cambridge University Press.

Buzsáki, György. 2006. *Rhythms of the Brain*. Oxford: Oxford University Press.

Buzsáki, György & Brendon O. Watson. 2012. Brain rhythms and neural syntax: Implications for efficient coding of cognitive content and neuropsychiatric disease. *Dialogues in Clinical Neuroscience* 14, 345–367.

Catani, Marco & Marsel Mesulam. 2008a. What is a disconnection syndrome? *Cortex* 44, 911–913.

Catani, Marco & Marsel Mesulam. 2008b. The arcuate fasciculus and the disconnection theme in language and aphasia: History and current state. *Cortex* 44, 953–961.

Chomsky, Carol. 1986. Analytic study of the Tadoma method: Language abilities of three deaf-blind subjects. *Journal of Speech and Hearing Research* 29, 332–347.

Chomsky, Noam. 1965. *Aspects of the Theory of Syntax*. Cambridge, MA: MIT Press.

Chomsky, Noam. 1967. The formal nature of language. Appendix A in Lenneberg (1967), pp. 397–442. New York: Wiley.

Chomsky, Noam. 1986. *Knowledge of Language: Its Nature, Origin and Use*. New York: Praeger.

Chomsky, Noam. 2013. Problems of projection. *Lingua* 130, 33–49.

Chomsky, Noam. 2016. *What Kind of Creatures Are We?* New York: Columbia University Press.

Chomsky, Noam. 2017a. The language capacity: Architecture and evolution. *Psychonomic Bulletin & Review* 24, 200–203.

Chomsky, Noam. 2017b. Language architecture and its import for evolution. *Neuroscience and Biobehavioral Reviews*. doi:10.1016/j.neubiorev.2017.01.053.

Crain, Stephen, Loes Koring & Rosalind Thornton. 2016. Language acquisition from a biolinguistic perspective. *Neuroscience and Biobehavioral Reviews*. doi:10.1016/j.neubiorev.2016.09.004.

Curtiss, Susan. 1977. *Genie: A Psycholinguistic Study of a Modern Day "Wild Child."* New York: Academic Press.

DaSilva, Ednéa, A., Diane C. Chungani, Otto Muzik & Harry T. Chungani. 1997. Landau-Kleffner syndrome: metabolic abnormalities in temporal lobe are a common feature. *Journal of Child Neurology* 12, 489–495.

Deonna, Thierry. 1991. Acquired epileptiform aphasia in children (Landau-Kleffner syndrome). *Journal of Clinical Neurophysiology* 8, 288–298.

Deonna, Thierry. 2000. Acquired epileptic aphasia (AEA) or Landau-Kleffner syndrome: from childhood to adulthood. In Dorothy V.M. Bishop & Laurence B. Leonard (eds), *Speech and Language Impairments in Children: Causes, Characteristics, Intervention and Outcome*, 261–272. London/New York: Routledge.

Deonna, Thierry, Anne Beaumanoir, François Gaillard & Gil Assal. 1977. Acquired aphasia in childhood with seizure disorder: a heterogeneous syndrome. *Neuropädiatrie* 8, 263–273.

Deonna, Thierry, Anne-Claude Prelaz-Girod, Claire Mayor-Dubois & Eliane Roulet-Perez. 2009. Sign language in Landau-Kleffner syndrome. *Epilepsia* 50, 77–82.

Deonna, Thierry & Eliane Roulet-Perez. 2010. Early-onset acquired epileptic aphasia (Landau-Kleffner syndrome, LKS) and regressive autistic disorders with epileptic EEG abnormalities: the continuing debate. *Brain & Development* 32, 746–752.

Deonna, Thierry & Eliane Roulet-Perez. 2016. *The Epilepsy-Aphasia Spectrum: From Landau-Kleffner Syndrome to Rolandic Epilepsy.* London: Mac Keith Press.

Everaert, Martin B.H., Marinus A.C. Huybregts, Noam Chomsky, Robert C. Berwick & Johan J. Bolhuis. 2015. Structures, not strings: Linguistics as part of the cognitive sciences. *Trends in Cognitive Sciences* 19, 729–743.

Faria, Paula, Felipe Fregni, Fernando Sebastião, Ana I. Dias & Alberto Leal. 2012. Feasibility of focal transcranial DC polarization with simultaneous EEG recording: preliminary assessment in healthy subjects and human epilepsy. *Epilepsy & Behavior* 25, 417–425.

Fernández, Javier Ramírez. 2015. Locality in language and locality in brain oscillatory structures. *Biolinguistics* 9, 74–95.

Giraud, Anne-Lise, Andreas Kleinschmidt, David Poeppel, Torben E. Lund, Richard S.J. Frackowiak & Helmut Laufs. 2007. Endogenous cortical rhythms determine cerebral specialization for speech perception and production. *Neuron* 56, 1127–1134.

Gleitman, Lila & Barbara Landau. 2013. Every child an isolate: Nature's experiments in language learning. In Massimo Piattelli-Palmarini & Robert C. Berwick (eds.), *Rich Languages from Poor Inputs*, 91–104. Oxford: Oxford University Press.

Gordon, Neil. 1990. Acquired aphasia in childhood: the Landau-Kleffner syndrome. *Developmental Medicine and Child Neurology* 32, 270–274.

Gordon, Neil. 1997. The Landau-Kleffner syndrome: increased understanding. *Brain & Development* 19, 311–316.

Gou, Zhenkun, Naseem Choudhury & April A. Benasich. 2011. Resting frontal gamma power at 16, 24 and 36 months predicts individual differences in language and cognition at 4 and 5 years. *Behavioural Brain Research* 220, 263–270.

Hickok, Gregory. 2009. The functional neuroanatomy of language. *Physics of Life Reviews* 6, 121–143.

Hickok, Gregory. 2012. Computational neuroanatomy of speech production. *Nature Reviews Neuroscience* 13, 135–145.

Hickok, Gregory, John Houde & Feng Rong. 2011. Sensorimotor integration in speech processing: Computational basis and neural organization. *Neuron* 69, 407–422.

Hickok, Gregory & David Poeppel. 2007. The cortical organization of speech processing. *Nature Reviews Neuroscience* 8, 393–402.

Hinzen, Wolfram & Joana Rosselló. 2015. The linguistics of schizophrenia: thought disturbance as language pathology across positive symptoms. *Frontiers in Psychology* 6. doi:10.3389/fpsyg.2015.00971.

Hinzen, Wolfram, Joana Rosselló, Otávio Mattos, Kristen Schroeder & Elisabet Vila. 2015. The image of mind in the language of children with autism. *Frontiers in Psychology* 6. doi:10.3389/fpsyg.2015.00841.

Hirsch, Edouard, Maria Paola Valenti, Gabrielle Rudolf, Caroline Seegmuller, Anne de Saint Martin, Pierre Maquet, Norma Wioland, Marie-Noëlle Metz-Lutz, Christian Marescaux & Alexis Arzimanoglou. 2006. Landau-Kleffner syndrome is not an eponymic badge of ignorance. *Epilepsy Research* 70, 239–247.

Hoshi, Koji & Kyoko Miyazato. 2016. Architecture of human language from the perspective of a case of childhood aphasia—Landau-Kleffner syndrome. *Biolinguistics* 10, 136–196.

Jenkins, Lyle. 2000. *Biolinguistics: Exploring the Biology of Language*. Cambridge: Cambridge University Press.

Kaga, Makiko. 1999. Language disorders in Landau-Kleffner syndrome. *Journal of Child Neurology*, 118–122.

Kaga, Makiko. 2000. Yoosyoozi no tyookakusitunin-Landau-Kleffner syookoogun to herupesu nooen kooisyoo [Auditory agnosia in children—Landau-Kleffner syndrome and herpes encephalitis sequela]. In Kimitaka Kaga (ed.), *Tyuusuusei tyookaku syoogai no kiso to rinsyoo* [*Basics and Clinical Medicine in Central Nervous Auditory Disorders*], 90–94. Tokyo: Kanehara Shuppan.

Kaga, Makiko. 2011. Landau-Kleffner syndrome. *Rinsyoo Seisin Igaku* 40, 325–327.

Kuhl, Patricia K. 1993. Early linguistic experience and phonetic perception: implications for theories of developmental speech perception. *Journal of Phonetics* 21, 125–139.

Kuhl, Patricia K., Barbara T. Conboy, Denise Padden, Tobey Nelson & Jessica Pruitt.

2005. Early speech perception and later language development: Implications for the "critical period." *Language Learning and Development* 1, 237–264.

Landau, William & Frank Kleffner. 1957. Syndrome of acquired aphasia with convulsive disorder in children. *Neurology* 7, 523–530.

Lenneberg, Eric. 1962. Understanding language without ability to speak: A case report. *Journal of Abnormal and Social Psychology* 65, 419–425.

Lenneberg, Eric. 1964. Language disorders in childhood. *Harvard Educational Review* 34(2), 152–177.

Lenneberg, Eric. 1967. *Biological Foundations of Language*. New York: Wiley.

Lenneberg, Eric. 1969. On explaining language. *Science* 164, 635–643.

Lenneberg, Eric. 1975. In search of a dynamic theory of aphasia. In Eric H. Lenneberg & Elizabeth Lenneberg (eds.), *Foundations of Language Development: A Multidisciplinary Approach, vol.2*, 3–20. New York: Academic Press / Paris: The UNESCO Press.

MacNeilage, Peter F. & Barbara L. Davis. 2001. Motor mechanisms in speech ontogeny: phylogenetic, neurobiological and linguistic implications. *Current Opinion in Neurobiology* 11, 696–700.

Massa, Rita, Anne de Saint-Martin, Edouard Hirsch, Christian Marescaux, Jacques Motte, Caroline Seegmuller, Catherine Kleitz, Marie-Noëlle Metx-Lutz. 2000. Landau-Kleffner syndrome: sleep EEG characteristics at onset. *Clinical Neurophysiology* 111 (Suppl. 2), 87–93.

Mikati, Mohamad Abdul, Rana M. Kurdi & Alhan N. Shamseddine. 2010. Landau Kleffner syndrome. In Harry A. Whitaker (ed.), *Concise Encyclopedia of Brain and Language*, 259–263. Oxford: Elsevier.

Murphy, Elliot. 2015. The brain dynamics of linguistic computation. *Frontiers in Psychology* 6. doi:10.3389/fpsyg.2015.01515.

Murphy, Elliot. 2016. The human oscillome and its explanatory potential. *Biolinguistics* 10, 6–20.

Murphy, Elliot & Antonio Benítez-Burraco. 2016a. Language deficits in schizophrenia and autism as related oscillatory connectomopathies: An evolutionary account. *Neuroscience and Biobehavioral Reviews.* doi:10.1016/j.neubiorev.2016.07.029.

Murphy, Elliot & Antonio Benítez-Burraco. 2016b. Bridging the gap between genes and language deficits in schizophrenia: An oscillopathic approach. *Frontiers in Human Neuroscience* 10, 422. doi:10.3389/inhum.2016.00422.

Nitsche, Michael A. & Walter Paulus. 2000. Excitability changes induced in the human motor cortex by weak transcranial direct current stimulation. *Journal of Physiology* 527, 633–639.

O'Grady, William D. 2005. *How Children Learn Language*. Cambridge: Cambridge University Press.

O'Hare, Anne. 2008. Commentary: Age of onset and outcome in Landau-Kleffner syndrome (1985). *Developmental Medicine and Child Neurology* 50, 724.

Pearl, Philip L., Enrique J. Carrazana & Gregory L. Holmes. 2001. The Landau-Kleffner syndrome. *Clinical Science* 1, 39–45.

Penfield, Wilder & Lamar Roberts. 1959. *Speech and Brain-Mechanisms*. Princeton: Princeton University Press.

Petitto, Laura-Ann, Clifton Langdon, Adam Stone, Diana Andriola, Geo Kartheiser

& Casey Cochran. 2016. Visual sign phonology: Insights into human reading and language from a natural soundless phonology. *WIREs Cognitive Science*. doi:10.1002/wcs.1404.

Piekarski, David J., Carolyn M. Johnson, Josiah R. Boivin, A. Wren Thomas, Wan Chen Lin, Kristen Delevich, Ezequiel M. Galarce & Linda Wilbrecht. 2017. Does puberty mark a transition in sensitive periods for plasticity in the associative neocortex? *Brain Research* 1654, 123–144.

Pinker, Steven. 1994. *The Language Instinct: How the Mind Creates Language*. New York: Harper Collins.

Poeppel, David. 2003. The analysis of speech in different temporal integration windows: cerebral lateralization as 'asymmetric sampling in time'. *Speech Communication* 41, 245–255.

Pullens, Pim, Will Pullens, Vera Blau, Bettina Sorger, Bernadette M. Jansma & Rainer Goebel. 2015. Evidence for normal letter-sound integration, but altered language pathways in a case of recovered Landau-Kleffner Syndrome. *Brain and Cognition* 99, 32–45.

Ramanathan, Ramnath Santosh, Tina Ahluwalia & Ankush Sharma. 2012. Landau-Kleffner syndrome- a rare experience. *Eastern Journal of Medicine* 17, 36–39.

Rapin, Isabelle, Steven Mattis, A. James Rowan & Gerald G. Golden. 1977. Verbal auditory agnosia in children. *Developmental Medicine and Child Neurology* 19, 192–207.

Robinson, Richard O., Gillian Baird, Gary Robinson & Emily Simonoff. 2001. Landau-Kleffner syndrome: Course and correlates with outcome. *Developmental Medicine & Child Neurology* 43, 243–247.

Shultz, Sarah, Athena Vouloumanos, Randi H. Bennet & Kevin Pelphrey. 2014. Neural specialization for speech in the first months of life. *Developmental Science* 17, 766–774.

Stefanatos, Gerry. 2011. Changing perspectives on Landau-Kleffner syndrome. *The Clinical Neuropsychologist* 25, 963–988.

Steinlein, Ortrud K. 2009. Epilepsy-aphasia syndromes. *Expert Review of Neurotherapeutics* 9, 825–833.

Sugisaki, Koji. 2016. Structure dependence in child English: New evidence. In Koji Fujita & Cedric Boeckx (eds.), *Advances in Biolinguistics: The Human Language Faculty and Its Biological Basis*, 69–82. London: Routledge.

Tharpe, Anne Marie & Barbara J. Olson. 1994. Landau-Kleffner syndrome: Acquired epileptic aphasia in children. *Journal of the American Academy of Audiology* 5, 146–150.

Tsimpli, Ianthi Maria, Maria Kambanaros & Kleanthes K. Grohmann. Language pathology. To appear in Ian G. Roberts (ed.), *The Oxford Handbook on Universal Grammar*. Oxford: Oxford University Press.

Tsuru, Noriko & Thomas J. Hoeppner. 2007. Brain mechanism of language and its disorder: From the viewpoint of Landau-Kleffner syndrome. *Rinshounouha* 49, 305–311.

Uldall, Peter, Lene Sahlholdt & Jørgen Alving. 2000. Landau-Kleffner syndrome with onset at 18 months and an initial diagnosis of pervasive developmental disorder. *European Journal of Paediatric Neurology* 4, 81–86.

Yang, Charles, Stephen Crain, Robert C. Berwick, Noam Chomsky & Johan J. Bolhuis. 2017. The growth of language: Universal Grammar, experience, and principles of computation. *Neuroscience and Biobehavioral Reviews*. doi:10.1016/j.neubiorev.2016.12.023.

Koji Hoshi
Keio University
Faculty of Economics
4-1-1 Hiyoshi Kohoku-ku
223-8521 Yokohama, Kanagawa
Japan

khoshi@a7.keio.jp

On the Biological Foundations of Language: Recent Advances in Language Acquisition, Deterioration, and Neuroscience Begin to Converge

Barbara Lust, Suzanne Flynn, Janet C. Sherman, Charles R. Henderson, Jr., James Gair[†], Marc Harrison & Leah Shabo

In this paper, experimental results on the study of language loss in prodromal Alzheimer's disease (AD) in the elderly are linked to experimental results from the study of language acquisition in the child, via a transitional stage of Mild Cognitive Impairment (MCI). Recent brain imaging results from a pilot study comparing prodromal AD and normal aging are reported. Both, behavioral results and their underlying neural underpinnings, identify the source of language deficits in MCI as breakdown in syntax–semantics integration. These results are linked to independent discoveries regarding the ontogeny of language in the child and their neural foundations. It is suggested that these convergent results advance our understanding of the true nature of maturational processes in language, allowing us to reconsider a "regression hypothesis" (e.g., Ribot 1881), wherein later acquisition predicts earliest dissolution.

Keywords: Language acquisition; language loss; brain; maturation; Prodromal Alzheimer's disease

1. Introduction

Since Lenneberg's (1967) landmark work on the *Biological Foundations of Language*, the fields of language acquisition, language deterioration, neuroscience (including study of the brain's "language network"), as well as the linguistic theory of a language faculty, have all developed exponentially. At the same time, we are still far from fulfilling Lenneberg's fundamental challenge, that is, "we must try to understand the nature of the maturational processes" (Lenneberg 1967: 126). Now, however, through converging recent interdisciplinary advances, we are poised for new advances in our understanding of maturational processes involved in language acquisition; not only new advances in developmental theory of language acquisition but new advances in realization of brain–behavior relations in the area.

The authors would like to thank the following institutions and individuals: Cornell Bronfenbrenner Center for Life Course Development; Cornell Institute for Translational Research on Aging (CITRA) Pilot Study Program; Cornell University Cognitive Science program; Cornell University Institute for Social Sciences; Hatch Grant/Federal Formula Funds #2013-14-292, 321420; NSF CI 0753415 to Lust and Blume; NSF 1463196 to Lust, Blume and Pareja-Lora (PIs); Dr. Bradley Hyman, Liang Yap, and Jeanette Gunther of the Massachusetts General Hospital Massachusetts Alzheimer's Disease Research Center (MGH ADRC)NIH/NIA P50 AG005134; Sarah Mancuso of the MGH Psychology Assessment Clinic; Azraa Janmohamed for assistance

ISSN 1450–3417

In this paper, we provide one example of such recent interdisciplinary convergence. We link recent discoveries from experimental results on our study of language loss in prodromal Alzheimer's disease (AD) in the elderly to comparable early experimental results from our study of language acquisition in the child. We report recent brain imaging results from a pilot study comparing prodromal AD and normal aging. Our brain imaging results cohere with our behavioral results documenting language loss in prodromal AD, allowing us to adumbrate selected brain-behavior relationships in language dissolution and to begin to identify the nature of language loss in prodromal AD. We then link these results to new independent discoveries on the ontogeny of the neural foundations for language in the child that are emerging from research led by Angela Friederici at the Max Planck Institute for Human Cognitive & Brain Sciences (e.g., Friederici 2016, 2017, this issue). Although such new results have begun to reveal the neural basis for language development and its impairment, until now there has not been clear principled mapping of language acquisition facts or theory to these precise neurobiological results, especially with regard to later language acquisition (although see Friederici 2006, 2016, 2017, Friedrich & Friederici 2005, 2010, Skeide et al. 2014, Vissiennon, Friederici, Brauer & Wu 2017).

Here, we suggest that our recent results on language acquisition and language loss cohere with what has now been independently discovered regarding underlying maturation of the language network. In doing so, these results suggest an expanded approach to the study of language across the lifespan. They allow us to reconsider a *regression hypothesis* (RH; Ribot 1881/2012, 1881, Lust et al. 2015b) as an explanation of the relation between the ontogeny and dissolution of language knowledge. Although our previous results had disconfirmed a version of RH with regard to the acquisition of syntax, our present results invite us to reconsider this hypothesis with regard to syntax–semantics integration. Together, our results advance our understanding of the true nature of maturational processes in language.

2. Comparing Language Loss in Prodromal AD to Language Acquisition in the Child

In our recent work, we have tested language production in populations (ages 58–98) with *mild cognitive impairment* (MCI). In what is now appreciated as a continuum in the development of AD, MCI is a stage of increased risk for AD diagnosis. Individuals at this stage demonstrate a cognitive decline from baseline that is not at the level of dementia (Petersen 2003, 2004, Wicklund & Petersen 2014). Individuals with a diagnosis of MCI are know to convert to AD at a higher level than cognitively normal individuals (Morris & Cummings 2005, Talbert et al. 2006, Dickerson et al. 2007, Chapman et al. 2010, Roberts et al. 2014). We compared language in MCI to *healthy aging* (HA; 62–87 years), *healthy young* (HY; 20–29 years), and children (ages 3;5 to 7;6 in years;months) with matched experimental designs. Through an interinsitutional collaborative infrastructure we compared 51 MCI subjects to 24 HA to

---

in data management; Ryutaro Uchiyama for graphic image conversion; Professor Nathan Spreng for consultation and guidance in imaging analyses, and manuscript comments; Tom Wasow for comments on linguistic design; Rebecca Goldweber and Prajakta Kalangutkar for assistance in manuscript preparation. Lastly, we appreciate two incisive anonymous reviews.

10 HY across a series of linguistic experiments testing various forms of sentence formation (relative clauses, coordinate sentences, adjoined clause sentences), using an elicited imitation (EI) task. The EI task has been shown to require reconstructive analysis of both syntax and semantics in sentence structure (e.g., Lust et al. 1996). Used with controlled and standardized experimental designs, results are shown not to depend solely on memory but on analytic sentence reconstruction (Blume & Lust 2017). The EI experiments were complemented by two other tests of language knowledge, and by a general cognitive assessment (Addenbrook's cognitive exam revised; ACE-R; see Mioshi et al. 2006) and a test of working memory, the Brown-Petersen test (also referred to as the Auditory Consonant Trigram Test; Brown 1958, Belleville, Chertkow, & Gauthier 2007), as well as by a general sociodemographic background assessment.

For example, one such experiment compared production of complex sentences with varied forms of relative clauses across these groups, using an experimental design that had been previously used with children. All sentences within each study were controlled for structural variables, as well as length and lexical frequency, in conjunction with an EI task. The children had been tested earlier with this experimental design using sentences such as exemplified in Table 1a. Data, methodology, and results from this child study were archived in the Cornell Language Acquisition Lab Virtual Center for Language Acquisition.[1] Results from the child study had revealed a developmental progression in the first language acquisition of relative clause structure (Flynn & Lust 1980).

Determinate Head	S	Big Bird pushes the balloon which bumps Ernie.
	O	Ernie touches the balloon which Big Bird throws.
Headless	S	Cookie Monster hits what pushes Big Bird.
	O	Cookie Monster pushes what Big Bird throws.

(a)

Determinate Head	S	The attorney presented the evidence which freed the defendant.
	O	The shopkeeper discounted the merchandise which the customer bought.
Headless	S	The state policeman discovered what troubled the private detective.
	O	The philosophy teacher pondered what the research scientist said.

(b)

Table 1: Example Sentences for elicited imitation for children in (a), for adults in (b).

---

[1] Experimental design, methods, and results from this early study are banked in the web-based Virtual Center for Language Acquisition Database, DTA tool (Data Transcription and Analysis tool) and are available on request. See Pareja-Lora, Blume, & Lust (in press), as well as Blume, Flynn, & Lust (2012).

Adult populations, including HA, HY, and MCI, were more recently tested on structurally matched sentences such as exemplified in Table 1b (Lust et al. 2015). MCI participants were assessed at Massachusetts General Hospital (MGH) through neurologic evaluations and a battery of standardized neuropsychological and clinician evaluations. Subjects' diagnoses of MCI were based on neurological evaluation including Clinical Dementia Rating scores (CDR; Morris 1993) and performance on neuropsychological tests from the Uniform Data Set (UDS; Morris et al. 2006, Weintraub et al. 2009, Monsell et al. 2012), as well as additional standardized neuropsychological measures. HA participants were recruited from MGH and administered the same tests as the MCI population to provide a control HA population.[2] HY adults were recruited at the Massachusetts Institute of Technology from the student and administrative population. Based on a sociodemographic questionnaire, all participants in this study reported no history of neurological disorders or events.

Results from this comparative study were surprising. The relative clause structure that was last developed by the child and most difficult in language production through childhood to approximately 7 years of age was the lexically headed type; that is, those with a determinate lexical noun head such as *balloon* in the examples in Table 1a. We have argued that this English lexically determinate headed relative clause represents complex language-specific computation, explaining its later acquisition (Flynn et al. 2005). However, it was this determinate lexically headed relative clause structure that was best retained in the MCI subjects. The headless

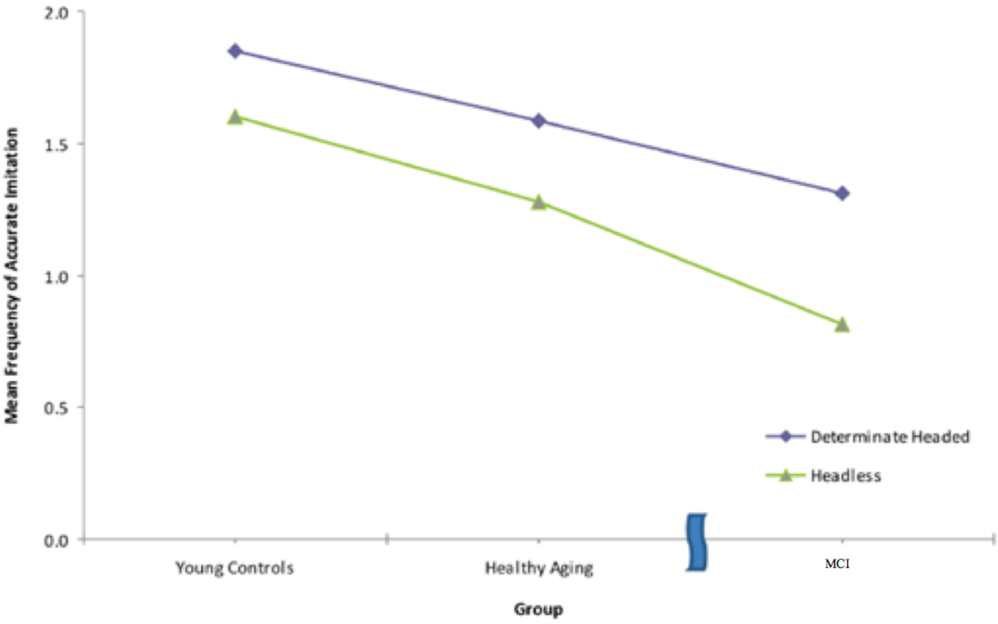


Figure 1: Frequency of accurate imitation for each type of relative for each adult group. (See Lust et al. 2015b for details.)

---

[2] A subset of 13 HA were tested at Cornell, and self-declared cognitively normal, i.e., although they presented as non-impaired, they did not receive the same diagnostic tests as did the clinical MGH subjects and the MGH tested HA controls.

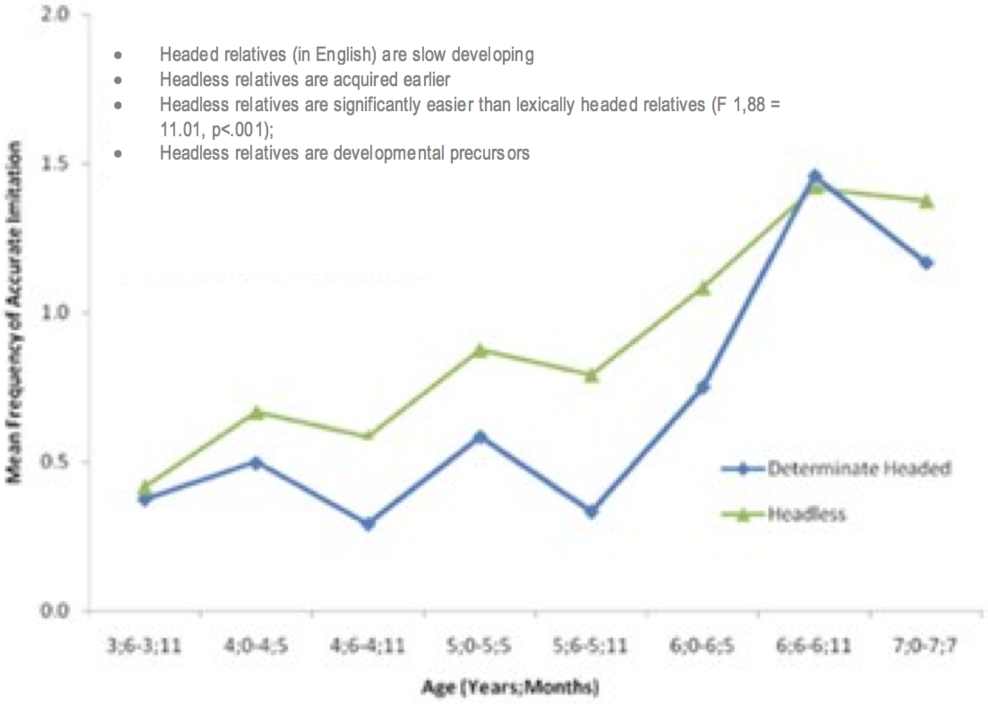


Figure 2: Frequency of accurate imitation by children for each type of relative for each age group. (See Flynn & Lust 1980 for details. The total N of 96 children was divided to eight 6-month age groups.)

relative clause type that was predominant in child language production early and throughout development was the "headless relative", e.g., the *what*-headed structure in the examples in Table 1a. Yet, it was this headless relative type of structure that posed significantly greater difficulty for the MCI. As we have argued (Lust et al. 2015b, 2017), these results disconfirmed a RH ("Ribot's hypothesis") with regard to the acquisition of syntax. It was not true that what was last acquired by the child was first lost by the adult in language deterioration (see Figures 1 and 2).

At the same time, results from this study also began to provide evidence on the fundamental nature of language deterioration in prodromal AD. MCI subjects differed significantly from HA and HY in their overall language production. However, this was not the case with regard to their production of the determinate headed relatives, the complex syntactic structure that is last acquired by the child. In spite of the syntactic complexity of headed relatives, MCI subjects produced these with significantly greater facility than they did the headless relative. The MCI subjects failed in production especially of the headless relative, the very structure that had been developmentally prominent. Example of imitation performance by an MCI individual is shown in (1a) and (1b). In a randomized set of EI sentences, the individual correctly reproduced lexically headed relatives such as in (1a), without change, while distorting headless relatives as in (1b).

(1) a. *Headed Relatives EI (TS033): Successful imitations (no change)*
The attorney presented the evidence which freed the defendant.

The shopkeeper discounted the merchandise which the customer bought.
The physician formulated the therapy which cured the patient.

b. *Headless Relatives EI (TS033): Unsuccessful imitations (changes from stimulus sentences)*
   i. Target: The state policeman discovered what troubled the private detective.
      Response: The st. . . policeman something discovered what.
   ii. Target: The philosophy teacher pondered what the research scientist said.
      Response: The pondered what the philosopher said.
   iii. Target: The peace activist protested what the vice president suggested.
      Response: The peace activist suggested other someone suggested.

This distinction in EI performance (summarized in Figure 1) held in spite of the experimental controls on length (all 17 syllables), general structure, and lexical frequency that characterized all sentences across the design.

An example of the structure of an MCI production error on a headless relative is shown in Figure 3.

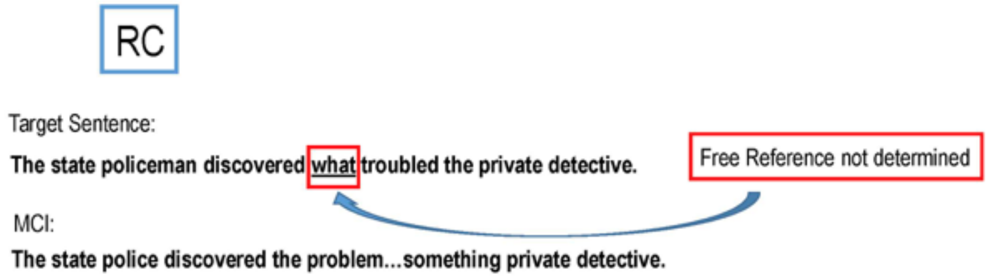


Figure 3: Example of structure of MCI subject's (GC533) production of a headless relative clause (RC).

3. The Nature of Language Deterioration in MCI

These results not only verified language deterioration in prodromal AD (MCI), but also began to provide evidence on the nature of this language deficit. It appeared that the complex syntax of a well-formed lexically headed relative clause was relatively well retained in MCI language. What was impaired was a case where the subject must specify an undetermined reference (e.g., whatever it was that troubled the detective in the example in Figure 3 above) and link it to the sentence structure. The difficulty that these sentences demonstrate appears to concern the integration of syntax, necessary for sentence production, with the semantics of the sentence, especially the computation of external reference, necessary for sentence interpretation. Specifically, this was a case when the subject had to compute an interpretation of indeterminate reference reflected in the headless relative and then integrate this semantic and pragmatic computation with computation of the syntax of the sentence. Although all sentences require an integration of syntax and semantics, the headless sentences appear to provide a particular challenge to this integration, which must be involved in all sentence processing.

These results suggest that language deterioration in prodromal AD targets the syntax–semantics interface in the language faculty (Figure 4; see Berwick et al. 2013) and suggests that this interface is the locus of this early language breakdown, rather than core syntactic computation. Subsequent experiments with other types of complex sentences which vary indeterminate reference (e.g., coordinate and adverbial subordinate sentences with or without free pronouns that are indeterminate in reference), and experiments with other tasks, confirmed this area of language breakdown (e.g., Lust et al. 2014, Lust et al. 2015a, Sherman et al. 2015a,b, Sherman 2017).

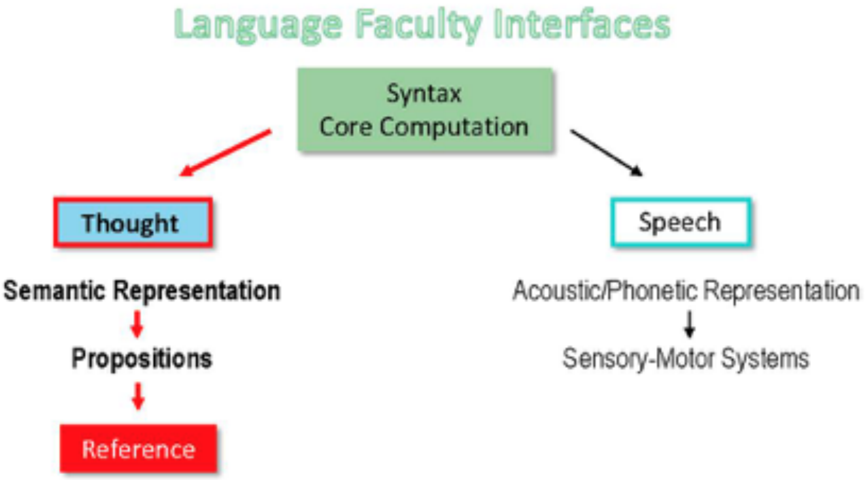


Figure 4: Schematic of the language faculty and its interfaces with other systems. (Illustration based on Berwick et al. 2013.)

3.1. Dissociating Memory and Language

Although memory and language performance are inextricably linked, it is clear from a comparison of headed and headless sentences in our controlled design, for example, (1a) and (1b), where headed sentences of equal length and general syntactic complexity are imitated significantly better than the headless sentences in (1b), that memory alone cannot explain these results in our MCI population. This conclusion was supported by our regression analyses of memory tests against linguistic performance. Both the ACE-R and the Brown Petersen tests did show that the groups significantly differed, with MCI showing significantly deficited performance. However, performance of subjects on the ACE-R memory subcomponent did not significantly predict linguistic performance on the RC sentences overall groups (regression estimate = .06577, p = .19) or within any group. More specifically, the Brown Petersen test of Working Memory also did not, not overall groups (regression estimate = .00141, p = .81) or within any group, including MCI (regression estimate = .002819, p = .79). These results would appear to cohere with independent psycholinguistic research which has failed to find effects of working memory on online processing, suggesting its alternative role on post interpretive processing (Caplan et al. 2011; also see Waters, Caplan, & Rochon 1995 for generalization to studies of Alzheimer's disease populations).

Thus we conclude that the nature of language deterioration that has been discovered in MCI is language-related, rather than determined simply by domain-general cognitive deficits, such as memory.

4. Pursuing Biological Foundations

Recent advances in neuroscience with regard to the biological foundations of a language network in the brain have allowed us to begin to test a hypothesis regarding the neurobiological foundations for language dissolution in MCI, which was suggested by our linguistic behavioral results.

Independent analyses of language processing have yielded a model wherein sentence processing involves distinct, incremental sequential components serially ordered such that syntax–semantics integration is distinct from earlier phonological and syntactic sentence computation. Integration of syntax and semantics occurs finally, as a late and distinct stage in serial processing within the dominant (i.e. left) hemisphere, as suggested in Figure 5. This model sketches the neural underpinnings of the temporal process of sentence comprehension as schematized in the illustration. The final integration step is focused on a posterior portion of the superior temporal gyrus (STG), an area of the language network that merges with the angular gyrus and supramarginal gyrus of the inferior parietal lobule (IPL; Friederici 2002, 2011, 2012, Friederici & Kotz 2003). Integration takes place at a later phase of language comprehension—in a neural network implicating the IPL/STG hub at later phases (Fengler et al. 2016).

Recent studies from independent labs have begun to map psycholinguistic data to the neural foundations of processing. For example, semantic processing in AD patients has provided evidence for the contribution of superior temporal and inferior parietal regions of the left hemisphere (Grossman et al. 1997). Studies of pronoun reference resolution have suggested a "two component model for resolving a pronoun's reference" and observe inclusion of inferior parietal cortex (IPC) activation to account for an "integration of probabilistic and value information" in resolving a pronoun's reference (McMillan et al. 2012: 674, 685).

Independent advances have also identified the IPL as a critical integrative hub in a network of functionally connected brain regions referred to as the Default Network (DN). The DN is generally implicated in internalized or associative processing and is typically suppressed during externally directed attention tasks (Bar et al. 2007, Buckner et al. 2008, Andrews-Hanna, Smallwood, & Spreng 2014). The

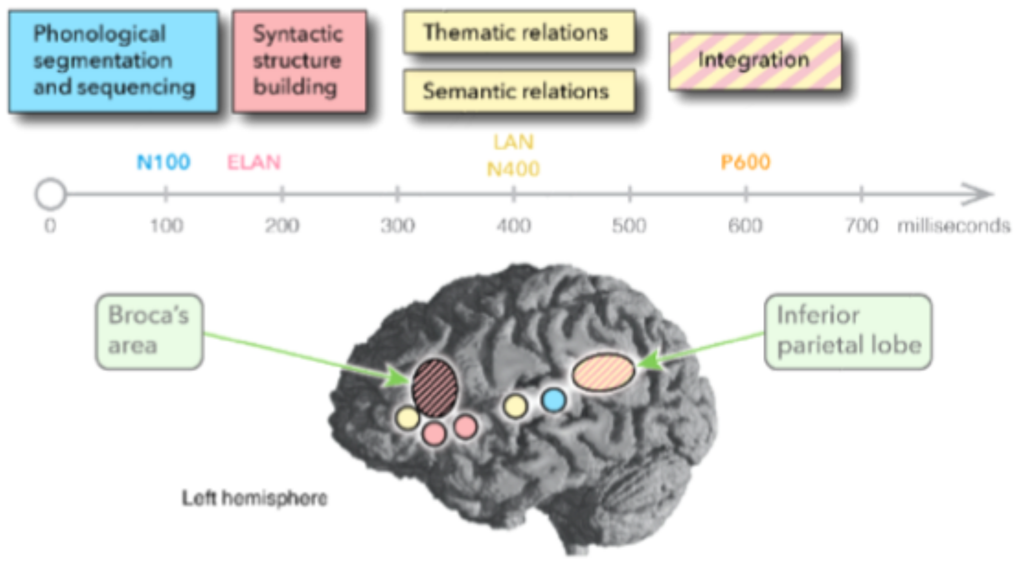


Figure 5: Schematic of the Serial Sentence Processing Model. (Adapted from Friederici 2002, Friederici & Kotz 2003.)

IPL, part of the DN, provides a cross-modal integrative hub, with connectivity to both the Posterior Cingulate Cortex (PCC), a core hub of the DN, and also to frontal and temporal regions involving classic Broca's and Wernicke's areas. The default-aligned node, left IPL, has been observed to facilitate modulation (i.e. suppression) of the DN (Menon and Uddin 2010, Spreng et al. 2013: 83; also see Friederici 2011). Recent research is investigating the degree to which the DN underlies cognition (Marguiles et al. 2016).

It has been suggested that loss of functional and structural integrity of the DN and its interactions with other brain networks occurs in older adulthood and may have clinical significance (Greicius et al. 2004, Seeley et al. 2009, Anticevic et al. 2012: 586, Turner & Spreng 2015, Spreng et al., in press). In AD, DN is disrupted "early as the disease progresses" evidencing "hypometabolism" and accelerated atrophy in DN connectivity hubs (Buckner et al. 2005, 2008: 28, Andrews-Hanna et al. 2014).

The linguistic behavioral evidence we found regarding the nature of the language difficulties in our MCI subjects suggests deterioration in the integration of external reference and the syntax of sentence construction, the last phase in the Serial Sentence Processing Model described above, targeting the IPL area in language connectivity. This deficit would be consistent in general with DN disruption and with deterioration in the DN connectivity hub involving the left IPL. We may assume that the syntactic composition of sentence structure must comprise internal cognition; and that some form of suppression of such internal computation must be involved in the cognitive computation involved in determining semantic and pragmatic reference to the external world.

Thus, both linguistic and neurocognitive foundations lead us to the hypothesis that damage in the IPL area would cohere with deterioration of the integration of syntax and semantics, which our behavioral language data have suggested is compromised in MCI. Our linguistic behavioral results, which characterize prodromal AD in MCI, suggest that cortical degeneration may occur early in this area, at the MCI phase, even before AD is clinically diagnosed. Independent studies have shown that the IPL appears to deteriorate early in the course of neural degeneration in prodromal AD, evidenced by relatively early atrophy of cortical grey matter in this area (e.g., Greene et al. 2010: 1304, Jacobs et al. 2011, Hanggi et al. 2011; the finding of early prodromal temporo-parietal involvement with frontal areas relatively spared is also indicated with other methodologies, e.g., position emission tomography [PET]; Minoshima et al. 1997, Small et al. 2006). This early atrophy of the IPL differentiates from Broca's area (i.e. Brodmann areas 44 and 45), for example. Atrophy in prodromal AD is not global: '[E]ven in end stage AD, distinct language-associated gyri are spared while others show severe atrophy" (Harasty et al. 1999: 682).

Since our linguistic behavioral data suggest good retention of syntax per se (e.g., as reflected in the relatively good performance on lexically headed relatives) in the MCI population, both our language data and neuroscientific data, as well as current sentence processing and cognitive modeling, predict that we may find relatively damaged IPL area in our MCI subjects, in contrast to relatively spared Broca's area (i.e. inferior frontal gyrus) which is generally associated with syntax (e.g., Friederici, this issue).

4.1. Hypothesis

On the basis of the neuroscientific results discussed above as well as our linguistic behavioral results regarding language deterioration in MCI, we hypothesized that, in our MCI subjects, the IPL area, critical to syntax–semantics integration, would show significantly more grey matter deterioration in contrast to HA than Broca's area as a frontal area implicated in syntactic processing.

4.2. Participants

In a first pilot study, we have now conducted volumetric analysis of brain images of six MCI subjects from the total group who participated in our experimental linguistic tests (Lust et al. 2015b) and compared them to a sample of HA. Structural MRI scans of this subset of MCI subjects were acquired during clinical diagnostic testing (Siemens 3T TIM Trio). Their scans were compared to healthy control matched templates derived from ADNI (Alzheimer's Disease Neuroimaging Initiative; Mueller et al. 2005, Weiner et al. 2010, adni-info.org 2016). Three ADNI scans were matched

Group	n	Age: Mean	Age: Range	Males	Yrs. Education: Mean	Yrs. Education: Range	Handedness (% Right)
MCI	6	77	68–88	6	17.33	14–20	83.33 %
Healthy Aging (ADNI)	18	78–79	65–88	18	15.78	8–20	100 %

Table 2: Participants—MRI scanning. Note: One scan was eliminated in linguistic analyses because of missing linguistic data.

to each MCI participant in age, sex, and education, providing a total of 18 healthy aging scans for comparison with the selected MCI subjects.

Five of the six MCI subjects were diagnosed as MCI amnestic, single domain; one as multi domain, non-amnestic. MCI subjects had MMSE scores ranging from 23 to 29, mean 26.8, while ADNI subjects all had perfect scores of 30.

4.3. Methods

Volumetric analysis was conducted on the structural MRIs of the MCI subjects using Voxel Based Morphometry (VBM) methods (Kurth et al. 2015). Volumetric data were compared to the 18 HA matched scans (derived from ADNI). Regions of Interest (ROIs) of IPL and Broca's area were created to assess regional specificity. We examined the relation between cortical volume in these two left hemisphere (LH) ROIs linked to linguistic processing with the goal of initiating a test of our hypothesis and a prototype methodology for identifying neural correlates of observed linguistic behavior.

All scans involved structural T1-weighted images obtained on a 3-tesla (T) scanner. Scanning of MCI subjects took place at MGH Radiology division as an independent component of diagnostic clinical testing. Deidentified scans were provided by MGH to study investigators (per IRB) for research purposes. ADNI dataset participants were scanned using 3-T GE Medical Systems scanners. The scanners collected T1-weighted (T1w) 3D anatomical spoiled gradient echo (SPGR) sequences (256×256 matrix; voxel size = $1.2 \times 1.0 \times 1.0$ mm3; TI = 400 ms; TR = 6.98 ms; TE = 2.85; flip angle = 11°).

4.4. Analyses by VBM

MRI scans (deidentified structural T1 scans) were imported into SPM in DICOM format and transformed to NIFTI. Scans were corrected for left/right orientation. Scans were then segmented into grey, white, and cerebral spinal fluid using the VBM toolbox in SPM8. Next, grey matter sections were quality controlled for homogeneity of variance again using VBM, then coregistered to MNI space using a FSL "152T1-avg" template.

All subject scans were preprocessed using SPM8 and the protocol specified in Kurth et al. (2015). Segmented and smoothed grey matter masks were linearly aligned to a DARTEL template that was manually created from 20 HA and 20 MCI ADNI subjects. Left and right IPL volumes were taken from Harvard-Oxford Cortical Atlas (HOA) masked and binarized using FSL Maths. Subsequently, masked IPL regions from subjects were extracted from the HOA and ROI volumes were calculated for each subject. Using FSL, ROIs of left Broca's area and left IPL were created using HOA, with a threshold at 50 % and binarized using FSL Maths. Thereafter mean values of voxels in each ROI were extracted and tabulated using FSL Stats by applying the mask to scans and isolating mean voxel values.

4.5. Statistical Analyses

We examined two types of models. The first model looks at brain volume as a function of brain area and MCI status in the sample of 30 subjects. IPL versus

Broca's area is a repeated measure on each individual, a fixed classification factor denoted ROI. Group (HA vs. MCI) is a second fixed classification factor. The model includes these two factors and their interaction; individuals are included as levels of a random classification factor. Analysis was by a general linear mixed model. Brain volume for IPL and Broca's area is the proportion of grey matter relative to a template for heathy aging.

To study the relation between cognitive status as measured by brain imaging and status as measured by linguistic tests, the second type of model examined RC (relative clause experiment) scores for proportion correct as the dependent variable, with the repeated measures for that task, TR (type of relative) and FC (functional role, i.e., subject or object in relative clause), as fixed classification factors; a brain volume measure as a covariate, with regressions specified separately by TR × FC; and individuals as levels of a random classification factor. Separate models were analyzed for each of IPL and Broca's area volume measures. This analysis was carried out on MCI individuals only. The key test is of homogeneity of regressions of RC scores on brain volume variables by TR and FC subclasses. An alternative model that had greater numerical stability specified the IPL and Broca's area regressions separately only for the 3 levels of TR, pooling across FC. Analysis was by a logistic-linear mixed model with binomial error assumption and a logit link function. Degrees of freedom were computed by a first-order Kenward-Rogers method.

4.6. Results

The analyses described above revealed:

1. Lower IPL grey matter integrity in MCI than HA; similar Broca's area integrity between them.
2. Greater mean difference between IPL (DN connectivity hub) and Broca's area (syntax related) neurodegeneration in MCI than HA scans (significant Group × Area interaction: $p = .03$).
3. The regressions of syntactic performance on ROI IPL were significantly different for structures requiring reference resolution and syntax integration (determinate headed vs. headless relative clauses; see Table 3). Test of homogeneity of regressions: $p = .011$.

Results from initial brain scan analyses suggest that a significant pattern of biomarking may link to an observed pattern of behavioral linguistic deficits. MCI may involve significant early neural degeneration in an area central to syntax–semantics integration in sentence processing and an integration hub of the DN, including the IPL. This area appears to be significantly more compromised in prodromal AD (MCI) compared to that in inferior frontal gyrus or Broca's area (Brodmann areas 44 and 45). This may indicate correlation with the linguistic pattern we have found

Sentence Type		**ROI IPL**
	Estimate	−.118
Relative Headed	*SE*	5.376
	P	.983
	Estimate	4.701
Relative Headless	*SE*	6.549
	P	.476

Table 3: Results of regression analysis: Relative clause type and ROI IPL.

in the language deterioration of MCI: deficit in syntax-semantic integration, compared to relatively spared basic syntactic computation (cf. Payne et al. 2016).

Linguistics behavioral results cohere with current structural and functional connectivity models of language knowledge and processing, as well as with observed neurodegeneration patterns in progressive development of prodromal AD.

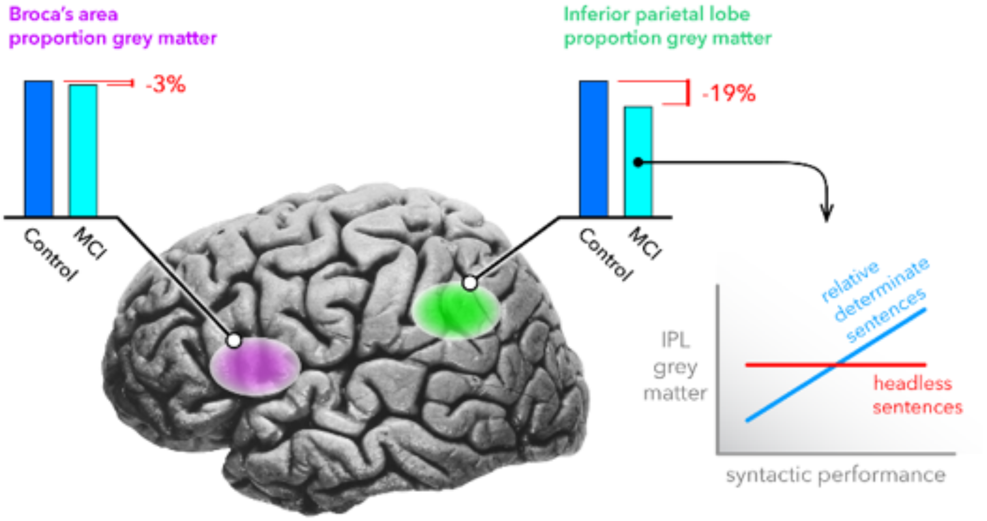


Figure 6: Proportion grey matter (relative to template) in Broca's area and inferior parietal lobe (IPL).

5. The Neurobiology of Brain Development

Independently, results from study of the neural bases of language development (analyzing both grey and white matter; Friederici 2006, 2012, 2016, see Friederici et al. 2017 for a review) have now provided evidence that "brain systems underlying language processing are in place already early in development" (Friederici 2006: 949). Connectivity of the language network (involving both white and grey matter connectivity) develops over time, taking until 7 years of age (or even adolescence) before completion. Specifically, it is proposed that although ventral systems of language network connectivity are in place even at birth, aspects of the dorsal system "connecting the temporal cortex to Broca's area" develop "much later and [are] still not fully matured at the age of seven" (Friederici 2012: 1; also see Friederici 2017, this issue). This connectivity implicates the posterior area of the language network, which is focal in syntax–semantics integration, as an area of final late development of the architecture of the language network in the child. Given the language processing model we reviewed above, this delayed brain development may involve not the acquisition of syntax per se, but of syntax–semantics integration.

5.1. Convergence of the Neurobiology of Brain Development and the Neurology of Language Loss in Prodromal AD

The results from the brain development studies and our initial studies of cortical loss with dementia suggest a correlation. The area of functional connectivity in the language network that has been discovered in early pilot imaging studies to underlie language deterioration in early prodromal AD generally corresponds to the area Friederici and collaborators have identified as the last developing in normal maturational development, viz., the posterior area of the language network and the default network converging on the IPL. If so, these data and this neurobiological model would appear to support a form of a RH regarding lifespan maturation of language. An area lost early in the course of dementia matures later in childhood.

5.2. *The Nature of Language Acquisition Over Time: Pursuing the Nature of maturational processes*

The challenge now for the development of a full maturational theory is to understand why neurobiological development of the language network appears to take the time it does (until age 7, or even later, adolescence), while much of basic syntax is generally acquired by the age of 3 (see, e.g., Lust 2006 for a review). Here we suggest that a coherent theory may be possible. When we look back to the evidence from language acquisition, we find that acquisition of complex syntax in the headed relative clause was late acquired, while another form of relativization, that is, headless forms (the ones we have argued to be more universally available given cross-linguistic typology; Flynn et al. 2005), is accessible early (Flynn & Lust 1980). This evidence was derived from analyses of language production in the child; and in Lust et al. (2015b) it was compared to production data from the prodromal MCI group in dispute of a regression hypothesis of language maturation. However, when we consider the comprehension data from the same child subjects we see that headless relatives are in fact always, across development until 7 years of age and beyond, recognized as semantically difficult. In this case, the child is performing an act-out task where they demonstrate their interpretation of a sentence by moving dolls (see Blume & Lust 2017). The child is naturally challenged to determine the unnamed reference of the head of the relative, that is, the referent of *what* in *Cookie Monster pushes what Big Bird throws*, in the face of several possible referents in the pragmatic context presented to the child.

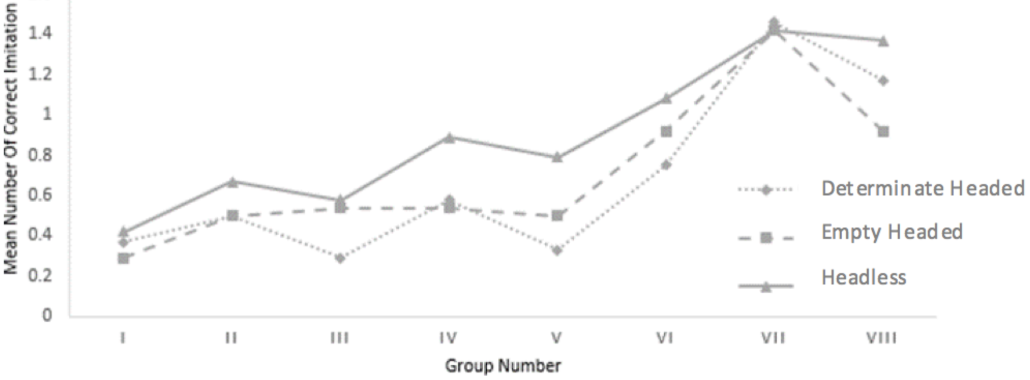


Figure 7: Mean number correct imitation for each head type by age group divided into eight 6-month age groups, ranging from 3.6–3.11 to 7.0–7.7; see Figure 2.

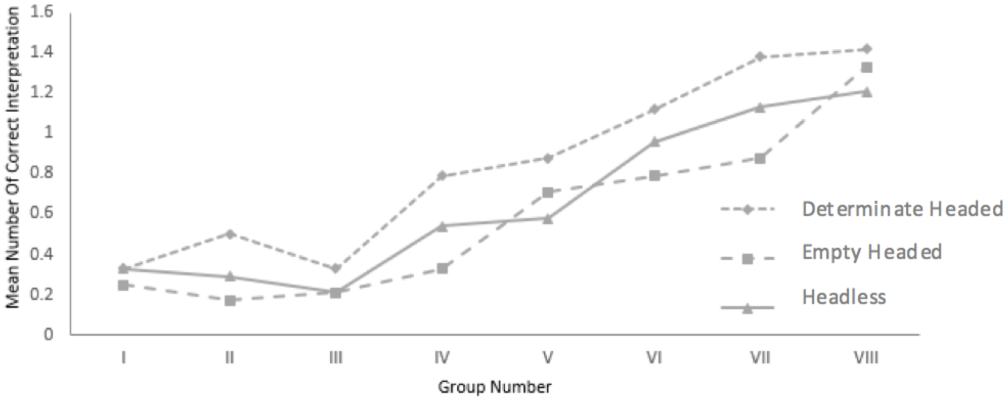


Figure 8: Mean number correct comprehension for each head type by age group. (Figures 8 and 7 result from a full experimental design in which Determinate (concrete headed) relatives are compared both to Headless relatives (cf. Table 1) and also to "empty headed" relatives with the noun head "thing". This 'thing' condition was tested in order to try to dissociate the syntactic property of 'head' vs the semantic property of indeterminacy. See Flynn & Lust 1980.)

Results from comparison of the development of children's production (Figure 7) and comprehension (Figure 8) in this study suggest that syntactic development (more clearly demonstrated in the production task) and semantic development (more clearly demonstrated in the comprehension task) are distinct. Even while comprehension of a headless relative is necessarily challenging semantically to children of all ages, it precedes the headed relative clause in development in the production task; it is only for ages 6.05–6.11 (group 7) and 7.0–7.7 (group 8) that comprehension and production appear to cohere in the integrated production and comprehension of the syntax and semantics in these sentences.

Thus, evidence may suggest a new hypothesis: Just as syntax–semantics integration is compromised in language loss with cerebral deterioration in prodromal AD, so syntax–semantics integration may develop only over time in normal first language acquisition, requiring a more protracted course of development than syntax per se. If so, these developmental results would converge with the neuroscientific evidence suggested by Friederici and collaborators, both for language processing and for language development.

6. Toward A New Developmental Theory

Our results lead us to support the view that in some ways "the brain basis of language develops continuously over time" (Friederici 2006: 941). At the same time, however, we argue that our results do not support a proposal that in language maturation "syntax gradually segregates from semantics in the developing brain" (Skeide, Brauer and Friederici 2014: 1), and "mastery of complex syntax is delayed" in the child. Rather, our results, including converging evidence from language acquisition and language loss, would suggest that syntax and semantics are to some degree independent continuously through development. The child does not show dominance of semantics, for example, in the case of relative clause acquisition, but development of production of syntactically complex sentence formation proceeds even while semantics continues to develop independently. What "matures" or develops is the integration of syntax and semantics. That is, the older child and the adult, including the young and HA adult, efficiently integrate the independent knowledge of syntax and semantics, exemplifying efficient computation at the "language faculty interface" between these in sentence processing. In contrast, language deterioration, as in the case of prodromal dementia with cerebral degeneration, begins to sever this integration, leading to specific deficits where the syntax–semantics integration is most challenging; not in syntax itself. The MCI subjects first begin to fail not in syntactic structures that do not require more indeterminate semantic computation but where semantic computation challenges syntax–semantics integration (as in the computation of indeterminate reference in the case of headless relatives, for example).

This conclusion regarding language maturation coheres with both Friederici's proposed serial processing model for sentence comprehension and the neuroscientific results regarding brain development during language maturation. At the same time, it coheres with our language acquisition studies as well as with our preliminary pilot neuroscientific results in language deterioration in MCI reported here.

Our results also argue for a re-interpretation of the "regression hypothesis" or "Ribot's hypothesis", which seeks to link acquisition with loss in a comprehensive developmental theory. We have seen that this RH does not hold in the case of acquisition of syntax per se (Lust et al. 2015b). The most complex, last-developed syntactic forms of relative clause structures are not the first lost in prodromal AD (MCI). In fact, in contrast to the RH, these are the structures that are best retained. However, based on results from the converging evidence reviewed here, there is new support for the RH hypothesis, at a more general level of analysis: If as we have seen,

(i) neural structures involved in syntax–semantics integration are in fact the last to be developed,

(ii) child language acquisition continuing post age 3 does involve a protracted development of integration of syntax and semantics, and

(iii) first signs of language deterioration and neural degeneration in developing dementia do involve deterioration of the syntax–semantics integration component of language processing,

then, based on our findings and analysis, it can be argued that what takes the most time in the process of language acquisition, and is last developed in the child (i.e. the integration of syntax and semantics), is the most vulnerable and first lost in language deterioration. This would suggest that in a more comprehensive theory of language maturation, the RH deserves new examination.

References

Andrews-Hanna, Jessica R., Jonathan Smallwood, & R. Nathan Spreng. 2014. The default network and self-generated thought: Component processes, dynamic control, and clinical relevance. *The Annals of the New York Academy of Sciences* 1316(1), 29–52. doi:10.1111/nyas.12360.

Anticevic, Alan, Michael W. Cole, John D. Murray, Philip R. Corlett, Xiao-Jing Wang, & John H. Krystal. 2012. The role or default network deactivation in cognition and disease. *Trends in Cognitive Sciences*, 16(12), 584–592.

Bar, Moshe, Elissa Aminoff, Malia Mason, & Mark Fenske. 2007. The units of thought. *Hippocampus* 17(6), 420–428. doi:10.1002/hipo.20287.

Belleville, Sylvie, Howard Chertkow, & Serge Gauthier. 2007. Working memory and control of attention in persons with Alzheimer's disease and mild cognitive impairment. *Neuropsychology* 21(4), 458–469.

Berwick, Robert C., Angela D. Friederici, Noam Chomsky, & Johan J. Bolhuis. 2013. Evolution, brain, and the nature of language. *Trends in Cognitive Sciences* 17(2), 89–98.

Blume, Maria & Barbara Lust. 2017. *Research Methods in Language Acquisition*. Berlin, Germany: Mouton de Gruyter.

Blume, Maria, Suzanne Flynn, & Barbara Lust. 2012. Creating linked data for the interdisciplinary international collaborative study of language acquisition and use: Achievements and challenges of a new virtual linguistics lab. In Christian Chiarcos, Sebastian Nordhoff & Sebastian Hellmann (eds.), *Linked Data in Linguistics: Representing Language Data and Metadata*, 85–96. Heidelberg, Germany: Springer.

Brown, John.1958. Some tests of the decay theory of immediate memory. *Quarterly Journal of Experimental Psychology* 10, 12–21.

Buckner, Randy L., Jessica R. Andrews-Hanna & Daniel L. Schacter. 2008. The brain's default network: Anatomy, function, and relevance to disease. *Annals of the New York Academy of Sciences* 1124(1), 1–38.

Buckner, Randy L., Abraham Z. Snyder, Benjamin J. Shannon, Gina LaRossa, Rimmon Sachs, . . . , & Mark A. Mintun. 2005. Molecular, structural and functional characterization of Alzheimer's disease: Evidence for a relationship between default activity, amyloid and memory. *Journal of Neuroscience* 25(34), 7709–7717.

Caplan, David, Gayle DeDe, Gloria Waters, Jennifer Michaud, & Yorghos Tripodis. 2011. Effects of age, speed of processing and working memory on comprehension of sentences with relative clauses. *Psychology and Aging* 26(2), 439–450.

Chapman, Robert M., Mark Mapstone, John W. McCrary, Margaret N. Gardner, Anton Porsteinsson, Tiffany C. Sandoval, Maria D. Guillily, Elizabeth DeGrush, & Lindsey A. Reilly. 2010. Predicting conversion from mild cognitive impairment to Alzheimer's disease using neuropsychological tests and multivariate methods. *Journal of Clinical and Experimental Neuropsychology* 33(2), 187–199. doi:10.1080/13803395.2010.499356.

Dickerson, Bradford C., Reisa A. Sperling, Bradley T. Hyman, Marilyn S. Albert, & Deborah Blacker. 2007. Clinical prediction of Alzheimer disease dementia across the spectrum of mild cognitive impairment. *Archives of General Psychiatry* 64(12), 1442–1450.

Fengler, Anja, Lars Meyer, & Angela D. Friederici. 2016. How the brain attunes to sentence processing: Relating behavior, structure, and function. *NeuroImage* 129, 268–278.

Flynn, Suzanne, & Barbara Lust. 1980. Acquisition of relative clauses: A preliminary report. In W. Harbert (ed.), *Cornell University Working Papers*, 33–45. Ithaca, New York: Department of Modern Languages and Linguistics, Cornell University.

Flynn, Suzanne, C. Foley, James Gair & Barbara Lust. *Developmental Primacy of Free Relatives in First, Second and Third Language Acquisition: Implications for their Syntax and Semantics*. Paper presented at the Linguistic Association of Great Britain Annual Meeting, Cambridge, England. [University of Cambridge, 31 Aug–03 Sep 2005.]

Friederici, Angela D. & Sonja A. Kotz. 2003. The brain basis of syntactic processes: Functional imaging and lesion studies. *NeuroImage* 20(S1), S8–S17.

Friederici, Angela D. 2011. The brain basis of language processing: From structure to function. *Physiological Reviews* 91(4), 1357–1392.

Friederici, Angela D. 2002. Towards a neural basis of auditory sentence processing. *Trends in Cognitive Sciences* 6(2), 78–84.

Friederici, Angela D. 2006. The neural basis of language development and its impairment. *Neuron* 52, 941–952.

Friederici, Angela D. 2012. The cortical language circuit: From auditory perception to sentence comprehension. *Trends in Cognitive Sciences* 16(5), 262–268.

Friederici, Angela D., & Sarah M. E. Gierhan. 2013. The language network. *Current Opinion in Neurobiology* 23(2), 250–254.

Friederici, Angela D. 2016. *Neural Basis of Language Development*. Keynote address to the 41st Annual Boston University Conference on Language Development. Boston, MA. [Boston University, 06–10 Nov 2016.]

Friederici, Angela D. 2017. *Language in our Brain: The Origins of a Uniquely Human Capacity*. Cambridge, MA: MIT Press.

Friederici, Angela D. 2017 [= this issue]. Neurobiology of syntax as the core of human language. *Biolinguistics*.

Friederici, Angela D., Noam Chomsky, Robert C. Berwick, Andrea Moro, & Johan J. Bolhuis. 2017. Language, mind and brain. *Nature Human Behaviour* 1, 713–722.

Friedrich, Manuela & Angela D. Friederici. 2005. Semantic sentence processing, reflected in the event-related potentials of one- and two-year-old children. *Neuroreport* 16(6), 1801–1804.

Friedrich, Manuela & Angela D. Friederici. 2010. Maturing brain mechanisms and developing behavioral language skills. *Brain and Language* 114, 66–71.

Greene, Sarah J., & Ronald J. Killiany. 2010. Subregions of the inferior parietal lobule are affected in the progression to Alzheimer's disease. *Neurobiology of Aging* 31(8), 1304–1311.

Greicus, Michael D., Gaurav Srivastava, Allan L. Reiss, & Vinod Menon. 2004. Default-mode network activity distinguishes Alzheimer's disease from healthy

healthy aging: Evidence from functional MRI. *Proceedings of the National Academy of Sciences of the United States of America* 101(13), 4637–4642.

Grossman, Murray, Franz Payer, Kris Onishi, Tammy White-Devine, Donald Morrison, Mark D'Esposito, Keith Robinson, & Abass Alavi. 1997. Constraints on the cerebral basis for semantic processing from neuroimaging studies of Alzheimer's disease. *Journal of Neurology, Neurosurgery and Psychiatry* 63, 152–158.

Hänggi, Jürgen, Johannes Streffer, Lutz Jäncke & Christoph Hock. 2011. Volumes of lateral temporal and parietal structures distinguish between healthy aging, mild cognitive impairment, and Alzheimer's disease. *Journal of Alzheimer's Disease* 26, 719–734.

Harasty, J. A., G. M. Halliday, J. J. Kril & C. Code. 1999. Specific temporoparietal gyral atrophy reflects the pattern of language dissolution in Alzheimer's disease. *Brain* 122, 675–686.

Jacobs, Heidi I.L., Martin P. J. Van Boxtel, Harry B. M. Uylings, Ed H. B. M. Gronenschild, Frans R. Verhey, Jelle Jolles. 2011. Atrophy of the parietal lobe in preclinical dementia. *Brain and Cognition* 75, 154–163.

Kurth Florian, Christian Gaser, & Eileen Luders. 2015. A 12-step user guide for analyzing voxel-wise gray matter asymmetries in statistical parametric mapping (SPM). *Nature Protocols* 10, 293–304.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Lust, Barbara. 2006. *Child Language: Acquisition and Growth*. Cambridge, England: Cambridge University Press.

Lust, Barbara, Janet Cohen Sherman, Suzanne Flynn, J. Gair, C. Cordella, Charles R. Henderson, J. Behrendt, S. Gutz, S. Whitmore & N. Ramkumar. 2014. *Three-Word Bricolage: A New Task Distinguishes Mild Cognitive Impairment From Healthy Aging*. Paper presented at the International Neuropsychological Society, Seattle, WA. [February 2014.]

Lust, Barbara, Suzanne Flynn, Janet Cohen Sherman, J. Gair, Chalres R. Henderson, Leah Shabo, Marc Harrison & S. Mancuso. 2015a. *Disintegration of the Syntax–Semantics Interface May Reveal Prodromal Alzheimer's Disease*. Paper presented at the International Convention of Psychological Science, Amsterdam, The Netherlands. [Beurs van Berlage, 12–14 March 2015]

Lust, Barbara, Suzanne Flynn, Janet Cohen Sherman, J. Gair, J. Whitlock, C. Cordella, Charles R. Henderson, S. Mancuso, Z. Chen, A. Costigan & A. Immerman. 2015b. Reversing Ribot: Does regression hold in normal aging or prodromal Alzheimer's disease? *Brain and Language* 143, 1–10.

Lust, Barbara, Suzanne Flynn, Janet Cohen Sherman & C. Foley. 1996. What children know about what they say: Elicited imitation as a research method. In Dana McDaniel, Cecile McKee, & Helen Smith Cairns (eds.), *Methods for Assessing Children's Syntax*, 55–76. Cambridge, MA: MIT Press.

Lust, Barbara, Suzanne Flynn, Janet Cohen Sherman, J. Gair, & Charles R. Henderson. 2017. *Last in. First Out. Does Language Loss Reverse Language Acquisition?* Paper presented at American Association for the Advancement of Science, Boston, MA.

Marguliesa, Daniel S., Satrajit S. Ghosh, Alexandros Goulas, Marcel Falkiewicz, Julia M. Huntenburg, Georg Langs, Gleb Bezgin, Simon B. Eickhoff, F. Xavier

Castellanos, Michael Petrides, Elizabeth Jefferies, & Jonathan Smallwood. 2016. Situating the default-mode network along a principal gradient of macroscale cortical organization. *Proceedings of the National Academy of Sciences of the United States of America* 113(44), 12574–12579.

McMillan, Corey T., Robin Clark, Delani Gunawardena, Neville Ryant, Murray Grossman. 2012. fMRI evidence for strategic decision-making during resolution of pronoun reference. *Neuroschchologia* 50(5), 674–687.

Menon, Vinod & Lucina Q. Uddin. 2010. Saliency, switching, attention and control: A network model of insula function. *Brain Structure and Function* 214, 655–667.

Minoshima, Saroshi, Bruno Giordani, Stanley Berent, Kirk A. Frey, Norman L. Foster, & David E. Kuhl. 1997. Metabolic reduction in the posterior cingulate cortex in very early Alzheimer's disease. *Annals of Neurology* 42(1), 85–94.

Mioshi, Eneida, Kate Dawson, Joanna Mitchell, Robert Arnold & John R. Hodges. 2006. The Addenbrooke's cognitive Examination Revised (ACE-R): A brief cognitive test battery for dementia screening. *International Journal of Geriatric Psychiatry* 21, 1078–1085.

Monsell, Sarah E., Danping Liu, Sandra Weintraub, & Walter A. Kukull. 2012. Comparing measures of decline to dementia in amnestic MCI subjects in the National Alzheimer's Coordinating Center (NACC) Uniform Data Set. *International Psychogeriatrics* 24(10), 1553–1560.

Morris, John C. 1993. The clinical dementia rating (CDR): Current version and scoring rules. *Neurology* 43 (11), 2412–2414.

Morris, John C. & Jeffrey Cummings. 2005. Mild cognitive impairment (MCI) represents early stage Alzheimer's Disease. *Journal of Alzheimer's Disease* 7, 235.

Morris, John C., Sandra Weintraub, Helena C. Chui, Jeffrey Cummings, Chalres DeCarli, . . . , & Walter A. Kukull. 2006. The Uniform Data Set (UDS): Clinical and cognitive variables and descriptive data from Alzheimer Disease Centers. *Alzheimer Disease and Associated Disorders* 20(4), 210–216.

Mueller, Susanne G., Michael W. Weiner, Leon J. Thal, Ronald C. Petersen, Clifford R. Jack, William Jagust, John Q. Trojanowski, Arthur W. Toga, & Laurel Beckett. 2005. Ways toward an early diagnosis in Alzheimer's disease: The Alzheimer's Disease Neuroimaging Initiative (ADNI). *Alzheimer's and Dementia* 1(1), 55–66.

Pareja-Lora, Antonio, Maria Blume, & Barbara Lust (eds.). In press. *Development of Linguistic Linked Open Data Resources for Collaborative Data-Intensive Research in the Language Sciences*. Cambridge, MA: MIT Press.

Payne, Brennan R., & Elizabeth A. L. Stine-Morrow. 2016. Risk for mild cognitive impairment is associated with semantic integration deficits in sentence processing and memory. *Journals of Gerontology, Series B: Psychological Sciences and Social Sciences* 71(2), 243–253.

Petersen, Ronald C. 2003. *Mild Cognitive Impairment: Aging to Alzheimer's Disease*. Oxford, England: Oxford University Press.

Petersen, Ronald C. 2004. Mild cognitive impairment as a diagnostic entity. *Journal of Internal Medicine* 256(3), 183.

Ribot, Théodule-Armand. 1881/2012. *Diseases of Memory: An Essay in the Positive Psychology*. New York, NY: Appleton.

Ribot, Théodule-Armand. 1881. *Les Maladies de la Memoire* [Diseases of Memory]. Paris, France: Bailliere.

Roberts, Rosebud O., David S. Knopman, Michelle M. Mielke, Ruth H. Cha, V. Shane Pankratz, ..., & Ronald C. Petersen. 2014. Higher risk of progression to dementia in mild cognitive impairment cases who revert to normal. *Neurology* 82(4), 317–325.

Seeley, William W., Richard K. Crawford, Juan Zhou, Bruce L. Miller, Michael D. Greicius. 2009. Neurodegenerative diseases target large-scale human brain networks. *Neuron* 62, 42–52.

Sherman, Janet C. 2017. *Is There a Language Prodrome for Alzheimer's Disease? A Clinical Perspective*. Presented at American Association for the Advancement of Science Annual Meeting, Boston, Massachusetts.

Sherman, Janet C., Suzanne Flynn, Charles R. Henderson, S. Mancuso, J. Gair, Leah Shabo, K.-L. Capelli, J. Behrendt & Barbara Lust. 2015a. *Pronoun Problems in MCI: New Research Begins to Reveal the Source of Difficulty*. Paper presented at the 43rd Annual Meeting of The International Neuropsychological Society (INS), Denver, CO. [Hyatt Regency Denver, 4–7 February 2015.]

Sherman, Janet C., Suzanne Flynn, J. Whitlock, C. Cordella, Charles R. Henderson, J. Behrendt, K.-L. Capelli, A. Janmohamed, Leah Shabo, C. Sullivan, A. Immerman, A. Costigan, A., J. Gair & Barbara Lust. 2015b. *New Tests of Language Help Differentiate Mild Cognitive Impairment From Healthy Aging*. Paper presented at the Massachusetts Alzheimer's Disease Research Center, Boston University Alzheimer's Disease Center & the Harvard NeuroDiscovery Center's 28th Annual Scientific Poster Symposium, Boston, MA. [27 February 2015.]

Skeide, Michael A., Jens Brauer & Angela D. Friederici. 2014. Syntax gradually segregates from semantics in the developing brain. *NeuroImage* 100, 106–111.

Small, Gary W., Vladimir Kepe, Linda M. Ercoli, Prabha Siddarth, Susan Y. Bookheimer, ..., & Jorge R. Barrio. 2006. PET of brain amyloid and tau in mild cognitive impairment. *New England Journal of Medicine* 355(25), 2652–2663. doi:10.1056/NEJMoa054625.

Spreng, R. Nathan, Jorge Sepulcre, Gary R. Turner, W. Dale Stevens, & Daniel L. Schacter. 2013. Intrinsic architecture underlying the relations among the default, dorsal attention and frontoparietal control networks of the human brain. *Journal of Cognitive Neuroscience* 25(1), 74–86.

Spreng, R. Nathan & Gary R. Turner. 2013. Structural covariance of the default network in healthy and pathological aging. *Journal of Neuroscience* 33(38), 15226–15234.

Spreng, R. Nathan, Amber W. Lockrow, Elizabeth DuPre, Roni Setton, Karen A. P. Spreng, Gary R.Turner. In press. Semanticized autobiographical memory and the default – executive coupling hypothesis of aging. *Neuropsychologia*. doi:10.1016/j.neuropsychologia.2017.06.009.

Tabert, Matthias H., Jennifer J. Manly, Xinhua Liu, Gregory H. Pelton, Sara Rosenblum, ..., & D. P. Devanand. 2006. Neuropsychological prediction of conversion to Alzheimer disease in patients with mild cognitive impairment. *Archives of General Psychiatry* 63, 916–924.

Turner, Gary R. & R. Nathan Spreng. 2015. Prefrontal engagement and reduced default network suppression co-occur and are dynamically coupled in older adults: The default-executive coupling hypothesis of aging. *Journal of Cognitive Neuroscience* 27(12), 2462–2476.

Vissiennon, Kodjo, Angela D. Friederici, Jens Brauer, Chiao-Yi Wu. 2017. Functional organization of the language network in three- and six-year-old children. *Neuropsychologia* 98, 24–33.

Waters, Gloria S., David Caplan, & Elizabeth Rochon. 1995. Processing capacity and sentence comprehension in patients with Alzheimer's disease. *Cognitive Neuropsychology* 12(1), 1–30.

Weiner, Michael W., Paul S. Aisen, Clifford R. Jack, Jr., William J. Jagust, John Q. Trojanowski, . . . , & Mark Schmidt. 2010. The Alzheimer's Disease Neuroimaging Initiative: Progress report and future plans. *Alzheimer's & Dementia* 6(3), 202–211.e7.

Weintraub, Sandra, David Salmon, Nathaniel Mercaldo, Steven Ferris, Neill R. Graff-Radford, . . . , & John C. Morris. 2009. The Alzheimer's Disease Centers' Uniform Data Set: The neuropsychological test battery. *Alzheimer Disease and Associated Disorders* 23(2), 91–101.

Wicklund, Meredith, & Ronald C. Petersen. 2014. Mild cognitive impairment. In Bradford C. Dickerson & Alireza Atri (eds.), *Dementia: Comprehensive Principles and Practices*, 432–448. Oxford, England: Oxford University Press.

Barbara Lust
Cornell University
Department of Human Development
Ithaca, NY 14853
United States of America

bcl4@cornell.edu

Janet C. Sherman
Massachusetts General Hospital
Psychology Assessment Clinic
Boston, MA 02114
United States of America

jsherman@mgh.harvard.edu

James Gair†
Cornell University
Department of Human Development
Ithaca, NY 14853
United States of America

In memoriam

Suzanne Flynn
Massachusetts Institute of Technology
Department of Linguistics and Philosophy
Cambridge, MA 02139
United States of America

sflynn@mit.edu

Charles R. Henderson, Jr.
Cornell University
Department of Human Development
Ithaca, NY 14853
United States of America

crh2@cornell.edu

Marc Harrison
Cornell University
Department of Human Development
Ithaca, NY 14853
United States of America

mbh92@cornell.edu

Leah Shabo
Cornell University
Department of Human Development
Ithaca, NY 14853
United States of America

lms372@cornellprod.onmicrosoft.com

What Lenneberg Got Right: A Homological Program for the Study of Language Evolution

Sergio Balari & Guillermo Lorenzo

By 1967, it was clear to Eric Lenneberg that reconstructing the phylogenetic history of language should require the adoption of a non-functional (or Owenian) homology concept for grounding relevant comparisons. Fifty years later, most biolinguistic approaches have betrayed this project, for they routinely derive their conclusions regarding the unique/shared status of language on merely folk grounds—as dramatically illustrated in Hauser, Chomsky & Fitch vs. Pinker & Jackendoff's debate, or based on functional considerations—as in Chomsky's recent conceptualization of language as a unique tool for thought. Here we claim that Lenneberg's project needs to be resumed and we articulate some suggestions about how to conduct it, taking advantage of recent findings and new conceptual insights concerning two crucial levels of analysis actually pinpointed by him—namely, anatomical/molecular structure and physiological function.

Keywords: homology thinking; character concept; evolutionary novelties; computational mind

> *Homologies cannot be established by relying on similarity*
> *that rests on superficial inspection [. . .];*
> *on logical rather than biological aspects [. . .];*
> *and on anthropocentric imputation of motives.*
> —Eric Lenneberg (1969: 641)

1. Introduction

It takes an easy exercise of folk comparative biology to persuade a freshman of the uniqueness of language when compared to the closest nonhuman behaviors that could possibly come to mind—say, the songs of oscine birds, the alarm calls of vervet monkeys, etc. As far as one can say, none of these otherwise sophisticated capabilities appears to provide the means for establishing complex sound-meaning pairings—alternatively, gesture-meaning pairings, ranging across any imaginable experiential domain, and with an open-ended capacity for composing new complex expressions from a finite array of preexistent basic units. In other words, a relatively shallow awareness of some Hockett-style definitional properties of language

---

We are grateful to Alessandro Minelli and Günter Wagner for their clarifications and feedback, as well as to two *Biolinguistics* reviewers for their insightful and helpful comments. This paper has benefitted from a grant of the Generalitat de Catalunya (2014-SGR-1013) to the Centre de Lingüística Teòrica at the Universitat Autònoma de Barcelona (SB).

ISSN 1450–3417

(Hockett 1960), together with some familiarity with songs, calls, or other nonhuman ways of signaling, appear to suffice to strongly fix the persuasion that language is uniquely human. At its proper (folk) level of observational accuracy, the statement is innocuous—perhaps even true, in the sense that it is true that France is hexagonal at a certain level of observational accuracy, as famously stated by Austin (1962: 143). But at a deeper level, one already subject to the strictures of biology proper, the matter becomes empirical and one to which expedient answers should not be welcome. To put it plainly, whether language is or is not a uniquely human capacity is clearly a matter of scientific discovery, the fulfillment of which obviously asks for a collaborative effort of theoretical linguistics and comparative biology at different levels of organization—say, from molecules to behavior. Moreover, and crucially to the point to be made here, such an effort is pointless if not conducted under the overarching umbrella of 'homology thinking', in the sense recently put forward by the likes of Ereshefsky (2012) and G.P. Wagner (2015). It may strike outsiders as surprising that the upsurge of evolutionary linguistics in the last few decades has made its way alien to such a reasonable guideline.

But it should strike insiders likewise, for it was already clear to Eric Lenneberg, as soon as in 1967, that any serious statement concerning the phylogenetic status of language must rely on *bona fide* structural and functional comparisons—be it at the anatomical, physiological or molecular levels of analysis, and disregarding the kinds of design and teleological considerations on which folk statements are commonly based. It clearly is a historical mistake of today's biolinguistic approach to language evolution to have sidestepped Lennenberg's wise advice in paragraphs like the following:

> A study of design features may give us insight into some of the biases that enter into the process of natural selection, into the biological usefulness of certain features of animal communication but it is not relevant to the reconstruction of phylogenetic history. For the latter we are only interested in the relation of types of anatomical structure (including molecular structure) and physiological function (including motor coordination and sensory acuity), but we disregard the *usefulness* or *efficiency* of these features to the contemporary form.
>
> (Lenneberg 1967: 234, emphasis in original)

The issue of deciding whether language is a new, unique organ of the cognitive make-up of humans, or rather an old, more or less widely shared one also present in the cognitive constitution of other species, is clearly a concern for homology thinking, understood along the lines of Lenneberg's suggestion.

Let us clarify before proceeding that it is not the case that deciding whether what humans do with language is unique or at least special is a matter devoid of any biological interest. It is, of course, a biologically interesting question, but in the context of 'population thinking'—a complementary branch to homology thinking within evolutionary biology (G.P. Wagner 2015), interested in deciding which selective pressures, if any, could possibly have favored the stabilization of language in the primeval human populations. But note that regarding this question, whether language is or is not an organ—new or otherwise, is not particularly demanding. Let us also note that for other evolutionary concerns—namely, the ones related to so-called 'tree thinking' (G.P. Wagner 2015), considering language as 'new' (strictly speaking, an 'autapomorphy') may be critical if one credits it a role in fixing the position of humans within their closest nonhuman relatives. But in that case a loose notion of 'organ' may be enough. Actually, the role of autapomorphy is routinely credited to the absence of organs, or to traits that do not qualify as *bona fide* organs. In as much as 'something' (or 'nothing') offers a clue of the relative kinship between members of a group of species, it qualifies as an autapomorphy (Müller & G.P. Wagner 1991).

Let's stress this from the outset. Our main aim in this paper—in conformity with the goals of 'homological thinking'—has mostly to do with the individuation of organs, i.e. (1) how their identities can be established, so as to (2) verify how their shared or unique character can possibly be settled. In doing this, issues extremely important to the concerns of 'population' and 'tree thinking'—like adaptive function, adaptive change, kinship degree, etc.—become of a secondary interest. Their omission in this paper is thus not due to their lack of intrinsic importance, but to their low explanatory profile regarding the issues to be dealt with.

In this paper, we want to work out the idea that if one's goal has to do with establishing whether the natural history of 'language as an organ' starts with humans or it rather has an older pedigree, then practicing homological thinking is inescapable. And not only this, it is also inescapable doing it along the lines suggested by Eric Lenneberg, which are but the lines solidly grounded—but largely marginalized—by Richard Owen in the mid 19th century. One should not understand this as if questions of uniqueness/novelty only make sense as associated to organs—of course, they don't. But as a matter of fact, the idea of 'language as an organ' (Anderson & Lightfoot 2002) has been a persistent one all through the generativist tradition—but maybe not for the right reasons, and it is one that we believe will gain more credit and support in the near future. Both questions will be presently dealt with. Central to our claims is that it makes a lot of sense to consider the unique/shared condition of language at this particular level of analysis, departing from the most common position which holds that it only makes sense to break up language as a composite or mosaic of sorts (Hauser et al. 2002, Boeckx 2012) and considering the issue on a piecemeal basis.

The main take home message of this paper is thus that the rehabilitation of Lenneberg's phylogenetic thought is crucial for the evolutionary biolinguistic project, a claim that boils down to the idea that it is urgent to inject a good dose of (Owenian) homology thinking into it. The paper is organized as follows: Section 2 is a critical one, in which we document the lack of genuine homology thinking in some of the most reputed recent biolinguistic approaches to language evolution. Section 3, in turn, makes some positive suggestions about the application of homology thinking to the case of language at different levels of biological analysis. It also reflects on the landscape that such an application opens regarding the 'unique vs. shared' issue. A brief concluding remark closes the paper.

2. The Achilles' Heel of Evolutionary Biolinguistics—How Lenneberg Anticipated it

A new instrumental use of an organic structure may prove critical for the survival of a given species; yet natural evolution (not to be confounded with Natural Selection) is not about the evolution of instruments: Natural evolution is about the evolution of organic structures. An instrument is an 'intentional' object, so one to be defined attending to the purpose that it stands for—or 'intends'. But obviously enough, the intentionality of an instrument is derivative of that of a mind capable of executing such a designated purpose—or 'intention.' As a matter of fact, a long philosophical tradition, customarily traced back to Brentano's (1874) work, holds that intentionality is the hallmark of the 'mental', and that only minds are intrinsically intentional. Organic structures thus are not (cannot be) instruments, for assuming the stance that they are is tantamount to derivatively deem them intentional things, the primary source of which could not possibly be but Mother Nature. An instrumental conception of organisms and their organ constitution thus entails the underlying (anthropocentric) stance of ascribing a mind to nature (Fodor & Piattelli-Palmarini 2010, Richards 2002, 2005). Clearly, and despite the pervading instrumentalism that for centuries has been present in naturalistic thought, organs are not instruments; they are just systems, connected to and embedded within overarching systems, with characteristic activity regimes that pave the way to emergent, somehow unpredictable practical effects, given the intricacies of the internal constitution of the organism itself and of its trade-offs with the environment (Cummins 1975, Love 2007, Wouters 2003). Starting evolutionary thinking at this latter, practical interface of sorts implies locating evolutionary explanation too much ahead of where it should start.

So, while it is important to understand how the human species has taken advantage of its organic constitution along its natural history, the issue is nevertheless irrelevant when what is in dispute is how purportedly new aspects of the human natural constitution have possibly come into existence. In the case that concerns us here, all this boils down to the conclusion that deciding whether language is 'for' communicating, or 'for' thinking, or 'for' communicating or thinking about this or that, is a negligible question when debates revolve around its evolutionary origins, including its innovative or conservative character. To such an aim, the focus must rather be put on comparisons between the organic structure(s) for which the word 'language' is used as a shortcut, and other nonhuman organic structures that can reasonably be suspected of being related with it (or them) in terms of anatomy, molecular underpinnings, or physiological activity. This methodological principle was crystal clear to Eric Lenneberg in 1967, as the quote in section 1 demonstrates. Let us refer to it as 'Lenneberg's Phylogenetic First Principle'—henceforth, LPFP.

It should strike observers and practitioners of the biolinguistic program currently being developed under the habitually explicit advocation of Lenneberg's book, that some of the most influential approaches to the evolutionary origins of language have been conducted in the last years completely sidestepping LPFP. The case of Chomsky's delimitation and compartmentalization of language for evolutionary concerns in recent works provides a dramatic illustration of this claim—see, for example, Chomsky 2013 and 2016. To start with, Chomsky symptomatically accepts the 'communication vs. thought-related' character of language at face value in this regard, and he concludes with the following Solomonic stance: Language is to be compartmentalized into a communication-related component and a thought-related component. On this conclusion he subsequently bases his evolutionary claim that the former, communication-related side of language—an Externalization device—belongs to a very old ancestry, so clearly is not a new language-specific component; only the latter, thought-related side—a Language of Thought— is genuinely new and purportedly the most distinctive seal of human nature. Chomsky's evolutionary tenets are however flawed, precisely because they run, and strongly indeed, against LPFP. Let's examine this with some detail.

Note, first, that Chomsky is delineating the evolved linguistic phenotype—both in the 'broad' and 'narrow' senses of Hauser et al. (2002)—exclusively attending to functional considerations: Language is—broadly speaking—a tool for communicating thoughts, within which the component for generating thoughts and the one for externalizing them can be safely taken apart; but above all—or narrowly speaking—language is an 'instrument' for thought—the word 'instrument' is taken from his own fragments (e.g., Chomsky 2016: 16). However, Chomsky's move is useless as a way of identifying language as an evolved phenotype: Firstly, because pinpointing such allegedly natural functions of language, no organic structure whatsoever shows up that one can subsequently evolutionarily explain; and secondly, because the notions of 'communication' or 'thought' are being used in statements like these in merely a folk, intuitive way that hardly can serve the task of individuating a *bona fide* organic entity. Curiously enough, Chomsky is perfectly aware of the oddities of functional adscription exercises, starting with the oddity of thinking that language has a purpose (Chomsky 2016: 15, Berwick & Chomsky 2016: 63), which makes his confidence in loose functional criteria for delineating the language phenotype and making conjectures about ancestry and innovativeness even more perplexing. If this critique is on track—and we strongly believe that it is—such influential works like Berwick and Chomsky 2011, Berwick & Chomsky 2016, Bolhuis et al. 2014, or Hauser et al. 2014, even Hauser et al. 2002, to be fair, are clearly vitiated from the start, for they all rely on Chomsky's functionally inspired conjectures about the language phenotype.[1]

So, what is so vexing about functional ascription—as the likes of Berwick and Chomsky aptly acknowledge? In a nutshell, it is that organs such as, for example, bones, "do not have a single, unambiguous function", and that "what is true for bones is also true for human language" (Berwick & Chomsky 2016: 63). An organ's purpose may change from one to another situation, without even composing some-

---

[1] Chomsky's position regarding this whole issue is, to say the least, obscure. On the one hand, his functional characterization of Externalization appears to be easily reducible to biological standards, as a system adapted to the communicative needs of Thought, the evolution of which required selecting suitably available genes, like FOXP2, according to the 'printer' story put forward in Berwick & Chomsky (2011)—see below. But Thought—i.e. Language proper, on the other hand, while also functionally defined, does not appear to respond to a history of adaptive evolution along similar lines—Chomsky's preferred alternative, at least from his 1968 on, having been that it derives from a spontaneous auto-organizational process of sorts. But if so, according to most authorities (e.g., Millikan 1984), it is not clear how Thought/Language could possibly have acquired its functional credentials. This eventually leads us to our conviction that Chomsky's 'Thought' is not a biologically sanctionable category, but a metaphysical one.

thing like that particular organ's functional repertoire. And more crucial to this point, an organ's purpose(s) may change from one to another species, a common observation that does not compromise that particular organ's interspecies identity. As a matter of fact, the 'homology' concept historically grew out of these kinds of observations—for example, that the forelimb may serve as a wing or as flipper,[2] among many other things, in different species, which justified an underlying identity concept capable of sidestepping functional considerations. When projecting this simple piece of homology thinking onto language, one easily discovers that Chomsky's uniqueness statement is, to say the least, premature, for the emergence of a new—even qualitatively—way of thinking, does not automatically entails the emergence of a new associated organic structure. For the sake of accuracy, note also that from the fact that vocal or gestural signing is widespread in the organic world, one cannot automatically derive the conclusion that language does not incorporate brand new means for doing it.

Chomsky's disregard of LPFP is not a trivial issue, for in the end it leaves the biolinguistic approach orphan of a phenotype to be evolutionarily explained. Obviously enough, the task of explaining whether it is an innovative or conservative aspect of the constitution of humans becomes vacuous in the absence of such an object. However, Chomsky is not alone in this particular side of the question. In the last years, many efforts have been directed to judge the unique or shared character of language, namely by disentangling which aspects of language can reasonably be linked with other aspects of animal cognition—see Fitch 2005, for a synthesis. Leaving aside that such comparative efforts are usually aimed at establishing connections at an observable/behavioral level—thus dealing with 'design features' or 'use' considerations, very much against LPFP, they add to this shortcoming the extra one of not being assisted by any technically established concept of 'evolutionary novelty'. We hasten to clarify that there is not a unique, consensual 'evolutionary novelty' concept in biology—see A. Wagner (2011) and G.P. Wagner (2014), for two recent non-coincidental approaches to the issue. But what is substantial to our point is that there exist some respectable definitions of such concepts. However, none of them is consistently applied in the biolinguistic approaches to the 'unique vs. shared' issue. As an illustration, let us concentrate on the Hauser, Chomsky and Fitch vs. Pinker and Jackendoff debate, which is to a great extent responsible for the incorporation of the issue into the biolinguistic agenda (Hauser et al. 2002, Fitch et al. 2005, Pinker & Jackendoff 2005, Jackendoff & Pinker 2005).

To begin with, it is worth mentioning that the contributions that articulated the debate were mostly focused on the topic of the level of abstraction that linguistic theory must adopt so that relevant comparisons with non-linguistic behaviors

---

[2] We keep off this discussion an important insight due to John Searle, who observes that functions are basically in the eye of the beholder, since deciding what a given organic structure is exactly for is clearly biased by the observer's familiarity with instruments, cultural practices, and so on (Searle 1992: 237–240). Richard Owen almost expressed the same intuition in his 1849, when he observed that it adds nothing to the characterization of the mole's forelimb to say that it is for 'digging' or for 'swimming' in the soil. For similar considerations, see also Canguilhem (1952) and, more recently, Fodor and Piattelli-Palmarini (2010), where the issue is treated under the general rubric of the 'problem of intentionality'. Incidentally, Lenneberg also suggested something along similar lines: "The human observer at times is forced to make predictions about what would be useful to a certain way of life, but predictions may be purely the result of his anthropocentric outlook" (Lenneberg 1967: 25).

can aptly be made in order to shed some light upon the issue. In fact, the main source of dissent between the contenders was that each party defended the application of a very different resolution of the linguist's lenses in conducting the enterprise. As a consequence, Pinker and Jackendoff found every reason to set apart language from any other form of nonhuman cognition/behavior, for they defended to respect the results of linguistic theory at a very fine-grained level of detail as the relevant base of comparison. In contrast, Hauser, Chomsky and Fitch concluded that language is for the most part homogenous with non-animal cognition/behavior, as one can easily appreciate by relaxing the strictures of linguistic analyses and conducting comparisons on the basis of the minimal architectural components and design specifications of the human faculty of language. We don't need to enter here into the details of the discussion that ensued. Suffice to say here that the underlying strategy that both parties shared is dubious to begin with, for it relies in a linguistic/anthropocentric stance that necessarily biases and vitiates conclusions from the outset: Namely, the acritical adoption of human language—one of the terms of comparison—as the basis for comparison at the same time. Curiously enough, Owen (1849) was perfectly aware of this potential contamination of the comparative method when he suggested that the homologies that one may reasonably conclude between organs of different species—instances of 'special' homology—might be meticulously differentiated from homologies as set by reference to an abstract, third term of comparison—instances of 'general' homology. For many, this inescapably (and fatally) leads to the original sin of Platonism. But this is not relevant here, for the right take home lesson is a different one, and Lenneberg was well aware of it: Namely, that we need neutral grounds for framing the comparative endeavor, and that such grounds must be based on independently well-established biological criteria—to repeat his own words:

> [This endeavor must be based on] the relation of types of anatomical structure (including molecular structure) and physiological function (including motor coordination and sensory acuity). (Lenneberg 1967: 234)

Claims of homology or novelty are of necessity to be framed in agreement with LPFP. But this is not enough, for well-defined and sufficiently consensual 'homology' and 'novelty' concepts are also required to conduct the task on a meaningful basis. Which is another fatal flaw of the debate, as Table 1 below illustrates. As can easily be grasped, different and non-coincidental senses were used, not just by each of the contender parties—which obviously enough prevented the discussion from attaining any possible point of understanding, but also by each party along a single contribution—which prevented the debate from being fully intelligible.[3]

Against this background, we believe that radical changes are urgently needed in the biolinguistic enterprise in order to attain the desired convergence with evolutionary biology at large.

---

[3] It is also a symptom of the looseness with which the discussion was conducted in the course of the debate, given the fact that no single entry in the reference list of the papers referred to any biological treatment of the subject. By 2005, however, such landmark papers like Mayr (1960), Müller & G.P. Wagner (1991, 2003) and a whole volume like Nitecki (1990), might have served this purpose

Hauser, Chomsky & Fitch		Pinker & Jackendoff	
Shared	*Unique*	*Shared*	*Unique*
Homolog; Analog; Huge overlap in the mechanisms; Largely in place before; Minor modifications; Given; Unchanged; Quantitatively different; Overlaps with others; Streamlined; Based upon a foundation shared; Built upon ancient foundations; Hypertrophied; Otherwise specialized	Specifically adapted; Uniquely adapted; Lack of analog; Innovative; Special; Specifically evolved; Qualitatively new; Difference of kind	Modified; Similarities found; Homologous; Analogous; Augmented; Altered; Retained; Extended; Minor extension; Discerned in others; Partly overlapping; With other general purposes; All properties in common; Reminiscent of another trait	With nothing remotely similar; (In part) newly evolved; Differences found; Special; Sui generis, specific; Absent in others; (Only) some properties in common; Different in significant aspects

Table 1: The multifarious folk semantics of the 'unique vs. shared' distinction in the Hauser, Chomsky & Fitch vs. Pinker & Jackendoff's debate.

3. A new Heel for Achilles—With Lenneberg on our Side

LPFP precludes claims of homology from being based on either design features or functional considerations. In the previous section, we have sought to very generally justify why functional considerations having to do with 'usefulness' or 'efficiency' are pointless in this regard, but less space was devoted to design or formal criteria. Curiously enough, Lenneberg's claim on the issue almost mimics Owen's historical statement about what the homology concept boils down to, for he states that claims about homology were to be settled abstracting away from both formal and functional details:

> Homology.—The same organ in different animals under every variety of form and function. (Owen 1843: 379)

This definition is particularly suggestive, especially when considering that in Owen's times the most reputed method for claiming homologies was a formal one, namely the conservation of parts and patterns of correlation among them in different animals. Owen himself had to routinely recur to this method in his daily practice as a comparative anatomist, but he had the strong intuition that homologies should ultimately be based on the generative or developmental resources putatively shared by the corresponding organs (Balari & Lorenzo 2012, 2015a). It is probably not too much of a stretch to say that Owen found formal considerations rife with the same kinds of difficulties that one finds when managing with functional ones: Namely, that forms are continuous, so determining when formal varieties are tokens of the same or different formal types, again, is only in the eye of the beholder. Pinpointing generative/developmental criteria as a neutral ground for deciding anatomical sameness was certainly within the purview of 19th century anatomists, even if the adequate operationalization means were still lacking.

The previous comments amount to the conclusion that anatomical homologies ultimately are to be resolved on developmental grounds, which nowadays mostly—but not exclusively—means on molecular grounds (G.P. Wagner 1989). Subsection 3.1. below is devoted to introducing the prospects of such a project in the case of language and points to some tentative conclusions regarding the 'unique vs. shared' issue that, we think, can plausibly be drawn at this point in time. After this, subsection 3.2 reflects on how one should conceptualize the functional/physiological level of analysis, also pinpointed by Lenneberg, in order to operationalize it for the same goal.

3.1. Anatomical/Molecular Structure

Individuating organic systems is not an easy task. Yet, it is a crucial one for deciding the case of the putative novelty of a particular such system. Actually, having a robust rationale for the individuation of organs is part and parcel of the attribution of a unique or shared identity of a particular organ at a given evolutionary time. This is, for example, the route recently taken by Günter P. Wagner, whose homological theory primarily relies on the distinction between the 'character identity' and the 'character state' concepts (G.P. Wagner 2014: 51–54): A character identity refers to an underlying 'sameness' to different organ tokens, while character states refer to the more or less varying ways in which these organ tokens actually surface. As stated in the previous formulation, a given character identity is not something that one may expect to be open to direct inspection: It is an 'underlying' property, not a 'superficial' one, contrary to formal or functional traits—thus in agreement with the classical, Owenian homology concept discussed above. According to G.P. Wagner (2014: Ch. 3), what makes different organ tokens of the same organ type is their sharing an underlying Character Identity Network (ChIN), which he defines as a reiterative pattern of interactions between genetic sequences—and products thereof—that interfaces with, on the one side, positional information provided by inductive signals, and, on the other side, 'realizer' genetic machinery, the activity of which brings about alternative character states in different developmental scenarios. The ChIN concept thus provides an explanation for the developmental individuality of a body part, that is its ability to express different sets of realizer genes than other body parts. Diverse illustrations are provided by G.P. Wagner (2014), which underpin traditional homological attributions—for example, fin/limb identity (Owen 1849, G.P. Wagner 2014: Ch. 10), but put into question some others—like the one that putatively holds of different vision organs (Gehring & Kazuko 1999, G.P. Wagner 2014: 111). In any event, it is the big picture, not particular applications, which is relevant for our present concerns.

It is good news that G.P. Wagner's model locates us on a developmental terrain, for knowledge about the developmental and, ultimately, genetic basis of language, while still limited, has dramatically increased in the last years, thanks to the window provided by congenital language impairments—which already were one of Lenneberg's focuses of attention (Lenneberg 1967: Ch. 6). We know now of the central role of the *FOXP2* hub gene in the sustained regulation of genetic activity that leads to the configuration of a complex neural circuitry that comprises parts of the basal ganglia, the cerebellum, the frontal cortex, and re-entering tracts of fibers connecting them (Lai et al. 2001, Enard et al. 2002, Lai et al. 2003, Liégeois et al. 2003, Enard et al. 2009, Reimers-Kipping et al. 2011; see Newbury et al. 2010, Enard

2011, Preuss 2012, and Graham & Fisher 2013, for some state-of-the-art reviews). Besides, we also know how *FOXP2* promotes neural differentiation at these particular sites by interacting with the retinoic acid signaling pathway, which regulates neuronal migration (reducing it) and neurite outgrowth (increasing it), two key processes for neural circuit formation (Vernes et al. 2011, Devanna et al. 2014, van Rhijn & Vernes 2015). Progress has also been made in the identification of genes targeted by *FOXP2* during the corresponding developmental loop, which reasonably enough contribute either to keep it active or to realize the form/function specificities of the resulting human characteristic circuitry (Johnson et al. 2009, Konopka et al. 2009, Vernes & Fisher 2009, Newbury & Monaco 2010, Roll et al. 2010, Vernes et al. 2011, Konopka et al. 2012, Ayub et al. 2013, Chiu et al. 2014, Rodenas-Cuadrado et al. 2014, Webb 2015). Finally, important insights have been gained with regard to enhancers and protein regulators—for example, SUMO proteins—of *FOXP2*'s own activity (Bonkowsky et al. 2008, Becker et al. 2015, Estruch et al. 2006, Becker 2016, Torres-Ruiz et al. 2016, Usui et al. 2016).

Needless to say, what we at present have at our disposal is certainly only a small fragment of a very intricate net of molecular products and interactions, to be hopefully more extensively unveiled in the years to come—for a visualization of its present state, see Konopka et al. (2012: 619; fig.6). In any event, what we already have starts to look very much like a Wagnerian ChIN, as figure 1 tries to illustrate. The skeleton of the figure is taken from G.P. Wagner (2014: 97; figure 3.8), we simply super-add a few representative data from the sources referred to in the previous paragraph.

Let us stress that the hypothesis above amounts to the tenet that *FOXP2* is part of a regulatory network that developmentally individuates parts of the brain, which, in humans, correlates with/embodies the language capacity. Assumedly, such a hypothetical network still waits for stronger experimental support than hitherto available; yet we contend that known putative fragments may already serve the task of guiding a homological enterprise. Regarding this enterprise, it is also crucial to keep in mind that such a task is not merely one of detecting gene names here and there, but of witnessing relevant interactive patterns between reasonable orthologs and related materials.

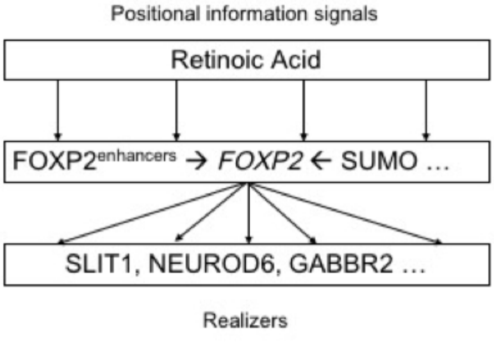


Figure 1: A first take on the language ChIN. The hub FOXP2 gene is represented in the image as interfacing key positional clues and a complex machinery of self-sustaining or realizer molecular stuff.

The previous comment also points to the reason why we believe that the ChIN concept provides a more powerful tool for conducting such a homological program than the closely related, yet different 'deep homology' one, which refers to extremely conservative genetic sequences that demonstrably have a key role in the making of organisms (Shubin et al. 1997, 2009; see Fitch 2011 and Scharff & Petri 2011, for its application to the case of language). Clearly enough, deep homologies reveal non-trivial evolutionary relations; yet, they clearly do not manage by themselves to support claims of 'sameness', like the ones that the ChIN concept is aimed at capturing. Deep homology concerns the natural proclivity to redeploy resources, but this is so even in cases where, considering the whole context, resources have been recruited for the making of (more or less radically) different organs. Besides, ChINs only make sense when what is at stake is the individuation of organs, while deep homologies liberally apply at lower levels of analysis—for example, protrusions and other intermediate structures, particular aspects of an organ's composition, etc. (G. P. Wagner 2014: 36).

Partial as the available information surely is about the putative ChIN represented in figure 1, we however insist on the claim that it is rich enough to start carrying out a *bona fide* homological project as regards human language (Balari & Lorenzo 2015b). Such a claim is partially based on our conviction that, for the first time, an amenable theoretical model exists to confidently guiding the task, but also on the fact that abundant comparative data already exist, from which some preliminary conclusion can already be achieved (Webb & Zhang 2005, White et al. 2006, Fisher & Scharff 2009, Konopka & Geschwind 2010, Scharff & Petri 2011, Fontenot & Konopka 2014). To cite but a few, orthologs of *FOXP2* have been exhaustively studied in organisms such as as the fly—*(d)FoxP* (DasGupta et al. 2014, Lawton et al. 2014, Mendoza et al. 2014), the bee—*AmFoxP* (Kiya et al 2008), zebrafish and medaka—*foxP2* (Bonkowsky & Chien 2005, Shah et al. 2006, Itakura 2008), the bat—*FoxP2* (Li et al. 2007, Chen et al. 2013, Vernes 2017), and the mouse—*Foxp2* (Ferland et al. 2003, Lai et al. 2003, Shu et al. 2005, Enard et al. 2009, Enard 2014, Fujita & Momoi 2014, Schreiweis et al. 2014, Medvedeva 2015, Castellucci et al.

organism	genetic proxy	loci of expression
Drosophila melanogaster	*(d)FoxP*	Protocerebral bridge (PB) (≈ Striatum) / Central complex (CX) (≈ Basal ganglia) / Optic,glomeruli
Apis mellifera	*AmFoxP*	Optic lobes / Central Complex / Dorsal lobes / Protocerebral lobes (connected by PB)
zebrafish	*foxP2*	Telencephalon / Diencephalon / Cerebellum / Hindbrain / Tectum / Retinal ganglion cells / Spinal cord
echolocating bats	*FoxP2*	Suprageniculate nucleus (SG) /Anterior cingulated cortex (ACC) (≈ BA 32, 33, 24)
mouse	*Foxp2*	Cerebral cortex / Thalamus / Cerebellum / Spinal cord

Table 2: Some preliminary bases for a putative cross-species ChIN comprising language.

2016, Chabout et al. 2016). Plotting observations made in these papers, results in an anatomical continuum that comprises neighboring or equivalent structures to the ones routinely pinpointed as correlates of *FOXP2* expression in the case of humans, as reflected in table 2.

In any event, the most dramatic results of this comparative enterprise have been provided to date for the case of songbirds. More concretely, the role of the FOPX2 transcription factor in the development and activity of the brain of these avian species has been firmly established (Haesler et al. 2004, Teramitsu et al. 2004), and a structure considered to be equivalent to (parts of) the basal ganglia (Area X) has been pinpointed as the one with which FOXP2 more strongly correlates (Haesler et al. 2007, Phillmore et al. 2014). Moreover, the role of retinoic acid as a key inductive signal in the development of this and some closely related structures (e.g., HVC, RA, IMAN) was known even before (Denisenko-Nehrbass et al. 2000, Denisenko-Nerhbass & Melo 2001, Roeske 2010, Roeske et al. 2014). Finally, an impressive amount of information regarding active (putatively realizer) genes of the referred structures has been recently provided, which strongly supports the homological relation with the human brain's candidate correlates (Pfenning et al. 2014): Namely, 78 genes are identified as active both in the songbird Area X and the human putamen, 40 in the birdsong RA and the surroundings of the human central sulcus, and an unspecified number (but in the order of the tens) in the birdsong RA and the human laryngeal motor cortex (LMC). Figure 2 offers a visualization of these results.

The resistance to admit all this background as informative of cases of *bona fide* homology strikes us as surprising. In this regard, Berwick's and Chomsky's position is paradigmatic: On the one hand, they agree that such data point to a case of deeply shared evolutionary history; on the other hand, they believe that it only touches the (peripheral) Externalization component of human language. Language 'proper'— that is the generative engine in charge of composing unboundedly internal expressions (aka bare thoughts)— has nothing to do with the kinds of molecular and anatomical findings that we have been reviewing (Berwick & Chomsky 2011, Berwick & Chomsky 2016, Chomsky 2016). Their stance is how-

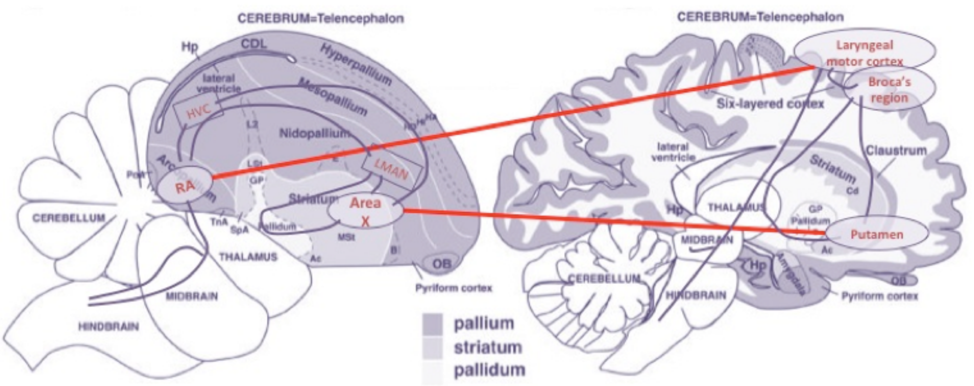


Figure 2: Some homological relations between the birdsong and the human brain, well established on developmental grounds. Illustration reproduced from Balari & Lorenzo (2015b: 12; fig. 4).

ever deceiving, for some animal models make us suspect that the cognitive import of the putative ChIN that we endorse is, as a matter of fact, orthogonal to the externalization/thought distinction. For example, Schreiweis et al. (2014) conclude that, in mice, it correlates with the declarative vs. procedural styles of learning and processing, and Chandrasekaran et al. (2015) corroborate that this appears to be also the case in humans.

But let us put aside these kinds of functional considerations for the time being. Berwick's and Chomsky's position is still, in all likelihood, conceptually unsound: Their discriminating between an externalization-related component and an internal thought-related component within the faculty of language, is merely made by fiat—that is, independently of any biological criterion. Ultimately, it conspicuously runs against Lenneberg's LPFP. But note that even if the biolinguistic claim were admissible that language is an instrument for thought— as Chomsky believes it to be—it still is without any biological motivation that an instrument for thought cannot be the same instrument that different animals use for, say, communication. In any event, such are bold claims, made without caring about the level(s) of organic activity on which claims of homology— or of lack thereof— can legitimately be made. We presently turn to this issue.

3.2. *Physiological Function*

Biological Foundations of Language can be safely described as the best compendium of neurophysiological knowledge concerning language, for 1967. One must not forget that, at the time of writing, the data coming from neuroscience Lenneberg had at his disposal had not yet gone much beyond the pioneering work of Edgar Adrian in the late 1920s.[4] No wonder then that our vindication of Lenneberg in this context will have little to do with empirical findings. As it was the case in the previous subsection we will focus on questions of method and insight.

If we were to highlight what in our opinion are the most relevant aspects of Lenneberg's stance with respect to the role of neurophysiology for the case of language, we think we would underline the following:

(i) Lenneberg's conviction that gross differences in cognitive capacities would translate to relatively small differences in structure, but would correlate instead with differences in the way a number of fairly well-preserved (i.e. homologous) structures interact.

(ii) Lenneberg's conception of biolinguistic explanation.

As for the first, we will have little to say, since this is today considered common knowledge in the field of neuroscience: "The main difference between brains of simple and complex animals is merely the number of neuronal loops that link

---

[4] A few examples will suffice. The Hodgkin and Huxley model of the action potential was first presented in 1952, but it remained a model for at least 20 years; the first experiments by Hubel and Wiesel on the visual cortex of the cat were first presented in 1962, but these just define a research project that, again, spans for more that 20 years; finally, the Society for Neuroscience was only founded in 1969 (https://www.sfn.org/About/Mission-and-Strategic-Plan; accessed 16/07/2017). These and many other examples can be found in any contemporary neuroscience book; see Churchland (1986), Churchland & Sejnowski (1992), Rieke et al. (1997), Craver (2007). Also see Arbib (this issue) who makes the same point.

the outputs to the inputs" (Buzsáki 2006: 32; see also Schneider 2014). To be sure, another point on which everyone appears to agree is that we want to understand brain 'computation' and that this is impossible without knowing something about the basic connectivity of the brain (Eliasmith & Anderson 2003: Ch. 1, Buzsáki 2006: Ch. 2, Seung 2012 for an introduction to 'connectomics'). Lenneberg did not express it with these words, but it is obvious that this is what he meant when he wrote that "[a]ll aspects of behavior may be considered to be based upon modulation of activity in neuronal nets" (Lenneberg 1967: 215). The crucial point however is the role Lenneberg attached to neurobiological descriptions in accounts of human linguistic capacities, since it is pretty clear that for him a complete characterization of the underlying mechanisms constituted a full-fledged explanation of the phenomena in question. This idea permeates the whole book, but perhaps the most illustrative quotation is this:

> It would be presumptuous to try to explain the nature of the innate events that control the operations of language. We may, however, assume that mechanisms are involved, such as (1) the modulation of firing characteristics of nerve cells; (2) the triggering of temporal patterns in neuronal chains; (3) the modulation of oscillatory characteristics of endogenous activities; and (4) the production of spreading of disturbances. These are some of the components of the automaton. How these phenomena interact to elaborate language remains a mystery.
>
> (Lenneberg 1967: 221)

In the purest Cartesian tradition (note his reference to the automaton), Lenneberg's conception of explanation is mechanistic through and through. But why should we bother to emphasize this point?—Some may ask: Is it not the case that explanation in the biolinguistic tradition has always been mechanistic? Well, not really (or not quite), we would contend, which obviously deserves some elaboration and to which we will devote the remainder of this section.

The issue is a delicate one. And it is because, although it is generally assumed that cognitive science/psychology and (bio)linguistics have been sailing in the same boat for the last sixty years, it is certainly not the case that both have adopted the same explanatory standards.[5] Take the case of cognitive science first. In this field, the debate has mostly centered around Jerry Fodor's notion of a 'special science' and the specific explanatory requirements that, according to him, these sciences impose (see Fodor 1965, 1968, 1974, 1975: 1–26, 1997). In essence, Fodor's model of explanation is a two-step account, where the first step (the phase one explanations of Fodor 1965) is to be set in terms of functionally characterized notions like 'beliefs,' 'desires,' and so on, complemented by research "directed towards determining the nature of the mechanisms whose functional characteristics phase one theories specify" (Fodor 1965: 176). We have dug into the roots of the issue, because Fodor's early writings already delineate a view in which functional and mechanis-

---

[5] Our use of the labels 'cognitive science,' 'psychology,' and 'biolinguistics' is merely instrumental and certainly not intended to capture the complex geography of the field. The intended idea, as it will become clear presently, is that Chomsky's position needs to be set apart from mainstream approaches to cognition, hence, to simplify further, in the text we will as of now just confront 'cognitive science' with 'biolinguistics'.

tic explanations are presented as different, albeit complementary, things and, consequently, autonomous from each other. The autonomy thesis erected by Fodor in 1965 and later propped up in Fodor (1974, 1997) is one of the dominant perspectives in contemporary cognitive science generically known as 'functionalism'.[6] In a nutshell, functionalism is the idea that cognitive systems are to be analyzed at a functional level that is not reducible to the neurophysiological level, mostly because macrolevel functional descriptions may be multiply realized at the microlevel, rendering any attempt to reduce the former to the latter impossible or, at least, uninformative and, therefore, not really explanatory; see, in addition to the already cited works, Putnam (1975) for a construction of the argument parallel to Fodor's, and Polger & Shapiro (2016) for a detailed exposition of the issues involved. The net effect of this perspective is the well-known attitude observed within cognitive science that research can proceed without paying too much attention to what is going on in the neurosciences. But the historical reasons of such a divorce run deeper and stem from the more or less tacit acceptance by most cognitive scientists of the deductive nomological model of explanation articulated by Hempel & Oppenheim (1948) coupled with the unity of science view of Oppenheim & Putnam (1958). According to this model, genuine scientific explanation amounts to reduction of (the laws of) some higher-level science to (the laws of) a lower-level science, where the most fundamental science is physics, such that, eventually, all science would be reduced to physics. This is a somewhat caricaturized exposition of the model, which doesn't affect the fact that it has been taken very seriously by many philosophers and cognitive scientists, in particularly those within the eliminativist camp (Churchland 1989, especially chapters 1 and 5, Bickle 1998, 2003). Thus, functionalism may be understood as a movement within cognitive science which, feeling uncomfortable with the prospects of elimination wielded by reductionist models of explanation, opts for a middle-ground position accepting a weaker (non-eliminativist) version of reduction that supposedly would keep it away from the dangers of (metaphysical) dualism (see Kim 2005, for a paradigmatic exercise along these lines).

It is interesting to notice to what an extent has Chomsky remained alien to these debates. Indeed, to great disappointment for many functionalists (e.g., Lycan 2003), he has mostly kept a (censoring) silence towards functionalist positions only broken to effectively dismantling it (Chomsky 2003, also 1997: 29–31). And for good reason: Chomsky's thought may be subject to many different criticisms but it certainly cannot be charged with incoherence, and one of the most salient (and often misunderstood) of Chomsky's philosophical positions is the one summarized by the following quotation: "Lacking a concept of 'matter' or 'body' or 'the physical,' we have no coherent way to formulate issues related to the 'mind-body problem'" (Chomsky 1995a: 4–5).[7] As a consequence, Chomsky's 'naturalism' concerning the mind is at most homophonous with the 'naturalism' typically observed within

---

[6] "Functionalism is a mess", Polger (2004: 71) observes, but as far as the argument in the text is concerned, little hinges on the nuanced variety of different functionalisms currently en vogue. See Polger (2004) for a taxonomy.

[7] The most explicit elaboration of this position by Chomsky goes back to the late 1980s through the 1990s, roughly in parallel to the first steps of the Minimalist Program (Chomsky 1995b)—presumably not a coincidence, but it certainly could be identified in most of his more philosophical works prior to that; see Chomsky (2000) for a collection of texts where the idea is articulated, and Poland (2003) for an illuminating analysis.

Anglo-American philosophy of mind (Dennett 2017, for the most recent example), as it is not an exercise of ‘naturalization’ but simply the assumption that minds are natural objects and therefore constrained by the very same principles applying to other entities of the world, whatever they are.[8] Only in this context can Chomsky’s iterated affirmation that eliminativism as a doctrine is not really intelligible be understood (e.g., Chomsky 2000: 25, among many other sources). It is not intelligible because, if we interpret Chomsky’s naturalism correctly (and we think we do), the kind of reductive eliminativism[9] so feared by the likes of Fodor and Putnam only makes sense when relying on the Oppenheim-Putnam Unity of Science framework for explanation, which is not Chomsky’s. His framework is Cartesian (i.e., mechanistic) from root to branch, with the only proviso referred to above concerning the status of the ‘physical’—just like Lenneberg’s, by the way.

The biolinguistic tradition thus appears to converge with a new wave of mechanistic philosophy of science, whose foundational text is Machamer et al. (2000) and which has given rise to a number of relevant monographs (Craver 2007, Bechtel 2008, Craver & Darden 2013, Piccinini 2015).[10] The idea behind mechanistic explanation is a priori fairly simple, but actuality suggests that it is not as easy to apply consistently as it seems. Take, for example, a standard definition of mechanism like the following:

> Mechanisms are how things work, and in learning how things work we learn ways to do work with them. Biologists try to discover mechanisms because mechanisms are important for prediction, explanation, and control.
>
> Biologists seek mechanisms that produce, underlie, or maintain a phenomenon.[11]
>
> (Craver & Darden 2013: 15)

As the definition suggests, the basic methodology of mechanistic explanation consists in identifying a phenomenon we want to explain—say, Long-Term

---

[8] See, in particular, Chomsky (2000: Ch. 4). In a slightly different context we adopted the same strategy in our attack to functionalism in Balari & Lorenzo (2015a). As a matter of historical detail, however, we are not entirely sure that the Newtonian revolution had the effect of turning “Newton’s anti-materialism” into “scientific common sense” (Chomsky 1995a: 5), simply because ‘immaterial’ at the time had a variety of meanings, ranging from ‘not directly perceptible’ to ‘poorly understood’, all of them falling within what was considered to be ‘natural’ (vs. the ‘supernatural’); see Balari & Lorenzo (2013b) where some illustrative examples taken from the work of Richard Owen and Michael Faraday, for example, are presented.

[9] The epithet ‘reductive’ is pertinent here, because not all varieties of eliminativism are reductive. Thus, for example, Churchland’s (1989) or Stich’s (1983) eliminativism towards folk-psychological categories is not motivated by the idea that these will eventually be reduced to neurophysiological categories but by the conviction that folk psychology is nothing but a “culturally entrenched theory” (Churchland 1989: 17) that will vanish as science progresses; similarly (and for similar reasons), we have exhibited our eliminative tendencies towards teleological functions in Balari & Lorenzo (2010).

[10] Perhaps not surprisingly, no tradition acknowledges the other but both locate their historical roots in references to Cartesianism (cf. Chomsky 1966, Bechtel 2008: Ch. 1, Craver & Darden 2013: Ch. 1).

[11] “We want to discover how actual systems work” (Chomsky 1997: 31). “While teleology seeks to answer a why-is-it-there question by answering a prior what-is-it-for question, functional analysis does not address a why-is-it-there question at all, but a how-does-it-work question” (Cummins 2002: 158).

Potentiation (LTP), to take a classical example—and describing the mechanism that produces, underlies or maintains it—in the case of LTP, the release of glutamate from a pre-synaptic neuron that results in changes in a post-synaptic neuron; see Craver (2007: 65–72), for details. Essentially, then, the strategy involves identifying a series of "entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions" (Craver & Darden 2013: 15). It would therefore seem that there's nothing new under the sun (and, in some sense, there isn't) because this looks very much like the kind of 'functional analysis' that, for example, Fodor (1965) and Cummins (1975, 1983) claim is an integral part of a complete psychological explanation. The (crucial) difference has to do with how the relevant levels of analysis are conceptualized in one and the other approach. Remember that Fodor justified a two-level approach in order to argue for the autonomy of functional analysis from mechanistic explanation and that we traced this back to Fodor's more or less tacit acceptance of a conception of levels based on the unity of science view. What Fodor failed to see[12] is that rejecting autonomy is not an argument for reductionism, it has never been, because the conception of 'level' that is predominant in the mechanistic tradition has nothing to do with the Oppenheim-Putnam model and, de facto, renders the classical idea of reduction nonsensical (as Chomsky would probably put it). Levels of mechanisms are not levels of science, because they involve a kind of part-whole relation in the sense that they are levels of behaving components that, in turn, may be identified as mechanisms, and therefore the part-whole relation makes only sense in the context of the mechanistic decomposition, often failing to correspond to the spatial boundaries one identifies in a purely mereological analysis; see Craver (2007: Ch. 5) and Craver (2015) for discussion and examples.

And thus we come to one of the cruces of the matter, since it is our contention that most self-declared biolinguistic approaches—but according to our exegesis, closer to the cognitivist orthodoxy; see fn. 4—have systematically misapplied the notion of 'level' in their attempts at solving the unification problem, in Chomsky's (2000: 103) sense.[13] Take the case of David Poeppel's proposals as to how to carry out the program originally delineated by himself and David Embick (Poeppel & Embick 2005, Poeppel 2012). In his paper, Poeppel argues, convincingly, that most current research in cognitive neuroscience is wrongheaded because it insists on drawing maps of the brain, associating functions to specific areas, when "localization and spatial mapping are not explanation" (Poeppel 2012: 35). As an alter-

---

[12] The case of Cummins is a bit more elaborate and a close examination would take us too far afield; see Piccinini & Craver (2011) for discussion.

[13] Chomsky has not been very specific about what he means by 'unification', but in Chomsky (1993: 46) he associates the concept with the question "How can organized matter have [the properties of mind identified by the Cartesians]?", so we believe it is safe to identify 'unification' with what Craver (2007: Ch. 7) terms 'interlevel integration' (roughly, identifying an item as a component of a higher-level mechanism and, in turn, identifying the lower-level mechanisms that constitute that component qua mechanism). Also, although Chomsky sometimes uses the word 'reduction' when considering one of the possible consequences of unification, he cannot possibly mean 'reduction' in the classical sense and we take it that his use is roughly synonymous to what Craver calls 'intralevel integration' (roughly, integration of different disciplines into an encompassing discipline); in fact, one of the examples Chomsky usually refers to (the incorporation of biology within known biochemistry) is better interpreted in this sense (Craver & Darden 2013).

native, Poeppel makes a case for what he calls addressing the 'mapping problem', which he defines as the

> [. . .] investigation of the (ultimately necessary) *formal relations* between two sets of hypothesized inventories, the inventory constructed by the language sciences and that constructed by the neurosciences.
>
> (Poeppel 2012: 35; emphasis in original)

To the extent that the meaning of the phrase 'formal relations' remains suitably vague, Poeppel's approach may count as mechanistic (as opposed to reductionist), but it is not obvious that he succeeds in really dismissing the classical view (if this is really his goal).[14] The main problem lies in Poeppel's invocation of David Marr's levels—which has become standard in cognitive science—as the guiding model to achieve explanatory theories. But appealing to Marr's levels can only generate a paradox, because Marr never believed in the explanatory power of neuroscience,[15] while Poeppel is clearly assuming otherwise. To be sure, Marr's model of explanation could be better characterized as intentional, even teleological, given the preponderance of 'why' components as opposed to the 'how' components typical of mechanistic explanations (Shagrir 2010), and therefore, without actually denying that the kind of computational analysis that Marr proposes may play some explanatory role, it cannot be the central element of any true computational mechanistic approach (Piccinini 2007, Shagrir 2010, Piccinini & Craver 2011, Piccinini 2015). More to the point, Marr's 'computational' and 'algorithmic' levels are not levels in the up-down interpretation that (almost) everybody gives to them (e.g., Poeppel 2012: 52), but rather slightly different perspectives from which a particular mechanism can be looked at and, hence, not actually autonomous from the 'implementation' level nor from each other, as Marr suggested (see Piccinini & Craver 2011: 302–303 and Piccinini 2015: 97–98, for discussion and further justification). Again, this is not to deny that a computational analysis may be relevant, but as we will suggest presently it will only share some elements with that of David Marr, namely those that clearly involve a breakup of the computational mechanism into parts, together with an assignment of functions and organization to those parts that is capable of showing that the capacities of the system are an effect of how the parts perform their activities (Piccinini 2007, 2015).

The issue of Marr's levels has percolated to other approaches trying to fulfill Poeppel's program. For example, Boeckx and Theofanopoulu (2014: 405 and figure 1) propose a stratified approach that supposedly bottoms out at the 'genome' and tops off at the 'phenome' or phenotypic level, spanning a number of intermediate levels like the 'connectome' (the set of neural connections), the 'dynome' (the linking of brain connectivity with brain dynamics), and the 'cognome' (roughly Marr's

---

[14] For example, the kinds of relations typical reductionist approaches describe between theories at different levels are inferential (i.e., formal; Craver 2005). Also, while Poeppel often appears to be arguing against reductionism (e.g., Poeppel 2012: 36, 51, 52), his putative attacks are lukewarm at best, as he mostly seems in fact to be refurbishing Fodor's autonomy thesis by vindicating the equal status as fundamental science of cognitive psychology with respect to neuroscience.

[15] "The key observation is that neurophysiology and psychophysics have as their business to *describe* the behavior of cells or of subjects but not to *explain* such behavior" (Marr 1982: 15; emphasis in original). By the way, Chomsky has also expressed his reservations to finding points of contact between his approach and Marr's (Chomsky 1997: 23).

computational levels; Poeppel 2012: 35). Consider the case of the 'connectome' and the 'dynome', for example. These are clearly not levels in any possible sense, since, as Kopell et al.—the inventors of the term 'dynome'—observe:

> What is needed is not only *what* is connected, but *how* and in what directions regions of the brain are connected: what signals they convey and how those signals are acted upon as part of a neural computational process. (Kopell et al. 2014: 1319; emphasis in original)

Thus, in a typical mechanistic fashion, we identify the connections (parts) and the signals and operations over signals (activities) these parts perform, and it is this organization together with its behavior in specific circumstances what makes this the mechanism underlying a phenomenon, not their position in a predefined level. In fact, talk of levels defined a priori is useless, because, as pointed out by Craver (2007: 191), this has to be solved on a case-by-case basis—what is explanatory relevant for each phenomenon "cannot be read off a menu of levels in advance."

Failure to carry out a mechanistic analysis may result in misrepresenting Lenneberg's prediction that "it is not possible to assign any specific neuro-anatomic structure to the capacity for language" (Lenneberg 1967: 72) in the sense that we "need to think of language-related tasks as 'whole-brain' affairs" (Boeckx & Theofanopoulou 2014: 411). This, we surmise, is inaccurate and a mere side effect of the fact that, more often than not, "the components picked out in a mechanistic decomposition fail to correspond to paradigmatic entities with clear spatial boundaries" (Craver 2007: 190), because the unifying principle is organized behavior or, in other words, "the peculiar way in which the various parts of the brain work together" (Lenneberg 1967: 72). Part of the problem stems, no doubt, from lack of consensus—and, we should add, accuracy—in the characterization of what constitutes the 'linguistic cognome', to use Poeppel's (2012) expression and diagnostic, which may be worked out also by applying Poeppel's recipe of 'radical decomposition' in the characterization of the phenomena liable to mechanistic analysis, 'language' being just an umbrella term ranging across several phenomena, each subserved by a manifold of (possibly overlapping) underlying mechanisms.

Notwithstanding, Poeppel's strategy, promising as it is, still faces in our opinion a more recalcitrant problem: the problem of computation (Balari & Lorenzo 2016). Few would challenge today the contention that the foundational hypothesis of cognitive science is that cognition is computation.[16] Lenneberg, for one, already foresaw that the core of an explanatory account of language would necessarily incorporate a computational account of the biological operations involved (physiological function)[17]—hence our digression above to reach this point. Unfor-

---

[16] Some do, for example Tim van Gelder (van Gelder 1995, 1998), but, as shown by Kaplan & Craver (2011), the kinds of models championed by dynamicists are holistic models that describe the behavior of some complex system without actually paying attention to how it does work and are, therefore, nonmechanistic.

[17] This point is more explicitly expressed by Lenneberg in his 1969 paper than in the 1967 book, thus:

> it is [. . .] reasonable to assume that individuals who speak Turkish, English, or Basque (or who spoke Sanskrit some millennia ago) all have (or had) the same kind of brain, that is, a computer with the same operating principles and the same sensorium; (Lenneberg 1969: 640)

tunately, paraphrasing John Haugeland (2002: 160), we don't seem to know anymore what computation really is. Which probably is but the tip of the iceberg of our poor current understanding of cognition at a functional/physiological level. Computation, to be sure, is a complex notion, susceptible to many different interpretations and formalizations (Smith 2002, for at least seven of these) and therefore we still need to find an answer to the question: "What *type* of computation is cognition?" (Smolensky & Legendre 2006: 5; emphasis in original). But the question itself may be deceiving, for we do not even know whether computation exhausts cognition. So a first, more urgent question would perhaps be: "What type of cognition is computation?" Some higher-order notion should come to the rescue here, a natural (cover) kind of sorts for all types of cognition—computational or otherwise. Some insistently claim that the 'dynamical system' concept may do the job (see, for example, Wheeler 2005). But if so: What type of dynamical system is a computational system? For many, the answer is straightforward: Dynamical computational systems are those that specifically feed on units that matter for their informational value, not just for their metabolic or energetic import. But this just introduces us into the not less slippery vocabulary of vehicles, symbols, representations, and so forth (Balari & Lorenzo 2016), the prospect of naturalizing which is for many beyond the limits of the human science-forming capacity.

Even putting aside most such complicating factors, the most plausible formalization of computation capable of accounting for higher cognitive processes, digital computation, doesn't appear to match what we know so far about what is going on in the brain. In other words, neural computation (if it is computation at all) appears to be sui generis (Piccinini & Bahar 2013), and at present we do not have a comprehensive notion of computation encompassing the traditional view and the kind of computational activity that brains are presumed to perform—computational neuroscientists simply assume that nervous systems compute, no one has ever proven that this is so (Piccinini & Shagrir 2014). In any event, it might be the case that a concept of computation not very different from the traditional one will be suitable enough for the particular case of the computation of internal linguistic expressions, considering that this is a task in which the subtle and precise synchronization processes with the complex ongoing flux of environmental stimulation, which for some marks an upper limit for classical computation (Wheeler 2005), do not dramatically arise. So far, however, all this is beyond anyone's guess.

We raise these issues hopefully not for provoking a paralyzing effect, but to caution against an excessively enthusiastic reading of certain recent proposals concerning the computational character of brain oscillations (e.g., those of Murphy 2015, 2016) which do not seem to have taken into account the complications we just alluded to. To repeat, this is not to deny the potential relevance of brain oscillations in an eventual account of neural computation, but evidence so far is only correlational, in the sense that oscillations do play some role in linguistic tasks (e.g., Lewis et al. 2015, Lewis & Bastiaansen 2015, Ding et al. 2016), but we have so far been unable to disentangle the computational role they purportedly play. Compared to the bulk of data coming from the neuroscience camp, relatively little effort is being devoted to articulate detailed computational analyses capable of "[challenging]

---

and "[t]he human brain is a biochemical machine; it computes the relations expressed in sentences and their components" (Lenneberg 1969: 642–643).

neurobiologists to define and characterize the neural circuitry that can underpin [them]", as Poeppel (2012: 52) would put it. The theory of computation is our main tool to carry out this project as it makes it possible to construct hypotheses and to identify constraints under the assumption that, if brains compute, then cognition is tractable (Frixione 2001, van Rooij 2008, Balari & Lorenzo 2013a). An example of this could be the proposal articulated both by Gallistel & King (2009) and by Balari & Lorenzo (2013a), each based on different grounds, that a basic component in the architecture of the computational system would be a memory workspace, given the inherent complexity of certain tasks carried out both by humans and other animals. A project not too detached form Lenneberg's conjecture that

> The cognitive function underlying language consists of an adaptation of a ubiquitous process (among vertebrates) of categorization and extraction of similarities. (Lenneberg 1967: 374)

4. Concluding Remarks

In *Language and Thought*, Chomsky conjectures that if the human cognitive system was

> embedded in different performance systems in some hypothetical (perhaps biologically impossible) organism, [lexical items] could serve as instructions for some other activity. (Chomsky 1993: 48)

In this paper we have tried to show that the research program delineated by Lenneberg, with his views on phylogenetics and mechanistic explanation, already contains the seeds for showing that Chomsky's hypothesized organism is not only not biologically impossible but that it may have been, and may be, quite actual. Bad times for human uniqueness? We believe so. In any event, not worse than 50 years ago, when Eric Lenneberg had already broadcasted it.

References

Anderson, Stephen R. & David W. Lightfoot. 2002. *The Language Organ: Linguistics as Cognitive Physiology*. Cambridge: Cambridge University Press.

Arbib, Michael A. 2017 [= this issue]. "Language and Brain: Developmental Aspects": Eric Lenneberg at the Neurosciences Research Program in 1972. *Biolinguistics* 11(2).

Austin, John L. 1962. *Sense and Sensibilia*. Oxford: Oxford University Press.

Ayub, Qasim, Bryndis Yngvadottir, Yuan Chen, Yali Xue, Min Hu, Sonja C. Vernes, Simon E. Fisher & Chris Tyler-Smith. 2013. FOXP2 targets show evidence of positive selection in European populations. *The American Journal of Human Genetics* 92, 696–706.

Balari, Sergio & Guillermo Lorenzo. 2010. Communication. Where evolutionary linguistics went wrong. *Biological Theory* 5, 228–239.

Balari, Sergio & Guillermo Lorenzo. 2012. Apología del villano. Richard Owen y el origen de Evo Devo. In Richard Owen, *Discurso sobre la naturaleza de las extremidades* [Sergio Balari & Guillermo Lorenzo (eds.)], 11–71. Oviedo: KRK.

Balari, Sergio & Guillermo Lorenzo. 2013a. *Computational Phenotypes. Towards an Evolutionary Developmental Biolinguistics*. Oxford: Oxford University Press.

Balari, Sergio & Guillermo Lorenzo. 2013b. Richard Owen and the mind/body problem. *Theoretical Biology Forum* 106, 131–146.

Balari, Sergio & Guillermo Lorenzo. 2015a. Ahistorical homology and multiple realizability. *Philosophical Psychology* 28, 881–902.

Balari, Sergio & Guillermo Lorenzo, 2015b. It is an organ, it is new, but it is not a new organ. Conceptualizing language from a homological perspective. *Frontiers on Ecology and Evolution* 3. doi:10.3389/fevo.2015.00058.

Balari, Sergio & Guillermo Lorenzo. 2016. Evo-devo of language and cognition. In Laura Nuño de la Rosa & Gerd Müller (eds.), *Evolutionary Developmental Biology. A Reference Guide*. doi:"10.1007/978-3-319-33038-9_43-1".

Bechtel, William. 2008. *Mental Mechanisms. Philosophical Perspectives on Cognitive Neuroscience*. New York: Psychology Press.

Becker, Martin. 2016. On the identification of FOXP2 gene enhancers and their role in brain development. Nijmegen: Radboud University Nijmegen dissertation.

Becker, Martin, Paolo Devanna, Simon E. Fisher & Sonia C. Vernes. 2015. A chromosomal rearrangement in a child with sever speech and language disorder separates *FOXP2* from a functional enhancer. *Molecular Cytogenetics* 8, 69.

Berwick, Robert C. & Noam Chomsky. 2011. The Biolinguistic Program: The current state of its development. In Anna Maria di Sciullo & Cedric Boeckx (eds.), *The Biolinguistic Enterprise. New Perspectives on the Evolution and Nature of the Human Language Faculty*, 19–41. Oxford: Oxford University Press.

Berwick, Robert C. & Noam Chomsky. 2016. *Why Only Us. Language and Evolution*. Cambridge, MA: MIT Press.

Bickle, John. 1998. *Psychoneural Reduction: The New Wave*. Cambridge, MA: The MIT Press.

Bickle, John. 2003. *Philosophy of Neuroscience: A Ruthlessly Reductive Approach*. Dordrecht: Kluwer.

Boeckx, Cedric. 2012. The I-language mosaic. In Cedric Boeckx, María de Carmen Horno-Chéliz & José Luis Mendívil-Giró (eds.), *Language, From a Biological Point of View*, 23–51. Newcastle upon Tyne: Cambridge Scholars Publishing.

Boeckx, Cedric & Constantina Theofanopoulou. 2014. A multidimensional interdisciplinary framework for linguistics: The lexicon as a case study. *Journal of Cognitive Science* 14, 403–420.

Bolhuis, Johan J., Ian Tattersall, Noam Chomsky & Robert C. Berwick. 2014. How could language have evolved? *PloS Biology* 12. doi:10.1371/journal.pbio.1001934.

Bonkowsky, Joshua L. & Chi-Bin Chien. 2005. Molecular cloning and developmental expression of foxP2 in zebrafish. *Developmental Dynamics* 234, 740–746.

Bonkowsky, Joshua L., Xu Wang, Esther Fujimoto, Ji Eun Lee, Chi-Bin Chien & Richard I. Dorsky. 2008. Domain-specific regulation of *foxP2* CNS expression by *lef1*. *BCM Developmental Biology* 8, 103.

Brentano, Franz C. 1874 [2009]. *Psychology from an Empirical Standpoint*. London: Routledge & Kean Paul.

Buzsáki, György. 2006. *Rhythms of the Brain*. Oxford: Oxford University Press.

Canguilhem, Georges. 1952. *Le connaissance de la vie.* Paris: Hachette.

Castellucci, Gregg A., Matthew J. McGinley & David A. McCormick. 2013. Knockout of *Foxp2* disrupts vocal development in mice. *Scientific Reports* 6, 23305. doi:10.1038/srep23305

Chabout, Jonathan, Abhra Sarkar, Sheel R. Patel, Taylor Radden, David B. Dunson, Simon E. Fisher & Erich D. Jarvis. 2016. A Foxp2 mutation implicated in human speech deficits alters sequencing of ultrasonic vocalizations in adult male mice. Front*iers in Behavioral Neuroscience* 10, 197. doi:10.3389/fnbeh.2016.00197.

Chandrasekaran, Bharath, Han-Gyol Yi, Nathaniel J. Blanco, John E. McGeary & W. Todd Maddox. 2015. Enhanced procedural learning of speech sound categories in a genetic variant of FOXP2. *The Journal of Neuroscience* 35, 7808–7812.

Chen, Qui, Tengteng Zhu, Gareth Jones, Junpeng Zhang & Yi Sun. 2013. First knockdown gene expression in bat (*Hipposideros armiger*) brain mediated by lentivirus. *Molecular Biotechnology* 54, 564–571.

Chiu, Yi-Chi, Ming-Yang Li, Yuan-Hsuan Liu, Jing-Ya Ding, Jenn-Yah Yu & Tsu-Wei Wang. 2014. Foxp2 regulates neuronal differentiation and neuronal subtype specification. *Developmental Neurobiology* 74, 723–738.

Chomsky, Noam. 1966. *Cartesian Linguistics. A Chapter in the History of Rationalist Thought*. New York: Harper & Row.

Chomsky, Noam. 1968. *Language and Mind*. New York: Harcourt, Brace and World.

Chomsky, Noam. 1993. *Language and Thought*. Wakefield, RI: Moyer Bell.

Chomsky, Noam. 1995a. Language and nature. *Mind* 104, 1–61.

Chomsky, Noam. 1995b. *The Minimalist Program*. Cambridge, MA: The MIT Press.

Chomsky, Noam. 1997. Language and cognition. In David Martel Johnson & Christina E. Erneling, eds., *The Future of the Cognitive Revolution*, 15–31. New York: Oxford University Press.

Chomsky, Noam. 2000. *New Horizons in the Study of Language and Mind*. Cambridge: Cambridge University Press.

Chomsky, Noam. 2003. Reply to Lycan. In Louise M. Antony & Norbert Hornstein, eds., *Chomsky and His Critics*, 255–263. Malden, MA: Blackwell.

Chomsky, Noam. 2013. Problems of projection. *Lingua* 113, 39–49.

Chomsky, Noam. 2016. *What Kinds of Creatures Are We?* New York: Columbia University Press.

Churchland, Paul M. 1989. *A Neurocomputational Perspective. The Nature of Mind and the Structure of Science*. Cambridge, MA: The MIT Press.

Churchland, Patricia S. 1986. *Neurophilosophy. Toward a Unified Science of the Mind/Brain*. Cambridge, MA: The MIT Press.

Churchland, Patricia S. & Terrence J. Sejnowski. 1992. *The Computational Brain*. Cambridge, MA: The MIT Press.

Craver, Carl F. 2005. Beyond reduction: Mechanisms, multifield integration and the unity of neuroscience. *Studies in History and Philosophy of Biological and Biomedical Sciences* 36, 373–395.

Craver, Carl F. 2007. *Explaining the Brain. Mechanisms and the Mosaic Unity of Neuroscience*. Oxford: Oxford University Press.

Craver, Carl F. 2015. Levels. In Thomas Metzinger & Jennifer M. Windt, eds. *Open MIND*, 8(T). Frankfurt am Main: Open MIND Group. doi:10.15502/

9783958570498.

Craver, Carl F. & Lindley Darden. 2013. *In Search of Mechanisms. Discoveries across the Life Sciences*. Chicago: The University of Chicago Press.

Cummins, Robert. 1975. Functional analysis. *The Journal of Philosophy* 72, 741–765.

Cummins, Robert. 1983. *The Nature of Psychological Explanation*. Cambridge, MA: The MIT Press.

Cummins, Robert. 2002. Neo-teleology. In André Ariew, Robert Cummins & Mark Perlman, eds. *Functions. New Essays in the Philosophy of Psychology and Biology*, 157–172. Oxford: Oxford University Press.

DasGupta, Shamik, Clara H. Ferreira & Gero Miesenböck. 2014. FoxP influences the speed and accuracy of a perceptual decision in *Drosophila*. *Science* 344, 901–904.

Denisenko-Nehrbass, Natalia I., Eric D. Jarvis, Constance Scharff, Fernando Nottebohm & Claudio V. Mello. 2000. Site-specific retinoic acid production in the brain of adult songbirds. *Neuron* 27, 359–370.

Denisenko-Nehrbass, Natalia I. & Claudio V. Mello. 2001. Molecular targets of disulfiram action on song maturation in zebra finches. *Molecular Brain Research* 87, 246–250.

Dennett, Daniel C. 2017. *From Bacteria to Bach and Back. The Evolution of Minds*. London: Allen Lane.

Devanna, Paolo, Jeroen Middlebeek & Sonja C. Vernes. 2014. FOXP2 dirves neuronal differentiation by interacting with retinoic acid signaling pathways. *Frontiers in Cellular Neuroscience* 8, 305. doi:10.3389/fncel.2014.00305.

Ding, Nai, Lucia Melloni, Hang Zhang, Xing Tian & David Poeppel. 2016. Cortical tracking of hierarchical linguistic structures in connected speech. *Nature Neuroscience* 19, 158–164.

Eliasmith, Chris & Charles H. Anderson. 2003. *Neural Engineering: Computation, Representation, and Dynamics in Neurobiological Systems*. Cambridge, MA: The MIT Press.

Enard, Wolfgang. 2011. FOXP2 and the role of cortico-basal ganglia circuits in speech and language evolution. *Current Opinion in Genetics and Development* 21, 415–424.

Enard, Wolfgang. 2014. Mouse models of human evolution. *Current Opinion in Genetics and Development* 29, 75–80.

Enard, Wolfgang, Sabine Gehre, Kurt Hammerschmidt, . . . , & Svante Pääbo. 2009. A humanized version of Foxp2 affects cortico-basal ganglia circuits in mice. *Cell* 137, 961–971.

Enard, Wolfgang, Molly Przeworski, Simon E. Fisher, Cecilia S.L. Lai, Victor Wiebe, Takashi Kitano, Anthony Monaco & Svante Pääbo. 2002. Molecular evolution of FOXP2, a gene involved in speech and language. *Nature* 418, 868–872.

Ereshefsky, Marc. 2012. Homology thinking. *Biology and Philosophy* 27, 382–400.

Estruch, Sara B., Sarah A. Graham, Pelagia Deriziotis & Simon E. Fisher. 2016. The language-related transcription factor FOXP2 is post-translationally modified with small ubiquitin-like modifiers. *Scientific Reports* 6, 20911.

Ferland, Russell J., Timothy Cherry, Patricia O. Preware, Edward E. Morrisey & Chistopher A. Walsh. 2003. Characterization of Foxp2 and Foxp1 mRNA and protein in the developing and mature brain. *Journal of Comparative Neurology*

26, 266–279.

Fisher, Simon E. & Constance Scharff. 2009. FOXP2 as a molecular window into speech and language. *Trends in Genetics* 26, 166–177.

Fitch, W. Tecumseh. 2005. The evolution of language: A comparative review. *Biology and Philosophy* 20, 193–230.

Fitch, W. Tecumseh. 2010. Deep Homology. In Ana M. Di Sciullo & Cedric Boeckx (eds.), *The Biolinguistic Enterprise: New Perspectives on the Evolution and Nature of the Human Language Faculty*, 135–166. Oxford: Oxford University Press.

Fitch, W. Tecumseh, Noam Chomsky & Marc D. Hauser. 2005. The evolution of the language faculty: Clarifications and implications. *Cognition* 97, 179–210.

Fodor, Jerry A. 1965. Explanations in psychology. In Max Black (ed.), Philosophy in America, 161–179. Ithaca, NY: Cornell University Press.

Fodor, Jerry A. 1968. *Psychological Explanation: An Introduction to the Philosophy of Psychology*. New York: Random House.

Fodor, Jerry A. 1974. Special sciences. *Synthese* 28, 77–115.

Fodor, Jerry A. 1975. *The Language of Thought*. Cambridge, MA: Harvard University Press.

Fodor, Jerry A. 1997. Special sciences: Still autonomous after all these years. *Noûs* 31 (Supplement: Philosophical Perspectives, 11, Mind, Causation, and World), 149–163.

Fodor, Jerry A. & Massimo Piattelli-Palmarini. 2010. *What Darwin Got Wrong*. New York: Farrar, Straus & Giroux.

Fontenot, Miles & Konopka, Genevieve. 2014. Molecular networks and the evolution of human cognitive specializations. *Current Opinion in Genetic and Development* 29, 52–59.

Frixione, Marcello. 2001. Tractable competence. *Minds and Machines* 11, 379–397.

Fujita-Jimbo, Eriko & Takashi Momoi. 2014. Specific expression of FOXP2 in cerebellum improves ultrasonic vocalization in heterozygous but not in homozygous FoxP2 (R552H) knock-in pups. *Neuroscience Letters* 566, 162–166.

Gallistel, C. Randy & Adam Philip King. 2009. *Memory and the Computational Brain. Why Cognitive Science Will Transform Neuroscience*. Malden, MA: Wiley-Blackwell.

Gehring, Walter J. & Ikeo Kazuko. 1999. Pax6: Mastering eye morphogenesis and eye evolution. *Trends in Genetics* 15, 371–377.

Graham, Sarah & Simon E. Fisher. 2013. Decoding the genetics of speech and language. *Current Opinion in Neurobiology* 23, 43–51.

Haesler, Sebastian, Kazuhiro Wada, A. Nshdejan, Edward E. Morrisey, Thierry Lints, Eric D. Jarvis and Constantine Scharff. 2004. FOXP2 expression in avian vocal learners and non-learners. *The Journal of Nerosciences* 24, 3164–3175.

Haesler, Sebastian, Christelle Rochefort, Benjamin Georgi, Pawel Licznerki, Pavel Osten & Constance Scharff 2007. Incomplete and inaccurate vocal imitation after knockdown of FoxP2 in songbird basal ganglia nucleus Area X. *PLoS Biology* 5, e321. doi:10.1523/JNEUROSCI.4369-03.2004.

Haugeland, John. 2002. Authentic intentionality. In Matthias Scheutz, ed. *Computationalism. New Directions*, 159–174. Cambridge, MA: The MIT Press.

Hauser, Mark D., Noam Chomsky & W. Tecumseh Fitch. 2002. The faculty of language: What is it? Who has it? How did it evolve? *Science* 298, 1569–1579.

Hauser, Mark D., Charles Yang, Robert C. Berwick, ..., & Richard C. Lewontin. 2014. The mistery of language evolution. *Frontiers in Psychology* 5. doi:10.3389/fpsyg.2014.00401.

Hempel, Carl & Paul Oppenheim. 1948. Studies in the logic of explanation. *Philosophy of Science* 15, 135–175.

Hockett, Charles F. 1989. The origin of speech. *Scientific American* 6, 80–111.

Itakura, Tatsuo, Abhishek Chandra, Zhi Yang, Xiao Dong Xue, Wataru Kimura, Keisuke Hikosaka, Keiji Inohaya, Akira Kudo, Tedayoshi Uezato & Naoyuki Miura. 2008. The medaka FoxP2, a homologue of human language gene FOXP2, has a diverged structure and function. *The Journal of Biochemistry* 143, 407–416.

Jackendoff, Ray & Steven Pinker. 2005. The nature of the language faculty and its implications for the evolution of language (Reply to Fitch, Hauser and Chomsky). *Cognition* 97, 211–225.

Johnson, Matthew B., Yuka Imamura Kawasaka, Chistopher E. Mason, Zeljika Krsnik, Giovanni Coppola, Darki Bodganovic, Daniel H. Geschwind, Shrikant M. Mane, Matthew W. State & Nenad Sestan. Functional and evolutionary insights into human brain development through global transcriptome analysis. *Neuron* 62, 494–509.

Kaplan, David Michael & Carl F. Craver. 2011. The explanatory force of dynamical and mathematical models in neuroscience: A mechanistic perspective. *Philosophy of Science* 78, 601–627.

Kim, Jaegwon. 2005. *Physicalism, or Something Near Enough*. Princeton, NJ: Princeton University Press.

Kiya, Takeo, Y. Itoh & T. Kubo. 2008. Expression analysis of the FoxP homologue in the brain of the honeybee, Apis mellifera. *Insect Molecular Biology* 17, 53–60.

Konopa Genevieve, Jamie M. Bomar, Kellen Winden, Giovanni Coppola, Zophonias O. Jonsson, Fuying Gao, Sophia Peng, Todd M. Preuss, James A. Wohlschlegel & Daniel H. Geschwind. 2009. Human-specific transcriptional regulation of CNS development genes by FOXP2. *Nature* 462, 213–217.

Konopka, Genevieve, Tara Friedrich, Jeremy Davis-Turak, Kellen Winden, Michael C. Oldham, Fuying Gao, Leslie Chen, Guang-Zhong Wang, Rui Luo, Todd M. Preuss & Daniel Geschwind. 2012. Human-specific transcriptional networks in the brain. *Neuron* 75, 601–617.

Konopka, Genevieve & Geschwind, Daniel H. 2010. Human brain evolution: Harnessing the genomics (r)evolution to link genes, cognition, and behavior. *Neuron* 68, 231–244.

Kopell, Nancy J., Howard J. Gritton, Miles A. Whittington & Mark A. Kramer. 2014. Beyond the connectome: The Dynome. *Neuron* 83, 1319–1328.

Lai, Cecilia S., Simon E. Fisher, Jane A. Hurst, Faraneh Vargha-Khadem & Anthony P. Monaco. 2001. A forkhead-domain gene is mutated in a severe speech and language disorder. *Nature* 413, 519–253.

Lai, Cecilia S.L., Dianne Gerrelli, Anthony P. Monaco, Simon E. Fisher & Andrew J. Copp. 2003. FOXP2 expression during brain development coincides with adult sites of pathology in a severe speech and language disorder. *Brain* 126, 2455–2462.

Lawton, Kristy J., Taryn L. Wassmer & David L. Deitcher. 2014. Conserved role of *Drosophila melanogaster* FoxP in motor coordination and courtship song. *Behavioral Brain Research* 268, 213–221.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York: John Wiley & Sons.

Lenneberg, Eric H. 1969. On explaining language. *Science* 164, 635–643.

Lewis, Ashley G. & Marcel Bastiaansen. 2015. A predictive coding framework for rapid neural dynamics during sentence-level language comprehension. *Cortex* 68, 155–168.

Lewis, Ashley G., Lin Wang & Marcel Bastiaansen. 2015. Fast oscillatory dynamics during language comprehension: Unification versus maintenance and prediction?. *Brain & Language* 148, 51–63.

Li, Gang, Jinhong Wang, Stephen J. Rossiter, Gareth Jones & Shuyi Zhang. 2007. Accelerated *FoxP2* evolution in echolocating bats. *PLoS ONE* 2, e900. doi:10.1371/journal.pone.0000900.

Liégeois, Frédérique, Torsten Badelweg, alan Connelly, David G. Gadian, Mishkin, Mortimer & Faraneh Varga-Khadem. 2003. Language fMRI abnormalities associated with *FOXP2* gene mutation. *Nature Neuroscience* 6, 1230–1237.

Love, Alan C. 2007. Functional homology and homology of function: Biological concepts and philosophical consequences. *Biology & Philosophy* 22, 691–708.

Lycan, William G. 2003. Chomsky on the Mind-Body Problem. In Louise M. Antony & Norbert Hornstein, eds., *Chomsky and His Critics*, 11–28. Malden, MA: Blackwell.

Machamer, Peter, Lindley Darden & Carl F. Craver. 2000. Thinking about mechanisms. *Philosophy of Science* 67, 1–25.

Marr, David. 1982. *Vision*. San Francisco: W. H. Freeman.

Mayr, Ernst. 1960. The emergence of evolutionary novelties. In Sol Tax (ed.), *Evolution After Darwin. The University of Chicago Centennial, Vol.1: The Evolution of Life. Its Origin, Evolution and Future*, 349–380. Chicago, IL: The University of Chicago Press.

Medvedeva, Vera. 2015. *Characterization of Foxp2 functions in the mouse cortex*. Paris: Université Pierre et Marie Curie dissertation.

Mendoza Ezequiel, Julien Colomb, Jürgen Rybak, Hans-J. Pflüger, Troy Zars, Constance Scharff & Björn Brembs. 2014. Drosophila FoxP mutants are deficient in operant self-learning. *PLoS One* 9, e100648. doi:10.1371/journal.pone.0100648.

Millikan, Ruth G. 1984. *Language, Thought, and Other Biological Categories. New Foundations for Realism*. Cambridge, MA: The MIT Press.

Murphy, Elliot. 2015. The brain dynamics of linguistic computation. *Frontiers in Psychology* 6: 1515. doi:10.3389/fpsyg.2015.01515.

Murphy, Elliot. 2016. The human oscillome and its explanatory potential. *Biolinguistics* 10, 6–20.

Müller, Gerd B. & Günter P. Wagner. 1991. Novelty in evolution: Restructuring the concept. *Annual Review of Ecology and Systematics* 22, 229–256.

Müller, Gerd B. & Günter P. Wagner. 2003. Innovation. In Brian K. Hall & Wendy M. Olson (eds.), *Keywords and Concepts in Evolutionary Developmental Biology*, 218–227. Cambridge, MA: Harvard University Press.

Newbury, Dianne F., Simon E. Fisher & Anthony Monaco. 2010. Recent advances in the genetics of language impairment. *Genome Medicine* 2, 6. doi:10.1186/gm127.

Newbury, Dianne F. & Anthony Monaco. 2010. Genetic advances in the study of speech and language disorders. *Neuron* 68, 309–320.

Nitecki, Matthew H. (ed.). 2016. *Evolutionary Innovations.* Chicago, IL, and London: The University of Chicago Press.

Oppenheim, Paul & Hilary Putnam. 1958. Unity of science as a working hypothesis. In Herbert Feigl, Michael Scriven & Grover Maxwell, eds. *Concepts, Theories, and the Mind-Body Problem. Minnesota Studies in the Philosophy of Science II*, 3–36. Minneapolis: University of Minnesota Press.

Owen, Richard. 1843. *Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals.* London: Longman, Brown, Green and Longmans.

Owen, Richard. 1849. *On the Nature on Limbs. A Discourse.* London: John Van Voorst.

Pfenning, Andreas R., Erina Hara, Osceola Whitney, ..., & Eric D. Jarvis. 2014. Convergent transcriptional specializations in the brains of humans and song-learning birds. *Science* 346, doi:10.1126/science.1256846.

Phillmore, Leslie S., Heather L. MacGillivray, K. Ryan Wilson & Stephanie Martin. 2014. Effects of sex and seasonality on the song control system and FoxP2 protein expression in black-capped chickadees (*Poecile atricapillus Developmental Neurobiology* 75, 203–216.

Piccinini, Gualtiero. 2007. Computing mechanisms. *Philosophy of Science* 74, 501–526.

Piccinini, Gualtiero. 2015. *Physical Computation. A Mechanistic Account*. Oxford: Oxford University Press.

Piccinini, Gualtiero & Sonya Bahar. 2013. Neural computation and the computational theory of cognition. *Cognitive Science* 34, 453–488.

Piccinini, Gualtiero & Carl F. Craver. 2011. Integrating psychology and neuroscience: functional analyses as mechanism sketches. *Synthese* 183, 283–311.

Piccinini, Gualtiero & Oron Shagrir. 2014. Foundations of computational neuroscience. *Current Opinion in Neurobiology* 25, 25–30.

Pinker, Steven & Ray Jackendoff. 2005. The faculty of language: What's special about it. *Cognition* 95, 201–236.

Poeppel, David. 2012. The maps problem and the mapping problem: Two challenges for a cognitive neuroscience of speech and language. *Cognitive Neuropsychology* 29, 34–55.

Poeppel, David & David Embick. 2005. Defining the relation between linguistics and neuroscience. In Anne Cutler, ed., *Twenty-First Century Psycholinguistics: Four Cornerstones*, 103–118. Mahwah, NJ: Lawrence Erlbaum.

Poland, Jeffrey. 2003. Chomsky's challenge to physicalism. In Louise M. Antony & Norbert Hornstein, eds., *Chomsky and His Critics*, 29–48. Malden, MA: Blackwell.

Polger, Thomas W. 2004. *Natural Minds*. Cambridge, MA: The MIT Press.

Polger, Thomas W. & Lawrence A. Shapiro. 2016. *The Multiple Realization Book*. Oxford: Oxford University Press.

Preuss, Todd M. 2012. Human brain evolution: From gene discovery to phenotype discovery. *Proceedings of the National Academy of Sciences U.S.A.* 109, 10709–10716.

Putnam, Hilary. 1975. The nature of mental states. In *Mind, Language and Reality: Philosophical Papers, Vol 2*, 429–440. Cambridge: Cambridge University Press.

Reimers-Kipping, S. W. Hevers, S. Pääbo & W Enard. 2011. Humanized Foxp2 specifically affects cortico-basal ganglia circuits. *Neuroscience* 175, 75–84.

Richards, Robert J. 2002. *The Romantic Conception of Life. Science and Philosophy in the Age of Goethe*. Chicago: The University of Chicago Press.

Richards, Robert J. 2005. Darwin's metaphysics of mind. In V. Hösle & Ch. Illies, eds., *Darwinism and Philosophy*, 166–180. Notre Dame, IN: University of Notre Dame Press.

Rieke, Fred, David Warland, Rob de Ruyter van Steveninck & William Bialek. 1997. *Spikes. Exploring the Neural Code*. Cambridge, MA: The MIT Press.

Rodenas-Cuadrado, Pedro, Joses Ho & Sonja C. Vernes. 2014. Shining a light on *CNTNAP2*: Complex functions to complex disorders. *European Journal of Human Genetics* 22, 171–178.

Roll, Patrice, Sonja C. Vernes, Nadine Bruneau, Jennifer Cillario, Magali Ponsole-Lenfnt, Annick Massacrier, Gabrielle Rudolf, Manal Khalife, Edouard Hirsh, Simon E. Fisher & Pierre Szepetowski. 2010. Molecular networks implicated in speech-relates disorders: FOXP2 regulates the SRPX2/uPAR complex. *Human Molecular Genetics* 19, 4848–4860.

Roeske, Christina. 2010. The role of retinoid signaling in the song control system of the zebra finch: From molecules to behavior. The neurobiological basis of song learning in songbirds. Berlin: Freie Universität dissertation.

Roeske, Tina C., Constance Scharff, Christopher R. Olson, Arpik Nshdejan & Claudio V. Mello. 2014. Long-distance retinoic signaling in the zebra finch brain. *PLoS ONE* 9, e111722. doi:10.1371/journal.pone.0111722.

Scharff, Constance & Jana Petri. 2011. Evo-devo, Deep homology and FoxP2: Implications for the evolution of speech and language. *Philosophical Transactions of the Royal Society B* 266, 2124–2140.

Schneider, Gerald E. 2014. *Brain Structure and Its Origins in Development and in Evolution of Behavior and the Mind*. Cambridge, MA: The MIT Press.

Schreiweis, Christiane, Ulrich Bornschein, Eric Burgière, . . . , & Ann. M. Graybel. 2014. Humanized Foxp2 accelerates learning by enhancing transitions from declarative to procedural performance. *Proceedings of the National Academy of Sciences U.S.A.* 111, 14253–14258.

Searle, John R. 1992. *The Rediscovery of Mind.* Cambridge, MA: MIT Press.

Seung, Sebastian. 2012. *Connectome: How the Brain's Wiring Makes Us Who We Are*. London: Penguin.

Shagrir, Oron. 2010. Marr on computational-level theories. *Philosophy of Science* 77, 477–500.

Shah, Rina, Olga Medina-Martínez, Li-Fang Chu, Rodney C. Samaco & Milan Jamrich. 2006. Expression of *FoxP2* during zebrafish development and in the adult brain. *International Journal of Developmental Biology* 50, 435–438.

Shu, Weiguo, Julie Y. Cho, Yuhui Jiang, . . . , & Joseph D. Buxbaum. 2007. Altered ultrasonic vocalization in mice with a disruption in the *Foxp2* gene. *Proceedings of the National Academy of Sciences U.S.A.* 102, 9643–9648.

Shubin, Neil, Cliff Tabin & Sean Carroll. 1997. Fossils, genes and the evolution of animal limbs. *Nature* 388, 639–648.

Shubin, Neil, Cliff Tabin & Sean Carroll. 2009. Deep homology and the origins of evolutionary novelty. *Nature* 457, 818–823.

Smith, Brian Cantwell. 2002. The foundations of computing. In Matthias Scheutz, ed., *Computationalism. New Directions*, 23–58. Cambridge, MA: The MIT Press.

Smolensky, Paul & Géraldine Legendre. 2006. Harmony optimization and the computational architecture of the mind/brain. In Paul Smolensky & Géraldine Legendre, eds. *The Harmonic Mind: From Neural Computation to Optimality-Theoretic Grammar. Volume 1: Cognitive Architecture*, 3–61. Cambridge, MA: The MIT Press.

Stich, Stephen. 1983. *From Folk Psychology to Cognitive Science. The Case Against Belief.* Cambridge, MA: The MIT Press.

Teramitsu, Ikuko, Lili C. Kudo, Sarah E. London, Daniel H. Geschwind & Stephanie A. White. 2004. Parallel FoxP1 and FoxP2 expression in songbird and human brain predicts functional interaction. *Journal of Neuroscience* 24, 3152–3163.

Torres-Ruiz, Raul, Antonio Benítez-Burraco, Marta Martinez-Lage, Sandra Rodríguez-Perales & Paloma García-Bellido. 2016. Functional genetic characterization by CRISPR-Cas9 of two enhancers of FOPX2 in a child with speech and language impairment. *bioRxiv*. doi:10.1101/064196.

Usui, Noriyoshi, Marissa Co, Matthew Harper, Michael A. Rieger, Joseph D. Dougherty & Genevieve Konopka. 2017. Sumoylation of FOXP2 regulates motor function and vocal communication through Purkinje cell development. *Biological Psychiatry* 81, 220–230.

van Gelder, Tim. 1995. What might cognition be, if not computation?. *The Journal of Philosophy* 92, 345–381.

van Gelder, Tim. 1998. The dynamical hypothesis in cognitive science. *Behavioral and Brain Sciences* 21, 615–665.

van Rhijn, Jon-Ruber & Sonja C. Vernes. 2015. Retinoic Acid signaling: A new piece in the spoken language puzzle. *Frontiers in Psychology* 6, 1816. doi:10.3389/fpsyg.2015.01816.

van Rooij, Iris. 2008. The Tractable Cognition thesis. *Cognitive Science* 32, 939–984.

Vernes, Sonja C. 2017. What bats have to say about speech and language. *Psychonomic Bulletin and Review* 24, 111–117.

Vernes, Sonja C. & Simon E. Fisher. 2009. Unravelling neurogenetic networks implicated in developmental language disorders. *Biochemistry Society Transactions* 37, 1263–1269.

Vernes, Sonja C., Peter L. Oliver, Elizabeth Spiteri, . . . , & Simon E. Fisher. 2011. Foxp2 regulates gene networks implicated in neurite outgrowth in the developing brain. *PLoS Genetics* 7, e1002145. doi:10.1371/ journal.pgen.1002145.

Wagner, Andreas. 2011. *The Origins of Evolutionary Innovations.* Princeton, NJ, & Oxford: Princeton University Press.

Wagner, Günter P. 1989. The biological homology concept. *Annual Review of Ecology and Systematics* 20, 51–59.

Wagner, Günter P. 2014. *Homology, Genes, and Evolutionary Innovation.* Princeton, NJ: Princeton University Press.

Wagner, Günter P. 2015. What is 'homology thinking' and what is it for? *Journal of Experimental Zoology (Molecular and Developmental Evolution)* 326, 3–8.

Webb, D.M. & J. Zhang. 2005. *FoxP2* in song-learning birds and vocal-learning mammals. Jou*rnal of Heredity* 96, 212–216.

Webb, Helen Susannah. 2015. *Binding studies of the FOXP2 forkhead domain and its cognate DNA sequences*. Johannesburg: University of Witwatersrand dissertation.

Wheeler, Michael. 2005. *Reconstructing the Cognitive World. The Next Step*. Cambridge, MA: MIT Press.

White, Stephanie A., Simon E. Fisher, Daniel H. Geschwind, Constance Scharff & Timothy E. Holy. 2006. Signing mice, songbirds, and more: Models for FOXP2 function and dysfunction in human speech and language. *Journal of Neuroscience* 26, 10376–10379.

Wouters, Arno G. 2003. Four notions of biological function. *Studies in History and Philosophy of Biological and Biomedical Sciences* 34, 633–668.

Sergio Balari
Universitat Autònoma de Barcelona
Department de Filologia Catalana
Facultad de Lletres, Edifici B
Bellaterra, Barcelona 08193
Spain

sergi.balari@uab.cat

Guillermo Lorenzo
Universidad de Oviedo
Departamento de Filología Española
Facultad de Filosofía y Letras
Oviedo 33011
Spain

glorenzo@uniovi.es

Justifications for a Discontinuity Theory of Language Evolution

Callum Hackett

In Chapter 6 of *Biological Foundations of Language*, Lenneberg argues against continuity theories of language evolution, which claim that language evolved from simpler communication systems. Although Lenneberg was pessimistic about even discontinuity theories explaining how language evolved, discontinuity has become significant in the Minimalist program, which posits that our species' acquisition of Merge was the key discontinuity that made language possible. On the basis of a unified description of natural communication systems, I show that language is indeed based upon a cognitive discontinuity, which is moreover specific to linguistic ability. However, I argue that even Minimalist theories must recognise this discontinuity as the sensorimotor interface with syntax, rather than syntax itself. This ultimately supports the view that syntactic structures are structures of thought, but taking this claim seriously means reimagining how syntax relates to semantics and morphology, as the traditional 'lexical item' is no longer a tenable primitive of generative theory.

Keywords: continuity; communication systems; minimalism; syntax-semantics interface

1. Introduction

One thing that makes language so fascinating—and its origins so difficult to pin down—is that it is unprecedented in evolutionary history. This need not necessarily make it mysterious or problematic, as uniqueness is simply a corollary of diversity, which is plentiful in the natural world. It is therefore tempting to agree with Fitch, who says that

> the fact that humans [alone] have this particular capacity is no more surprising […] than other unusual features like the elephant's trunk, bat echolocation, or "radar" in electric fish. (Fitch 2010: 5)

Yet, the idiosyncrasies of other animals tend nonetheless to serve common biological needs, like ingestion, navigation, and object manipulation, while human language serves functions that no other species has been capable of, like creative and

---

I would like to thank Noel Burton-Roberts, Anders Holmberg, and Maggie Tallerman for many insightful conversations on the issues addressed here. I am also grateful to two anonymous reviewers for comments regarding clarity and precision—I trust that all the remaining bad ideas are cleanly articulated.

ISSN 1450–3417

unbounded expression. These functions having moreover made civilization possible, it is the emergence of language—and not the elephant's trunk—that Maynard Smith & Szathmáry (1997) give as much weight to in their history of life as the origins of genes, eukaryotes and sexuality.

Of course, to talk of the origins of language, we must bear in mind that it is an integration of many traits with evolutionary histories that are largely independent of language and independent of each other. Some of these traits—such as our vocal physiology, motor control, imitative abilities, social intelligence, and so on—may have been selectively refined for the roles that they now play in language, while others may have been recruited by language mostly unchanged. Some may have homologues in closely (or even distantly) related species, while others may only have precursors in hominins, if any at all. Here I argue that, however this human tapestry was woven, a sensorimotor interface with syntactically structured thought is a core linguistic ability that is unique to humans, so the cognitive basis of language must have originated within our lineage during the past six million years (the time of our last common ancestor with chimpanzees; see Patterson et al. 2006).

Theories like this one, which claim that there is no evolutionary precedent for language cognition, are often taken by their critics as claiming that language was something that came from nothing (e.g., Deacon 2003, Lieberman 2015): Some uniquely human capacity, whatever that is, must have been bolted onto our biology by sheer good fortune, rather than having developed from it by any ordinary means like adaptive selection. If this is how we are to interpret 'discontinuity', then I agree that we should reject it—the only genuine possibility for something to have come from nothing is to be left to cosmologists. In life, all new traits, no matter how unique or how quickly acquired, arise through the variation and selection that acts upon existing organic material. In a very real sense, then, evolutionary discontinuity *must* be construed as just a special case of continuity, and this paper will in part explain how that can be sensibly done.

In section 2, I reformulate Lenneberg's discussion of continuity and discontinuity theories of language evolution, with a consideration for how the debate has progressed in the fifty years since *Biological Foundations of Language*. Owing to some deficiencies in the terms of that debate, in section 2.1 I introduce some clarifying definitions. With those in place, in section 2.2 I adapt the notion of intentionality to describe the necessary properties of all natural communication systems, and I discuss why the defining property of language—its mapping of words to meanings—must be accounted for separately. I then consider whether there is any evidence for this property or its potential in nonhuman species, and I conclude that there is not, so language must be an evolutionary discontinuity. In section 2.3, I consider the significance of this, and I demonstrate that there is no reason to think of discontinuities as 'miracles'.

Given the need for a discontinuity theory, in section 3 I investigate the role of discontinuity in the Minimalist program. I begin in section 3.1 with Chomsky's standard conception, which I defend as generally plausible, though I argue that it is on the whole unconvincing in its integration with all that we know about human and nonhuman cognition. In section 3.2 I propose an alternative Minimalist discontinuity theory, which does not stipulate that Merge is biologically recent and unique to humans. I claim instead that the only evolutionary innovation in language was sensorimotor access to already available syntactic structures, which do not themselves provide linguistic ability. For this to be true, the input to Merge must be purely conceptual, not lexical, and I sketch some support for this by considering how syntactic and semantic structures might be maximally transparent. I end by highlighting some broader programmatic issues.

2. Justifications for a Discontinuity Theory

2.1. The Terms of the Debate

Lenneberg does not give especially principled definitions of the terms 'continuity' and 'discontinuity', essentially because he adopts them for rhetorical reasons.[1] At the beginning of his discussion in chapter 6, he lists examples of animal noise-making that have been considered primitive forms of communication and then summarises the rationale underlying continuity theories in this way:

> Since Darwin has shown that man is not the product of special creation but that he descended from more primitive animal forms, neither his structure nor his behavior are special creations. His forms of communication must have descended from primitive animal forms of communication, and a study of the latter is likely to disclose that there is indeed a straight line of evolution of this feature. (Lenneberg 1967: 227–228)

After exposing several issues with these assumptions (developed in section 2.2), Lenneberg introduces the alternative of discontinuity in these highly subjective terms:

> A discontinuity theory is not the same as a special creation theory. No biological phenomenon is without antecedents. The question is, "How obvious are the antecedents of the human propensity for language?" It is my opinion that they are not in the least obvious.
>
> (Lenneberg 1967: 234)

Lenneberg did not need any more precise definitions than this, as his intention was simply to provoke a reassessment of the similarities between human and nonhuman forms of communication. However, this paper will range beyond that reassessment and we will benefit from some more thoroughly worked out terminology.

Minimally, 'continuity' and 'discontinuity' ought to specify something to do with how traits in different species are related to each other. As Lenneberg noted, 'continuity' has been used opportunistically for every kind of cross-species similarity, no matter whether there is any underlying connection, such as a shared genetics. This lax equivalence of continuity with similarity effectively means that any trait we identify as similar to any other is a continuity, while any trait we identify as unique to one species is a discontinuity.

---

[1] He was aware of this and consequently sought to clarify his usage of these terms in the preface to the Japanese translation (see the editorial of this special issue for brief discussion).

Yet, this precludes the use of 'continuity' and 'discontinuity' in theories of trait *evolution*, as a trait's uniqueness is irrelevant to its origins and development. A trait might be shared by two species purely because it has evolved more than once through convergence, each time with a distinct genetic basis. In such cases, the sharing of the trait is a mere coincidence having nothing to do with its genesis and inheritance in any particular lineage. Just imagine that we wanted a theory of human bipedalism, for example. By examining closely related species, we can infer that the trait must have originated and developed in hominins, as it is a distinctively hominin characteristic among the larger family of hominids (Richmond et al. 2001). To understand that hominin heritage, it would be useless for us to catalogue instances of convergent bipedalism in, say, dinosaurs and birds, yet we would be compelled to do so to establish the trait's continuity, if its continuity is its similarities.

Presuming that we do intend 'continuity' to specify trait relationships with respect to their evolution, this means that the term must be restricted at least to trait similarities that have a shared origin, so that they are related by genealogy. Still, even a shared origin is too coarse a measure for addressing another key relationship in evolution—the development of traits over time—as two traits having such a connection is coincidental to their development through descent. Thus, while there was likely one origin for bipedalism in all bipedal hominins, the evolution of the trait in the lineage that led to *Homo sapiens* was independent of its evolution in lineages that were not ancestral to our species.

As an evolutionary term, then, 'continuity' is best limited to shared traits that have shared origins and a shared line of descent. These qualifications have a very straightforward expression:

(1) *Continuity*: The inheritance of a trait in an individual or population.

When a trait is inherited, it is passed on through descent, it extends from one origin, and it resembles its antecedent, permitting some variation. All of the apparent concerns of continuity are therefore contained in this one relationship, while none can be improperly singled out. So far, this definition simply ensures that any intuitions we have about continuity are made appropriate for an evolutionary context, but there are some difficulties in applying it to the evolution of language.

Crucially, to determine the origins of language, we must determine to what extent the various traits that make up linguistic ability have a continuous inheritance, yet this is difficult to do by any direct means, as we have little understanding of their genetics, they leave no environmental imprints, and their physical bases are challenging to identify in whatever dregs of our ancestors the earth occasionally preserves (Tattersall 2014). Instead, we can try to draw inferences about trait continuities by looking for homologues in other living species, with the cross-species reach of a homologous trait giving an indication of how deep into history its continuity likely runs (i.e. as far back as the common ancestor of the most distantly related species with the trait). But while this method is simple in its conception, what homology *is* exactly has been debated and revised in recent biological theory, so we must briefly examine the homology concept itself.

In line with the traditional notion, Mayr states that " [a] feature in two or more taxa is homologous when it is derived from the same (or corresponding) feature of their common ancestor " (Mayr 1982: 45). With respect to the definition of continuity I gave in (1), the following is an equivalent restatement:

(2) *Homology*: The intersection of trait continuities.

If we know the continuities of two traits in two species, their intersection would be their point of origin in a common ancestor, while having no intersection would indicate an independent evolution. It is implicit for Mayr (and explicit for, e.g., Wake 1999) that this definition is unassailable, yet it has been challenged. As observed by Butler & Saidel (2000), the homology relation is meant to allow us to identify traits in different species as fundamentally the 'same', despite variation in form and function, but this 'sameness' cannot be established absolutely. Phenotypic traits are constituted at a minimum of genes, developmental processes, and morphology, with each level in this biological hierarchy having its own continuity, so there is an inherent ambiguity in the continuity and homology of traits.

For example, when two distantly related species have traits that are only functionally similar, with structural dissimilarities that do not share an origin, we generally categorise them as convergent. However, such traits can nonetheless share deeply conserved genetic regulation, as is the case with eye development in vertebrates and *Drosophila* flies—despite the profound differences between camera and compound eyes, as well as the structures' lack of common ancestry, they turn out to be regulated by homologous *Pax-6* genes (Callaerts et al. 1997; see Fitch 2011 for the potential relevance of this phenomenon to language evolution). Also, many anatomical structures across species that *are* clearly derived from a common ancestor may nonetheless be produced from different developmental mechanisms, owing to embryological epigenetics. This is the case with Meckel's cartilage in vertebrates, its formation being stimulated by different tissue interactions in amphibians, birds, and mammals (Hall 1989).

In more closely related species, genetic mechanisms can have an unbroken inheritance in several lineages and yet be expressed in morphology only sporadically, so that phylogenies can appear to exhibit multiple convergences that are in fact genetic homologues (e.g., the nucleus rostrolateralis cell group found in some fish brains, as discussed by Saidel & Butler 1997). Moreover, even unequivocal instances of convergence might be best understood as arising from a common inheritance of design limitations, rather than from chance similarities in responses to selective pressures, as the regularity of some analogues is too improbable to be coincidence. Wake (1991) provides an example of this involving changes to digit numbers in *Plethodontidae* salamanders.

Some biologists have tried to impose order upon this chaos of continuities by introducing more fine-grained terms that apply at different levels of organisation (e.g., Butler & Saidel 2000), while others have sought to limit the domain of homology entirely to one level, that of developmentally individualised structures (e.g., Wagner 1989, Müller & Wagner 1996). This latter, so-called 'biological' homology concept treats many ordinarily analogous traits as homologous (such as camera and compound eyes), and its advocates have criticised the traditional homology concept, which they label 'historical'. But their criticisms, while valid, have been levelled particularly at the prioritisation of *genetic* history above all else—the basic temporal relation defined by (1) and (2), which does not specify any level of organisation, remains fundamental to every definition of homology that has been proposed. Thus, we find in Wagner's 'biological' homology a criticism of Mayr's 'historical' conception and at the same time an inevitable resonance with it:

> The homology concept is an attempt to identify and name the units of phenotypic organization (i.e. *those body parts that have historical continuity and that can often be found in many species derived from a common ancestor*).
>
> (Wagner 2014: 44; emphasis added)

As in many terminological disputes, what we find advertised as a disagreement on the substance of a concept is really a disagreement on how we ought to use a label to carve up a theoretical space. At heart, attempts to constrain the homology concept to one level of organisation have been motivated by a desire to sift the ambiguous continuities of traits for those that seem somehow more *meaningful*, while an acceptance of a level-free homology concept is open to all kinds of observations that are simply more or less *informative* (often hardly at all). As Wagner points out (2014: 43f.), it would be fatuous for us to consider the continuity of a particular red blood cell, as opposed to the system that generates red blood cells, but it is in the end a matter for terminological hygiene (not biological theory) whether we rework the homology concept so that fatuous applications of it are inexpressible, when we could just as well choose to not apply it to fatuous cases.

The original, quick and dirty definition of homology therefore suffices, so long as we are careful to refer to a level of organisation when using it (Bolker & Raff 1996). If we are interested in the wings of bats and birds, for example, we must identify them as homologous forelimbs, but not homologous wings, as the common ancestor of bats and birds had forelimbs that were not wings (Dickinson 1995). Likewise, although we might expect to find conserved genetic mechanisms for their development, these would be homologues of forelimb (not wing) construction. As I detail further in section 2.3, continuity and homology are always partial in this way, so we must be sensitive to how traits integrate various interacting and non-interacting levels.

To apply these refined notions of continuity and homology to language, we must first observe a critical consequence of defining continuity as trait inheritance (and therefore discontinuity as the lack of it): *All* trait evolution is predicated on *both* continuity and discontinuity, and so all theories of trait evolution must likewise deal in both. A trait's discontinuity is simply its origin or end, at which time it must be created or eliminated rather than inherited, while its continuity is its stasis or change through inheritance. Of course, few traits originate in a single transition between generations or species, so we must not think of discontinuities as clean breaks in evolutionary history. Nonetheless, all traits originate somehow and we can call these origins 'discontinuities', whatever the details turn out to be. But how does this definition, which brings continuity and discontinuity together in all trait histories, relate to the debate on language evolution, which treats continuity and discontinuity as opposites? For this, we need to be certain what we mean by 'language'.

The target that we all share is the species' language faculty: A biological property comprising all the anatomical and cognitive features that make it possible for us to acquire and use languages, which are themselves individual, mind-internal systems (‘I-languages’, after Chomsky 1986) that provide some means of mapping between sensorimotor expressions and conceptual content (i.e. sound and meaning). In this context, I will use ‘ideas’ to refer exclusively to the kinds of thoughts that I-languages access, and ‘utterances’ to refer to any, even non-linguistic sensorimotor expressions.

In principle, utterances and ideas need not be related at all. In humans, emotive vocalisations, like laughter and crying, are utterances that do not stand for any conceptual content, instead having only pragmatic interpretations, and it is clear from our own internal lives that ideas can form and go unuttered (often for the better). The same is true of other species—vocal productions in birds vary acoustically with no matching variation in meaning, such as there is any (Catchpole & Slater 2008), and at least socially intelligent species seem capable of complex thought without having a capacity to express it (Cheney & Seyfarth 2008).

Whether there are any nonhuman thoughts that are the same in kind as the ideas accessed by human I-languages is a more difficult issue that I return to in section 3.2. Yet, regardless of whether language introduced a new kind of thought, it is clear that the ubiquitous biological systems that are used to utter and to think exist independently of language and are insufficient for it, so an organism can only have language if it has mechanisms for mapping between these systems. This amounts to a species-level equivalent of Chomsky’s (1965) distinction between competence and performance: To have a language faculty, a species must have competence mechanisms for doing the mapping between utterances and ideas, and performance systems for putting these mappings to use in actual utterances and ideas. As the fundamental property of language is the utterance-idea mapping, no matter what substance is mapped in particular individuals, I will refer to these competence mechanisms as the mapping mechanisms.

This distinction between the performance systems and their mapping gives theories of language evolution *two* histories to reconstruct. One is the history of how the language faculty as a whole came to be as it is, with its particular integration of all the traits that underlie it; the other is of how a language faculty became possible at all, through the first emergence of a mapping between utterances and ideas. There was no doubt co-evolution, with the performance systems adapting to make effective use of their mapping (e.g., with newly intricate control of vocal anatomy), and likely contributing to utterance structure where it is under-determined by the mapping mechanisms (e.g., Holmberg 2010). All the same, these are nuances rather than counter-examples to the simple observation that language consists of things that are mapped to each other and things that do the mapping.

With these qualified ideas of continuity, homology, mapping, and performance, we can try to determine the continuity of the language faculty by searching for homologues of the human performance systems and the mapping between them. Given that homologues must be sought at particular levels of biological organisation, we are of course constrained by having little understanding of what constitutes the language faculty at any levels besides cognition and behaviour, as its genetic and neurological underpinnings remain mostly obscure. Moreover, what we do know of language cognition is largely an inference from behaviour, which underlies almost all syntactic theory in the guise of acceptability judgements (even our own introspections are of course behavioural data, which we rightly try to explain with theories of cognition, but which do not themselves reveal our cognitive processes).

As far as nonhuman behaviours are concerned, there are no other living species that can learn and use languages, whatever talents some have to mimic fragments of them, but we can't infer any more from this than that there aren't any homologues of the entire language faculty, developed and integrated as it is in humans. Although our particular configuration of performance systems and their mapping must have originated in hominins, the date for the whole may not be the same as for its parts—it could be that the performance systems have a pre-hominin history and their mapping a hominin origin, or their mapping could be pre-hominin, with the inability of other animals to use language being due to severe performance limitations.

Setting aside polemic, both continuity and discontinuity theories recognise hominin discontinuities in performance. The most obvious example is speech, for which there are no homologues (there may be homologues of its genetic regulation, but the behaviour and much of its neuroanatomy is uniquely human; see Lieberman 2007). Recall that all trait origins are by definition discontinuities, so all that needs to be agreed upon for this is that speech arose in hominins. It should come as no surprise that we all embrace a discontinuity of *some* sort, otherwise it would be bewildering that humans have anything unique about them. However, even where there are disagreements about the inheritance of performance traits, these are unhelpful for categorising theories of language evolution more generally, as such disagreements regard the inheritance of traits that could have evolved differently while leaving our fundamental capacity for language unaffected, and it is the inheritance of that capacity that we are interested in. What matters instead, then, is what theories have to say about the homologues (and concomitant continuity) of the mapping between utterances and ideas, irrespective of the character of the performance systems involved.

Continuity theories are those that find these homologues in abundance. Corballis (2017), for example, suggests that nonhuman great apes use manual utterances to convey ideas, even though they are relatively imprecise and ambiguous. Though these homologues are rudimentary, their prevalence suggests a long, pre-hominin history of inheritance. Continuity theories do not deny that human language has remarkable properties, but all are argued to result from changes to the performance systems and their interactions with general intelligence (syntactic structure, for example, is often dealt with as an epiphenomenon due to constraints on learning and use; e.g., Hawkins 2004). It remains a truism that the utterance-idea mapping must have had *some* discontinuous origin, but continuity theories bury it deep in the histories of our species and others. Lieberman 2002 and Tomasello 2008 are other examples of continuity theories.

Discontinuity theories, meanwhile, go looking for nonhuman utterance-idea mappings and come back empty-handed. All behaviours that have been candidates are viewed as unconvincing abilities of some other sort, making it necessary to conclude that mapping had a hominin origin. There is still a place for continuity in these theories, as the many parts of the language faculty that are not unique to our species must have pre-hominin histories of inheritance, and even the parts that *are* unique have of course been continuous for the short time that they have been around. There is almost as much variation among discontinuity theories as there is among theories of language cognition generally—Bickerton (2014), Bouchard (2013), and Jackendoff (2002) all argue for discontinuities of very different kinds.

In terms of continuity, then, what distinguishes theories of language evolution is the time that they propose for the origin of the human capacity to map between utterances and ideas, as determined by searching for nonhuman homologues. Either it originated in some pre-hominin ancestor, leaving many traces in many species, or it originated in hominins, leaving no traces in any species but our own (and possibly other extinct hominins). It has undoubtedly been misleading to use 'continuity' and 'discontinuity' as labels for this distinction, as it does not regard *whether* there was discontinuity, but rather *when* there was discontinuity of the relevant kind. It is only because these theories have such radically different answers, correlating with categorical claims about the existence or not of homologues, that these labels have stuck as shorthand for what is really a division over pre- versus post-hominin discontinuity.

Lenneberg (1969: 642) rightly warned that it would be futile to ever speculate on the evolutionary circumstances that gifted ancient hominins with language. The purpose of biolinguistics, informed though it must be by evolutionary theory, is not to tell stories about our ancestors, but rather to tell stories about ourselves, by giving answers to the sorts of straightforward engineering questions posed by Searle:

> Subtract language from a species like us: What do you have? Now add language: What are you adding? (Searle 2009: 178)

Nonetheless, it is worth noting that the need for continuity and discontinuity in all traits makes it meaningless to say that some traits evolve 'continuously' and others 'discontinuously'. These are not terms that apply to evolutionary processes. It is therefore wrong to think that continuity theories are gradualist theories, and discontinuity theories saltational. Although these pairings have dominated mainstream debate, the time of a trait's origin is nonetheless independent of the mechanisms of its evolution. There is certainly a connection between the amount of time a theory allows for the evolution of a trait, the size of its phenotypic difference to be evolved, and the mechanisms that might reasonably bring about that difference in that time, but these are only implications to be explored. As such, many anti-generative theories, though gradualist, are also discontinuity theories (e.g., Deacon 1997), and there are now some saltational generative theories that are continuity theories (see section 2.2).

Thus, if we are led to a discontinuity theory when answering Lenneberg's question, "how obvious are the antecedents of the human propensity for language?", we are not automatically committed to the belief that a gradual evolution of language was impossible, or even unlikely. If we find that there are no obvious homologues of linguistic ability, this *could* suggest a sudden emergence of language's entire complexity, but it might turn out that language has antecedents that are simply *not* obvious, and which required only small discontinuities in hominins for the language faculty as we have it to come into being. If so, its relatively recent origin would be compatible with gradual evolutionary change. Before considering that possibility, in the next section I consider the natures of linguistic and non-linguistic communication to demonstrate the basic implausibility of continuity theories.

2.2. *A Theory of Communication*

2.2.1. *Giving and Taking Meaning*

The major claim of continuity theories is that the human capacity to map between utterances and ideas has homologues in other living species, so it has a continuous inheritance extending further back than the first hominins (from here, I will frame the debate in terms of whether these homologues exist, but readers should recall from section 2.1 that this is a shorthand for whether the mapping is pre- or post-hominin). As the utterance-idea mapping is the fundamental property of language, continuity theories of course deny what I took to be a basic fact in my introduction, which is that language is biologically unprecedented. For these theories, human language really is just language as *humans* have it; it is only a hypertrophied form of widespread communicative behaviours that are simpler, but which have essentially the same mental basis.

One way to challenge continuity theories is to argue that the human utterance-idea mapping must be carried out by particular mechanisms, and that these mechanisms appear to be absent in other species. Generative theories in the Chomskyan tradition typically do this by arguing that our performance systems are mapped by a generative module that builds syntactic structures which each system must interpret, so an absence of syntactic structure in utterances means they mustn't be generated by the kind of module that is necessary for utterances to map to ideas. There is now a rich literature addressing the possibility for syntax in nonhumans, but most studies have been undermined by shortcomings in experimental design (Watumull et al. 2014), misinterpretations of data (ten Cate & Okanoya 2012), and a lack of clarity on what we should even be looking for (Rogers & Pullum 2011). They are further vexed by the systematic ambiguity with which 'syntax' is used to refer both to *any* regularity in utterance sequencing (whatever its computational complexity) and *only* regularities that match the particular complexity of human morpheme sequences. Nonetheless, while some nonhuman utterances (especially bird calls) do seem to be generated by cognitive rule-systems, their syntax is never more complex than that of human phonology, which has no mapping to ideas (Berwick et al. 2011).

Of course, it only follows *directly* from this that there is no nonhuman mapping between utterances and ideas if that mapping demands a syntax at least as complex as the one that structures human morphemes. Certainly, a mapping like ours, with hierarchical concept composition, requires a suitably powerful syntax, but it is at least conceivable that a much simpler generative module could map between non-compositional utterances and non-composed concepts. If so, hominins might have evolved only the syntactic complexity of language through some new computational apparatus, rather than the utterance-idea mapping itself, and we would require a continuity theory of language evolution. This is a latent possibility in Minimalist theories like Hornstein 2009 and Boeckx 2015, where the emergence of language is ascribed to computational discontinuities that are compatible with (though do not mandate) a continuity in mapping mechanisms (this is unlike Chomsky's theory, where a computational discontinuity *is* the mapping mechanism; see section 3.1).

Although these theories have much to recommend them, in what follows I seek to establish that such syntactic discontinuities are unnecessary and burdensome to theories of language evolution, provided that a non-syntactic discontinuity is inescapable and is alone sufficient to account for human uniqueness. All the same, readers should take a moment to appreciate the significance of these theories having been developed at all. Generative linguists have ordinarily taken the evolutionary discontinuity of language for granted, owing to the richness of language-specific primitives that have seemed to be innate and which are transparently lacking in nonhuman communication systems (such as the principles relating to case, binding, argument structure, and so on). Now, however, some linguists have pursued the Minimalist reduction of language-specific primitives to such extremes that they flirt with the possibility of continuity, albeit a new kind, with continuity in the utterance-idea mapping and rapid discontinuity in its structural complexity. Proponents of this continuity Minimalism might object to it being considered a continuity theory at all, as a discontinuity in the structure of utterances and thoughts would make human language clearly distinct from all other animal communication systems. However, we have already seen that everyone acknowledges at least one discontinuity in the performance systems, so our strength of feeling about some specific discontinuity is irrelevant to its place in the overall debate about the origins of language. We must always maintain a distinction between the history of the particularities of *our* language faculty, and the history of the defining property of *any* language faculty, which is the utterance-idea mapping—any theory that couples a continuity in that mapping with a discontinuity in its structure (or in anything else) can only be identified as a continuity theory, though its proponents may set themselves apart in other ways from gradualists.

Altogether, this new theoretical landscape makes arguing for discontinuity much less trivial than it has often appeared, with many more consequences for linguistic theory. For now, I will delay further discussion of what constitutes the utterance-idea mapping in humans, as it will be more fatal to continuity theories to reject them without having any particular theory of our human faculty in mind. Instead, another means of challenging continuity theories—this one taken up by Lenneberg—is to show that *any* discontinuity theory is preferable because all continuity theories are inherently implausible. This would be the case if either: (i) none of the nonhuman utterance-idea mappings that are said to exist are of a type that the human mapping could have evolved from, or (ii) there are no nonhuman mappings at all. Justifying this second, stronger claim will be the focus for the rest of this section (Lenneberg's approach is quite different to the one I develop here, though I consider the importance of that difference in section 2.3).

It is vital to recognise what identifying utterance-idea mappings in nonhuman species demands. It is not enough to notice that an organism utters and thinks, or that it utters when it thinks, or even that it reliably utters one thing when it has a particular thought, as the mapping of utterances and ideas is more than their mere coincidence. If I see a leopard and yell in fear, my yell will be *related* to ideas, in that it will occur alongside my perception of danger and will allow others to infer that a danger exists, but my yell will have no mapping to ideas, in that it would not *represent* any of the ideas that it is related to. It might, for example, lead others to think that there is a predator on their trail, that there is a person in need of help, or that someone in a north-westerly direction is making an awful racket, but it would not stand for any of these thoughts, or any of the concepts that they contain. A yell communicates simply *that* someone has yelled, with the association of yells and heightened emotional states allowing a certain variety of useful inferences about the yell's possible causes.

This is totally unlike genuine utterance-idea mappings, such as the word 'leopard' representing the concept LEOPARD in the expression of ideas that contain that concept. Of course, the relation between words and concepts is not straightforward. It has been clear since Grice 1957, 1969 that the interpretation of linguistic utterances is just as context-dependent as the interpretation of yells, as words do not denote concepts in a one-to-one correspondence. Moreover, some have argued that this is not just pervasive ambiguity, but rather that all words (as types) intrinsically under-specify the ideas that they can refer to (as tokens) in contexts of use (there is an interesting convergence on this point in both pragmatic and syntactic theories; see Recanati 2004 on the former and Borer 2013 on the latter). However, the fundamental difference between something like a yell and something like the word 'leopard' is that, *in* context, only word-like things stand for concepts.

It might therefore seem that we ought to search for nonhuman utterance-idea mappings by searching for nonhuman words, but this is ill-advised (if it is not simply tautology), as many of the properties that human words have in addition to their idea-mapping are not necessary for that mapping. For example, I have already dismissed syntactic and semantic compositionality, just in case nonhuman species have an unstructured, non-compositional mapping. We also cannot presume that meaningful units (i.e. morphemes) will decompose into strings of meaningless ones (i.e. phonemes), as this 'duality of patterning' may be necessary for large vocabularies of mappings (Nowak & Krakauer 1999), but it is not necessary for small ones (Sandler et al. 2011 argue that Al-Sayyid Bedouin Sign Language has no phonological patterning). Moreover, there are phonologically complex units, like 'hello' and 'abracadabra', that are not syntactically compositional and do not map to non-composed concepts, but which we nonetheless think of as words (notice that human language seems to lack syntactically non-compositional words that *do* map to concepts—I think this tells against the very possibility of it even in other species, but I leave it as an open question).

As such, we can *only* consider whether utterances have a mapping to ideas, ignoring all other incidental properties, but we face a difficulty here, as this is a mind-internal property that we cannot observe. While we can probe the invisible boundaries of our own linguistic capacity using circuitous introspective methods, we obviously cannot do the same with nonhuman species. Thus, without access to nonhuman minds, we are limited to a study of nonhuman behaviours, and so must examine their utterances for literally superficial signs of an underlying mapping to ideas. Yet, what visible features of utterances could possibly tell us that they have this invisible mental property? The classic inventory of utterance features is Hockett 1960, and though none of Hockett's features (as he defines them) is guaranteed of all and only utterances with a mapping to ideas, *en route* to such a feature, it is crucial to understand why it cannot be his most likely candidate: 'Semanticity'.

There are really two ways that people think about 'semantics', only one of which is sensible, though both are unhelpful to the task we are embarked upon. Hockett defines 'semanticity' in terms of "relatively fixed associations between elements in messages (e.g., words) and recurrent features or situations in the world around us." This is an unabashed behaviourist definition, which may be attractive for locating semantics in things we can observe, but, like classic behaviourism, these observables give no indication whatsoever of mind-internal properties. As Hockett notes, it is semantic by this standard for gibbons to produce alarm calls in alarming situations, though he stops short of recognising that such calls would *still* be semantic by this standard even if produced with no conscious control, and with no representation of conceptual content, just like my yell at the sight of a leopard. All that this semantics requires is a reliable association between an utterance (e.g., a yell) and its contexts of production (e.g., nearby predators), so that hearers can respond automatically (e.g., by running away), *just as though* they can recognise and react to a concept-representing statement like, "there's a predator!"

Hockett's kind of semantics was rebirthed as 'functional reference' by Marler et al. (1992), with their emphasis on functionalism suggesting that the term should be agnostic about mind-internal properties. So it should be, but it's important to see that this is not because our knowledge of those properties is limited (though it is), but because functional reference is just irrelevant to them. Primate vocalisations have always been the troublesome case, as primates' use of the vocal-auditory channel has a seductive familiarity that tempts us to treat their utterances as word-like, when we might treat them as yell-like if they were produced by humans (the classic example being vervet monkey alarm calls, which vary with particular predators, *just as though* they have words for each of them; Seyfarth et al. 1980). Luckily, we needn't take sides on this issue yet, as all we need to notice is that functional reference *makes no distinction* between words and yells, whatever alarm calls happen to be more like—all it cares about is whether there is an observable association between an utterance and some salient element in its contexts of production, which is true of many kinds of utterances that nobody even suspects to have a mapping to ideas.

If we turn away from primate vocalisations, we find exactly this kind of meaningful association in avian courtship dances, where individuals interact with visual scenes, rather than acoustic ones, to make the same sorts of contextually relevant inferences. Of course, there is no suggestion that a dance might refer to a concept in the dancer's mind, so functional reference has no grip on the issue (see Wheeler & Fischer 2012 for a detailed discussion of how permissive 'functional reference' is). In fact, all of this can be easily extended to non-biological phenomena, where there are no minds producing utterances at all, let alone ones that might possibly be producing utterances to represent concepts. Smoke clouds can be said to have Hockett's kind of semantics, as their fixed association with fire can be learned by an organism, so that it can infer from any instance of smoke clouds that there must be a fire somewhere. While the notion of functional reference might seem to exclude these mindless cases, it does so only by stipulating an interest in evolved rather than non-evolved associations and responses—it would otherwise construe meaning identically in each scenario, and thus contributes nothing to our understanding of meaning's mental character.

Having said all this, it is difficult to finally dispatch behaviourist semantics and its reincarnations, as it remains trivially true that a word can only represent a concept by somehow coming into an association with it. Quine (1960) showed that words having associations with observable features in the world is by itself hopelessly insufficient for determining a word's meaning (see also Gleitman 1990 for a syntactic perspective), but it is at least a precondition for word meaning that a word's use should have some contiguity with the concept that it is meant to represent (i.e. for 'leopard' to come to mean LEOPARD, it is necessary, though insufficient, for 'leopard' to be used *alongside* thoughts of LEOPARD). As such, associations must have some role in a theory of semantics, and therefore in a theory of language origins, but only insofar as our interactions with smoke clouds have a role. What is at issue in all this is only the capacity that some animals have to perceive things as meaningful, rather than any capacity they may have for language in particular, which is an entirely different, derived phenomenon.

To put this more concretely, consider the extremes of a smoke cloud compared to some uttering of "there's a fire." In the right circumstances, both events could be *taken* to mean THERE IS A FIRE (i.e. an observer could have that thought as a consequence of observing the event), but only the linguistic utterance would *give* the meaning that is taken from it—the smoke cloud is not produced in order to specify the thought that the observer should have. What Hockett's semantics does is identify those associations that have a potential to be taken to mean something, and functional reference identifies those evolved behaviours that are in fact taken to mean something, but neither notion addresses whether these meanings are taken purely through observer inference, or through the meaning being given by what the observer observes. Moreover, note that if a meaning is always something like an idea in the mind of an observer (we will explore other possibilities, but the lesson here won't change), then 'given meaning' is simply a reformulation of the utterance-idea mapping, and as this is the defining property of language, language is thus beyond Hockett's semantics and functional reference. These ideas are not inadequate or wrong in theories of the nature and origins of language, they just have nothing to say about language at all, despite their use of linguists' jargon.

At this point, we can settle upon a conception of 'semantics' that is more sensible. Presuming that we do not want to say that smoke clouds have semantics, as this would empty the term of its purpose in a characterisation of language, we must concede that having semantics is not the same thing as having meaning, as smoke clouds can be meaningful. Semantics must therefore constitute some particular, limited contribution to meaning, in which case it could either be observers' inferences, which determine the meanings that are not given by an observed event, or it could be mappings to ideas, where the meanings are given. Of course, linguistic theory already has well-worn terms for these things: Observers' inferences are the subject of pragmatics, and so semantics simply *is* the utterance-idea mapping (that 'semantics' is therefore just another label for what we are trying to identify is why this more sensible conception is also of no help).

It follows from this that the utterances that animals take to be meaningful without there being any mapping to ideas ought to be classified as having pragmatic meaning (and no semantics). That 'pragmatics' is not commonly used this way is no doubt due to the false equivalence typically drawn between semantics and meaning, as well as the to reputation that 'pragmatics' has for referring to somehow peripheral, ineffable, and even socially complex aspects of meaning determination, rather than to the simple process of inference from observation. However, as I will argue presently, post-Gricean, cognitive pragmatic frameworks (like Relevance Theory after Sperber & Wilson 1986) provide a robust basis, even if not fully realised, for understanding pragmatics as inferential meaning *wherever* we find it in the natural world, even in socially unintelligent and non-conscious interactions. It's worth bearing in mind here that, because linguistic utterances depend (like non-linguistic ones) on pragmatics for their meaning, the 'utterance-idea mapping' is specifically the non-pragmatic element of linguistic meaning, as it is this alone that distinguishes it from non-linguistic meaning (Carston 2002 has an insightful discussion of these distinctions, though the finer details will not bother us here).

2.2.2. Communication Systems

There remains one other possibility for continued confusion that leads us now to undertake a more formal analysis of the distinctive properties of language. While we might be willing to accept that there can be meaning without semantics, and therefore that wholly pragmatic meanings in animal interactions are no more informative about the origins of language than wholly pragmatic meanings derived from smoke clouds and other such cues, it nonetheless seems that there is a boundary being crossed here: Even the most mindless animal interactions can be regarded as *communication*. Naturally, the notion of communication, and its intimations of information transfer, suggests something like the giving of meaning, even though there is no question that most communicative behaviours do not have semantics as I have described it. What we need in order to navigate the difficulties raised by these intuitions is a principled description of the necessary properties of animal communication, which can allow us to appreciate where the utterance-idea mapping fits in a grander evolutionary scheme.

Note that an essential premise of the description I will give is that there is no such thing as metaphorical communication—if we feel that we are licensed to describe something as communicative, that is because it is, even if we are describing a non-conscious interaction, like a poison dart frog dissuading a predator from eating it by having a vivid yellow colour that marks it as poisonous. For the moment, I will focus particularly on what unifies non-linguistic communication, and I will afterwards consider what must be added to form a minimal linguistic system.

Let's begin by focusing on the simplest case of communication, where there is one signaller and one receiver. Every instance of communication involves some thing, which we'll call the *object*, that the signaller wants the receiver to understand (I will use such mentalistic language as 'wanting' and 'understanding' for exposition, and will encompass non-conscious communication later, so the exposition should not be read as claiming a psychological reality for beliefs and desires in nonhuman communication). There are two important limitations on objects in communicative acts: An object must be something that the signaller and receiver *can* perceive, and something that the signaller *does* perceive. To put this impressionistically, a signaller can only communicate about something that both parties can think about, and the signaller must be in some sense thinking about that thing to communicate about it. Of course, by 'perceive', I mean something much broader than sensory contact with an external reality, as humans are exquisitely capable of talking about fictions, like fairies at the bottom of the garden, as well as about real things that are abstract or not present to the senses. Instead, I take something to be perceptible so long as it can be thought about at all, and so something *is* perceived when it is thought about, in which case, it is a *percept* (I deliberately avoid 'conceive' for this, so that we can later have a different understanding of 'concepts').

We can be more precise about this if we consider the property of intentionality. For now, I don't mean the term as it has generally been used in previous discussions of nonhuman communication, as there it often has only a common sense meaning (i.e. that of volition and desire), or it refers only to a capacity to represent others' beliefs, following Premack & Woodruff (1978) and Dennett (1983). This latter sense preserves more of the philosophical meaning, but not quite enough of it. Instead, we must begin with intentionality as it was revived by Brentano (1874), and as explored by Searle (1983) and especially Dennett (1987). Quite simply, intentionality is the property of mental states and events that are *about* something, or directed towards something. Belief and desire are certainly the classic examples: If I desire an apple pie, my desire is an intentional state in being directed at the pie. If I believe that the pie is too hot, my belief is intentional in being about that particular proposition. However, these states do not exhaust the possibilities—if I am scared of clowns, my fear is an intentional state about clowns; if I am irritated by a fly, well, you see how the story would go. With this in mind, to say that the object in a communicative act must be a percept (i.e. that it must be something thought about) is to say that it must be something that an intentional state is directed towards. This is satisfactory for now, but we will later on break down this equivalence between thinking-about and intentionality, and it is vital that we understand percepts exclusively in terms of intentionality.

So far, so much stipulation, but why should any of this be true? It's important to appreciate that the physical reality that is external to an organism never enters directly into communication any more than apple sauce gets smeared across my brain when I think about it. However accurately or inaccurately our thoughts represent the world, all that we know of the world is what we perceive of it through the prism of our biology, and everything that we can communicate about is derivative of this mental organisation of experience. Thus, when we communicate about apple sauce, we really communicate about our mental representations of it, rather than about the sticky stuff itself, and any such mental representation is by definition a percept when it is thought about. More generally, whenever we talk about having any intentional disposition towards something, the disposition must be mediated by a mental representation of the physical thing itself. As such, it is always that mental representation that is the percept, and this is why communication must always be about things as we think of them, rather than about things as they actually are (here lie the beginnings of a criticism of referential theories of meaning, which, like Chomsky 2000, I reject).

For exactly the same reasons that a signaller's object must be a mind-internal percept, so a receiver's understanding of the object must also be a mind-internal percept (a receiver is also a biological entity that communicates in mental representations). Of course, these percepts are not publicly accessible—even when we are in some sense thinking the same thing, we do not literally have the same thought, as thoughts are fundamentally physical properties of individual brains—so the percept that is the signaller's object cannot be the same percept as the receiver's understanding, which must instead have an independent existence in the receiver's mind.

This second percept introduces a second intentionality into the communicative dynamic, and we shall call it the *interpretant* in imitation of Charles Sanders Peirce's philosophy, which has many important points of contact with this discussion that I won't develop explicitly (see Short 2007 for an introduction to Peirce's work). Note that communication is not perfect, and there are always two perspectives on interpretants: That of the signaller's intended meaning, and that of the receiver's actually taken meaning (e.g., I might point to something on the windowsill, and you might look through the window). As we are primarily interested in the mental basis and content of utterances, the utterer's desires are paramount here, so for us the 'interpretant' is the percept in the receiver as the signaller wishes it to be.

So far, we have a signaller and a receiver who are synchronous in perceptual abilities, and we have labels for the percepts that they have in a communicative interaction. For example, if I want to communicate with you about some book, we had both better be able to think about it, and, if we can, my thought of it will be the object, and the thought that I want you to have of it, the interpretant. However, we have not yet equipped signaller and receiver with a communicative competence, so that an object can in fact be related to an interpretant. For this, the signaller requires some kind of *sign*, such as an utterance, that can direct the receiver to have the interpretant that the signaller desires, as would be the case if I directed you to share my percept of some book by pointing at the book that my percept represents. Here, the use of a sign embodies a third intentionality, involving the desire that the signaller has towards provoking some particular interpretant. Now, if we have an intentionality, we must also have the percept that it is directed towards, but we must be careful here, as this percept is not quite the interpretant that the signaller desires. I could want you to notice a stain on the carpet, perhaps, and yet do absolutely nothing about that desire, in which case it would have no place in a story about communication. Instead, if I decide to use a sign, it is because I know the sign is a means of bringing about a response, so it is the whole complex of the sign *and* its interpretant that is the percept of this desire.

Signs do not have their meaning intrinsically, so they must have a *ground* that gives them some connection to their object. The grounds that are traditionally recognised are iconicity (resemblance of sign and object), indexicality (existential connection), and symbolism (arbitrary connection), with the three often mixed in one sign (Short 2007). The details of this taxonomy are unimportant here, except for the fact that all grounds acquire an additional property in communicative interactions that it is worth keeping in mind. A non-communicative sign (e.g., a rash) might be taken to represent its object (e.g., an illness) by being indexically linked to it, such that the connection will hold independently of any specific instance of the sign (rashes just signify illness generally by brute association, wherever we find them). A communicative sign, however, must always be grounded with some element of *self-reference*, in that its object is inevitably dependent upon a specific token of the sign. With pointing, for example, though we all have a general plan for how to interpret the gesture when we encounter it, it has no pre-determined object independent of its particular uses (there are as many objects as there are things you can point at). This is true even for symbols, which need not be linguistic—culturally variable hand gestures for approval, for example, do not signify approval abstractly, but rather approval as an attitude of the signaller (note that rashes do not signify illness as a property of their signaller, as there *isn't* one—the rash is an effect of the illness, not produced by the person who is ill). The reason that this self-reference is necessary is simply that signs that are produced by signallers always have mind-internal objects, and so they are always used to represent properties of their users.

With the object, interpretant, sign, and ground, we have enough in place for many varieties of meaning, but not quite enough for communicated meaning. There are many interactions in which an object and an interpretant are brought into a relation by a sign, but which are *not* communicative. For example, I might have a piece of music in mind that I want you to think about, and I could achieve this by inconspicuously whistling its melodies while you're nearby, without you realising when you think of the piece that I intended you to think of it. Although you thus perceive the object and I get my desired interpretant by whistling, the sign has not been *mutually recognised* for what it is. Similarly, I might go about whistling with no intentions at all, and you might notice the whistling and interpret it as a sign of my good mood, though I had no desire to communicate that state of mind (although I could do just that if I whistled ostentatiously while kicking my heels). Thus, in order for the use of a sign to constitute a communicative act, there must be mutual recognition that the sign is intended to bring an object and an interpretant into a relation (I will comment on the obvious connection this has with Gricean pragmatics momentarily).

Here, we add yet further degrees of intentionality that we must be careful with. First, recognition is itself an intentional state, directed at whatever it is that is recognised. In this case, it is an intentional state in the receiver, directed at the signaller's *use* of the sign (i.e. it is recognition not only of what the sign means, but of the sign being meant to mean what it means). Second, to ensure mutuality, the signaller must have a desire for the receiver to recognise their sign as having communicative purpose, and the percept here is thus the complex of the sign and its recognition. As an example, if I point at something, in addition to noticing what I am pointing at, you must recognise that I am in fact pointing, rather than merely stretching out my arm, and in performing the action, I must want you to have that recognition, as it aids the interpretant I intend.

In the description up to this point, we have encountered all of the elements that we need. To recap, we have five intentional states, each directed at one or more of the object, the interpretant, and the sign (and it is the relations among these that collectively determine the sign's ground). Of the intentional states, two are simply the perceptions of the object and interpretant, respectively; the third is a signaller's desire directed at use of a sign to bring about its interpretant; the fourth is a receiver's recognition of this desire for the sign and its interpretant; and the fifth is the signaller's desire for this purpose in the sign to be so recognised. There are many interdependent elements to juggle in this account, and they are easily confused, but all can be arranged into a more palatable definition:

(3) *Communication*: Use of a sign to produce an interpretant through mutual recognition of the sign as grounded to an object.

Human non-linguistic communication lends itself most readily to description in these terms. For example, if I communicate to you that someone else's cat is crouching in the garden again by looking wide-eyed from you to the patio door, a fuller description we could give of this is that I use a facial gesture to produce (in you) a percept of the cat through mutual recognition of the gesture as standing (indexically) for (my) percept of the cat. My claim, however, is that this definition, with one important adjustment, can extend to all possible varieties of communication in the natural world, including those where all of the elements in the communicative act are non-conscious.

Returning to an earlier example, we essentially have to ask what it could mean for a poison dart frog to have communicative desires that it fulfils by signalling a property of itself. Although intentionality was originally believed by Brentano to be the hallmark of mental phenomena, it is not difficult to extend it to non-mental phenomena if we adopt the intentional stance advocated by Dennett (1971). We already do exactly this when we think, say, that a frog *wants* to make its poisonousness known in order to be avoided, and there are reasons why this doesn't seem so ridiculous, even though we don't believe that frogs have desires in the sense that humans do. To adopt the intentional stance towards a system (such as an organism, or even a computer) is to describe it as having beliefs and desires, with a view to predicting its behaviour. Thus, when we say that a frog *wants* to make its poisonousness known, we can predict the development of a sign to represent that property as a fulfilment of its desire.

Unsurprisingly, there has been considerable controversy about the adequacy of using these anthropomorphic descriptions for non-conscious systems, as well as about whether human beliefs and desires might in fact reduce entirely to non-conscious equivalents (see Dennett 1987 and Ross et al. 2000 for more discussion). However, the purpose of the intentional *stance* is not to make claims about the reality of beliefs and desires as psychological states, but rather to predict behaviours through the idiomatic ascription of such states, when this may be the best (and even only) means of understanding behaviour. In this context, we can adopt the stance just instrumentally, without concern for its metaphysics, and we need only think about organic systems.

The critical point is that it is in the nature of evolution by natural selection to produce organisms that can *all* be characterised as intentional systems. It is fundamental to the selective process that it favours those organisms that survive and reproduce, and it favours those that do so most effectively (note that the ubiquitous talk of natural selection as 'favouring' anything at all treats it as an intentional system), so all organisms that exist can be fruitfully described as having a *desire* to survive and reproduce, as well as *beliefs* about how best to go about it. Thus, it is not so startling to say that a poison dart frog *wants* to communicate its poisonousness because it *believes* that doing so will keep predators away, as this is just one way of employing certain vocabulary to talk about targets of natural selection. Of course, we still have all our work cut out for us in ensuring that the traits we talk about *are* so targeted, but, if they are, the intentional stance applies automatically as a consequence. One way of viewing this is to say that any attribute an organism has that it would want to have in aid of its fitness, if it could think about it that way, should be regarded as a percept of its survival intentions. Perhaps a less awkward way of thinking about this is simply to say that any adapted property of a species is a percept of selective intentions, and it is thus natural selection, rather than the frog, that perceives the benefits of the frog being yellow.

With this expanded notion of intentionality, we can understand percepts either as things thought about, or as targets of natural selection. This imposes some important limits on what might otherwise seem to be an unconstrained metaphor, in that our definition of communication requires its elements to be percepts, so if a property of an organism is *neither* a thought or an adaptation, then we should expect that it cannot enter into communicative acts. This moreover means that nonhuman communication cannot be distinguished from human language by thinking of the former as being externally referential, as the only things that nonhumans can communicate are things that are relevant to their fitness and are thus percepts of natural selection—there cannot be referential meaning, human or otherwise. Bearing all this in mind, we need not alter the definition of communication in (3) to have it apply to non-conscious varieties—all we have to do is be clear that the five intentionalities could be either psychological or selective. Compare the following descriptions of communicative interactions, for example:

(4) a. Use of a facial gesture to produce a mental percept of a cat through mutual mental recognition of the gesture as grounded to a mental percept of the cat.

b. Selection of yellowness to produce selection of predator avoidance through mutual selected recognition of the yellowness as grounded to a selected property of poisonousness.

The parallelism is exact even with regards to mutual recognition, as we would discount as non-communicative any interactions where a trait has a signification that evolution did not intend it to have (e.g., when the size of an animal is taken as a deterrent, though that is not its evolved purpose). In the intentional idiom, a frog's colour is rightly regarded as communicative because it is *meant* to be recognised as representing its poisonousness, and it *is* recognised as such, as evidenced by the avoidance response.

Before seeing where this leaves us with language, we should appreciate quite how general this account of communication is, given my claim that it can apply to any communicative system. There is in fact no element of this description that is novel, though it brings together several different perspectives. The triadic conception of the sign, object, and interpretant is of course Peirce's (though Peirce would have rejected such thorough mind-internalism), but, though the triad is excellent for capturing taken meaning generally, it is only by making all three elements intentional percepts that we limn communicated meaning especially. Of course, my particular use of intentionality is Dennett's, whose (1983) discussion of nonhuman intentionality was heavily influenced by Grice, though he construed Grice's approach more in terms of theory of mind than in terms of necessary elements in a communicative dynamic. More important than Grice's approach, however, is its development into the definition of ostensive-inferential communication in Sperber & Wilson 1986:

(5) *Ostensive-Inferential Communication*: The communicator produces a stimulus which makes it mutually manifest to communicator and audience that the communicator intends, by means of this stimulus, to make manifest or more manifest to the audience a set of assumptions **I**.

It is by deliberate design that every aspect of this definition has a correspondent in the definition of communication I gave, with the exception of the ground, which is an important but often overlooked interaction of the other elements. In translation, the stimulus is the sign, the set of assumptions is the object, their desired manifestness is the interpretant, and that manifestness is mutual. The definition also has a correlate of the signaller's desire towards the sign and interpretant complex (Sperber & Wilson call this the 'informative intent'), as well as to the signaller's desire towards the sign and its recognition (what they call the 'communicative intent'). The five intentionalities are all here, categorised in the same way, with no more and no less. The only limitation of this definition of ostensive-inferential communication is that it treats human interactions as possibly a special case, dependent upon the subtleties of our social intelligence. Not so—this simply *is* communication as it *must* be, and its ostensive-inferential character is simply the result of the informative and communicative intents being realised mentally, rather than selectively. Compare this with a completely different definition of communication, that of animal signalling given by Maynard Smith & Harper (2003):

(6) *Signal*: Any act or structure which alters the behaviour of other organisms, which evolved because of that effect, and which is effective because the receiver's response has also evolved.

Note first that the inclusion of 'structure' here allows physical traits, like skin colour, to be signals, which is what we allow by making it possible for signs to be percepts of natural selection. In this definition, we find that the sign corresponds to the act or structure, the interpretant to the altered behaviour, and the mutual recognition of the sign to the necessary co-evolution of signal and response. There are things missing in this definition, but they are all implicitly necessary—a signal can only have some effect on another organism if it has an object (or it would be meaningless), which it is grounded to (this relation being what underlies the well-studied problem of honest signalling; see Higham 2014 for a recent overview). Maynard Smith & Harper also circumscribe the intentionalities for an interpretant and for recognition by contrasting signals with other kinds of interactions: 'Cues' are those where the trait has a signification it has not evolved to have (i.e. there is no intention for the interpretant), and 'coercion' is where a response is forced without communicative engagement (i.e. there is no intention for recognition).

These approaches to communicated meaning appear very different on the surface as they were designed to deal with very different phenomena, but I find that they all present facets of one underlying dynamic that could be no other way. Once we properly identify the five intentionalities of communication and the various interactions of their percepts, we can abstract from their particular psychological or selective character in particular acts and species, and thereby account for every variety of communication in one like manner.

2.2.3. Language is More than Communication

Recall that, to establish the discontinuity of language, we must establish the nonexistence of the utterance-idea mapping in nonhuman species. Our account of communication now allows us to ask more pointedly how utterance-idea mappings fit in the overall scheme of utterances relating to ideas. Consider, then, the difference between our poison dart frog, and, say, one person uttering to another, “it’s poisonous”, to dissuade them from eating a death-cap mushroom. What are the differences, and which (if any) are especially linguistic?

The most immediately obvious difference is the mental, rather than selective, nature of the intentions in the communicative act. If language were selective like a frog’s colour, we would all be born speaking the same words, and speaking them automatically in the same situations. Fully selective vocal utterances are in fact what yells are: Selected calls that produce selected alertness in conspecifics through mutual selected recognition of the calls as grounded to a selected property of panic. Note how the fact that yells only *relate* to, rather than *represent*, ideas is partly captured in this by their objects and interpretants being affective states of the signallers and receivers; any ideas that the receivers have are inferences from this affective stimulation, which are not given by the sign.

We must therefore recognise the intentions in linguistic communication as mental, but which ones? So far, I have only discussed cases where all of the intentions are in harmony, but they need not be. For example, it is possible to make conscious *mental* use of a sign as standing for a *selected* object, to produce a *selected* interpretant. If I voluntarily fall about in tears (it being an act, rather than a physiological response), it will be to produce sympathy (a selected response to crying) through mutual selected (i.e. programmed) recognition of the crying as grounded towards an unhappy state of mind (states of mind without percepts also being products of organic design). We therefore have to ask whether all, or only some, of the intentions must be mental for utterance-idea mappings. It is certain that the object and interpretant must be, as mappings are always from and to meaning. Even when an intended interpretant is a behaviour, as in an imperative, it is always targeted indirectly through perception of the mental content specified by the utterance. It is also necessary that the signaller’s intention towards the sign and its interpretant is mental, as not having this mental intention would mean that the sign must be produced automatically, while linguistic utterances are voluntary.

The receiver’s recognition, and the signaller’s intention for it, are similarly clear-cut, though they are more nuanced. If there were a one-to-one correspondence between utterances and the ideas they refer to (i.e. if language were entirely unambiguous), then the receiver would not need to know of the signaller’s intentions towards the sign and its interpretant, as each interpretant could be derived without any understanding of the signaller’s psychology. However, this is nowhere close to reality—language is deeply ambiguous and, as previously discussed, pragmatic enrichment of utterances is an all-pervasive necessity, so there is no possibility for language unless a receiver has some mental recognition of the purpose to which a signaller is putting their utterance, and the signaller likewise intends this recognition. Unlike non-linguistic and non-conscious communication, any communication with a mapping of utterances and ideas therefore requires *all* the intentional components to be mental in character.

We reach an important juncture here, as a lesson we learn from this is that mutual recognition of communicative intentions (i.e. ostensive-inference) is a biological precondition for utterance-idea mappings. This mutual understanding of intentions is the property of ‘shared intentionality’ analysed by Tomasello et al.

(2005), and it is generally agreed to be a human development of theory of mind. In other words, we are all implicitly or explicitly in agreement that a precondition for utterance-idea mappings is not met by nonhuman species, so they cannot possibly have such mappings. The few scholars who suggest otherwise (e.g., Corballis 2017) largely focus on the gestural repertoires of other great apes, which are 'intentional' in the sense of being voluntary, but there is no evidence that they have *shared* intentionality (Byrne et al. 2017). Instead, apes gesture mostly unsuccessfully and repetitively until some behavioural goal is achieved, as their actions are structured to give cues for physical responses, rather than to communicate mental percepts (this is true whether they are interacting with conspecifics or humans). They realise that conspecifics are agents with abilities to satisfy desires, and they may even understand that others have beliefs and desires of their own, but they do not cognize that an individual's beliefs could be about another's, so they lack the leverage of shared intentionality to exchange mental content (note that their interactions can be described as communicative despite not having shared intentionality, as mutual recognition is still instantiated by natural selection, the gestures and their responses being innate). As See (2014) observes, even if we are generous about primate vocalisations, interpreting those as volitional and goal-directed rather than automatic and non-conscious, they would also have the same intentional limitations.

In a moment, I will describe a visible property of utterances that we could use to identify them as mapping to ideas, but so long as we accept the uncontroversial point that only humans have shared intentionality, we have already ruled out nonhuman mappings as an impossibility. Why is it, then, that we are not all discontinuity theorists? Well, we are interested in something more precise than mappings themselves—we are interested in the cognitive mechanisms that facilitate the development of mappings in the right circumstances. We know that shared intentionality is not itself this mechanism, as there is shared intentionality in much non-linguistic communication (i.e. communication that is all pragmatics and no semantics). Continuity theories are therefore those that claim that the mechanism that facilitates the association of utterances and ideas pre-exists shared intentionality, and the evolution of shared intentionality led inevitably to utterance-idea mappings through a drift to abstract linguistic signs (e.g., Tomasello 2008 and Scott-Phillips 2014).

Stating this in some other equivalent terms may set some alarm bells clanging: Continuity theories are those that claim that the mechanism underlying utterances' mappings to semantics pre-exists shared intentionality, and the evolution of shared intentionality led inevitably to semantics, *as just a highly derived form of pragmatics*. Discontinuity theories maintain a categorical difference between pragmatics and semantics, and claim instead that the mechanism for semantic mappings came after shared intentionality and is distinct from it. We therefore need to consider whether this categorical distinction is justified.

We have a framework in place for the meanings involved in pragmatic communication: They are mental or selected percepts, and, as semantic meanings are never selected meanings, we need only be concerned with mental ones. These percepts are *individual* in two very important respects. First, they are mental representations of individuated perceptible entities, whether real or fictional. For example, I can think about and point at some particular chair, but I cannot gesture non-linguistically at CHAIR, the abstraction of all things that can count as some kind of chair. These meanings are also individual in the sense of their grounds being self-referential—the chair that I point at can only be identified as the object of my pointing by recognising my signaller's intention and following the line of my arm.

Utterances with semantics are also partially individual, as they all require pragmatic interpretation. It is the purpose of any linguistic utterance, like many non-linguistic ones, to specify an individuated mental percept, and this requires an amount of self-reference for ambiguity resolution. However, the essence of utterance semantics is what is *general*, rather than individual. Although I have no non-linguistic gesture for CHAIR, I have a word for it, and I have words for all kinds of properties that cross-cut individuated mental representations—how could one non-linguistically gesture at the colour of a wall, or at the exact number of a heap of stones without counting them out individually? All of these things are *concepts* rather than percepts, and it seems that concepts cannot be percepts, in that we cannot bring concepts before our mental attention. Although we may try to think of our concept BIRD, say, what we will think *about* is some particular prototypical bird standing for the properties that our concept organises, though that prototype cannot be the concept itself (see Armstrong et al. 1983, Connolly et al. 2007). We cannot have a mental perception of the concept as it subsumes everything from the peewit to the penguin, and if we otherwise tried to describe it, we would resort to using other words for other concepts as periphrastic definitions. Concepts are thus like 'filenames' that organise our thoughts (Fodor 2008), but the only things we can actually think *about*, in intentional terms, are perceptible file contents.

While the individuality of pragmatic communication requires the ground of a sign to refer to a signaller, the generality of semantic communication allows non-self-referential grounds, as concepts are not specific to individuals—concepts are relations among percepts, and though such relations must have individual neural instantiations, they are also abstractions over individuals, in just the same way that we must all have different neural instantiations of structurally identical sentences. We can therefore identify utterances with semantics by observing whether their use has a non-self-referential ground (i.e. an independence of signallers' communicative intentions). Of course, this is impossible to observe with any *single* utterance, as all utterances have a pragmatic element, but if we look at *patterns* of use, semantic utterances ought not to be correlated with characteristic intentions for recognition and interpretation:

(7) *Non-Self-Referential Grounding*: Utterances that map to ideas can be identified by a pattern of use that is independent of particular communicative intentions on occasions of use.

This applies quite neatly to humans, as we can use it to distinguish words that do and do not have a mapping to ideas. For example, 'hello', which is not syntactically or semantically compositional, is strongly correlated with a specific intention to be recognised as signalling a greeting. Take semantically contentful words, however, and they have no strong correlation with specific intentions (we put other words to use for all kinds of different purposes). In case we are not convinced of other species' deficiency in shared intentionality, we can instead observe that none of their utterances exhibit this patterning—the most advanced vocal and manual gestures in closely related primate species are all directed towards specific communicative goals, and are thus fully pragmatic. This evokes some of the old arguments against behaviourism, the first page of Fodor 1975 stating:

> the contribution of [an] organism's internal states to the causation of its own behavior seems sufficiently undisputable, given the spontaneity and freedom from local environmental control that behavior often exhibits. (Fodor 1975: 1)

Here, we could just as well say that an organism's internal mapping of utterances to ideas is sufficiently undisputable when the utterances have a spontaneity and freedom from local environmental control.

This much establishes that semantic and pragmatic meanings are mentally different in kind, but mightn't shared intentionality nonetheless be sufficient for both, as continuity theories claim? There are two points to consider here. In a semantically compositional system, where composition is driven by syntactic structure, accepting standard arguments for a poverty of the stimulus entails that there must be some innate syntactic capacity over and above our pragmatic abilities that is responsible for the semantic composition of morpheme sequences. One can try to get around this by denying the poverty of the stimulus and arguing that syntactic structure is a byproduct of cognitive efficiency or learning constraints, but in setting out the theoretical landscape, I will simply assume the generative position that these arguments are fundamentally misguided.

Earlier, however, I stated that compositionality (and thus syntax) is not an obviously necessary component of the utterance-idea mapping. We can at least imagine a scenario in which shared intentionality at first permits holistic utterances to be associated with situation-specific pragmatic meanings, with forces in cultural evolution later leading to holistic utterances being associated with abstract concepts, thus being more recognisable as morphemes with semantics. To get compositionality, an innate syntactic faculty would still need to be added afterwards, as there is no route from holism to compositionality without it (Tallerman 2007), but still, some form of utterance semantics might have preceded syntactic structure. At heart, what this claim amounts to is that there is no cognitive, mechanistic difference between acquiring the purely pragmatic significance of a word like 'hello' and acquiring the semantic significance of a word like 'cat'—in both cases, shared intentionality, mixed with domain-general associative learning mechanisms, are sufficient for the morphemes to get their meanings, even though these abilities would be insufficient for morpheme *sequences* to get *compositional* meanings.

Though this seems admittedly more plausible, I think it has to be false. As with syntactic structure, the problem is again a poverty of the stimulus, though this time with the non-self-referential grounding that characterises semantic utterances. Utterances with pragmatic meanings, like 'hello', are by their nature identifiable with the intentions of their users—what their users intend by them on individual occasions simply *is* constitutive of their meaning, and associative learning can handle this without issue. However, semantic utterances are *more* than their intended use on individual occasions. It is not just that semantic utterances refer to abstract concepts that are context-independent, but also that their use extends beyond the conceptual referents to which we are exposed, such that there are cases in which it seems implausible to account for the novel use of a word as being a pragmatic modulation of some meaning that we have observed it to have in a prior instance.

The most obvious examples are metaphors. Consider the word, 'open', for example. It has various ordinary uses, as in 'open the door', 'open the letter', 'open the shop', and 'open the wound'. Each of these uses requires a contextual interpretation—taking a scalpel to a door would not satisfy the meaning as it would for a wound (Recanati 2004)—but it superficially seems that there is some highly abstract sense of 'opening' that applies to them all. Yet, consider the use of the word in Bellow's *The Dean's December*, where a dog, barking as if in protest at the limits of its experience, is imagined to be saying, 'open the universe a little more!' There is no difficulty in assigning an interpretation to this unusual use of the word, but what schematic sense of 'open' applies equally to this use as it does to all the others? It seems rather that 'open' has no core conceptual meaning that is modulated situationally, but rather that it can refer to entirely novel concepts, so long as these concepts have a sufficient resemblance to those of previous uses (Carston 2013). If this is true, then the meaning of a word is strictly speaking not a concept or even a range of possible concepts, but rather some network of conceptual features that implicate concepts as more or less likely to be referred to by the word. As these features and their connection to morphemes is a form of tacit knowledge not supported by experience, there must be some domain-specific mapping capacity that is responsible for organising an individual's lexicon in this way, and this would have to be an evolutionary discontinuity that post-dates shared intentionality. Exactly what this lexical capacity consists in is as yet unclear, but in §3 I will consider some of its broader implications for a Chomskyan theory of syntax. First, however, a few brief comments are warranted on the overall significance of such a discontinuity.

2.3. *The Significance of Discontinuity*

In section 2.1, I stated that defining discontinuity as a lack of inheritance is compatible with evolutionary gradualism, so the fact that a discontinuity theory is necessary should not dishearten those who feel that an adaptive explanation of language origins is essential. To understand the relation between discontinuity and gradualism more precisely, we first need to acknowledge that choosing traits to study can be somewhat artificial if it is not done carefully, as we are led to deconstruct whole, integrated organisms into collections of interacting parts. Often, we are biased in this by how we, as humans, have evolved to categorise the things we perceive in the world, and this need not relate in any informative way to the actual genetic, developmental and structural organisation of organisms. We therefore tend to end up with false impressions of traits as being somehow isolated from the rest of the organism in which they are incorporated, suggesting that they have been targeted by selection pressures, even though they may be nothing more than non-selected byproducts of other traits, or 'spandrels' (Gould & Lewontin 1979).

The 'biological' conception of homology that I discussed in section 2.1 has tried to counteract these problems by focusing on just those traits that are developmentally individualised, with the expectation that their evolutionary development will likewise have been independent of much of the rest of the organism (Wagner 2016). However, even if we succeed in identifying such traits, this ontogenetic and phylogenetic independence does not eliminate a fundamental reliance that all traits have upon pre-existing organic material to come into existence at all, and this basic fact grounds all discontinuities in continuity.

To demonstrate this, it is perhaps unhelpful to search for developmentally individualised traits, as we know far too little about how language cognition individuates in this way. Instead, we can learn from the similarly messy, integrative trait of bipedalism. There are many other traits besides bipedalism with longer genetic histories that had to exist prior to its development—some are obviously necessary precursors, like having at least two legs, while others are more subtle, like having a particular configuration of the legs, hips, and spine. All of these traits would have evolved for their own independent benefits, and none with any foresight for making bipedalism inevitable or even possible, but only when they came together in conspiracy could a rudimentary form of bipedal behaviour be experimented with, and subsequently refined. In a sense, though we can identify the discontinuous origin of bipedalism with the narrow window of time in which something resembling bipedal behaviour began, the *possibility* of its origin was much more deeply rooted in the history of life. Every trait has these *substrates*: Traits that are the basis for other traits to evolve.

When we identify a trait of interest, then, establishing some vague time for its origin is only the first step in an explanation of its evolution, and the easiest. True understanding comes with identifying a trait's substrates in antecedent species, before the interactions of variation and selection created it in some primitive form and then developed it through descent. Of course, all substrates are themselves traits with their own substrates, each one implicating more and more, until the entire organism and its evolutionary history are involved. Part of the challenge we face in this is to delineate traits and trait relationships in ways that can elucidate an organism's functions and their history. With regards to language evolution, a useful example is speech physiology. Our species is capable of producing a variety of speech sounds, and we know from the highly limited abilities of other apes that this capacity arose as a discontinuity in hominins, but how did its substrates lead to the possibility of that origin?

The production of speech sounds has many phenotypic and genotypic substrates, minimally and most importantly including the anatomy of the vocal tract and its integration with fine motor control. Consider, then, a hypothetical scenario in which some hominin ancestor had a vocal tract configured roughly like that of modern humans, but without the neural integration necessary for any actual speech-making behaviour. Tattersall (1998) suggests that this is in fact close to the truth, and macaques provide an intriguing parallel—a study by Fitch et al. (2016), revising the methods and conclusions of an influential paper by Lieberman et al. (1969), showed that macaques have vocal tracts that are physically adequate for a variety of speech sounds, though the monkeys don't produce any, presumably because of a lack of neural control, rather than motivation. Given a scenario like this in hominins, we would obviously conclude that the ability for speech was brought about by modifications to our ancestors' motor control of the vocal tract. Thus, the discontinuous origin of one trait—speech—would have been predicated on underlying continuities in its substrates—vocal tracts and motor control—which underwent a gradually transitioned integration (see Ackermann et al. 2014 for one such story).

Ultimately, what this means is that it is never quite right to talk of a trait discontinuity as a discontinuity *tout court*. All traits are the integration of their substrates, and all trait continuities are the integration of their substrates' continuities. Moreover, as the evolutionary process is enormously conservative—recall that the average genomic difference between humans and chimpanzees is around 1–2% (Chen & Li 2001)—every discontinuity arises in a context of massive substrate continuity. It is in this sense that all discontinuities can and should be construed as special cases of continuity: All are tips of innovation on icebergs of conservation, with the tip unable to exist without its supporting structure.

Bearing these aspects of discontinuity in mind, Lenneberg's approach differed considerably from the one I developed in the previous section, as he regarded the discontinuity of language in a deliberately trivial way, noting only that every species is by definition discontinuous with every other, and he felt that the unique properties of language ought to be explained as much as possible through developmental reorganisations of commonalities (see Boeckx & Longa 2011 for a detailed discussion of this point from a more modern perspective). Thus, while Lenneberg did not deny that language is unique among animal communication systems, he avoided talk of language as an integration of other, independent traits (and especially the idea that some might be *sui generis*), while trying to explain characteristics like syntactic structure as developmentally complex expressions of widely shared conceptual capacities for categorisation and differentiation.

Initially, this seems antagonistic to any attempt to isolate a constituent property of language and claim it as an evolutionary capstone that brought other traits together in a new system of linguistic behaviour. However, note how the hypothesised discontinuity in speech described above was behavioural, and predicated on continuities in traits at the lower anatomical level. Just as was the case in the discussion of homology (and drawing parallels with Marr's 1982 plan for understanding the visual system), if we are careful to not mix up different levels of analysis, we will find that these different approaches to trait evolution are complementary.

By simply identifying a unique behavioural or cognitive feature, we are not forced to make any claims about it corresponding exactly and exclusively to some individualised developmental process. It could well be the case that the utterance-idea mapping, or Merge, is nothing more than a theoretical abstraction that labels a phenomenon which emerges from our complex biological design, but which has no encapsulated existence at a physical level. As such, Lenneberg's program is crucial for identifying how these sorts of discontinuities can result from gradual descent with modification. However, just as we mustn't take 'biological' homology too far in its obsession with developmental individuation, so we mustn't do the same here—if we don't develop the theoretical abstractions in the first place, we will not even identify what it is that is to be explained in terms of its physical implementation. Indeed, approaches that repudiate these abstractions inevitably have their own; they are just either so mindless or simplistic that they are not seen for what they are. Of course, our abstractions must be open to significant revision upon discoveries in neuroscience and developmental biology, but we will end up understanding nothing if we pretend that we can understand everything from the bottom up (Krakauer et al. 2017).

In sum, there is nothing upsetting about discontinuity, and nothing inherently saltational about it, as all trait origins are discontinuities, and all discontinuities have continuous substrates. Moreover, the task I have been engaged in is specifically the identification of a cognitive discontinuity, independent of any claims about its developmental organisation. Of course, the real test for such a discontinuity, as I will explore in the next sections, comes with actually explaining its origin. What discontinuity theories of language evolution must do is make clear what substrates preceded linguistic ability so that it could have had a hominin origin. As stated previously, there are many potential varieties of discontinuity theory, but I will only be concerned with Minimalist ones here.

3. Discontinuity in Minimalism

3.1. The Standard Account

In the context of language evolution, the main attraction of Minimalism—and admittedly its implausibility to some (e.g., Pinker & Jackendoff 2005)—is its offer of a clear response to what has been dubbed 'Darwin's problem'. This is simply the question, "how could language have evolved?", only asked quizzically because we appear to be dealing with an inconceivably speedy evolution of a biologically complex system. As Lieberman (2015) states, Darwin's problem does not exist in continuity theories, which hypothesise an evolution over many millions of years, but we've seen that continuity theories have more serious things to worry about.

Chomsky has also in various places (e.g. Chomsky 2010) referred to this as Wallace's problem to emphasise the doubt he shares with Wallace about the mechanism of natural selection being able to produce linguistic competence (Wallace 1869). However, Wallace also doubted that natural selection could be responsible for human hands and hairlessness, so his was not a cogent reflection on the relationship between human cognitive abilities and evolutionary processes. Ultimately, whether it's Wallace arguing incredulously for a guiding divinity or, say, Pinker & Bloom (1990) flogging adaptationism as an elixir for complex design, too many hypotheses about language's evolution begin with astonishment and then search for mechanisms that can create astonishing things. I think we are safer starting with biological properties.

Biologically, then, the core motivation of Minimalism is to narrow the difference between humans and other animals while maintaining a qualitative separation in linguistic competence, so that the origin of language can happily fit into the available time. Ideally, this should be accomplished by some very small genetic changes with large phenotypic effects, so that there would be no need for inconceivable amounts of mutation and selection. If this were possible, accusations of fanciful saltationalism would then fall flat.

Linguistically, Minimalism aims to do this by unifying the diverse structural phenomena that were at the heart of Government and Binding theory (GB), such as binding relations, case assignment, and argument structure, with more basic principles of syntactic derivation. As Hornstein (2009: 7f.) notes, the GB vision of the language faculty was internally modular, its phenomena having their own independent primitives, and this is a highly suspect property for the language faculty, as it is unlikely that such modules could have evolved and integrated in so short a time. Indeed, even over many millions of years, it is difficult to see how these modules could have developed as basic units of language ability, rather than their properties being emergent (see Poeppel & Embick 2005 on the 'mismatch problem' between the primitives of linguistic theory and cognitive neuroscience).

There are now many approaches within the Minimalist framework, but all try to stray as little as possible from the Strong Minimalist Thesis, which claims that all the structural properties of languages are derived from the binary set-forming operation, Merge, taking lexical items as input, building them into recursive hierarchical structures while constrained by third factor principles of efficient computation (Chomsky 2005), before converging at interfaces with the sensorimotor and conceptual-intentional systems.

Of many expositions, Berwick & Chomsky (2016), hereafter B&C, give a concise summary of how the Strong Minimalist Thesis ought to lend shape to our biological history:

> In some completely unknown way, our ancestors developed human concepts. At some time in the very recent past, apparently some time before 80,000 years ago if we can judge from associated symbolic proxies, individuals in a small group of hominids in East Africa underwent a minor biological change that provided the operation Merge—an operation that takes human concepts as computational atoms and yields structured expressions that, systematically interpreted by the conceptual system, provide a rich language of thought. These processes might be computationally perfect, or close to it, hence the result of physical laws independent of humans. The innovation had obvious advantages and took over the small group. At some later stage, the internal language of thought was connected to the sensorimotor system, a complex task that can be solved in many different ways and at different times.
>
> (Berwick & Chomsky 2016: 87)

Although I ultimately favour Minimalist descriptions of syntactic derivation, I think this account has some irreparable conceptual flaws, deeply rooted in a certain view of language that we must take pains to pick apart. First, though, let me discuss what is *not* wrong with this description.

An architecture consisting of Merge (or something like it), with some interface and third factor constraints, I fully take for granted. There is no issue with positing that a minor biological change could have produced Merge (though the talk of its adaptive benefits is perfunctory), as it is computationally basic. It may be a composite operation of, say, concatenation and labelling (Hornstein & Pietroski 2009), but, however it is implemented, a capacity to compute recursive set formation is an all-or-nothing deal.

Of course, a consequence of accepting both that syntax is fully captured by Merge and that Merge evolved almost instantaneously is that all syntactic phenomena ought to have appeared instantaneously too. At risk of contradicting myself, I share the doubts about this (e.g., Tallerman 2014), but in a specific weak sense. It has been the traditional approach in generative linguistics to have syntactic derivations fully determine utterance output. This is exemplified by Kayne 1994, where the Linear Correspondence Axiom was intended to ensure a one-to-one correspondence between the left-to-right sequence of morphemes in an utterance and their top-down sequence in a syntactic tree (this approach had a notable cross-linguistic application in Julien 2002). However, it is now common practice, if not universal, for syntactic derivations to *under*-determine utterance output, so that many features of left-to-right morpheme sequences must be imposed by the sensorimotor systems, creating many language-particular discrepancies with the underlying syntax. Although this inevitably makes the connection between utterances and syntax more oblique, so that it is harder to work out what syntax is doing (Anderson 2013), it means that many traditionally 'syntactic' word order phenomena may not be 'syntactic', in the sense that they arise after mental computations have finished and been spelled out. Thus, the full panoply of traditionally 'syntactic' (i.e. word order) phenomena would not have arisen instantaneously with Merge but this is because they are not due to Merge, in which case they are after all not 'syntactic' in the relevant sense (i.e. computationally derived). The small-print of this Minimalist offering is that Merge will give you everything, but everything isn't as much as it used to be.

Also unproblematic is the suggestion that concepts precede language, and the structures built by Merge are primarily connected to the conceptual system, rather than the sensorimotor systems. The most obvious reason for this is that there simply isn't any inherent hierarchical structure in utterances, as they are one-dimensional acoustic or visual strings. We are only able to infer that this structure is supposed to be represented in utterances, and perhaps form our expressions with prosody that respects and suggests it, because we have an innate mental prejudice to discover it, given that utterances map to semantic compositions that *are* syntactically structured by necessity. Moreover, while hierarchical structures have clear benefits in a system of thought, allowing the generation of internally complex ideas, there has been no convincing demonstration that a non-hierarchically structured signalling system would be improved by this additional complexity (Számadó & Szathmáry 2006)—unless, of course, it was to represent ideas that already possessed that structure.

The fact that utterances have syntactic structure only secondarily often creates a confusion about what exactly linguistic competence is, given that the syntactic structure of concepts is a precondition for anything we would identify as linguistic expression. Does this mean that linguistic competence after all cannot be the ability to map between utterances and ideas, as I have described it, and must instead be the ability to generate syntactic structures, even without actual or possible externalisation? While it is undeniable that linguistic *structure* is rooted in the conceptual system, it is stubborn to insist that linguistic *competence* is merely that structure-building capacity. On the standard Minimalist account, there was a time, however brief, when hominins had syntactically structured ideas that were not externalised, as the sensorimotor systems had not yet been integrated, and it would be foolish to call these intelligent but mute hominins linguistically competent. Allowing for the possibility that syntax originated in ideas therefore does not affect my statements characterising linguistic ability as the utterance-idea mapping. Competence is therefore not the same thing as the 'narrow' language faculty limned by Hauser et al. (2002), which may in any case not be helpfully defined (see especially Boeckx 2013 on this).

Finally, there is mounting evidence that refinement of the sensorimotor systems, with neural integrations allowing complex acoustic signals and a vocal learning capacity that far surpasses that of other primates, would have had sufficient time to evolve, even if the developments were entirely hominin. As stated already, there is evidence that our vocal tracts were speech-ready long before speech use, even if not quite so exquisitely (Tattersall 1998), and there appear to be deeply conserved genetic bases for vocal learning that may find a rapid implementation in the right circumstances (Pfenning et al. 2014).

Now to the problems. As stated in section 2.3, identifying the trait (Merge) and the time of its origin (in hominins) is only the easy work of setting the parameters for the question (perhaps the arrival of the Minimalist program over a century after *On the Origin of Species* should be a warning to us). What an actual theory of language evolution needs to do is identify the substrates of the identified trait in antecedent species to explain how its origin and spread was possible. For the language faculty, the substrates that we might identify could be genetic, anatomical, neurological, cognitive, behavioural, and even cultural. Although it is encouraging that B&C and Boeckx (2017) have attempted to identify a neurological basis for Merge (for a short review of neuroimaging data also see Friederici, this issue), in order to make it seem less like a bolt from the blue, I think there is still so much to be said about the computational substrates of the language faculty (again setting aside their developmental basis as a separate issue) that we don't yet need to venture into other territories.

Although Merge would have appeared rapidly, its first interactions would have been with the conceptual system, and the sensorimotor systems could have been refined quickly, what seems entirely unreasonable is that Merge's integration with the rest of our cognitive faculties had to come for free. Bear in mind, Merge is not just an operation with some output, it is an operation with very specialised input ('conceptual atoms', which are supposed to be unique to humans), as well as outputs that have to be interpreted by interfaces with independent cognitive modules of phonology and semantics.

Even if we can explain how syntactic computation originated in hominins (ignoring the mystery of its conceptual input), we would still have to account for the cognitive mechanisms that translate its structures into forms that are intelligible these other mental systems. For this, it does not matter that the sensorimotor and conceptual systems are comparatively ancient—syntactic structures are purported to be generated in a novel autonomous module that these other systems now interface with, so their mechanisms for interpreting syntactic structures must be every bit as novel as the syntactic process itself. For example, if the meaning of some syntactically structured utterance depends upon interface rules that translate syntactic configurations into logical forms, those translation rules must surely post-date the origin of syntax. What would it mean to say that the conceptual-intentional system was able to interpret syntactic structures before there were any such structures? Did Merge bring its own interface rules, or did the interfaces evolve their rules independently? Merge is generally thought to be constrained in its derivations by conceptual impositions like the functional hierarchy, but if Merge is just a set-forming operation whose autonomous structures are built for interpretation, how did the conceptual-intentional system ever come to impose these constraints? To date, the most prominent answer is only that Merge is not so constrained (e.g., Chomsky 2004) and that the interfaces filter out sequences they don't like. As I will come to explain more fully, these *ad hoc* stipulations are the result of an unnatural severance of a syntax that should be closely intertwined with its semantics.

To make a start at this resolution, let's return to the three as yet unexplained hominin discontinuities that the standard Minimalist account proposes. Paraphrasing the earlier quotation from B&C, these are (in order of origin):

A. The origin of (some relevant notion of) atomic concepts.

B. The origin of Merge within the conceptual system.

C. The origin of sensorimotor integration with the output of Merge.

Immediately, we can note that this improbable sequence must be incorrect if all of (A)-(B) are hominin origins *and* independent of each other. Although language, as an autapomorphy, is simply biologically improbable, if (A) does not predispose a species to (B), and (B) does not (C), it is inconceivable that hominins should have accrued all of these fortunes while nonhumans have been blessed with none. Nonetheless, it is basically correct to characterise the language faculty as constituted somehow of the elements in (A), (B) and (C), and in that order, so we have two routes to plausibility: Either these are *not* all hominin in origin (i.e. other species possess at least concepts, and perhaps Merge), or these are not independent.

The approach taken in B&C entails that language evolution, though described in three stages, after all required only *two* innovations—concepts and Merge—with sensorimotor integration in some sense inevitable or free (and thus not an independent development). What this means is either that sensorimotor integration is so cheap and valuable after Merge evolves that it is unavoidable, so a species simply cannot find itself at (B) and not proceed to (C), or the sensorimotor systems have always had some kind of access to the content of the conceptual system, so that (C) was not the evolution of lexical and phonological *capacities*, but was rather a mere refinement of sensorimotor physiology to make the externalisation of an unbounded system of ideas more effective. In either case, for B&C other species cannot have Merge because its presence is sufficient for its externalisation.

B&C in fact seem to believe that, whatever integration of the sensorimotor systems was necessary, sensorimotor access to concepts is ancient (and therefore free, in the sense above), as they take utterances in every species they consider to be straightforward reflections of conceptual ability (see Tallerman 2014: 208f. for more discussion of this). This effectively means that they draw an equivalence between words and concepts, or, more weakly, that they presume conceptual repertoires to guarantee lexical ones, so that the lack of words in other animals (which is undoubted for the reasons given in section 2.2) means that animals have no concepts. They do state (p. 84) that other primates have "conceptual structures", but by this they seem to mean that primates can make relational distinctions between things like actors and actions, singular and plural, without anything like property categorisations.

There are bizarre consequences to claiming that animals do not have concepts, as demonstrated by a discussion of the chimpanzee, Nim, where B&C cite Pettito (2005: 86f.) before concluding that he had neither words nor concepts:

> Chimps, unlike humans, use [...] labels in a way that seems to rely heavily on some global notion of *association*. A chimp will use the same label apple to refer to the action of eating apples, the location where apples are kept, events and locations of objects other than apples that happened to be stored with an apple (the knife used to cut it) [...] all simultaneously and without apparent recognition of the relevant differences or the advantages of being able to distinguish among them [...] Surprisingly then, chimps do not really have "names for things" [...] They have only a hodge-podge of loose associations with no Chomsky-type internal constraints or categories and rules that govern them.
>
> (Berwick & Chomsky 2016: 146)

Nim clearly did not use anything in a word-like way, but if the sign he used was used of eating apples, apple locations, apple actions, apple tools, and so on, then he likely had the *concept* of an apple, as that is the only psychological constant that relates all of these disparate references. However, by allowing him the concept, we are not compelled to somehow interpret his use of the sign as being intended to mean APPLE, which I agree is impossible. Rather, his haphazard pragmatics seem very much like a child at the one-word stage, where single words can represent complex thoughts that include the referent—for example, *doggie* for, "there's a dog!", "can I pet the dog?", and "the dog is biting me!" (Jackendoff & Wittenberg 2014). What Nim lacked may not have been the concept that his trainers wanted the word to signify, but rather an ability to form systematic associations between repertoires of concepts and signs, so that he could pass the one-word stage and begin expressing structured ideas in a way that respects things like lexical categories and functional relations. In other words, this is just what sign-use looks like when you have concepts and a reasonable pragmatic ability, but no mechanism for forming semantic mappings.

Moreover, just as Nim would have been incapable of mentally representing his handlers' intentions, his handlers appear to show a systematic oversight of Nim's own intentional competence. As we know chimpanzees to be limited by their lack of shared intentionality, we can only expect that, even if they have concepts, they must fail to communicate them, as being able to do so requires a degree of theory of mind that they don't possess. Indeed, Nim's behaviour is remarkably similar to the goal-orientated activity of chimpanzees in the wild, whose behaviour is highly likely to be structured by competences to categorise objects and properties in the world as concepts (see Fitch 2017 for more discussion of the false conclusions that B&C draw from limitations on communication of ideas).

The identity between word and concept that B&C tacitly assume is particularly evident in their conclusion from that same passage:

> It appears that chimpanzees are perfect examples of pure "associationist learners"—what they seem to have are direct connections between particular external stimuli and their signs. *They do not seem to regard the apple*

> *they see in some mind-dependent way* […] Rather, they have stored a list of explicit, mind-*independent* associations between objects in the external world and the ASL signs for them.
>
> (Berwick & Chomsky 2016: 146–147; emphasis added)

Again, rather than Nim using associative intelligence, B&C interpret Nim *as* an associative intelligence, his psychology a repository of statistical correlations between stimuli, with no conceptual understanding of types of objects and events that he encountered—Nim was just one of Skinner's superstitious pigeons in an ape costume. I in fact agree that Nim stored an explicit list of associations between external objects and signs (i.e. he had only Hockett's semantics, not the real deal), but the portion I italicised is a non-sequitur: Just because Nim was unable to form an association between utterances and concepts does not mean that Nim had no such concepts, as there are cognitive capacities *besides* having concepts that are necessary to form systematic sensorimotor representations of them. B&C are thus led to a catastrophic vision of human evolution, with language not just a capacity to communicate structured ideas, but the primary source of ideas, fundamentally reconstituting the way that our brains organise and process information:

> Human cognoscitive powers provide us with a world of experience, different from the world of experience of other animals.
>
> (Berwick & Chomsky 2016: 86)

Their continued underlying assumption is that utterances across species are a direct insight into their mental life. All one has to do is listen, and one knows what it is like to be a bat (see Nagel 1974).

Against these ideas, not only is there substantial empirical evidence that nonhuman species have concepts as humans have them (see Hurford 2007 for a comprehensive review), it also seems cognitively necessary, in that the same associative mechanisms that were rejected as a theory of utterance-idea mappings in the rejection of behaviourism are just as incapable of explaining *non*-linguistic behaviour—nonhuman species would simply not be capable of their richly structured behaviours were it not for a capacity for forming mental abstractions of sensory experience. Dupré (1996) has correctly pointed out that to assume otherwise is to be beholden to the Cartesian view that there is a gulf between humans and nonhumans, the latter being unthinking automata.

Importantly, if we are compelled to believe that other species have concepts, then, as they don't have words, words and concepts must be fundamentally dissociated, and this dissociation means that B&C must be wrong to think that sensorimotor access to the conceptual system was cheap or free—if so, animals with even the most basic conceptual systems should be capable of the most basic meaningful expressions, and this is not what we find. As explored in section 2.2, despite the abundance of nonhuman conceptual abilities, the *representation* of concepts in nonhuman utterances isn't tiny, it's nil, as it's impossible. As sensorimotor access to concepts must be secured by a specific capacity that uniquely evolved in humans, it's absolutely essential that we understand the role of Merge in this, as if Merge in addition to concepts is sufficient for their externalisation, then other animals ought not to have Merge, but if Merge is genuinely a conceptual tool, and concepts have no automatic connection with sensorimotor representation, then Merge might be found in non-speaking animals.

Though evidence of syntactically structured thought in nonhuman species is slim, and we perhaps don't even know how to confirm or deny it convincingly, it would arguably be a parsimonious outcome, as other animals could be said to possess the sorts of ideas that Merge can generate (taking concepts rather than words as input), and many species seem capable of mentally representing relationships like *who* did *what* to *whom*, with the kinds of argument structures that are secured by a hierarchical syntax (see Carruthers 2002 and Gallistel 2011 for discussion). What prevents them from having the same cultural and symbolic intelligence would therefore not be a lack of Merge, but a lack of an ability to externally represent what Merge generates, so that they could acquire more complex conceptual structures by building a shared lexicon (Tallerman 2009). Likewise, what caused the flourishing of symbolic behaviour in human societies around 100,000 years ago may not have been our species' acquisition of Merge, but rather our acquisition of a capacity to express its structures. Of course, if feasible, this would also be attractive within the Minimalist aesthetic, as it would reduce the chasm between humans and other species, while still maintaining some domain-specific linguistic capacity.

Arguably, a key motivation for B&C to hold to their position is that if we allow Merge to exist without externalisation, it is more literally a device for structuring thoughts than they ever intended, as syntax could no longer be understood as a fully autonomous module facilitating interaction between the performance systems—Merge would just build ideas, and whatever maps those ideas to utterances is a mystery again. Although the hierarchical structure of utterances and ideas would be as central to linguistic competence as ever, *narrow* syntax would be gone, and, as hinted at twice already, much of what we previously believed to be determined by mental computations would instead be effects of externalisation processes. In the remainder of this paper, I will briefly outline some ideas that lend support from a more linguistic perspective.

3.2. *There's Minimalism and then There's Minimalism*

If it is true that Merge has no necessary connection to utterances and can exist in many species purely to structure ideas, then there is no prior reason for the objects it derives to feature elements or relationships that have no semantic interpretation. Moreover, though syntax might in the end be imperfect and generate some things that are not semantically interpreted, none of its redundancies should be coincidentally targeted for sensorimotor legibility. For example, in a standard T-model of the language faculty, syntax may undertake a movement operation to ensure the left-to-right order of words in an utterance. However, if this movement operation has no purpose at logical form, it ought to be prohibited from syntax and the displacement explained by externalisation mechanisms (see Tallerman 2014 for some possibilities).

This suggests a broader research program to demonstrate that Merge can indeed exist as a conceptual device with no necessary sensorimotor connection, or, in other words, to demonstrate that Merge only needs and uses atomic concepts instead of lexical items (see Burton-Roberts & Poole 2006 for some reasons why this undertaking is attractive for reasons independent of the arguments presented here). Whereas the Minimalist program proffers the Strong Minimalist Thesis, claiming that Merge is an optimal solution to the needs of two interfaces with the performance systems, we can test what I will call, only half-facetiously, the Minimalist Minimalist Thesis, which claims that Merge meets only the needs of semantics, with all utterance phenomena that are not reducible to the derivation of a logical form being explained by externalisation pressures imposed upon completed derivations. Testing this properly is beyond this paper, but it has some initial promise.

We can approach the core issues by thinking about the ways in which syntactic structure can relate to semantics. If what we are after is some kind of equivalence between syntax and semantics, then it is immediately tempting to reconsider generative semantics, but I take it that that paradigm failed for good reasons (Newmeyer 1980). However, we should bear in mind that the fault with generative semantics may not have been its proposal that semantics should be homomorphic with syntax, but rather that it attempted its homomorphism *pre*-derivationally. Let us think about various possible derivational relations, then:

1. Configurational interpretation
2. Rule-to-rule interpretation
3. Piggybacking
4. Derivational equivalence

The first two terms are due to Bach (1976). In a configurational system, Bach says, interpretative rules take "a [syntactic] structure of such and such a form [and] translate the structure into an expression in the interpreted language of such and such a form" (Bach 1976: 184). In such a system, rules of arbitrary complexity map completed syntactic structures onto logical forms, with any input suitable for any output. Although this sounds unconstrained, limits are of course imposed by explanatory elegance and it has perhaps been the most common approach.

There is some variation in what rule-to-rule interpretation is supposed to mean, but the interpretative rules ought to be in some sense less arbitrary, with every application of some well-defined syntactic rule having a direct semantic consequence. Lohndal (2014) develops a version of this where every cyclical Spell-Out of syntactic structure creates a conjunct in a neo-Davidsonian semantics (Davidson 1967, Pietroski 2005). Conversely, this means that every conjunct in a logical form ought to correspond to a Spell-Out domain.

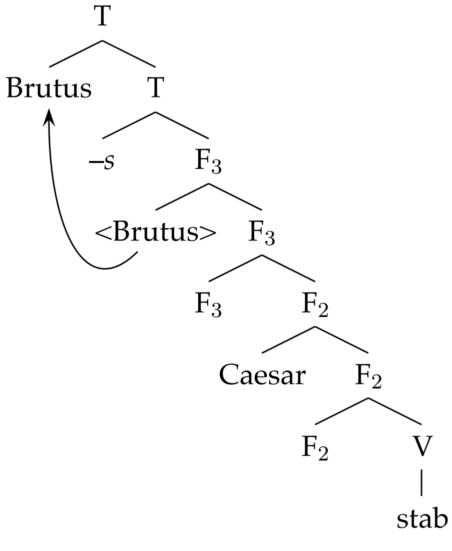
What I call 'piggybacking' is just a special type of rule-to-rule interpretation that is maximally transparent. For this, *every* syntactic operation ought to have an immediate logical interpretation. One such system is developed by Hornstein & Pietroski (2009), where Merge (renamed Combine) is first decomposed into the structure-building operations Concatenate and Label, and then all concatenations signify conjunction of monadic concepts, and all labelling signifies the building of thematic relationships.

These three approaches are all interpretative: Syntax first creates a structure on its own and, however soon afterwards, translation rules apply to that structure to create a logical form. However, abandoning any notion that the output of Merge must interface with a sensorimotor system means that having an independent syntactic module with just a semantic interface is unmotivated. Such an interface is only substantive when there is a many-to-one relationship between syntactic structures and logical forms, which is what we find in architectures where syntax takes lexical items (not concepts) as input, with that relationship holding of cross-linguistic differences in utterances that express synonymies. However, if atomic concepts are the input to syntax and, by extension, the syntactic process is *not* affected by its input, then there must be a one-to-one correspondence between structure and meaning, making an 'interface' vacuous. In essence, separating syntax from semantics is only credible if syntax takes input with non-semantic features, or in some other way interfaces with non-semantic systems. Especially given the possibility for there to be a maximal rule-to-rule correspondence between operations in syntax and semantics, if syntax has no necessary connection to any other cognitive faculties, then syntactic rules simply *are* semantic rules, and the two are derivationally equivalent.

Having talked about 'syntax' now in many different ways, a concrete example may help illustrate these points. Consider the utterance in (8), the simplified syntactic structure in (9), and the logical form in (10) (for the purposes of this discussion, we do not need a precise theory of the functional sequence, so I do not specify all functional heads):

(8) Brutus stabs Caesar.

(9)

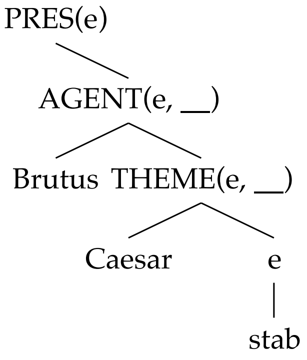


(10) $\exists e[\mathrm{stab}(e) \;\&\; \mathrm{THEME}(e, \mathrm{Caesar}) \;\&\; \mathrm{AGENT}(e, \mathrm{Brutus}) \;\&\; \mathrm{PRES}(e)]$

The claim of the Minimalist Minimalist Thesis is that Merge, taking concepts as input, is directly responsible for the derivation of (10). That is, the logical form is not created by an interpretation of a previously built syntactic structure; the logical form *is* a syntactic structure, from which the utterance 'Brutus stabs Caesar' ought to be derived. As such, the representation in (9) is equivalent to (10) to the extent that what it represents is also represented in (10), and whatever other features (9) has that correspond only to the utterance (e.g., agreement and possibly movement) need to be explained as artefacts of externalisation—they are not genuine features of a syntactic derivation.

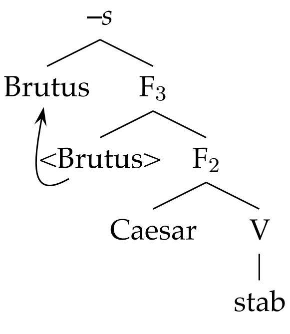
While the transparency between syntactic and semantic forms means that syntactic trees as normally drawn contain morpho-phonological information that is not truly part of syntax, we ought nevertheless to recognise richer structure in logical forms. To reiterate, I argue that logical forms themselves are produced by derivation, not by interpretation of prior derivations, so logical forms have derivational histories due to Merge, making them recursive hierarchical trees. Though that may not be clear from the appearance of (10), we can give it a better representation:

(11)



Although this looks simpler than the earlier syntactic tree, it contains the same structural, functional information and, in a more developed theory, should be sufficient to determine externalisation. Readers may notice that (11) strongly resembles Brody's (2000) Mirror theoretic representations of syntactic structures, which he argues for from entirely different motivations. In that system, (9) would look like this:

(12)



Clearly some questions immediately arise, presuming that a system like Hornstein & Pietroski (2009) can indeed establish a derivational equivalence between these semantic and syntactic forms: What exactly is the relation between the functional heads and the thematic roles (Borer 2005b, for example, replaces these thematic roles with ones that specify event structure)? How might movement be motivated at the sensorimotor interface, so that *Brutus* is linearised as the specifier of Tense? And is it possible to dispense with other canonical syntactic relations, like specifier-head-complement order, feature-valuing and variable argument structure?

It should be noted, of course, that syntactically structured logical forms undermine any potential objection that they are not suitable for externalisation, as such objections fail to treat logical forms as inherently structured. For example, one might say that because conjunction is commutative—AGENT & THEME = THEME & AGENT—pronunciation order is impossible to determine. However, as a semantic derivation by Merge has a temporal history and is constrained in its construction by whatever usually constrains Merge, like the functional hierarchy (Cinque 1999, Ramchand & Svenonius 2014), the proper shape for a derived logical form is the one in (11), which is suitable for linear ordering.

The most obvious difficulty for these ideas is that Merge taking atomic concepts, rather than lexical items, as input means that every identical meaning across languages ought to have an identical syntactic representation despite massive surface variation. One could argue that, once concepts have been connected with words, Merge could take feature-rich lexical items as input instead of concepts. This is essentially what Chomsky claims (with the presumption that the connection to words came for free), as he argues that Merge began with concepts, though the entire basis of parameterised language acquisition according to the Borer-Chomsky conjecture (Baker 2008) is that the input to syntax can be marked with syntax-affecting features (thus, they must be lexical items, rather than concepts, which do not bear such features). However, I believe this defeat can be avoided if we consider what it means to get rid of the lexical and morpho-phonological features that are normally associated with the lexical input to syntax.

In mainstream Minimalism, the secondary nature of externalisation is usually taken to mean that syntax is blind to morpho-phonological features, if they are present at all, though parameterised syntactic features are of course only possible if there are sensorimotor forms from which they can be inferred during acquisition. In other words, syntactic features are properties of lexical items that index concepts; they are not properties of concepts themselves. However, if concepts do not automatically entail lexical items, this system faces a fundamental problem, as the syntactic process at the time before externalisation either has unvalued features that cannot be valued, or if it can get by without feature-valuing, the origin of features and a sensitivity to them is unexplained. This much suggests that we should be aiming to get rid of non-semantic features if we believe that morpho-phonological features are irrelevant to syntax, but we need to approach the issue from another direction to really substantiate this.

Exo-skeletal (XS) syntax, following (Borer 2005a,b), has in the past ten years had great success with undermining the traditional view that syntactic structure projects from lexical specifications (with, for example, argument structure being licensed by features on verbs specifying how many and which theta-roles they must assign). Instead, XS syntax develops a framework where structure is templatic, universal, and (in a T-model) imposed upon lexical items, with meaning turning out to be variously acceptable, coerced or unacceptable (often with semantic unacceptability better explaining 'ungrammaticality'). Moreover, there are programmatic reasons for wanting to do away with lexical features even without pursuing a derivational semantics (e.g., Boeckx 2015), and there is interesting work being done to reformulate phenomena like case assignment in terms of structural configurations rather than feature-valuing (e.g., Preminger 2014).

For now, if we take it for granted that lexical features can be dispensed with, it becomes much more difficult to do away with morpho-phonological ones, and it is seriously tempting to conclude from studies like Borer (2013) that syntax is simply not possible without morpho-phonological input leading derivations, which seems counter to the basic intuition that syntactic structure is the structure of ideas. Perhaps we will one day have to concede that syntactically structured thoughts were only possible once spoken communication provided a means of organising conceptual structure, but this is an extraordinary claim requiring extraordinary evidence.

The main obstacle to reconciling XS syntax with a derivational semantics is the very clear mismatch between the units of morphology and units of meaning. In particular, basic (i.e. non-composed) meanings are sometimes matched with already syntactically structured words and word sequences, so the apparently necessary conclusion that has been reached by Borer is that morphemes are the true atomic input to syntax, with super-morphemic Spell-Out domains triggering conceptual access. This moreover means quite strikingly that morphemes (or 'roots') as listed in the lexicon are not associated with conceptual content, as they acquire it only when integrated within functional structure.

Of course, this must all be turned on its head to maintain the claims that other species have Merge, and that Merge structures concepts alone, and the only way to do this is to challenge the belief central to most syntactic theory that syntactic terminals correspond to actual or possible morphological units. With the morphology-concept mismatch, if concepts are to be the input to syntax, then syntactic terminals must be in many cases larger or smaller than morphemes, and morphemes must in many cases be externalisations of multiple terminals. This novel approach has had its first explorations in 'spanning' (Svenonius 2016), an outgrowth of Nanosyntax (Starke 2009) and Distributed Morphology (Halle & Marantz 1993), although these approaches still retain language-specific elements as the input to syntactic computation, rather than human-universal conceptual atoms.

To end this sketch, I would like to offer a metaphor for envisioning exactly the kind of system that is required, as technical implementations could have many guises, and the real sticking point here is what the *input* to syntactic computation must be. Especially as Chomsky already regularly claims that syntactic derivation is optimised for semantic interpretation, how is any of the preceding discussion genuinely different from his proposals? It is the input that matters, as syntactic analysis since the earliest days of generative grammar has always assumed, to a greater or lesser degree, an isomorphism between syntactic terminals and the morpho-phonological units of particular languages, whereas it is essential to this system that syntactic terminals are the atomic concepts that can enter into syntactic composition, which are *universal* for all speakers of all languages (and such concepts must exist if concepts and their composition are prior to linguistic ability). In other words, the 'lexical item', defined as a conjunction of phonological, syntactic, and semantic content, has no basis in the theory. Yet, if syntactic structures are so regularised, and if they do not structure morphemes, how is language variation possible? Note that frameworks like Distributed Morphology (Halle & Marantz 1993), which posit the phonological emptiness of syntactic roots, are not answers to this, as they still claim that roots are language-*particular*, and thus the invisibility of the phonology is sleight of hand.

Let's address this by asking quite simply: How do morphemes get their syntax if not from Merge, which is said to structure only concepts, prior to any morphological representation? Imagine first of all that you are a child again, and, presuming that your childhood was at all like mine, that you're on a school trip to the woods. You have in your hands a sheet of paper and a wax crayon, and you walk up to a tree, hold the paper against the trunk, and rub the crayon across the sheet. What you now hold in your hand is a two-dimensional picture of the three-dimensional trunk that was underneath. Of course, even though it is two-dimensional, there is sufficient contrast in light and depth of colour that you can look at it and accurately reconstruct an idea of how the tree must have been. It is a property of our visual systems, as with any painting or photograph, that we can look at a two-dimensional object and *see* it as three-dimensional. However, just because your tree rubbing has the appearance of three-dimensionality to you, you do not conclude that it must have been produced by a three-dimensional process. Whatever constructed the tree's trunk from carbon and nutrients in the soil did not construct your picture from the wax crayon. You placed against the tree an independent interface, and applied a different substance to represent the tree in lesser dimensions.

In the mental woodlands of our language faculty, we have syntactic trees, which are logical forms constituted of concepts, that are two-dimensional, and somehow they correspond to sequences of morphemes. Of course, when we produce an acoustic signal, the signal is entirely one-dimensional, though we are capable of identifying the two-dimensional mental object that it represents. Perceiving this correspondence, however, does not mean that the substance of the one-dimensional representation must be brought into sequence by the same process that is responsible for the two-dimensional structure it represents. Instead, it is perfectly possible that the two-dimensional object has a prior, independent existence, and the function of language—the utterance-idea mapping—is to hold up a one-dimensional interface against that structure and cover it with morpho-phonological substance in ways that are highly variable across languages. This would in part explain why it is that words do not straightforwardly associate with concepts, and it would ultimately support the view of Fodor (1998) that language is a non-compositional representation of a compositional Mentalese. It would moreover defuse the criticisms of Fodor's position in Pietroski 2006, as there would be no second morpho-syntax to account for, independent of the syntax of Mentalese—the structure of morpheme sequences comes not from a syntactic operation, but from an externalisation module. There are of course many intricacies in cross-linguistic variation that it would take detailed study to reconcile with these suggestions, but it seems more and more that independent developments in pragmatics, semantics, and syntax are converging to make this a possibility worth serious attention.

References

Ackermann, Hermann, Steffen R. Hage & Wolfram Ziegler. 2014. Brain mechanisms of acoustic communication in humans and nonhuman primates: An evolutionary perspective. *Behavioral and Brain Sciences* 37(6). 529–604.

Anderson, Stephen R. 2013. What is special about the human language faculty and how did it get that way? In Rudolf Botha & Martin Everaert (eds.), *The Evolutionary Emergence of Language: Evidence and Inference*, 18–41. Oxford: Oxford University Press.

Armstrong, Sharon Lee, Lila R. Gleitman & Henry Gleitman. 1983. What some concepts might not be. *Cognition* 13(3). 263–308.

Bach, Emmon. 1976. An extension of classical transformational grammar. In Jerry M. Sadock (ed.), *Problems in Linguistic Metatheory*, 183–224. East Lansing: Department of Linguistics, Michigan State University.

Baker, Mark C. 2008. The macroparameter in a microparametric world. In Theresa Biberauer (ed.), *The Limits of Syntactic Variation*, 351–373. Amsterdam: John Benjamins.

Berwick, Robert C. & Noam Chomsky. 2016. *Why Only Us: Language and Evolution*. Cambridge, MA: MIT Press.

Berwick, Robert C., Kazuo Okanoya, Gabriel J. L. Beckers & Johan J. Bolhuis. 2011. Songs to syntax: The linguistics of birdsong. *Trends in Cognitive Sciences* 15(3). 113–121.

Bickerton, Derek. 2014. *More Than Nature Needs: Language, Mind, and Evolution*. Cambridge, MA: Harvard University Press.

Boeckx, Cedric. 2013. Biolinguistics: Forays into human cognitive biology. *Journal of Anthropological Sciences* 91. 63–89.

Boeckx, Cedric. 2015. *Elementary Syntactic Structures: Prospects of a Feature-Free Syntax*. Cambridge: Cambridge University Press.

Boeckx, Cedric. 2017. A conjecture about the neural basis of recursion in light of descent with modification. *Journal of Neurolinguistics* 43. 193–198.

Boeckx, Cedric & Víctor M. Longa. 2011. Lenneberg's views on language development and evolution and their relevance for modern biolinguistics. *Biolinguistics* 5(3). 254–273.

Bolker, Jessica A. & Rudolf A. Raff. 1996. Developmental genetics and traditional homology. *BioEssays* 18(6). 489–494.

Borer, Hagit. 2005a. *Structuring Sense, Vol. 1: In Name Only*. Oxford: Oxford University Press.

Borer, Hagit. 2005b. *Structuring Sense, Vol. 2: The Normal Course of Events*. Oxford: Oxford University Press.

Borer, Hagit. 2013. *Structuring Sense, Vol. 3: Taking Form*. Oxford: Oxford University Press.

Bouchard, Denis. 2013. *The Nature and Origin of Language*. Oxford: Oxford University Press.

Brentano, Franz Clemens. 1874. *Psychologie vom empirischen Standpunkt*, vol. 1. Leipzig: Duncker & Humblot.

Brody, Michael. 2000. Mirror theory: Syntactic representation in perfect syntax. *Linguistic Inquiry* 31(1). 29–56.

Burton-Roberts, Noel & Geoffrey Poole. 2006. 'Virtual conceptual necessity', feature-dissociation and the saussurian legacy in generative grammar. *Journal of Linguistics* 42(3). 575–628.

Butler, Ann B. & William M. Saidel. 2000. Defining sameness: Historical, biological, and generative homology. *BioEssays* 22. 846–853.

Byrne, Richard W., Erica Cartmill, Emilie Genty, Kirsty E. Graham, Catherine Hobaiter & Joanne Tanner. 2017. Great ape gestures: Intentional communication with a rich set of innate signals. *Animal Cognition* 20(4). 755–769.

Callaerts, Patrick, Georg Halder & Walter J. Gehring. 1997. PAX-6 in development and evolution. *Annual Review of Neuroscience* 20(1). 483–532.

Carruthers, Peter. 2002. The cognitive functions of language. *Behavioral and Brain Sciences* 25(6). 657–674.

Carston, Robyn. 2002. *Thoughts and Utterances: The Pragmatics of Explicit Communication*. Oxford: Blackwell.

Carston, Robyn. 2013. Word meaning, what is said, and explicature. In Carlo Penco & Filippo Domaneschi (eds.), *What is Said and What is Not: The Semantics/Pragmatics Interface*, 175–203. Stanford, CA: CSLI Publications.

Catchpole, C. K. & P. J. B. Slater. 2008. *Bird Song: Biological Themes and Variations*. Cambridge: Cambridge University Press 2nd edn.

ten Cate, Carel & Kazuo Okanoya. 2012. Revisiting the syntactic abilities of non-human animals: Natural vocalizations and artificial grammar learning. *Philosophical Transactions of the Royal Society of London B: Biological Sciences* 367(1598). 1984–1994.

Chen, Feng-Chi & Wen-Hsiung Li. 2001. Genomic divergences between humans and other hominoids and the effective population size of the common ancestor of humans and chimpanzees. *American Journal of Human Genetics* 68(2). 444–456.

Cheney, Dorothy L. & Robert M. Seyfarth. 2008. *Baboon Metaphysics: The Evolution of a Social Mind*. Chicago: University of Chicago Press.

Chomsky, Noam. 1965. *Aspects of the Theory of Syntax*. Cambridge, MA: MIT Press.

Chomsky, Noam. 1986. *Knowledge of Language: Its Nature, Origin and Use*. New York: Praeger.

Chomsky, Noam. 2000. *New Horizons in the Study of Language and Mind*. Cambridge: Cambridge University Press.

Chomsky, Noam. 2004. Beyond explanatory adequacy. In Adriana Belletti (ed.), *Structures and Beyond: The Cartography of Syntactic Structures*, 104–131. Oxford: Oxford University Press.

Chomsky, Noam. 2005. Three factors in language design. *Linguistic Inquiry* 36(1). 1–22.

Chomsky, Noam. 2010. Some simple evo devo theses: How true might they be for language? In Richard K. Larson, Viviane Déprez & Hiroko Yamakido (eds.), *The Evolution of Human Language: Biolinguistic Perspectives*, 45–62. Cambridge: Cambridge University Press.

Cinque, Guglielmo. 1999. *Adverbs and Functional Heads: A Cross-Linguistic Perspective*. Oxford: Oxford University Press.

Connolly, Andrew C., Jerry A. Fodor, Lila R. Gleitman & Henry Gleitman. 2007. Why stereotypes don't even make good defaults. *Cognition* 103(1). 1–22.

Corballis, Michael C. 2017. The evolution of language: Sharing our mental lives. *Journal of Neurolinguistics* 43. 120–132.

Davidson, Donald. 1967. The logical form of action sentences. In Nicholas Rescher (ed.), *The Logic of Decision and Action*, 81–95. Pittsburgh: University of Pittsburgh Press.

Deacon, Terrence W. 1997. *The Symbolic Species: The Co-Evolution of Language and the Brain*. New York: W. W. Norton & Company.

Deacon, Terrence W. 2003. Multilevel selection in a complex adaptive system: The problem of language origins. In Bruce H. Weber & David J. Depew (eds.), *Evolution and Learning: The Baldwin Effect Reconsidered*, 81–106. Cambridge, MA: MIT Press.

Dennett, Daniel C. 1971. Intentional systems. *The Journal of Philosophy* 68(4). 87–106.

Dennett, Daniel C. 1983. Intentional systems in cognitive ethology: The "Panglossian paradigm" defended. *Behavioral and Brain Sciences* 6(3). 343–390.

Dennett, Daniel C. 1987. *The Intentional Stance*. Cambridge, MA: MIT Press.

Dickinson, W. J. 1995. Molecules and morphology: Where's the homology? *Trends in Genetics* 11(4). 119–121.

Dupré, John. 1996. The mental lives of nonhuman animals. In Marc Bekoff & Dale Jamieson (eds.), *Readings in Animal Cognition*, 323–336. Cambridge, MA: MIT Press.

Fitch, W. Tecumseh. 2010. *The Evolution of Language*. Cambridge: Cambridge University Press.

Fitch, W. Tecumseh. 2011. "Deep homology" in the biology and evolution of language. In Anna Maria Di Sciullo & Cedric Boeckx (eds.), *The Biolinguistic Enterprise: New Perspectives on the Evolution and Nature of the Human Language Faculty*, 135–166. Oxford: Oxford University Press.

Fitch, W. Tecumseh. 2017. On externalization and cognitive continuity in language evolution. *Mind & Language* 32(5). 597–606.

Fitch, W. Tecumseh, Bart de Boer, Neil Mathur & Asif A. Ghazanfar. 2016. Monkey vocal tracts are speech-ready. *Science Advances* 2. doi:10.1126/sciadv.1600723.

Fodor, Jerry A. 1975. *The Language of Thought*. Cambridge, MA: Harvard University Press.

Fodor, Jerry A. 1998. *Concepts: Where Cognitive Science Went Wrong*. Oxford: Oxford University Press.

Fodor, Jerry A. 2008. *LOT 2: The Language of Thought Revisited*. Oxford: Oxford University Press.

Gallistel, C. R. 2011. Prelinguistic thought. *Language Learning and Development* 7(4). 253–262.

Gleitman, Lila. 1990. The structural sources of verb meanings. *Language Acquisition* 1(1). 3–55.

Gould, Stephen J. & Richard C. Lewontin. 1979. The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society B: Biological Sciences* 205(1161). 581–598.

Grice, H. P. 1957. Meaning. *The Philosophical Review* 66(3). 377–388.

Grice, H. P. 1969. Utterer's meaning and intention. *The Philosophical Review* 78(2). 147–177.

Hall, Brian K. 1989. Genetic and epigenetic control of vertebrate embryonic development. *Netherlands Journal of Zoology* 40(1). 352–361.

Halle, Morris & Alec Marantz. 1993. Distributed morphology and the pieces of inflection. In Kenneth Hale & S. J. Keyser (eds.), *The View from Building 20*, 111–176. Cambridge, MA: MIT Press.

Hauser, Marc D., Noam Chomsky & W. Tecumseh Fitch. 2002. The faculty of language: What is it, who has it, and how did it evolve? *Science* 298(5598). 1569–1579.

Hawkins, John A. 2004. *Efficiency and Complexity in Grammars*. Oxford: Oxford University Press.

Higham, James P. 2014. How does honest costly signaling work? *Behavioral Ecology* 25(1). 8–11.

Hockett, Charles F. 1960. The origin of speech. *Scientific American* 203(3). 88–96.

Holmberg, Anders. 2010. Parameters in minimalist theory: The case of Scandinavian. *Theoretical Linguistics* 36(1). 1–48.

Hornstein, Norbert. 2009. *A Theory of Syntax*. Cambridge: Cambridge University Press.

Hornstein, Norbert & Paul Pietroski. 2009. Basic operations: Minimal syntax-semantics. *Catalan Journal of Linguistics* 8. 113–139.

Hurford, James R. 2007. *The Origins of Meaning*. Oxford: Oxford University Press.

Jackendoff, Ray. 2002. *Foundations of Language: Brain, Meaning, Grammar, Evolution*. Oxford: Oxford University Press.

Jackendoff, Ray & Eva Wittenberg. 2014. What you can say without syntax: A hierarchy of grammatical complexity. In Frederick J. Newmeyer & Laurel B. Preston (eds.), *Measuring Grammatical Complexity*, 65–82. Oxford: Oxford University Press.

Julien, Marit. 2002. *Syntactic Heads and Word Formation*. Oxford: Oxford University Press.

Kayne, Richard S. 1994. *The Antisymmetry of Syntax*. Cambridge, MA: MIT Press.

Krakauer, John W., Asif A. Ghazanfar, Alex Gomez-Marin, Malcolm A. MacIver & David Poeppel. 2017. Neuroscience needs behavior: Correcting a reductionist bias. *Neuron* 93(3). 480–490.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York: John Wiley & Sons.

Lenneberg, Eric H. 1969. On explaining language. *Science* 164(3880). 635–643.

Lieberman, Philip. 2002. On the nature and evolution of the neural bases of human language. *Yearbook of Physical Anthropology* 45. 36–62.

Lieberman, Philip. 2007. The evolution of human speech: Its anatomical and neural bases. *Current Anthropology* 48(1). 39–66.

Lieberman, Philip. 2015. Language did not spring forth 100,000 years ago. *PLOS Biology* 13(2). doi:10.1371/journal.pbio.1002064.

Lieberman, Philip, Dennis H. Klatt & William H. Wilson. 1969. Vocal tract limitations on the vowel repertoires of rhesus monkey and other nonhuman primates. *Science* 164(3884). 1185–1187.

Lohndal, Terje. 2014. *Phrase Structure and Argument Structure: A Case Study of the Syntax-Semantics Interface*. Oxford: Oxford University Press.

Marler, Peter, Christopher S. Evans & Marc D. Hauser. 1992. Animal signals: Motivational, referential, or both? In Hanuš Papoušek, Uwe Jürgens & Mechthild Papoušek (eds.), *Nonverbal Vocal Communication: Comparative and developmental approaches*, 66–86. Cambridge: Cambridge University Press.

Marr, David. 1982. *Vision: A Computational Investigation into the Human Representation and Processing of Visual Information*. San Francisco: W. H. Freeman and Company.

Maynard Smith, John & David Harper. 2003. *Animal Signals*. Oxford: Oxford University Press.

Maynard Smith, John & Eörs Szathmáry. 1997. *The Major Transitions in Evolution*. Oxford: Oxford University Press.

Mayr, Ernst. 1982. *The Growth of Biological Thought*. Cambridge, MA: Harvard University Press.

Müller, Gerd B. & Günter P. Wagner. 1996. Homology, *Hox* genes, and developmental integration. *American Zoologist* 36(1). 4–13.

Nagel, Thomas. 1974. What is it like to be a bat? *The Philosophical Review* 83(4). 435–450.

Newmeyer, Frederick J. 1980. *Linguistic Theory in America: The First Quarter-Century of Transformational Generative Grammar*. New York: Academic Press.

Nowak, Martin A. & David C. Krakauer. 1999. The evolution of language. *Proceedings of the National Academy of Sciences of the United States of America* 96(14). 8028–8033.

Patterson, Nick, Daniel J. Richter, Sante Gnerre, Eric S. Lander & David Reich. 2006. Genetic evidence for complex speciation of humans and chimpanzees. *Nature* 441(7097). 1103–1108.

Pettito, Laura-Ann. 2005. How the brain begets language. In James McGilvray (ed.), *The Cambridge Companion to Chomsky*, 84–101. Cambridge: Cambridge University Press.

Pfenning, Andreas R., Erina Hara, Osceola Whitney, Miriam V. Rivas, Rui Wang, Petra L. Roulhac, Jason T. Howard, M. Arthur Moseley, J. Will Thompson, Erik J. Soderblom, Atsushi Iriki, Masaki Kato, M. Thomas P. Gilbert, Guojie Zhang, Trygve Bakken, Angie Bongaarts, Amy Bernard, Ed Lein, Claudio V. Mello, Alexander J. Hartemink & Erich D. Jarvis. 2014. Convergent transcriptional specializations in the brains of humans and song-learning birds. *Science* 346(6215). doi:10.1126/science.1256846.

Pietroski, Paul. 2005. *Events and Semantic Architecture*. Oxford: Oxford University Press.

Pietroski, Paul. 2006. Character before content. In Judith Thomson & Alex Byrne (eds.), *Content and Modality: Themes from the Philosophy of Robert Stalnaker*, 34–60. Oxford: Oxford University Press.

Pinker, Steven & Paul Bloom. 1990. Natural language and natural selection. *Behavioral and Brain Sciences* 13(4). 707–784.

Pinker, Steven & Ray Jackendoff. 2005. The faculty of language: What's special about it? *Cognition* 95(2). 201–236.

Poeppel, David & David Embick. 2005. Defining the relation between linguistics and neuroscience. In Anne Cutler (ed.), *Twenty-first Century Psycholinguistics: Four Cornerstones*, 103–118. Mahwah, NJ: Erlbaum.

Premack, David & Guy Woodruff. 1978. Does the chimpanzee have a theory of mind? *Behavioral and Brain Sciences* 1(4). 515–526.

Preminger, Omer. 2014. *Agreement and its Failures*. Cambridge, MA: MIT Press.

Quine, W. V. O. 1960. *Word and Object*. Cambridge, MA: MIT Press.

Ramchand, Gillian & Peter Svenonius. 2014. Deriving the functional hierarchy. *Language Sciences* 46. 152–174.

Recanati, François. 2004. *Literal Meaning*. Cambridge: Cambridge University Press.

Richmond, Brian G., David R. Begun & David S. Strait. 2001. Origin of human bipedalism: The knuckle-walking hypothesis revisited. *Yearbook of Physical Anthropology* 44. 70–105.

Rogers, James & Geoffrey K. Pullum. 2011. Aural pattern recognition and the subregular hierarchy. *Journal of Logic, Language and Information* 20(3). 329–342.

Ross, Don, Andrew Brook & David Thompson (eds.). 2000. *Dennett's Philosophy: A Comprehensive Assessment*. Cambridge, MA: MIT Press.

Saidel, William M. & Ann B. Butler. 1997. An atypical diencephalic nucleus in actinopterygian fishes: Visual connections and sporadic phylogenetic distribution. *Neuroscience Letters* 229(1). 13–16.

Sandler, Wendy, Mark Aronoff, Irit Meir & Carol Padden. 2011. The gradual emergence of phonological form in a new language. *Natural Language and Linguistic Theory* 29(2). 503–543.

Scott-Phillips, Thom. 2014. *Speaking Our Minds: Why Human Communication is Different, and how Language Evolved to Make it Special*. New York: Palgrave Macmillan.

Searle, John R. 1983. *Intentionality: An Essay in the Philosophy of Mind*. Cambridge: Cambridge University Press.

Searle, John R. 2009. What is language? Some preliminary remarks. *Etica & Politica / Ethics & Politics* 11(1). 173–202.

See, Adam. 2014. Reevaluating chimpanzee vocal signals: Toward a multimodal account of the origins of human communication. In Marco Pina & Nathalie Gontier (eds.), *The Evolution of Social Communication in Primates: A Multidisciplinary Approach*, 195–215. New York: Springer.

Seyfarth, Robert M., Dorothy L. Cheney & Peter Marler. 1980. Monkey responses to three different alarm calls: Evidence of predator classification and semantic communication. *Science* 210(4471). 801–803.

Short, T. L. 2007. *Peirce's Theory of Signs*. Cambridge: Cambridge University Press.

Sperber, Dan & Deirdre Wilson. 1986. *Relevance: Communication and Cognition*. Oxford: Blackwell.

Starke, Michal. 2009. Nanosyntax: A short primer to a new approach to language. *Nordlyd* 36(1). 1–6.

Svenonius, Peter. 2016. Spans and words. In Heidi Harley & Daniel Siddiqi (eds.), *Morphological Metatheory*, 199–220. Amsterdam: John Benjamins.

Számadó, Szabolcs & Eörs Szathmáry. 2006. Selective scenarios for the emergence of natural language. *Trends in Ecology & Evolution* 21(10). 555–561.

Tallerman, Maggie. 2007. Did our ancestors speak a holistic protolanguage? *Lingua* 117(3). 579–604.

Tallerman, Maggie. 2009. The origins of the lexicon: How a word-store evolved. In Rudolf Botha & Chris Knight (eds.), *The Prehistory of Language*, 181–200. Oxford: Oxford University Press.

Tallerman, Maggie. 2014. No syntax saltation in language evolution. *Language Sciences* 46. 207–219.

Tattersall, Ian. 1998. *The Origin of the Human Capacity*. New York: American Museum of Natural History.

Tattersall, Ian. 2014. Communication and human uniqueness. In Marco Pina & Nathalie Gontier (eds.), *The Evolution of Social Communication in Primates: A Multidisciplinary Approach*, 219–227. New York: Springer.

Tomasello, Michael. 2008. *Origins of Human Communication*. Cambridge, MA: MIT Press.

Tomasello, Michael, Malinda Carpenter, Josep Call, Tanya Behne & Henrike Moll. 2005. Understanding and Sharing Intentions: The Origins of Cultural Cognition. *Behavioral and Brain Sciences* 28(5). 675–735.

Wagner, Günter P. 1989. The biological homology concept. *Annual Review of Ecology and Systematics* 20(1). 51–69.

Wagner, Günter P. 2014. *Homology, Genes, and Evolutionary Innovation*. Princeton: Princeton University Press.

Wagner, Günter P. 2016. What is "homology thinking" and what is it for? *Journal of Experimental Zoology B: Molecular and Developmental Evolution* 326(1). 3–8.

Wake, David B. 1991. Homoplasy: The result of natural selection, or evidence of design limitations? *American Naturalist* 138(3). 543–567.

Wake, David B. 1999. Homoplasy, homology and the problem of 'sameness' in biology. In Gregory R. Bock & Gail Cardew (eds.), *Novartis Foundation Symposium 222: Homology*, 25–46. New York: John Wiley & Sons.

Wallace, Alfred Russel. 1869. Sir Charles Lyell on geological climates and the origin of species. *Quarterly Review* (April). 359–392.

Watumull, Jeffrey, Marc D. Hauser & Robert C. Berwick. 2014. Conceptual and methodological problems with comparative work on artificial language learning. *Biolinguistics* 8. 120–129.

Wheeler, Brandon C. & Julia Fischer. 2012. Functionally referential signals: A promising paradigm whose time has passed. *Evolutionary Anthropology* 21(5). 195–205.

Callum Hackett
Newcastle University
School of English Literature,
Language, and Linguistics
Newcastle upon Tyne
NE1 7RU
United Kingdom

callum.hackett@newcastle.ac.uk

What's in (a) Label? Neural Origins and Behavioral Manifestations of Identity Avoidance in Language and Cognition

Evelina Leivada

The present work defends the idea that grammatical categories are not intrinsic to mergeable items, taking as a departure point Lenneberg's (1967, 1975) claim that syntactic objects are definable only contextually. It is argued that there are four different strands of inquiry that are of interest when one seeks to build an evolutionarily plausible theory of labels and operation Label: (i) linguistic constraints on adjacent elements of the same type such as Repetition/Identity Avoidance ([*XX]), (ii) data that flout these constraints ([XX]), (iii) disorders that raise questions as to whether the locus of impairment is a categorial feature per se, and (iv) operation Label as a candidate for human uniqueness. After discussing categorial identity through these perspectives, this work first traces the origins and manifestations of Identity Avoidance in language and other domains of human cognition, with emphasis on attention orienting. Second, it proposes a new processing principle, the Novel Information Bias, that (i) captures linguistic Identity Avoidance based on how the brain decodes types and tokens and (ii) explains the universal fact that generally the existence of adjacent occurrences of syntactically and/or phonologically identical tokens is severely constrained.

Keywords: attention; categories; Label; repetition avoidance/blindness

1. Introduction

Over the last five decades, linguists have observed a very interesting phenomenon: Elements of the same type are unlikely to occur in immediately adjacent positions; instead, they are usually mediated by other elements. This observation has been described in different, yet similar ways, in the linguistics literature: Obligatory Contour Principle (OCP; Odden 1986, Yip 1988), Identity Restrictions/Avoidance

---

I thank Patrick Trettenbrein for giving me the unique opportunity to participate in this Special Issue that celebrates the 50-year anniversary of the publication of Eric Lenneberg's *Biological Foundations of Language*. I am very grateful to Henk van Riemsdijk and Martin Prinzhorn for inviting me to present an early version of the ideas sketched here in the 2016 workshop 'What's in a Label? The categorial status of functional categories' in Arezzo. My gratitude extends to all the participants of that workshop for the helpful feedback, and especially, Norbert Corver and Jane Grimshaw. I also thank the audience of the AcqVA seminar in Trond-

ISSN 1450–3417

(Berent et al. 2012), Similarity Avoidance (Frisch et al. 2004), or Repetition Avoidance (Walter 2007) in phonology, Identity Avoidance (van Riemsdijk 2008), Distinctness (Richards 2006, 2010), or absence of X-within-X structures in syntax (Arsenijević & Hinzen 2012). In the syntactic domain, this is also the basis for anti-locality relations (Grohmann 2003). All these different designations essentially describe various versions of the same observation: adjacent elements of the same type or category are either constrained or prohibited (henceforth [*XX]), depending on how strong the ban is argued to be. The ban spans across not only domains of grammar, but languages too. As Berent et al. (2012) note, constraints on identity have been shown to generalize across both natural languages (Berent & Shimron 2003, Berent et al. 2001, 2004) and artificial ones (Marcus et al. 1999, Nevins 2010).

At the syntactic level, the postulation of such a ban that constrains or prohibits identity-adjacency requires *granting an identity* to X in [XX]. In other words, it requires the existence of categories. It has been recently argued that linguists today are in a position to observe how functional elements such as auxiliaries and determiners consistently appear in specific configurations that grant grammatical identity, but still our theory about them can *merely stipulate* the attested orders, with the hope to derive this stipulation in the future (Adger 2016 based on Grimshaw 1991). This is not the first time that this point is raised. Breheny & Adger argued that in

> the context of the current research programme the nature, number and order of functional projections is a descriptive explanandum rather than the explanation. It is a goal of current research to discover reasons why functional categories are structured as they appear to be in terms of interface properties. (Breheny & Adger 2005: 1674)

Overall, it seems that not adequate progress took place in the last decade for us to fully develop a theory of categories that has replaced stipulations and explananda with explanations.

Against this background, it will be argued that it is important to approach the process of 'granting identity' from different perspectives in order to understand what it corresponds to and how it is manifested cross-linguistically across cases of typical and atypical language development. The present work defends the idea that grammatical categories are not intrinsic to Mergeable Items (henceforth, MIs), building on Lenneberg's (1975) claim that syntactic objects are definable contextually. For Lenneberg, linguistic categories are to be understood as relationships, not as absolute constructs; an idea compatible with Ross' (1972) graded continuum of categories. If categorial features are not intrinsic to MIs, they can be eliminated from the Universal Grammar (UG) inventory. Eliminating them and subsequently approaching Label as an extrasyntactic operation that falls within the 'third-factor'

---

heim, and particularly Terje Lohndal and Dave Kush, for commenting on some of the material presented here. For helpful discussions and comments, I thank Antonio Benítez-Burraco, Antonio Fábregas, Kleanthes Grohmann, Maria Kambanaros, Björn Lundquist, Anna Martinez-Alvarez, Maria Mystakidou, Javi Ramírez-Fernández, Marit Westergaard, and two anonymous reviewers. I also thank Maud Westendorp for her help with Dutch data. I acknowledge funding support for my research from European Union's Horizon 2020 research and innovation programme under the Marie Skłodowska-Curie grant agreement N^{o} 746652.

domain would give rise to a picture that is compatible with a highly desirable, according to Chomsky (2007), bottom-up approach to UG. This, in effect, will result to a reduced degree of linguistic specificity that needs to be explained from an evolutionary perspective.

The aim of this work is twofold. First, to construct a theory of labels (understood for the purposes of the present work as containing only categorial information) and operation Label through bringing together different strands of inquiry: (i) linguistic constraints on adjacent tokens of the same type (e.g., van Riemsdijk's 2008 'Identity Avoidance'), (ii) data that flout these constraints (Leivada 2015), (iii) neurolinguistic arguments for category-specific impairments that raise questions as to whether the locus of the impairment is a categorial feature per se or parts of the information attached to the category at different levels of linguistic analysis (Barner & Bale 2002, Tsimpli 2013), and (iv) the operation Label as the key evolutionary novelty that distinguishes human language from the communication systems of other species through the development of grammatical categories which provided "a new mode of cognizing" (Hinzen & Sheehan 2013: 73; see also Hornstein 2009, Hornstein & Pietroski 2009, Murphy 2015a, and Goucha et al. 2017 on labelling as a candidate for human uniqueness). The second aim of this work is to show that cross-linguistic constraints on identity-adjacency are neither syntactic, nor linguistic in nature. They are the linguistic reflection of a general, cognitive bias that filters out multiple tokens of the same type when these occur in adjacent positions. In other words, it is true that elements of the same type do not generally appear in adjacent positions, but not because this would cause a derivation crash as it has been argued in the literature (Perlmutter 1971, Ross 1972, van Riemsdijk 1998, Grohmann 2000, Richards 2010), as the examples provided in section 2.2 show.

The next section deals with labels and operation Label. After defining [*XX] and analyzing counterexamples, the last two subsections will approach Label from a neurological and an ethological perspective respectively. Section 3 traces the origins of Identity Avoidance (also known as Repetition Avoidance) in cognition, linking anti-identity to how the brain decodes identical tokens, and presents the Novel Information Bias together with the rationale behind it.[1]

2. Labels and Operation Label

When reading Lewis Carroll's *Jabberwocky* for the first time, speakers of English understand that 'gyre' and 'gimble' in 'Twas brillig, and the slithy toves/Did gyre and gimble in the wabe' are verbs. Even if they do not know the meaning of these words, they are still able to determine their category based on grammatical context. It seems that these MIs became verbs the moment they were placed in the right environment. They can become nouns too (e.g., 'Did wabe and gimble in the gyre') and English speakers would immediately interpret them as such, if they see them appearing in a 'nouny' context.[2]

---

[1] The terms 'Identity Avoidance, 'Repetition Avoidance' and 'anti-identity' are used interchangeably throughout this work. The first term is more prominently used in linguistics, whereas the second is mostly used in the neurocognitive literature.

[2] The connection between Jabberwocky and an exocentric interpretation of categories à la Lenneberg is due to Laka (2013). In her words,

Eric Lenneberg has repeatedly argued that the categorization process is flexible and linguistic categories are not absolute constructs, but relationships that are definable only contextually (1967, 1975). In his words,

> I think it is a mistake to look at categories such as noun phrase, noun, verb, adjective, and so on, as absolute constructs. Instead, these terms are the names of *relations between concatenated words*. A word such as 'green' is no more an adjective, a verb, a noun, or a noun phrase when it appears in isolation than it is a subject or a predicate.
>
> (Lenneberg 1975: 24; emphasis added)

> Let us consider more closely the process of categorization that underlies semantics. Is it possible to characterize this cognitive activity any further? For instance, if the classification criteria are not usually physical dimensions, what are they? *The most outstanding feature of the "criteriality" is its great flexibility*. Sometimes the criterion is primarily one of "use that man makes of the objects"; sometimes it is a given aspect; sometimes a certain emotional state that all objects in that class may elicit in the viewer. *Any one category is not definable by only one, consistently applied criterion*. (Lenneberg 1967: 332–333; emphasis added)

In Minimalism, Label is defined as the operation that grants identification of the category of an MI (Chomsky 2013): Following the union of two MIs through Merge, Label gives an identity to the newly formed MI. The syntactic configurations between non-atomic, complex MIs are often viewed as *relational* and interpretable at the interfaces (Hornstein & Nunes 2008, Chomsky 2013). The important question is whether this identity is intrinsic to atomic, non-complex MIs. In other words, is a noun something that is interpreted in a 'nouny' way at the interfaces (Narita 2011) or does it have the 'nouny' feature already grafted on to it in syntax? Also, is Identity Avoidance at the syntactic level the result of a ban that operates within narrow syntax proper or are the attested constraints on identity-adjacency the result of the low information value that repeated instances of 'nouny' elements would have once they are 'read' at the interfaces? These questions will be dealt with in the next two subsections.

2.1. Where Does Identity Avoidance Come From?

When discussing labeling, many linguists have put forth the existence of a process or algorithm that interprets MIs contextually, at the interfaces (Pesetsky & Torrego 2004, Hornstein & Nunes 2008, Chomsky 2013). Others have argued in favor of certain MIs entering the derivation already endowed with categorial features (Cinque 1999, Rizzi 1997, Panagiotidis & Grohmann 2009, Panagiotidis 2011) or acquiring

---

> [t]he full meaning of those words eludes us, but the grammatical scaffold set by the recognizable function words allows us to make sense of the propositional structure of the poem, identifying nonsense words that speak of entities, properties and events. Very importantly, function words tell us how they are related.
>
> (Laka 2013)

them in the course of the derivation through 'inheriting' them from the material they are attached to (e.g., the 'Categorial Identity Thesis' according to which functional heads that extend the projection of a lexical head share the categorial status of that lexical head; van Riemsdijk 1998, Grimshaw 2005).

When it comes to Identity Avoidance in language, the precise target of the anti-identity ban (i.e. whether the ban and its haplological effects target items that are morphophonologically *and* vs. or syntactically identical) is elusive and varies across proposals (cf. (1)–(9)). Roughly, the relevant proposals can be split into three groups. In the first group, the interface that figures more prominently in the discussions of the ban on adjacent elements of the same syntactic and/or morphophonological type is the articulatory-perceptual one (Phonetic Form, PF), as in (1)–(5).

(1) I take it that at least some instances of haplology are purely morphophonological. For example, Bošković [(2002)] and Bošković & Nunes (2007) provide evidence that the ban against identical wh-phrases in multiple wh-fronting languages is a purely PF-matter. (Boeckx 2008: 113)

(2) In our account, haplology is either phonological or motivated by independent syntactic principles having to do with the size of the complement of a lexical head or with the functional hierarchy. (Fábregas 2014: 37)

(3) The Syntactic OCP: Multiple elements with an identical morphophonological specification are disallowed in the same Spell-Out domain at PF. (Hiraiwa 2010)

(4) Double Determiner Filter: *[D1…D2] where no lexical head intervenes between D1 and D2. Determiner Deletion: Delete one of two phonologically adjacent determiners. (Davis 2010: 23)

(5) Falling again within the realm of reduplication that 'accidentally' creates an exact copy—without intending to—are cases of consonantal fixed segmentism and spontaneous 'avoidance' that occur with echo word formation and related phenomena. For example, English *shm*-reduplication results in a reduplicant whose onset is *shm-*, e.g. flowers, shmowers. Nonetheless, as a large-scale survey by Nevins & Vaux (2003) shows, speakers do not tolerate such reduplication when the base itself begins with this sequence. Thus, the name *schmidt* must undergo alternate attempts, such as *schmidt-shpidt*, *schmidt-flidt*, etc. Surprisingly, this phenomenon extends to practically every case of echo reduplication that exists […]. (Nevins 2012: 109)

In the second group, the role of PF is diminished and [*XX] is described as a purely syntactic/grammatical constraint ((6)–(9)).

(6) *XX sequences of identical functional heads are ill-formed. (Grimshaw 1997: 170)

(7) Data concerning complementizer substitution in Dutch show that in syntax, as in morphology, a condition that bans adjacent identical heads can have the effect that one head is replaced by a counterpart that has the wrong fea-ture specification for the syntactic context. (Ackema 2001: 725)

(8) If a linearization statement <α, α> is generated, the derivation crashes. [. . .] Distinctness effects are crucially sensitive to syntactic structure, and are not about linear adjacency. (Richards 2006: 4, 12)

(9) [T]here is a grammatical (non-semantic) constraint in English that prohibits double negation, dubbed *NEG NEG [. . .]. (Collins 2016)

The third group is the smallest one and consists of few studies that do not approach the ban on identity as linguistic (either syntax- and/or PF-induced), but as the outcome of a more general principle of human cognition that may find application in language, the way it finds application in other cognitive domains ((10)–(13)).

(10) [. . .] Identity Avoidance is a general principle of biological organization: its effect can be detected at both interfaces, PF and LF. (van Riemsdijk 2008: 242)

(11) It seems, then, that repetition avoidance is universal in the sense that it seems to occur in virtually every human language, in every grammatical subdomain, and in other cognitive domains as well. (Walter 2007: 6)

(12) Rather, identity is created in a number of ways [. . .] but in some cases, it may be disfavored for reasons that range from perceptual difficulty to articulatory fatigue. (Samuels 2014: 357)

(13) Richard's (2010) Distinctness Condition, prohibiting the presence of multiple lexical units of the same label within a single phase complement, may be the consequence of how many distinct rhythms it is possible to couple in specific actions (Boeckx 2013). These XX-like structures (e.g., structures containing multiple phase-internal nouns such as *John Mary ate apples) may be ungrammatical because of the oscillatory patterns local language regions can sustain. (Murphy 2015b: 13)[3]

In line with the main claim of the third group, in this work I will defend the idea that Identity Avoidance is not linguistic in origin, but boils down to general cognitive architecture, and more specifically to a bias that I call *Novel Information Bias*. In what follows, I present grammatically licit, [*XX]-violating structures. Then, I approach labels and labelling from different perspectives with the aim to bring together all the necessary pieces in order to evaluate Novel Information Bias and the rationale behind it in section 3.

2.2. *Licit [XX]: Flouting Identity Avoidance*

Identity Avoidance is a well-observed condition that operates at different levels of linguistic analysis. At the syntactic level, many studies analyze 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 2014). However, despite the fact that languages indeed employ a range of grammatical options in order to avoid [XX], it would be wrong

---

[3] For another detailed analysis of anti-identity captured in terms of oscillatory brain rhythms, see Ramírez Fernández (2015).

to conclude that [XX] patterns do not exist in various spoken and signed languages (Leivada 2015).

The very fact that such patterns are possible suggests that, contra what has been repeatedly claimed in the relevant literature, *there is no [*XX] ban in syntax* that prevents two MIs of the same category from being merged together. More importantly, if Lenneberg's theory is on the right track, then syntax/Merge *cannot* even 'read' whether the two MIs that it puts together belong to the same category or not, because a category is only defined *post*-syntactically, in relation to the grammatical or discourse context.[4] I argue that this explains why [XX] patterns like the ones in (14)–(16) are licit and eventually surface cross-linguistically, even despite the fact that repeated occurrences of the same item obviously carry low information value.

(14) *European Portuguese*

Acho **que** amanhã **que** a Ana **que** vai conseguir acabar o trabalho.
think.1SG that tomorrow that the Ana that will manage finish the assignment

'I think tomorrow Ana will manage to finish the assignment.'
(Mascarenhas 2007: 10)

(15) *Greek*

a. **Kapçi** (**kapçi**) kala ϑa kanun na citun ti ðulia tus.
some some well FUT do.3PL SUBJ see.3PL the work POSS
'Some people would do well to mind their own business.'
(Leivada 2015: 54)

b. **Merici** (**merici**) ðen katalavenun tipota.
some some NEG understand.3PL nothing
'Some people don't understand a thing.'

(16) *Dutch*

Ik heb het anders eens epresteerd om mijn schoenen
I have.1SG it otherwise once manage.PTCP to POSS shoes
verkeerd-**om**-**om** aan te trekken.
wrong-around-around on INF put.INF

'I once managed to put my shoes on the wrong way around.'
(Aelbrecht & den Dikken 2013: 41)

These examples show adjacent occurences of functional heads, not mediated by other functional categories. (14) features adjacent complementizers, (15) indefinite articles, and (16) prepositions. The syntactic analyses of these structures have

---

[4] This idea is also compatible with standard assumptions about the contextual definition of categories within the framework of Distributed Morphology. Consider, for instance, the following claim by Marantz:

> [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. (Marantz 1997: 215)

been already discussed in detail elsewhere (Mascarenhas 2007, Aelbrecht & den Dikken 2013, Leivada 2015), hence they will not be presented any further here. However, it is important to note that the repeated elements in (14)–(16) are not subject to analyses that treat the two identical tokens as belonging to different syntactic categories. In this sense, Identity Avoidance violations concern syntactic categorization. Similarly, Lenneberg's (1967) argument about the great flexibility and contextual definition of categories does not concern only semantic categories. This is evident in his later work:

> Although words are discrete entities, they represent or are the product of underlying continuous cognitive and physiological processes. These deeper continuities are reflected *in the "fuzzy" nature of semantic, syntactic, and phonological categories, making sharp, formal distinctions and decisions difficult*. (Lenneberg 1975: 17; emphasis added)

In relation to the examples in (14)–(16), the important conclusion that can be reached is that depending on which version of the ban in (1)–(9) one assumes, these examples should be illicit, as they flatly violate Identity Avoidance on both the syntactic and the phonological level, yet they are perfectly well-formed in the languages in which they belong.

Although these examples suggest that Identity Avoidance is not a syntactic ban, there is no doubt that generally the existence of adjacent occurrences of syntactically and/or phonologically identical MIs *is* severely constrained. Therefore, one can still assume that Identity Avoidance is indeed operative in language, but crucially not in the form that most studies suggest. More specifically, first it is not a *hard constraint* or a *ban*, but a *flexible bias*. [XX] tends to be infrequent in languages, but it is realized in a cross-linguistic manner, regardless of modality of externalization (see Leivada 2015 for more examples of [XX] in spoken and signed languages). Second, it is not a syntactic or linguistic bias. It may find application in language, but it has parallels in other domains of human cognition (Walter 2007). If one adopts Chomsky's (2005) proposal about the existence of three factors in language design, then Repetition Avoidance, not being specific to language, would fall in the third factor.[5]

Insofar, the above examples have shown that licit [XX] patterns can flout Identity Avoidance across languages and levels of analysis. However, they have not revealed much about labels and operation Label. Is X endowed with categorial features in syntax proper? The fact that syntax can put [XX] together can be taken to suggest that syntactic objects acquire their label contextually, at the interfaces, as Lenneberg (1975) and Chomsky (2013), among many others, have suggested. However, it could equally be the case that precisely because the anti-identity bias is a bias and not an absolute ban, it can ignore the categorial features that X is endowed with in syntax. In other words, [XX] patterns do not suffice to answer questions about whether nouns and verbs are interpreted as such at the interfaces or they have the corresponding 'nouny' and 'verby' features already grafted on to them in syntax. Moreover, [XX] patterns cannot provide alone a complete understanding of the op-

---

[5] Chomsky (2005) argued that there are three sets of factors in language design: 1) biological endowment specific to language (UG), 2) experience, and 3) principles of general cognition that are not specific to language.

eration Label. For these reasons, the next two subsections will approach labels and Label from the neurolinguistic and the ethological perspective, respectively.

2.3. A Neurolinguistic Approach to Categories

Neurolinguistic arguments for category-specific impairments raise questions as to whether the locus of impairment is a categorial feature per se or parts of the information attached to the target form at different levels of linguistic analysis. For instance, in the literature on aphasia and other pathological phenotypes, one finds many case studies that show disproportionate impairment of noun vs. verb retrieval since the mid-1980s (Crepaldi et al. 2011, Kambanaros & Grohmann 2015). One wonders whether this differential performance can be the result of a type of "feature blindness" (Gopnik 1990) that is sensitive to categorial features.

Barner & Bale (2002) were among the first to address the issue of categorial identity from a neurolinguistic perspective. Asking,

> [h]asn't it been shown that certain patients are selectively impaired for nouns or verbs? Doesn't this evidence mean that nouns and verbs must be marked in the lexicon? (Barner & Bale 2002: 775)

Barner & Bale (2002) adduced two important arguments in order to give a negative answer to the last question. First, they observed that even in pathologies that typically involve a noun-verb dissociation, it is usually seen that patients do not omit target forms altogether. For example, if the deficit is mainly found in verbs, what happens is that patients commonly produce a target verb in its bare infinitive form rather than the inflected, target form. This suggests that the locus of impairment boils down to morphological markers and not to categorial features. In other words, failure to produce the inflected form entails an impairment in accessing the *surface* representation of this form, at the level where morphophonological specification takes place. This explains why patients that are unable to produce a fully inflected form can still access and correctly report information on its grammatical features (for example, gender in the case of the patient with Alzeimer's disease reported in Hernández et al. 2007).

Defining the different levels of access, models of language processing have proposed that word retrieval is a process that can be divided into discrete phases: (a) lemma selection and (b) lexeme retrieval (Levelt 1989). The lemma is an abstract conceptual form without morphophonological specification. After lemma selection, lexeme retrieval takes place: the lexeme that corresponds to the selected lemma is morphophonologically specified. Levelt's (1989) model has been highly influential in neurolinguistics precisely because findings across pathologies suggest that a lemma/lexeme distinction is necessary. Mapping this model to what looks like a category-specific impairment, the difficulty lies in lexeme retrieval, not lemma selection. The noun-verb dissociation that is found in many studies usually boils down to atypical morphology manifested through impaired lexeme retrieval.

The second argument of Barner & Bale (2002) is that selective impairments that affect one category are often shown to be modality-specific too. They cite the case report of Caramazza & Hillis (1991) that involves a patient who, although impaired in her spoken production of target verbs, showed no problem writing the same words. This dissociation between modalities would not be possible if the locus of the impairment was the Verb category, as Caramazza & Hillis (1991) also note. If a categorial feature was truly impaired or inaccessible in this patient, how is it possible that in some modalities it appeared completely unaffected?

It has been recently argued that the question as to whether the category distinction itself is the driving force of the selective impairment of some categories or whether semantic and perceptual distinctions in nouns and verbs are responsible for the attested effects is not resolved yet (Tsimpli 2013). For this reason, I will review below three case studies that show selective impairment of some category with the aim to show that the attested impairment is not due to a category deficit, but to morphophonological realization at the surface level (i.e. lexeme retrieval).

Hernández et al. (2007) present the case of a Spanish-Catalan bilingual woman (LPM) with Alzheimer's disease whose linguistic production shows a category-specific deficit. In naming tasks, her performance was significantly better for verbs compared to nous. Hernández et al. (2007) present this as a case of a category-specific impairment, but it is important to highlight three crucial aspects of their results before evaluating the 'category-specific' nature of the deficit. First, LPM has access to semantic representations for both verbs and nouns as evidenced by her high performance in a word-picture matching task that used the same material as the naming task. Second, she does not show the noun-verb dissociation in comprehension tasks. Third, some of her errors consist of circumlocutions that reveal intact access to both verbs and nouns, although not always the target ones (e.g., target response: *tostadora* 'toaster', actual response: *sirve para tostar el pan* 'used to toast bread'). In this example, LPM makes a naming error in the 'noun' condition. The target noun is not produced, but another noun *is* used in the circumlocution she employs. How is it possible that LPM can produce this noun in a grammatical way if the N feature is missing from her repository due to a category-specific feature blindness? In sum, LPM seems to have trouble accessing the target lexemes. Her naming errors involve morphological paraphasias (e.g., target response: *planxa* 'iron', actual response: **planxadora*, a neologism) and this suggests that the locus of the impairment is not a categorial feature, but impaired access to morphophonological specification at the lexeme level. As verbs and nouns involve different morphological markers, different degrees of disruption are possible, hence the attested verb-noun dissociation. Similar results have been robustly found in other impaired phenotypes too (e.g., anomic aphasia; Kambanaros 2008).

Laiacona & Caramazza (2004) present two cases that show verb-noun dissociation: EA, a man suffering from herpes simplex encephalitis and MR, a woman who has suffered a stroke. EA showed a better performance in naming actions, whereas MR was better in naming objects. Laiacona & Caramazza argue that their results suggest that "a possible grammatical-specific deficit could be detected" (2004: 110). In these cases, too, there are indications that the locus of impairment is not a categorial feature. First, both EA and MR performed at ceiling (above 98 %) in the picture-word matching task. This suggests that they had access problems: they were not able to retrieve and produce the target word, but they were able to correctly select it and match it with a picture, once presented with it. Also, both EA and MR had problems with morphology: EA with irregular noun morphology (mainly producing the plural forms) and MR with verbal morphology both regular and irregular. Finally, notice that the lowest accuracy performance in the picture naming task was 42 % (EA: actions: 82 %, objects: 42 %; MR: actions: 70 %, objects: 90 %); a strikingly high percentage for somebody that has a deficient N feature. Overall, it seems that in these case studies too, one cannot straightforwardly witness a missing or impaired categorial feature.

To claim a neural basis, one needs to refer to case studies that show (semantic) category dissociations because of a discrete lesion in a specific brain area. This does not imply the need to find a one-to-one mapping (i.e. a linguistic primitive that corresponds to a unique disorder or maps onto a single brain area). Some loci of impairment at the neural level are, however, expected to be consistently present. Indeed, certain category-specific semantic deficits *have* been associated to brain regions. However, these refer to highly specific semantic distinctions, such as vegetables vs. fruits, and not to broad semantic distinctions that are based on *lexical category*, such as nouns vs. verbs. For instance, the case study presented in Levin et al. (2005) is that of a man who exhibited a language impairment following a left anterior thalamic infarction. His naming abilities were selectively impaired with some categories (e.g., tools, clothes, transportation, fruits) being better preserved than others (e.g., vegetables, toys, animals and body parts) in the visual modality. Crucially, much like the comprehension-production dichotomy observed earlier, here too a dichotomy is found between naming in animate and inanimate categories in the visual and tactile modalities, but not when the patient responded to auditory stimuli. In Barbeau & Giusiano (2003), another patient showed lesions located primarily in the left inferior temporal lobe, and his linguistic behaviour involved a dissociation between the manufactured vs. natural objects category. In other research, a specific type of nouns, that of manipulable objects, has been linked to lesions in the posterior middle temporal gyrus (Campanella et al. 2010).

The message that emerges is clear: Topographical differences in brain activation are *not* driven by broad semantic distinctions that are based on lexical category (e.g., nouns vs. verbs), but by finer semantic distinctions (e.g., concrete noun/verb vs. abstract noun/verb; Moseley & Pulvermüller 2014). These results suggest that categories that are based on lexical class are *theoretical constructs* and *not discrete primitives* that drive brain representation as such. A variety of different reasons support this claim. First, there is no confirmed discrete lesion pattern for a noun-verb dissociation; the attested dissociations are based on finer semantic distinctions and a categorial feature such as 'noun' is never consistently impaired. Second, the dissociation is not preserved across modalities, and it should be if the locus of impairment truly was a categorial feature. Third, apart from modality effects, task effects have also been observed (e.g., see Kambanaros 2014 on how the vulnerability of verbs in children with Specific Language Impairment shows up in naming tasks but not in connected speech).

The discussion has so far revolved around nouns and verbs because this dissociation has been the focus of attention in the relevant literature. However, if categorial feature blindness existed, it could conceivably apply to functional categories. Yet, there is no known case of a patient with a developmental or an acquired disorder that has suffered a total loss of a category (e.g., no nouns can be produced/comprehended) or that consistently misapplies labels in a way that results to the use of complementizers where one expects to see determiners or clitics when one expects modals, due to categorial feature blindness. Also, a sharp difference is consistently found between affected production and intact comprehension across patients and pathologies. These observations suggest that in studies that report a category-specific deficit, the locus of impairment is not the categorial feature per se, but part of the information attached to the target forms at the various phases of lexical access.

The conclusion to be drawn is that the neurolinguistics literature does not seem to offer cases in support of missing or damaged categorial features. This absence can raise concerns as to whether categorial features exist at all as separate entities in a feature inventory. Going back to Narita's (2011) question, seeing that an impaired noun feature does not exist as such in the findings of neurolinguistics paves the way for arguing that a noun is something that is interpreted in a 'nouny' way in relation to the environment in which it is encountered. This conclusion supports Lenneberg's (1967, 1975) claim that nouns, verbs, and other categories, are not absolute constructs, but *flexibly established* and *contextually defined relations* between words.

2.4. *Operation Label as the Key Evolutionary Novelty?*

The main aim so far was to show that labels are not intrinsic to MIs and constraints on identity-adjacency do not have a linguistic —much less syntactic—origin. Instead, these constraints have been described as language-external and deriving from a general cognitive bias. In addition, it has been argued that nothing attests to categorial features having the status of individual primitives in the neurolinguistics/clinical linguistics literature. Based on what has been reviewed so far, it seems that Label is an extra-syntactic operation and that MIs are granted their identity contextually.

Switching to an ethological perspective, the picture somewhat changes. Several studies have suggested that the operation Label is the locus of human uniqueness, the novelty that distinguishes human language from the communication systems of other species (Hornstein 2009, Hornstein & Pietroski 2009, Murphy 2015a, Goucha et al. 2017). If a labelling algorithm that forms asymmetric hierarchical structures in syntax is indeed the key evolutionary novelty, Label belongs to the core of the language faculty (FL) and should fall in the first factor in language design (UG, according to Chomsky 2005) or FLN(arrow), if one adopts the distinction between narrow and broad FL (Hauser et al. 2002). Yet the theses reviewed so far suggest the opposite. More concretely, going back to how categories are treated in Lenneberg (1967), the difference between human language and systems of communication in other species is *quantitative*, not qualitative. Lenneberg has argued that most animals organize the world by some process of categorization which is not formally different than man's concept formation, but only differs in terms of the number of total possibilities for categorization that exist (1967: 331-332). If the categorization process is not peculiar to man—and it is not per Lenneberg—, we can employ a bottom-up approach to animal and human cognition(de Waal & Ferrari 2010) by discussing the shared properties of categories in human language and those in other species' 'language'.

Comparing human language to animal communication, Berwick et al. (2013) highlight the *flexibility of labels* in the latter, but not in the former, when they write that

> [a]s an example of this gap [between the two], Laura-Ann Petitto, one of the leading researchers of primate communication and early language acquisition, observes that a chimpanzee uses the label for 'apple' to refer to 'the action of eating apples, the location where apples are kept, events and locations of objects other than apples that happened to be stored with an apple (the knife used to cut it), and so on and so forth—all simultaneously, and without apparent recognition of the relevant differences or the advantages of being able to distinguish among them' ([Petitto 2005:] 86). (Berwick et al. 2013: 92)

However, it is precisely this great flexibility of categories that Lenneberg (1967) has called the "most outstanding feature of human 'criteriality'" in the context of *human language*. Data from languages that are not heavily grammaticalized suggest that he is right. Riau Indonesian in (17) is one such case.

(17) *Riau Indonesian*

ayam makan
chicken eat

(an association of CHICKEN and EAT)

(Gil 2009: 23)

The MIs in (17) receive their grammatical identity post-syntactically, depending on the context. (17) can thus mean that 'the chicken is/was eating' or 'the chickens that were eaten' or 'the reason chickens eat'. Overall, it seems that a great degree of flexibility is involved when interrelating the MIs that make up (17). If categories are defined post-syntactically, there are no 'nouny' or 'verby' features per se in syntax, which would then explain why such as a feature inventory fails to show up as atypical in the neurolinguistics literature.

One could observe at this point that Lenneberg talks about *broad* semantic distinctions when he argues that nouns and adjectives are not absolute constructs, whereas Berwick et al. (2013) talk about *narrow*, individual labels (e.g., apple to refer to APPLE, instead of the broader label 'noun'). However, flexibility characterizes both types of labels in human language. Data from Riau Indonesian paint only half of the picture in showing that broad distinctions—such as noun, verb, and tense—can be flexibly established as context-dependent relations between MIs (granting support to Lenneberg's claim). Data from recently emerged sign languages paint the other half, providing unique insights into how narrow labels are flexible too. Importantly, these data defy one of the design characteristics (in the sense of Hockett 1960) of human language: signifier-signified consistency. This term refers precisely to the (synchronically) inflexible nature of the association between apple and APPLE in human language. Unlike other species (see Berwick et al.'s earlier point), humans that speak English will form this association in a consistent fashion, without changing 'apple' into another string of sounds for denoting APPLE and without attaching to this string of sounds a different concept. Yet, studies on different 'newborn' sign languages have noted the *absence* of this consistency in the early stages of the development of a language (figure 1). Washabaugh (1986) on Providence Island Sign Language and Sandler et al. (2011) on Al-Sayyid Bedouin Sign Language give similar reports on how consistency *gradually* improves over new generations of speakers.

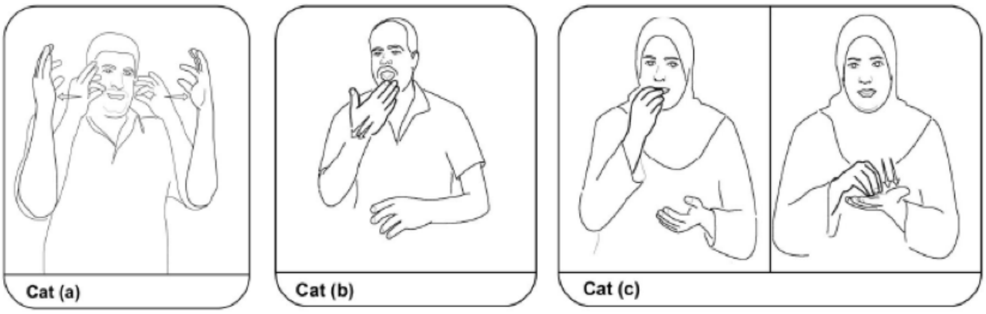


Figure 1: Absence of signifier-signified consistency in Al-Sayyid Bedouin Sign Language—three variants of CAT (Meir et al. 2010: 19).

Gradual development entails that the fact that some languages do not employ a verb-noun distinction (see Tkachman & Sandler 2013 for examples) does not mean that the speakers or signers of these languages cannot employ it. Absence of use does not entail absence of ability; the former may depend on many environmental factors. Crucially, the same argument can be evoked for other species. Consider, for instance, how Bengalese finches, the long-domesticated strain of white-rumped munia, have developed a phonologically and syntactically richer repertoire in comparison to their wild relatives (Okanoya 2012). White-rumped munia songs *could* have been equally complex, but they are not, because of the different environmental needs that drive their performance (e.g., strength of sexual selection; Okanoya 2015). In this context, other species may lack—*to the best of our knowledge*—a range of semantic categories that is quantitatively comparable to ours, but this speaks about actual use, not ability.[6]

---

[6] Green & Marler (1979) raise a similar point for primate languagelike behavior, when discussing the linguistic abilities of chimpanzee Sarah (Premack 1971):

> Within her repertoire of about 130 words were not only many nouns, verbs, and adjectives but also more complex constructions, such as *same*, and *different*, questions, and the conditional *if-then*. [...] The accomplishments of chimpanzees using languagelike systems of signaling to converse with an experimenter are surely the highest animal attainments demonstrated so far. Yet, they also raise a curious dilemma. If a chimpanzee can indeed achieve some elementary competence with language when provided with an appropriate vehicle, why is the evidence for symbolic usage in nature so limited? It may well be that the paucity of our knowledge of natural communication in animals is such that *we can hardly judge whether such abilities are demonstrated in nature or not*. However, it is also possible that in most social interactions, *animals have little use for languagelike patterns of communication* [...].
>
> (Green & Marler 1979: 133–134; emphasis added)

The inter- and intraspeaker variability found in ‘newborn’ sign languages is extensive (Washabaugh 1986), clearly pointing to the fact that both narrow and broad labels can allow for some degree of flexibility in human language. For the purposes of the present discussion, the important conclusion is that data from different spoken and signed languages attest to the fact that “category distinctions do not come ready-made into language” (Tkachman & Sandler 2013: 277). These data grant further support to Lenneberg’s (1967, 1975) view of categories as flexibly established and contextually defined relations.

Going back to the ethological literature, despite the line of studies that suggest that Label is the locus of humaniqueness, it seems that other species do have a range of broad, contextually-defined categories (i.e. broad labels) in their repertoire. In animal communication, vocal labeling refers to incidents in which an animal *consistently* uses an acoustic signal when presented with a specific object or class of objects (King & Janik 2013). Primatologists have argued that non-human primates possess a number of calls—alarm calls, copulation calls, moving grunts when foraging, infant handling grunts, contact barks, contest wahoos—that function as *semantic labels* linked both to real-world relations and to other elements in the repertoire itself (Cheney & Seyfarth 1997, 2010).[7] This is reminiscent of human language, where words may refer to real-world entities and be semantically related with other words. Second, similar to how nouns and verbs in human language “represent different viewpoints on concepts” (Panagiotidis 2015: 2), the calls of Diana monkeys seem to represent different viewpoints on the type of stimuli they denote. More specifically, when perceiving a threat, their alarm calls indicate the predator’s biological class: they represent *predator category* in a *referential way*,[8] and these calls are interpreted as such by conspecifics regardless of immediacy of threat or direction of attack (Zuberbühler 2000). Third, these alarm calls encode other types of semantic information (e.g., elevation, distance) through vocal tract filtering, another ability once thought to be restricted to human speech (Zuberbühler 2000, Riede & Zuberbühler 2003). Last, as happens in human language, non-human primates may take into account *pragmatic context* when interpreting call meaning (e.g., the peep calls of bonobos; Clay et al. 2015).

Narrow labels are also found in the communication systems of other species. Baboons can process argument structure and reference to the extent of understanding the difference between *Sylvia threatens Hannah* and *Hannah threatens Sylvia* (Cheney & Seyfarth 2007). Even more special is the case of Bottlenose dolphins, which have the *signature whistle* in their repertoire; a unique signal that encodes individual identity independently of voice features (King & Janik 2013). Much like narrow labels in human language, this whistle *labels individuals* in a way that involves a stable association between a signifier and a signified.

---

[7] As Cheney & Seyfarth (1997) clarify, these labels really deserve the designation ‘semantic’, because they evoke the same responses from recipients as do the stimuli to which they refer.

[8] Kaplan (2008) and Watson et al. (2015) have also found use of referential calls in the repertoire of Australian magpies and chimpanzees, respectively. With respect to the latter, it was found that the referential vocalizations of chimpanzees were not determined by arousal, leading Watson et al. (2015) to claim that the socially learned aspect of reference in human language has ancient evolutionary origins, uncovering yet another similarity between human language and non-human communication systems.

All these different labels that are scattered in the repertoire of different species beg the question of whether Label is truly the locus of humaniqueness. While it is probably true that other species do not possess as many fine-grained categorial distinctions as humans, this section aimed to show that they have some, in agreement with Lenneberg's (1967) proposal that the categorization process is not peculiar to man. What degree of uniqueness does the description "unique to human language" require then? When a property has parallels in the communication systems of other species, shouldn't some degree of difference be tolerated, given that we deal with quite different physiological implementations of this property across species? If the answer is positive, labels seem to fall in the domain of properties that are shared across different species.[9]

3. Decoding Adjacent Tokens of the Same Type: The Novel Information Bias

It has been argued that anti-identity derives from a cognitive bias that is diffused across domains of human cognition (Walter 2007, van Riemsdijk 2008, Samuels 2014). Not being specific to language, it would be a third factor bias, if one adopts Chomsky's (2005) proposal about the existence of three factors in language design. Within linguistics, few works have recognized this domain-general character of anti-identity (see ((10)–(13)). Two of the studies that elaborate on its origin are Walter (2007) and Murphy (2015b).

Starting from the latter, Murphy's effort to *derive* linguistic constraints by tracing their origins in the brain is definitely on the right track when the aim is to reach explanatory adequacy. There is, however, a slight caveat to be considered: his background assumptions in (13) seem too strong, given that grammatically licit counterexamples exist. If little verbs count as phase heads, as is standardly assumed, and Collins (2002) is right about serial verb compounds in ‡Hoan adjoining to a little verb *v*, the result is a phase head that has two elements of the same label in its complement. In other words, the presence of multiple lexical units of the same label within a single phase complement is *not* prohibited. Importantly, if Lenneberg is right about the contextual definition of categories, nothing could prohibit it, because an MI does not go into the derivation bearing categorial features such as N, V, or *v*. Put another way, if the labels of the elements that build up the complement of a phase head are (contextually) granted after transfer, how could identity-adjacency be first detected and then prohibited in syntax proper, within the narrow confine of a complement? Data from different languages ((14)–(16), see also Collins 2002 and Leivada 2015) show that it is not.

Walter (2007) offers an extensive analysis of anti-identity, dealing with its behavioral manifestations both in language and other cognitive domains, and identifying various reasons as an explanation of the fact that humans avoid adjacent repetitions of the same element. Her 'Biomechanical Repetition Avoidance Hypoth-

---

[9] Accepting the position that the categorization process is not unique to man does not alter the fact that labelling in humans is implemented through language. One could then ask whether any of the specifics of the labeling algorithm are unique to human language. In sum, labeling has parallels in the communication systems of other species, but this does not change the fact that when manifested in human language, labeling uses some linguistic machinery and inevitably acquires a linguistic dress.

esis' (BRAH) in (18) offers a *physiological* explanation, while she also recognizes the possibility of a *perceptual* motivation (19).

(18) *Biomechanical Repetition Avoidance Hypothesis*
Repetition of articulatory gestures is relatively difficult. This difficulty results in phonetic variation.

(19) *The Repetition Deficit*
Repeated items are at times not perceived as different occurrences at all. This tendency is a general characteristic of human cognition.

(18) entails that in [XX] sequences, it is likely that the two elements will not be realized identically, in agreement with those studies that highlight the role of PF in deriving anti-identity (i.e. ((1)–(5)). (19) has its roots in a phenomenon called *Repetition Blindness*: It has been shown that in rapid serial visual presentation of words—above normal reading rate but well within limits of accurate reading potential—, subjects have difficulty in detecting repeated words (Kanwisher 1987 et seq.). This phenomenon is not restricted to language. It also occurs between color patches (Kanwisher et al. 1995), pictures of natural objects (Buffat et al. 2013), and visually different items that are phonologically similar and semantically related (e.g., a picture of the sun and the word "sun") or phonologically similar and semantically unrelated (e.g., a picture of the sun and the word "son"; Bavelier 1994).

A concrete illustration of the fact that humans avoid repetitions is the 'apparent motion' illusion: Identical stimuli flashed in different locations are usually perceived as a single moving stimulus. People show a clear preference for perceiving these identical stationary tokens that blink on and off alternatively as one moving token (Vetter et al. 2012 and references therein). Language is not involved in this illusion; a fact that suggests that there is a bias towards anti-identity that is operative across domains of cognition.

Another example of repetition avoidance in human cognition outside the domain of language comes from experiments that measure random number generation. It has been consistently found that adjacent repetitions of the same element are avoided in human response sequencing (Towse 1998, Towse & Neil 1998), even if participants were not instructed to not produce them. The tendency is to avoid repetitions among immediately adjacent responses and to repeat after a lag of several responses. The explanation given for this performance is rooted in a general cognitive mechanism that inhibits responses that have been just produced (Towse 1998).

Comparing the different motivations behind anti-identity that are given in (18) and (19), it seems that the former would have trouble explaining the numerous cases where human language licenses multiple adjacent tokens of the same type for specific reasons.[10] For example, there are songs that repeat many times the same word. In telephone closings, a final farewell token such as 'bye' or 'ciao' is often repeated a couple of times (Auer et al. 1999 document cases of extensive repetition), probably serving as a conversation hedge. Reduplications of the same item when cursing are also typical in language (see Corver 2014 for data), strengthening

---

[10] I am grateful to Henk van Riemsdijk and Norbert Corver for bringing various cases of item repetition to my attention.

emotive content (Potts 1997). Moreover, item repetition is often used for achieving contrastive focus (Ghomeshi et al. 2004). Interestingly, even if in all those situations the repeated item is realized just a couple of times, there are occasions where the repetition is considerably broader than what one would expect if articulatory fatigue was the reason that humans avoid repetitions in language.[11]

Observing that there are contexts that call for adjacent repetitions of identical tokens, the next aim is to identify the motivation and exact circumstances that lead to flouting Identity Avoidance in language. The hypothesis put forth in (20) is developed on the basis of Kanwisher et al.'s (1995) observation that the attentional system responsible for integrating information of types and tokens may be unable to bind the appropriate identity (type) to distinct, adjacent representations (tokens). The novelty of this hypothesis boils down to offering an explanation as to why different degrees of Identity Avoidance-violations are realized in different linguistic circumstances, *depending on whether novel information is presented or not*. This explains why in (14)-(16) there are at most two or three repetitions of X—there are no grammatically licit structures that feature five adjacent complementizers—, but in a context where no new information is conveyed (e.g., telephone closings, songs, broadcasting while waiting for the match to resume etc.), five adjacent tokens of the same type are perfectly possible.

(20) *Novel Information Bias (NIB)*
Subjects avoid tokenizing multiple, adjacent occurrences of the same type, because of a general bias in the cognitive system to provide more attentional resources to novel information, enhancing perception and production processes accordingly.

At the phenotypic level, the applications of NIB are so central to human language that if the process that underlies self-avoidance is consistently disrupted in contexts where new information should be conveyed, the result is an impairment called palilalia. Found in various neurological and psychiatric disorders, such as (early-onset) schizophrenia (Ferrara et al. 2006) and, more generally, in the psychosis spectrum (Geschwind 1974), palilalia involves repetition of entire words or utterances.[12] It is not an articulation-related disorder confined to spoken languages; cases of palilalia in the signed modality have been documented too (Tyrone & Woll 2008). (21) offers an example of palilalia in a patient with schizophrenia.

(21) Doctor: Okay, so [PATIENT NAME], you've been hearing voices.
Patient: Yes, sir.
Doctor: Uh, is that, like, every day?
Patient: Not every day, yeah, every day, every –

---

[11] To give one example, presenting the 2015 Champions League final, Lluís Flaquer announced the first goal of the match, producing 'goal' more than 20 times in a row in a time frame of 22 seconds.

[12] Patients with schizophrenia also perform slightly worse than neurotypical controls in tasks that measure Repetition Blindness (Kammer et al. 1998). Interestingly, a disruption in the process that underlies anti-identity at the phenotypic level may occur in parallel to a disruption in self-avoidance at the cellular level. Dysfunction of protocadherins, a group of transmembrane proteins that regulate dentrite self-avoidance, has been repeatedly implicated in schizophrenia and other neurological disorders (Hirabayashi & Yagi 2014, Hayashi & Takeichi 2015).

> Doctor: Every day.
> Patient: Every, every . . .
>
> (Steuber 2011: 19)

Naturally, attention deficits are also fundamental in schizophrenia (Fioravanti et al. 2005), as NIB is directly related to how our system of attention filters incom-ing information, allowing the ‘proactive’ brain (Bar 2007) to extract the important parts and generate focused predictions about the future.

Kanwisher et al.’s (1995) observation that the attentional system responsible for decoding types and tokens may be unable to link repeated representations of the same type to different tokens can be connected with theories of how our parser deals with received messages and especially to the possibility of interference by various sources. More specifically, according to Shannon’s (1948) mathematical theory of communication (figure 2), the transmission of a message can be affected by *noise sources*.

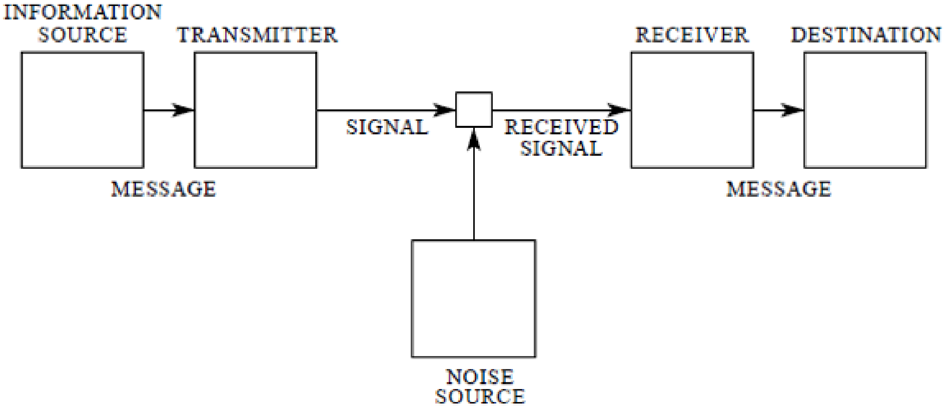


Figure 2: Shannon’s (1948) schematic diagram of a general communication system.

The signal that the receiver gets can be corrupted because of noise at both the transmitter and receiver ends, hence the receiver must reconstitute the original message. The crucial part here is what Gallistel & King call one of Shannon’s most “profound insights”:

> What was essential about a message was not its meaning but rather that *it be selected from a set of possible messages*. Shannon realized that for a communication system to work efficiently—for it to transmit the maximum amount of information in the minimum amount of time—both the transmitter and the receiver had to know what the set of possible messages was and the relative likelihood of the different messages within the set of possible messages.
>
> (Gallistel & King 2009: 6; emphasis in original)

Recognizing that the intended message is subject to noise due to a multitude of production errors that can interfere with its contents (e.g., wrong retrievals, tip-of-the-tongue states, self-interruptions, false starts, phonetic execution errors), the receiver knows that it is quite likely that many adjacent tokens of the same type would constitute noise. If the parser's goal is to receive the maximum amount of information in the minimum amount of time, the parser must keep track of the key characteristics of noise and orient attention accordingly. Generalizing this effect, one obtains NIB: Receivers reconstitute the message, focusing their attention to *novel* information and filtering out potential noise. Adjacent repetitions are a prime candidate for the latter. In this context, NIB predicts that in situations where no new information is supposed to be conveyed, Identity Avoidance can be flouted to a greater degree (e.g., telephone closings, songs, etc.). Also, since NIB is a cognitive bias and not a syntactic ban, [XX(X)] is possible in grammar (as shown in (14)–(16)), although heavily constrained for obvious reasons. No language would deploy sentences with ten adjacent prepositions, determiners, or complementizers, because a big part of that message would have a very low information value. If such a sentence was realized, the receiver would recognize that repetitions are due to production errors or, in more serious cases, a disorder that affects linguistic performance (e.g., palilalia).

NIB could also be the source of Towse's (1998) performance bias that is responsible for inhibiting responses that have been just produced in random number generation. Based on the knowledge that repetitions are often filtered out as noise, NIB diffuses across domains of cognition, across comprehension and production tasks, and across settings, effectively establishing optimal conditions for communication at both the transmitter and receiver ends. Support for this claim comes from a recent experiment that involved random number generation, in which it was found that the tendency to avoid repetitions is evident even in dyadic production settings (Towse et al. 2016). In other words, not only were self-generated repetitions avoided, but participants monitored the alternation of responses in the dyadic setting, consistently avoiding immediately adjacent repetitions. Towse et al. (2016) interpreted this finding as showing that repetition avoidance is *not* just an output effect. Since repetition avoidance is shown to be much broader than inhibition of one's own production, its origins should be searched elsewhere. NIB is one possible source.

The literature on the development of different types of attentional mechanisms seems to lend support to NIB. It has been shown that infants display an experience-independent bias which makes words that feature adjacent reduplications (e.g., *neenee*) easier to learn than non-reduplicated words (Ota & Skarabela 2016). This happens because in the course of development, *exogenous* attention, which is driven by salient environmental stimuli, becomes operative earlier than *endogenous* attention, which allows for voluntary, selective control of which stimuli will be attended (Posner & Cohen 1984). It has been even suggested that the register-specific reduplications that one founds in infant-direct speech (i.e. baby talk) possibly evolved as a response to the innate predisposition of infants to segment better words with adjacently repeated phonological elements than words that involve no such repetitions (Ota & Skarabela 2017).

Salient features such as adjacent repetitions function as attentional attractors that may facilitate language learning in the early stages of development (de Diego-Balaguer et al. 2016). For example, infants' ability to generalize the detection of non-adjacent dependencies (e.g., *this chair* vs. *these chairs*) occurs when they are first presented with familiar stems, whereas learning is altered if their attention is drawn by a *novel*, intervening nonce word (e.g., *this blem* vs. *these blems*; Soderstrom et al. 2002). At later stages of development, the maturation of the endogenous system enables infants to ignore 'salient stimulus'-driven properties of their environment, in favor of orienting their attention to the *relevant* information (de Diego-Balaguer et al. 2016). Relating these findings to NIB, receivers increasingly rely on endogenous attention when reconstituting messages, because the (mature) parser needs to filter out noise and communicate effectively in the least costly possible manner.

At the neural level, it has been argued that *goal-driven* attention (roughly corresponding to endogenous attention), involves the dorsal frontoparietal network, whereas the ventral frontoparietal network, which detects task-relevant stimuli, underlies *stimulus-driven* (i.e. exogenous) attention (Corbetta et al. 2008). The ventral pathway matures earlier than the dorsal pathway (Dubois et al. 2015), explaining why our attentional system is more 'salient stimulus'-driven at the beginning. At the subcortical level, the main structure of interest is the hippocampus. Goal-driven, endogenous attention affects the magnitude of encoding-related activity in the hippocampus (Aly & Turk-Browne 2016), the maturation of which is generally considered to be slower than that of other structures (Østby et al. 2009). Perhaps unsurprisingly then, its modulating signal has been shown to be implicated in repetition suppression and enhancement (Kremers et al. 2014). In recent research, the hippocampus has been directly connected with what one may call 'the neural signature of Identity Avoidance'. More specifically, it was found that when subjects are presented with identical patterns of events, this overlap triggers a 'repulsion' among hippocampal representations, which is resolved by the hippocampus through reducing similarity among representations (Chanales et al. 2017).

In this context, one can predict that the process of identity inhibition will be disrupted in patients with abnormal hippocampal representations, and indeed, repetition avoidance has been reported to be less evident among hippocampal amnesics (Brugger et al. 1992, 1996a). A high incidence of repetitions is also attested in patients with damage in the right frontal lobe (Brugger et al. 1996b). Aberrances in the frontal lobe, the hippocampus, and the parahippocampal gyrus have been often associated with a high risk for developing psychosis/schizophrenia (Du et al. 2017, Hill et al. 2017) and with conditions that involve palilalia (Peterson et al. 2007, Cho et al. 2009).

Overall, one can observe across different levels and types of stimuli that the human brain does not like to process identical representations, hence it employs a number of ways to reduce similarity. At the neural level, protocadherins underlie dentritic self-avoidance and self-/non-self-discrimination, which function as strategies that endow neurons with distinct molecular identities (Lefebvre et al. 2012). At the behavioural level, NIB is operative, leading again to the use of various strategies in order to break identity-adjacency (e.g., in the case of language through the addition of structure among identical nodes). It is important to recognize that the development of these strategies in language is not driven by a constraint that prevents a token from being merged with another token of the same type/category. Instead, the origin is a general bias in the cognitive system to provide more attentional resources to novel information, than to repeated information, given that the latter is a prime candidate for noise. In this context, Identity Avoidance is the linguistic manifestation of a more general, cognitive bias. In the linguistics literature, the fact that Identity Avoidance is simply a bias and not a hard constraint that leads to crashed derivations and unacceptable sentences, although usually overlooked, has been already recognized in Chomsky's (1967) appendix to Lenneberg's seminal book *Biological Foundations of Language*:

> The normal use of language relies in an essential way on this unboundedness, on the fact that language contains devices for generating sentences of arbitrary complexity. *Repetition of sentences is a rarity; innovation*, in accordance with the grammar of the language, *is the rule* in ordinary day-by-day performance.
>
> (Chomsky 1967: 400; emphasis added)

4. Outlook

The present work defended Lenneberg's (1967, 1974) idea that categories are not absolute constructs, but flexibly established and contextually defined relations between words. A review of categorial identity from the neurolinguistic perspective showed that categorial features lack a status of separate entities in a feature inventory, in agreement with Lenneberg's (1974) claim about categories existing on a continuum rather than being discretely demarcated entities. The review of the ethological literature has suggested that a number of semantic categories is scattered in the repertoire of different species, thus granting support to the claim that the categorization process is not peculiar to man (Lenneberg 1967). Having developed an understanding of what categories are, this work then focused on Identity Avoidance. It was shown that this is a flexible bias, and not a hard constraint or ban, which is why communicative needs can flout it.

Grammatically licit [XX] structures are found cross-linguistically, although it is true that the existence of adjacent tokens of the same type is severely constrained. The reason is that our parser focuses on novel information, avoiding to bind one type to distinct, adjacent tokens (Kanwisher et al. 1995). In relation to language, this hypothesis is formally captured under the *Novel Information Bias*, which predicts that in situations where no new information is supposed to be conveyed, Identity Avoidance can be flouted to a greater degree. The literature on the development of different types of attentional mechanisms grants support to this proposal, explaining why our attentional system is more 'salient stimulus'-driven at the early stages of development. On the contrary, the mature parser relies more on endogenous attention in order to focus on novel information and filter out noise.

All in all, the interdisciplinary take on labels and Identity Avoidance presented here might be a step in the process of developing a theory of categories that has replaced explananda with explanations. In this work, these explanations were based on insights coming not only from the study of language, but also from recent findings in attention, cognitive processing, neurology, and ethology. It is likely that this interdisciplinarity will eventually prove to be instrumental in finally understanding both what a label is and whether it is unique to man.

References

Ackema, Peter. 2001. Colliding complementizers in Dutch: another syntactic OCP effect. *Linguistic Inquiry* 32, 717–727.

Adger, David. 2016. *A Menagerie of Merges*. Baggett Lectures, Lecture I, University of Maryland.

Aelbrecht, Lobke & Marcel den Dikken. 2013. Preposition doubling in Flemish and its implications for the syntax of Dutch PPs. *The Journal of Comparative Germanic Linguistics* 16, 33–68.

Alexiadou, Artemis. 2014. Exploring the limitations of identity effects in syntax. In Kuniya Nasukawa & Henk van Riemsdijk (eds.), *Identity Relations in Grammar*, 199–224. Berlin: Mouton de Gruyter.

Aly, Mariam & Nicholas B. Turk-Browne. 2016. Attention promotes episodic encoding by stabilizing hippocampal representations. *PNAS* 113(4), E420–E429.

Arsenijević, Boban & Wolfram Hinzen. 2012. On the absence of X-within-X recursion in human grammar. *Linguistic Inquiry* 43, 423–440.

Auer, Peter, Elizabeth Couper-Kuhlen & Frank Müller. 1999. *Language in Time: The Rhythm and Tempo of Spoken Interaction*. New York: Oxford University Press.

Bar, Moshe. 2007. The proactive brain: using analogies and associations to generate predictions. *Trends in Cognitive Sciences* 11(7), 280–289.

Barbeau Emmanuel & Bernard Giusiano. 2003. Category-specific visual agnosia: Lesion to semantic memory versus extra-lesional variables in a case study and a connectionist model. *Brain and Cognition* 53, 433–440.

Barner, David & Alan Bale. 2002. No nouns, no verbs: psycholinguistic arguments in favor of lexical underspecification. *Lingua* 112, 771–791.

Bavelier, Daphne. 1994. Repetition blindness between visually different items: the case of pictures and words. *Cognition* 51, 199–236.

Berent, Iris, Joseph Shimron & Vered Vaknin. 2001. Phonological constraints on reading: Evidence from the Obligatory Contour Principle. *Journal of Memory and Language* 44, 644–665.

Berent, Iris, Vered Vaknin & Joseph Shimron. 2004. Does a theory of language need a grammar? Evidence from Hebrew root structure. *Brain and Language* 90, 170–182.

Berent, Iris & Joseph Shimron. 2003. Co-occurrence restrictions on identical consonants in the Hebrew lexicon: Are they due to similarity? *Journal of Linguistics* 39, 31–55.

Berent, Iris, Colin Wilson, Gary F. Marcus & Douglas K. Bemis. 2012. On the role of variables in phonology: Remarks on Hayes & Wilson 2008. *Linguistic Inquiry* 43(1), 97–119.

Berwick, Robert C., Angela D. Friederici, Noam Chomsky & Johan J. Bolhuis. 2013. Evolution, brain, and the nature of language. *Trends in Cognitive Sciences* 17, 89–98.

Boeckx, Cedric. 2008. *Bare Syn*tax. Oxford: Oxford University Press.

Boeckx, Cedric. 2013. Merge: Biolinguistic considerations. *English Linguistics* 30, 463–484.

Bošković, Željko. 2002. On multiple wh-fronting. *Linguistic Inquiry* 33, 351–383.

Bošković, Željko & Jairo Nunes. 2007. The copy theory of movement: A view from

PF. In Norbert Corver & Jairo Nunes (eds.), *The Copy Theory of Movement*, 351–385. Amsterdam: John Benjamins.

Breheny, Richard & David Adger. 2005. Commentary on 'exaption and linguistic explanation'. *Lingua* 115, 1673–1677.

Brugger, Peter, Theodor Landis & Marianne Regard. 1992. The brain as a random generator: The relevance of subjective randomization for neuropsychology. *Journal of Clinical and Experimental Neuropsychology* 14, 84.

Brugger, Peter, Andreas U. Monsch, David P. Salmon & Nelson Butters. 1996a. Random number generation in dementia of the Alzheimer type: A test of frontal executive functions. *Neuropsychologia* 34, 97–103.

Brugger, Peter, Andreas U. Monsch & Shannon A. Johnson. 1996b. Repetitive behavior and repetition avoidance: The role of the right hemisphere. *Journal of Psychiatry and Neuroscience* 21, 53–56.

Buffat, Stéphane, Justin Plantier, Corinne Roumes & Jean Lorenceau. 2013. Repetition blindness for natural images of objects with viewpoint changes. *Frontiers in Psychology* 3, 622.

Campanella, Fabio, Serena D'Agostini, Miran Skrap & Tim Shallice. 2010. Naming manipulable objects: Anatomy of a category specific effect in left temporal tumours. *Neuropsychologia* 48, 1583–1597.

Caramazza, Alfonso & Argye E. Hillis. 1991. Lexical organization of nouns and verbs in the brain. *Nature* 349, 788–790.

Chanales, Avi J. H., Ashima Oza, Serra E. Favila & Brice A. Kuhl. 2017. Overlap among spatial memories triggers repulsion of hippocampal representations. *Current Biology* 27(15), 2307–2317.

Cheney, Dorothy L. & Robert M. Seyfarth. 1997. Why animals don't have language. *The Tanner Lectures on Human Values*, Cambridge University, March 10–12.

Cheney, Dorothy L. & Robert M. Seyfarth. 2007. *Baboon Metaphysics: The Evolution of a Social Mind*. Chicago, IL: University of Chicago Press.

Cheney, Dorothy L. & Robert M. Seyfarth. 2010. Primate communication and human language: Continuities and discontinuities. In Peter M. Kappeler & Joan B. Silk (eds.), *Mind the Gap: Tracing the Origins of Human Universals*, 283–298. Berlin: Springer-Verlag.

Cho, Yang-Je, Sang-Don Han, Sook Keun Song, Byung In Lee & Kyoung Heo. 2009. Palilalia, echolalia, and echopraxia–palipraxia as ictal manifestations in a patient with left frontal lobe epilepsy. *Epilepsia* 50(6), 1616–1619.

Chomsky, Noam. 1967. The formal nature of language. In Eric Lenneberg (auth.), *Biological Foundations of Language*, 397–442. New York: John Wiley & Sons.

Chomsky, Noam. 2005. Three factors in language design. *Linguistic Inquiry* 36, 1–22.

Chomsky, Noam. 2007. Approaching UG from below. In Uli Sauerland & Hans-Martin Gärtner (eds.), *Interfaces + Recursion = Language?*, 1–29. Berlin: Mouton de Gruyter.

Chomsky, Noam. 2013. Problems of projection. *Lingua* 130, 33–49.

Cinque, Guglielmo. 1999. *Adverbs and Functional Heads: A Cross-Linguistic Perspective*. New York: Oxford University Press.

Clay, Zanna, Jahmaira Archbold & Klaus Zuberbühler. 2015. Functional flexibility in wild bonobo vocal behaviour. *PeerJ* 3: e1124.

Collins, Chris. 2002. Multiple verb movement in ‡Hoan. *Linguistic Inquiry* 33(1), 1–29.

Collins, Chris. 2016. *NEG NEG. [https://ling.auf.net/lingbuzz/003159].

Corbetta, Maurizio, Gaurav Patel & Gordon L. Shulman. 2008. The reorienting system of the human brain: from environment to theory of mind. *Neuron* 58, 306–324.

Corver, Norbert. 2014. Recursing in Dutch. *Natural Language & Linguistic Theory* 32, 423–457.

Crepaldi, Davide, Chiara Ingignoli, Ruggero Verga, Antonella Contardi, Carlo Semenza & Claudio Luzzatti. 2011. On nouns, verbs, lexemes, and lemmas: Evidence from the spontaneous speech of seven aphasic patients. *Aphasiology* 25(1), 71–92.

Davis, Henry. 2010. A unified analysis of relative clauses in St’át’imcets. *North-west Journal of Linguistics* 4, 1–43.

de Diego-Balaguer, Ruth, Anna Martinez-Alvarez & Ferran Pons. 2016. Temporal attention as a scaffold for language development. *Frontiers in Psychology* 7, 44.

Du, Yuhui, Susanna L. Fryer, Zening Fu, Dongdong Lin, Jing Sui, Jiayu Chen, Eswar Damaraju, Eva Mennigen, Barbara Stuart, Rachel L. Loewy, Daniel H. Mathalon & Vince D. Calhoun. 2017. Dynamic functional connectivity impairments in early schizophrenia and clinical high-risk for psychosis. *NeuroImage*. doi:10.1016/j.neuroimage.2017.10.022d.

Dubois, Jessica, Cyril Poupon, Bertrand Thirion, Hina Simonnet, Sofya Kulikova, François Leroy, Lucie Hertz-Pannier & Ghislaine Dehaene-Lambertz. 2015. Exploring the early organization and maturation of linguistic pathways in the human infant brain. *Cerebral Cortex* 26(5), 2283–2298.

Fábregas, Antonio. 2014. Phrasal spell out: an argument from haplology. *Linguistic Analysis* 39, 1–2.

Ferrara, Mauro, Francesca Freda, R. Massa & T. J. Carratelli. 2006. Frontal lobe syndrome or adolescent-onset schizophrenia? A case report. *Acta Psychiatrica Scandinavica* 114 (5), 375–377.

Fioravanti, Mario, Olimpia Carlone, Barbara Vitale, Maria Elena Cinti & Linda Clare. 2005. A meta-analysis of cognitive deficits in adults with a diagnosis of schizophrenia. *Neuropsychology Review* 15(2), 73–95.

Frisch, Stefan A., Janet B. Pierrehumbert & Michael B. Broe. 2004. Similarity avoidance and the OCP. *Natural Language & Linguistic Theory* 22, 179–228.

Gallistel, C. R. & Adam Philip King. 2009. *Memory and the Computational Brain*. Malden, MA: Wiley-Blackwell.

Ghomeshi, Jila, Ray Jackendoff, Nicole Rosen & Kevin Russell. 2004. Contrastive focus reduplication in English (the salad-salad paper). *Natural Language & Linguistic Theory* 22, 307–357.

Gil, David. 2009. How much grammar does it take to sail a boat? In Geoffrey Sampson, David Gil & Peter Trudgill (eds.), *Language Complexity as an Evolving Variable*, 19–33. Oxford: Oxford University Press.

Geschwind, Norman. 1974. *Selected Papers on Language and the Brain*. Dordrecht: Reidel.

Gopnik, Myrna. 1990. Feature blindness: A case study. *Language Acquisition* 1(2), 139–164.

Goucha, Tomás, Emiliano Zaccarella & Angela D. Friederici. 2017. A revival of the *Homo loquens* as a builder of labeled structures: neurocognitive considerations. *Neuroscience and Biobehavioral Reviews*. doi:10.1016/j.neubiorev.2017.01.036.

Green, Steven & Peter Marler. 1979. The analysis of animal communication. In Peter Marler & J. G. Vandenbergh (eds.), *Handbook of Behavioral Neurobiology. Volume 3, Social Behavior and Communication*, 73–158. New York & London: Plenum Press.

Grimshaw, Jane. 1991. Extended Projection. Ms, Brandeis University.

Grimshaw, Jane. 1997. The best clitic: Constraint conflict in morphosyntax. In Liliane Haegeman (ed.), *Elements of Grammar*, 169–196. Dordrecht: Kluwer.

Grimshaw, Jane 2005. Extended Projection. In Jane Grimshaw (ed.), *Words and Structure*, 1–69. Stanford: CSLI Publications.

Grohmann, Kleanthes K. 2000. Prolific peripheries: A radical view from the left. College Park, MD: University of Maryland dissertation.

Grohmann, Kleanthes K. 2003. *Prolific Domains: On the Anti-Locality of Movement Dependencies*. Amsterdam: John Benjamins.

Hauser, Marc D., Noam Chomsky & W. Tecumseh Fitch. 2002. The faculty of language: What is it, who has it, and how did it evolve? *Science* 298, 1569–1579.

Hayashi, Shuichi & Masatoshi Takeichi. 2015. Emerging roles of protocadherins: from self-avoidance to enhancement of motility. *Journal of Cell Science* 128, 1455–1464.

Hernández, Mireia, Albert Costa, Núria Sebastián-Gallés, Montserrat Juncadella & Ramón Reñé. 2007. The organisation of nouns and verbs in bilingual speakers: A case of bilingual grammatical category-specific deficit. *Journal of Neurolinguistics* 20, 285–305.

Hill, Kathryn, Nicolas Bolo, Suraj Sarvode Mothi, Paulo Lizano, Synthia Guimond, Neeraj Tandon, Elena Molokotos & Matcheri Keshavan. 2017. Subcortical surface shape in youth at familial high risk for schizophrenia. *Psychiatry Research Neuroimaging* 267, 36–44.

Hinzen, Wolfram & Michelle Sheehan. 2013. *The Philosophy of Universal Grammar*. Oxford: Oxford University Press.

Hirabayashi Takahiro & Takeshi Yagi. 2014. Protocadherins in neurological diseases. In Vladimir Berezin & Peter S. Walmod (eds.), *Cell Adhesion Molecules. Advances in Neurobiology*, vol 8, 293–314. New York: Springer.

Hiraiwa, Ken. 2010. The Syntactic OCP. In Yukio Otsu (ed.) *The Proceedings of the 11th Tokyo Conference on Psycholinguistics*, 35–56. Hituzi: Tokyo.

Hockett, Charles F. 1960. The origin of speech. *Scientific American* 203, 88–96.

Hornstein, Norbert. 2009. *A Theory of Syntax: Minimal Operations and Universal Grammar*. Cambridge: Cambridge University Press.

Hornstein, Norbert & Jairo Nunes. 2008. Adjunction, labeling, and bare phrase structure. *Biolinguistics* 2, 57–86.

Hornstein, Norbert & Paul Pietroski. 2009. Basic operations: Minimal syntax-semantics. *Catalan Journal of Linguistics* 8, 113–139.

Kambanaros, Maria. 2008. The trouble with nouns and verbs in Greek fluent aphasia. *Journal of Communication Disorders* 41, 1–19.

Kambanaros, Maria. 2014. Context effects on verb production in specific language impairment (SLI): confrontation naming versus connected speech. *Clinical Linguistics & Phonetics* 28, 826–843.

Kambanaros, Maria & Kleanthes K. Grohmann. 2015. Grammatical class effects across impaired child and adult populations. *Frontiers in Psychology* 6, 1670.

Kammer, Thomas, Fabian Saleh, Godehard Oepen, Theo Manschreck, Soraya Seyyedi, Nancy Kanwisher, Chris Furmanski & Manfred Spitzer. 1998. Repetition blindness in schizophrenic patients. *European Archives of Psychiatry and Clinical Neuroscience* 248, 136–140.

Kanwisher, Nancy. 1987. Repetition blindness: Type recognition without token individuation. *Cognition* 27, 117–143.

Kanwisher, Nancy, Jon Driver & Liana Machado. 1995. Spatial repetition blindness is modulated by selective attention to color or shape. *Cognitive Psychology* 29, 303–337.

Kaplan, Gisela. 2008. Alarm calls and referentiality in Australian magpies: between midbrain and forebrain, can a case be made for complex cognition? *Brain Research Bulletin* 76(3), 253–263.

King, Stephanie L. & Vincent M. Janik. 2013. Bottlenose dolphins can use learned vocal labels to address each other. *PNAS* 110, 13216–13221.

Kremers, Nico A. W., Lorena Deuker, Thorsten A. Kranz, Carina Oehrn, Juergen Fell & Nikolai Axmacher. 2014. Hippocampal control of repetition effects for associative stimuli. *Hippocampus* 24(7), 892–902

Laiacona, Marcella & Alfonso Caramazza. 2004. The noun/verb dissociation in language production: varieties of causes. *Cognitive Neuropsychology* 21, 103–123.

Laka, Itziar. 2013. Jabberwocky, or the poetry of function words. Mapping Ignorance. [https://mappingignorance.org/2013/12/13/jabberwocky-or-the-poetry-of-function-words/]

Lefebvre, Julie L., Dimitar Kostadinov, Weisheng V. Chen, Tom Maniatis & Joshua R. Sanes. 2012. Protocadherins mediate dendritic self-avoidance in the mammalian nervous system. *Nature* 488, 517–521.

Leivada, Evelina. 2015. X-within-X structures and the nature of categories. *Biolinguistics* 9, 50–73.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York: John Wiley & Sons.

Lenneberg, Eric H. 1975. The concept of language differentiation. In Eric H. Lenneberg & Elizabeth Lenneberg (eds.), *Foundations of Language Development. A Multidisciplinary Approach*, Volume 1, 17–33. New York: Academic Press.

Levelt, Willem. 1989. *Speaking. From Intention to Articulation*. Cambridge, MA: MIT Press.

Levin, Netta, Tamir Ben-Hur, Iftah Biran & Eli Wertman. 2005. Category specific dysnomia after thalamic infarction: a case-control study. *Neuropsychologia* 43(9), 1385–1390.

Marantz, Alec. 1997. No escape from syntax: Don't try morphological analysis in the privacy of your own lexicon. In Alexis Dimitriadis, Laura Siegel, Clarissa

Surek-Clark & Alexander Williams (eds.*), Proceedings of the 21st Annual Penn Linguistics Colloquium*, 201–225. Philadelphia, PA: University of Pennsylvania.

Marcus, Gary F., Sujith Vijayan, Shoba Bandi Rao & Peter M. Vishton. 1999. Rule learning by seven-month-old infants. *Science* 283, 77–80.

Mascarenhas, Salvador. 2007. Complementizer doubling in European Portu-guese. Ms., Amsterdam, ILLC.

Meir, Irit, Wendy Sandler, Carol Padden & Mark Aronoff. 2010. Emerging sign languages. In Marc Marschark & Patricia Elizabeth Spencer (eds.*), The Oxford Handbook of Deaf Studies, Language, and Education*, vol. 2, 267-280. Oxford: Oxford University Press.

Moseley, Rachel L. & Friedemann Pulvermüller. 2014. Nouns, verbs, objects, actions, and abstractions: Local fMRI activity indexes semantics, not lexical categories. *Brain and Language* 132, 28–42.

Murphy, Elliot. 2015a. Labels, cognomes, and cyclic computation: an ethological perspective. *Frontiers in Psychology* 6, 715.

Murphy, Elliot. 2015b. The brain dynamics of linguistic computation. *Frontiers in Psychology* 6, 1515.

Narita, Hiroki. 2011. *Phasing in Full Interpretation*. Ph.D. thesis, Cambridge, MA: Harvard University.

Nevins, Andrew. 2010. Two case studies in phonological universals: A view from artificial grammars. *Biolinguistics* 4, 218–233.

Nevins, Andrew. 2012. Haplological dissimilation at distinct stages of exponence. In Jochen Trommer (ed.), *The Morphology and Phonology of Exponence*, 84–116. Oxford: Oxford University Press.

Nevins, Andrew & Bert Vaux. 2003. Metalinguistic, Shmetalinguistic: The Phonology of Shm-Reduplication. *Proceedings of CLS* 39, 702–721.

Odden, David. 1986. On the role of the Obligatory Contour Principle in phonological theory. *Language* 62, 353-383.

Okanoya, Kazuo. 2012. Behavioural factors governing song complexity in Bengalese finches. *International Journal of Comparative Psychology* 25, 44–59.

Okanoya, Kazuo. 2015. Evolution of song complexity in Bengalese finches could mirror the emergence of human language. *Journal of Ornithology* 156, 65–72.

Ota, Mitsuhiko & Barbora Skarabela. 2016. Reduplicated words are easier to learn. *Language Learning and Development* 12, 380–397.

Ota, Mitsuhiko & Barbora Skarabela. 2017. Reduplication facilitates early word segmentation. *Journal of Child Language*. doi:10.1017/S0305000916000660.

Østby, Ylva, Christian K. Tamnes, Anders M. Fjell, Lars T. Westlye, Paulina Due-Tønnessen & Kristine B. Walhovd. 2009. Heterogeneity in subcortical brain development: a structural Magnetic Resonance Imaging study of brain maturation from 8 to 30 Years. *Journal of Neuroscience* 29, 11772–11782.

Panagiotidis, Phoevos. 2011. Categorial features and categorizers. *The Linguistic Review* 28, 365–386.

Panagiotidis, Phoevos. 2015. *Categorial Features. A Generative Theory of Word Class Categories*. Cambridge: Cambridge University Press.

Panagiotidis, Phoevos & Kleanthes K. Grohmann. 2009. Mixed projections: Categorial switches and prolific domains. *Linguistic Analysis* 35, 141–161.

Perlmutter, David M. 1971. *Deep and Surface Structure Constraints in Syntax*. New York: Holt, Rinehart, and Winston.

Pesetsky, David. 1995. *Zero Syntax*. Cambridge, MA: MIT Press.

Pesetsky, David & Esther Torrego. 2004. Tense, case and the nature of syntactic categories. In Jacqueline Guéron & Jacqueline Lecarme (eds.), *The Syntax of Time*, 495–537. Cambridge, MA: MIT Press.

Peterson Bradley S., HuiMahn A. Choi, Xuejun Hao, Jose A. Amat, Hongtu Zhu, Ronald Whiteman, Jun Liu, Dongrong Xu & Ravi Bansal. 2007. Morphologic features of the amygdala and hippocampus in children and adults with Tourette syndrome. *Archives of General Psychiatry* 64, 1281–1291.

Petitto, Laura-Ann. 2005. How the brain begets language. In James McGilvray (ed.), *The Cambridge Companion to Chomsky*, 84–101. Cambridge: Cambridge University Press.

Posner, Michael I. & Yoav Cohen. 1984. Components of visual orienting. *Attention and Performance* 32, 531–556.

Potts, Christopher. 2007. The expressive dimension. *Theoretical Linguistics* 33, 165–197.

Premack, David. 1971. Language in chimpanzee? *Science* 172(3985), 808–822.

Ramírez Fernández, Javier. 2015. Locality in language and locality in brain oscillatory structures. *Biolinguistics* 9, 74–95.

Richards, Norvin. 2006. A distinctness condition on linearization. Ms., MIT.

Richards, Norvin. 2010. *Uttering Trees*. Cambridge, MA: MIT Press.

Riede, Tobias & Klaus Zuberbühler. 2003. The relationship between acoustic structure and semantic information in Diana monkey alarm vocalization. *Journal of the Acoustical Society of America* 114, 1132–1142.

van Riemsdijk, Henk. 1998. Categorial feature magnetism: The endocentricity and distribution of projections. *Journal of Comparative Germanic Languages* 2, 1–48.

van Riemsdijk, Henk. 2008. Identity avoidance: OCP effects in Swiss relatives. In Robert Freidin, Carlos P. Otero & Maria Luisa Zubizarreta (eds.), *Foundational Issues in Linguistic Theory: Essays in Honor of Jean-Roger Vergnaud*, 227–250. Cambridge, MA: MIT Press.

Rizzi, Luigi. 1997. The fine structure of the left periphery. In Liliane Haegeman (ed.), *Elements of Grammar: Handbook in Generative Syntax*, 281–337. Dordrecht: Kluwer.

Ross, John R. 1972. The category squish: Endstation Hauptwort. *Chicago Linguistic Society* 8, 316-328.

Samuels, Bridget. 2014. On the biological origins of linguistic identity. In Kuniya Nasukawa & Henk van Riemsdijk (eds.), *Identity Relations in Grammar*, 341–364. Berlin: Mouton de Gruyter.

Samuels, Bridget. 2015. Biolinguistics in phonology: a prospectus. *Phonological Studies* 18, 161–171.

Sandler, Wendy, Irit Meir, Svetlana Dachkovsky, Carol Padden & Mark Aronoff. 2011. The emergence of complexity in prosody and syntax. *Lingua* 121, 2014–2033.

Shannon, Claude E. 1948. A mathematical theory of communication. *The Bell System Technical Journal* 27, 379–423.

Soderstrom, Melanie, Kenneth Wexler & Peter W. Jusczyk. 2002. English-learning toddlers' sensitivity to agreement morphology in receptive grammar. In Barbora Skarabela, Sarah Fish & Anna H.-J. Do (eds.), *Proceedings of the 26th Annual Boston University Conference on Language Development*, vol. 2, 643–652. Somerville, MA: Cascadilla Press.

Steuber, Lucas Carl. 2011. Disordered Thought, Disordered Language: A corpus-based description of the speech of individuals undergoing treatment for schizophrenia. MA thesis, Portland State University.

Tkachman, Oksana & Wendy Sandler. 2013. The noun-verb distinction in two young sign languages. *Gesture* 13, 253–286.

Towse, John N. 1998. On random generation and the central executive of working memory. *British Journal of Psychology* 89, 77–101.

Towse, John N. & Derek Neil. 1998. Analyzing human random generation behavior: A review of methods used and a computer program for describing performance. *Behavior Research Methods, Instruments, & Computers* 30(4),583–591.

Towse, John Nicholas, Andrea Sarah Towse, Satoru Saito, Yukio Maehara & Akira Miyake. 2016. Joint cognition: Thought contagion and the consequences of cooperation when sharing the task of random sequence generation. *PLoS ONE* 11(3), e0151306.

Tsimpli, Ianthi-Maria. 2013. (Evidence for) the language instinct. In Cedric Boeckx & Kleanthes K. Grohmann (eds.), *The Cambridge Handbook of Biolinguistics*, 49–68. Cambridge: Cambridge University Press.

Tyrone, Martha E. & Bencie Woll. 2008. Palilalia in sign language. *Neurology* 70, 155–156.

Vetter, Petra, Grace Edwards & Lars Muckli. 2013. Transfer of predictive signals across saccades. *Frontiers in Psychology* 3, 176.

de Waal, Frans & Pier Francesco Ferrari. 2010. Towards a bottom-up perspective on animal and human cognition. *Trends in Cognitive Sciences* 4(5), 201–207.

Walter, Mary Ann. 2007. Repetition avoidance in human language. Cambridge, MA: MIT dissertation.

Washabaugh, William. 1986. *Five Fingers for Survival: Deaf Sign Language in the Caribbean*. Ann Arbor, MI: Karoma Press.

Watson, Stuart K., Simon W. Townsend, Anne M. Schel, Claudia Wilke, Emma K. Wallace, Leveda Cheng, Victoria West & Katie E. Slocombe. 2015. Vocal learning in the functionally referential food grunts of chimpanzees. *Current Biology* 25, 1–5.

Yip, Moira. 1988. The Obligatory Contour Principle and phonological rules: A loss of identity. *Linguistic Inquiry* 19, 65–100.

Zuberbühler, Klaus. 2010. Referential labelling in Diana monkeys. *Animal Behaviour* 59, 917–927.

Evelina Leivada
UiT-The Arctic University of Norway
Department of Language and Culture
Hansine Hansens veg 14
9037 Tromsø
Norway

evelina.leivada@uit.no

Cognitive Phonetics:
The Transduction of Distinctive Features at the Phonology–Phonetics Interface

Veno Volenec & Charles Reiss

We propose that the interface between phonology and phonetics is mediated by a transduction process that converts elementary units of phonological computation, features, into temporally coordinated neuromuscular patterns, called 'True Phonetic Representations', which are directly interpretable by the motor system of speech production. Our view of the interface is constrained by substance-free generative phonological assumptions and by insights gained from psycholinguistic and phonetic models of speech production. To distinguish transduction of abstract phonological units into planned neuromuscular patterns from the biomechanics of speech production usually associated with physiological phonetics, we have termed this interface theory 'Cognitive Phonetics' (CP). The inner workings of CP are described in terms of Marr's (1982/2010) tri-level approach, which we used to construct a linking hypothesis relating formal phonology to neurobiological activity. Potential neurobiological correlates supporting various parts of CP are presented. We also argue that CP augments the study of certain phonetic phenomena, most notably coarticulation, and suggest that some phenomena usually considered phonological (e.g., naturalness and gradience) receive better explanations within CP.

Keywords: phonology–phonetics interface; Cognitive Phonetics; distinctive features; transduction; neurobiology of language

1. Introduction

This paper aims to elucidate the nature of a cognitive system that takes as its input a representation consisting of distinctive features (i.e., the output of the phonological module) and generates a representation directly interpretable by the neuromuscular system associated with speech production. This system we will call 'Cognitive Phonetics' and the representations it generates 'True Phonetic Representations'.[1]

This paper draws on both the phonological and phonetic literature. Unsurprisingly, as generative linguists, our interpretation of these two traditions conflicts rather sharply with that of more phonetically oriented scholars. Thanks to the critical comments of two such reviewers, we have tried to clarify our assumptions and inferences about both phonetics and phonology. Even if these perspectives remain incommensurable, we hope to have made the sources of disagreement and incompatibility more evident in light of the reviews we received.

[1] The way we use the term 'representation' here is slightly different than is customary in generative linguistics, where a representation is taken to be an abstract characterization of implicit

ISSN 1450–3417

Here we will concentrate solely on speech (pre)production, leaving the perceptual direction of this system aside whenever possible. In line with the theme of this volume, our inquiry is a resuscitation of certain proposals made by Eric Lenneberg 50 years ago (see section 2), recast in the modern biolinguistic research program advocated by David Poeppel and colleagues as an attempt to unify theoretical linguistics and cognitive neuroscience.

Our point of departure is a fairly well-established claim: Surface (also known as 'phonetic' or 'output') representations of the phonological component of a generative grammar are matrices of distinctive features (where columns represent segments).[2] During most of the 1960s, it was usually assumed that the features of underlying and surface representations are entities of a different kind, the former being binary, the latter gradual scales (Chomsky & Halle 1968: 297). However, one aspect of Postal's (1968) 'naturalness condition'—the statement that a surface representation is identical (and therefore composed from the same set of representational elements) to its underlying representation except as requested otherwise by phonological rules—seems to have been, often tacitly, adopted over the following decades, after a brief period of uncertainty. Thus in early 1970s, in an influential compendium on the contemporary issues in phonological theory, Maran (1973), discussing classificatory (phonological) and phonetic features, concluded that

> [w]e do not, however, claim at this stage that the set of abstract phonological features is identical in membership to the set of phonetic features. There are many things which remain unclear. (Maran 1973: 73)

But already by the late 1970s a consensus seems to have emerged that underlying and surface representations do consist of the same vocabulary of features:

> Assuming that utterances are best represented as a string of feature matrixes at the phonetic level, we can raise the question of how sounds are represented for the purpose of phonological description (i.e., in the UR and at all intermediate levels). [...] [A] fundamental tenet of generative phonology has been that sounds are most properly represented at these levels in the same way they are phonetically—namely, as feature matrixes in which each feature describes an articulatory and/or acoustic property of the sound. (Kenstowicz & Kisseberth 1979: 239)

If we assume that URs and SRs belong to the same cognitive module, that is, the phonological module, and if we assume that a 'module' may operationally be defined as an encapsulated computational system that operates over a particular kind of abstract units (Boeckx 2009: 125–127), if follows that all levels of phonological

---

linguistic knowledge. We use the term in a broader sense, as a scientific abstraction in general, similar to how H_2O 'represents' water in formal stating of chemical processes. The main difference between a surface representation and a true phonetic representation, as will be shown in greater detail in section 4, is that the former represents knowledge (competence), and the latter represents information feeding speech production. This more general sense of usage is in line with Marr's (1982/2010: 20) definition of 'representation' as "a formal system for making explicit certain entities or types of information together with a specification of how the system does this".

[2] Other data structures have been proposed, such as the feature geometry trees of Sagey (1986) and related work, but the simpler feature matrix structure is sufficient for our discussion.

representation are built from *the same* set of primitives (Hale & Kissock 2007: 83). Thus the output of the phonological module, the surface representation, also consists of matrixes of distinctive features.

We understand distinctive features here as a particular kind of substance-free units of mental representation, neither articulatory nor acoustic in themselves, but rather having articulatory and acoustic *correlates*, as Halle (1983/2002: 108–109) and Reiss (2018, chapter 15.7) have pointed out. Many influential phonological texts have stated over the last several decades that features serve as a bundle of information that the brain sends to the articulators (if speech is the chosen modality). Here are three examples of such statements:

> In articulatory terms each feature might be viewed as information the brain sends to the vocal apparatus to perform whatever operations are involved in the production of the sound, while acoustically a feature may be viewed as the information the brain looks for in the sound wave to identify a particular segment as an instance of a particular sound.
>
> (Kenstowicz & Kisseberth 1979: 239)

> [...] [T]he distinctive features correspond to controls in the central nervous system which are connected in specific ways to the human motor and auditory systems. [...] In producing speech, instructions are sent from higher centers in the nervous system to the different feature boxes in the middle part of (5) ['tone', 'vocal', 'labial' etc.—vv & cr] about the utterance to be produced. (Halle 1983/2002: 109)

> The [...] featurally specified representation constitutes the format that is both the endpoint of perception - but which is also the set of instructions for articulation. (Poeppel & Idsardi 2011: 179)

If one thinks about how exactly features engage the articulatory system, it becomes apparent that there is a substantial conceptual gap between features and neural structures or activities. At present there is no way to link either the general concept 'distinctive feature' or any of the particular features (e.g., [CORONAL]) to any known neural structure (e.g., dendron, neuron, cortical column etc.) or activity (e.g., long term potentiation, oscillation, synchronization etc.; see Embick & Poeppel 2015). In fact, there seems to be very little understanding of how the brain exactly represents and computes any of the units or processes that are part of linguistic competence (Chomsky 2000a; Gallistel & King 2010; Mausfeld 2012). In other words, the units of linguistic computation and the units of neurological computation—as currently understood—are mostly incommensurable. This problem was therefore dubbed 'the ontological incommensurability problem' by Poeppel & Embick (2005). The proposed solution to it is to decompose a particular linguistic domain (e.g., phonology) into formal units and operations that are as basic and as generic as possible, and then formulate biologically plausible and scientifically productive 'linking hypotheses' across the fields of linguistics and neuroscience (Poeppel & Embick 2005, Poeppel 2012, Embick & Poeppel 2015).

The main goal of this paper is to formulate a hypothesis about the 'intermodular bridge' (Pylyshyn 1984: 147) from the symbolic and substance free (phonology) to the physical and substantive (phonetics). By pursuing this line of inquiry a modest attempt is made to formulate a theory of the phonology–phonetics interface[3] in strict biolinguistic terms, that is, in such a fashion that it can be linked to the kind of neurobiological activity that we might plausibly find in a neuromuscular system. Distinctive feature theory was initially outlined by Roman Jakobson in a lecture delivered in 1928 (see Jakobson 1971: 3–6) and in an often overlooked paper from the late 1930s (Jakobson 1939), and subsequently elaborated by Jakobson, Fant & Halle (1952) and Jakobson & Halle (1956). The idea of a 'distinctive feature' was founded upon purely phonological—that is, non-biological and non-cognitive—insights about phonemic oppositions in the vein of Trubetzkoy (1939/1969), as shown in the following passage:

> Any minimal distinction carried by the message confronts the listener with a two-choice situation. Within a given language each of these oppositions has a specific property which differentiates it from all the others. The listener is obliged to choose either between two polar qualities of the same category, such as *grave* vs. *acute*, *compact* vs. *diffuse*, or between the presence and absence of a certain quality, such as *voiced* vs. *unvoiced*, *nasalized* vs. *non-nasalized*, *sharpened* vs. *non-sharpened (plain)*. The choice between the two opposites may be termed *distinctive feature*. The distinctive features are the ultimate distinctive entities of language since no one of them can be broken down into smaller linguistic units. The distinctive features combined into one simultaneous or [...] concurrent bundle form a *phoneme*. (Jakobson, Fant & Halle 1952: 2)

Despite many revisions of the theory during the following decades (e.g., Chomsky & Halle 1968: 298–329, Halle & Clements 1983, Clements 1985, Clements & Hume 1995), it stands to reason that distinctive feature theory was never meant to face one of the more difficult questions of modern biolinguistics and of cognitive neuroscience in general, namely, how to bridge the gap between a cognitive faculty, in this case phonological competence partly represented by features, and brain. The existence of features themselves should not be in question—they have withstood almost a century of rational and empirical scrutiny and are considered "to be a scientific achievement on the order of the discovery and verification of the periodic table in chemistry" (Jackendoff 1994: 60). Also clear is the fact that features *are* somehow interpreted by the sensorimotor (SM) system because utterances are effectively externalized and perceived/parsed. Therefore, a question that logically follows from these facts is how exactly to get from discrete, timeless, abstract cognitive entities (features), on the one hand, to temporally arranged articulatory movements and ultimately to continuously varying sound waves, on the other.

Here we will adopt the position that cognition, including linguistic cognition, is best understood as a set of modules (see Chomsky 1984 and Curtiss 2013 for justification), each of which is characterized by mappings involving inputs and outputs in a particular format (Reiss 2007, section 2.1). Modules are connected

---

[3] An influential source on this topic is the collection of papers in the special issue of the *Journal of Phonetics* (1990) dedicated to the relationship between phonetics and phonology. In the course of this paper, we will address what we consider as some shortcomings of these previous discussions of the phonology–phonetics interface.

via ‘interfaces’—configurations in which the outputs of one module serve as the inputs to another module. We argue that the interface between the phonological component of the grammar and phonetics (in this case starting with the neurophonetics of speech production, that is, with sending efferent neural commands to speech organs) is mediated by a system that transduces features into True Phonetic Representations—arrays of temporally coordinated neuromuscular information directly interpretable by the motor system in charge of speech production. An assumption that is interleaved in this proposal is that distinctive features, as currently conceived in modern literature, are not *directly* intelligible to the SM system. It is a non-trivial matter to show why this is so, and we return to this issue in section 3. Thus, our research question is that of transduction of distinctive features at the phonology–phonetics interface, which necessarily precedes speech production. A convenient and productive way to fractionate this question and begin to approach it is to adopt Marr’s (1982/2010) three level perspective that specifies—for any cognitive information-processing system—its computational level (‘What is computed and why?’), algorithmic level (‘How is it computed?’), and implementational level (‘How is it realized physically?’). It should be noted that these three levels of analysis *do not* state some fundamental truth about cognitive systems in general (e.g., that every cognitive system consists of three levels); rather, these are explanatory devices that provide a convenient way of dividing a cognitive system in order to study it, or in Marr’s (1982/2010: 24) words, these are “the different levels at which an information-processing device must be understood before one can be said to have understood it completely”. Since the cognitive system under study is an information-processing device, we will frame our discussion in Marr’s terms.

The rest of the paper is structured as follows. In section 2 we revisit Lenneberg’s (1967) *Chapter Three* where he introduces abstract neuromuscular schemata to account for the transformation of basic phonological units, segments in his case, into muscular events. In section 3, we state in more detail some general properties of Cognitive Phonetics, our proposed interface theory; we show how it can be constrained by both phonological and phonetic considerations; and we provide arguments for why features need to be transduced before a representation can be legible to the SM system. In section 4, we define the transduction of features into True Phonetic Representations following Marr’s (1982) tri-level approach and we explore its neurobiological substrate. In section 5, we pursue several direct consequences of viewing the phonology–phonetics interface this way and introduce the concept of ‘intrasegmental coarticulation’. We conclude (section 6) by summarizing our results and by pointing out some further research strategies that follow directly from our insights.

2. Lenneberg’s Neuromuscular Schemata

Lenneberg (1967: 89–90) was well aware of the complexity of the relationship between discrete, logically ordered phonological units (phonemes, segments) on the one hand, and continuous articulatory movements with concomitant acoustic results on the other. He recognized that although some acoustic discontinuities corresponding to segment transitions are detectable in a spectrogram, in general, these boundaries are not apparent, and the acoustic record of speech provides very limited information about phonological organization. This complexity is of course mirrored in speech production, since discrete sequences of segments correspond to continuous movements of physical systems: "[w]hen we think of the entire musculature of the speech apparatus in activity, we realize that there is a continuous waxing and waning in states of contraction throughout these muscles" (Lenneberg 1967: 90). The relation between phonological units and articulatory movements is further complicated by various directions, scopes and types of segmental coarticulation: "[t]he muscular activity associated with one phoneme is influenced by the phonemes that precede and follow it" (Lenneberg 1967: 92). As was already understood at that time (Öhman 1966, 1967), and as subsequent research has confirmed (Hardcastle & Hewlett 1999), coarticulation is a ubiquitous phenomenon that obliterates the neat, beads-on-a-string-like succession of phonological segments. A further problem that Lenneberg emphasized is that the order and duration of events at different levels of phonetic organization—perceptual, acoustic, neural—are not perfectly aligned:

> The perceptual order of speech sounds need not be identical with the order of acoustic correlates (we may ignore or fail to hear certain acoustic phenomena); the order of acoustic events need not be identical with the order of motor or articulatory events (movements occur that do not produce sound or sound-changes); the order of central neuronal events may be different from the order of peripheral motor events (certain nervous impulses must be initiated in advance of others because traveling time to the periphery is longer for some pathways [e.g., the recurrent nerve supplying the muscles of the larynx—vv & cr] than others [e.g., the trigeminal nerve innervating the muscles of the jaw—vv & cr]). (Lenneberg 1967: 93)

Lenneberg's discussion illustrates how segmental units of surface representations radically differ from their realizations. The former are discrete, timeless, neatly ordered mental abstractions, the latter continuous, dynamic, overlapping, coordinated movements of respiratory, phonatory and articulatory organs. The magnitude of this mismatch is even greater when we take into account the tremendous complexity of the neuromuscular mechanisms by which mental representations are realized. The production of speech is the most complex neuromuscular activity human beings ever come to master, requiring temporal coordination of over 100 muscles controlled by more than 1400 motor commands per second (Stetson 1951, Lenneberg 1967: 91–92, Laver 1994: 1). Stated this way, it becomes apparent that the mental unit represented as [t] on the one hand, and the sound of producing that unit on the other, are separated by a considerable gap. The problem, then, is to explicitly relate the two sides, taking into account their fundamentally different natures.

Lenneberg (1967: 98–107) proposed a two-step process which, essentially, transmutes segments into real-time muscular activity. A few caveats are due before sketching his proposals. First, Lenneberg's discussion is based on the production of idealized utterances. His examples are not drawn from observed speech, but are models of the process of speech production applied to hypothetical tokens. A related second point is that Lenneberg's proposal is not intended as part of a psycholinguistic theory of language use, what is sometimes called a 'psychologically real' model of speech production. Similar to the components of Marr's (1982) tri-level analysis, the components of Lenneberg's model are "theoretical stages that help us visualize the complications of speech production" (Lenneberg 1967: 99). Third, Lenneberg takes segments, not distinctive features, to be the basic phonological units, and uses a traditional structuralist terminology—'phonemes' for abstract segmental distinctive units, 'phones' for their intended realizations. One of our primary goals in this paper is to show how Lenneberg's insights can be further developed by combining them with a finer level of phonological representation using distinctive features.

Lenneberg's model, as shown in Figure 1, takes a string of phones as its input and applies two operations: (1) it assigns muscle activity to each phone; (2) it orders that muscle activity temporally.

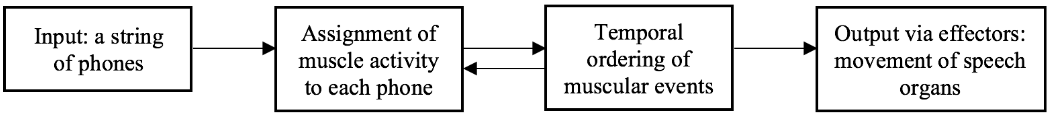


Figure 1: Diagram of hypothetical transduction processes involved in speech production. Based on Lenneberg (1967: 99).

Both medial processes of Figure 1 may be represented in a form of a schema. Lenneberg represented the assignment of muscle activity to each phone with a table where columns stand for successive phones, and rows for muscles relevant for their production (Figure 2).

	string of phones					
muscles	I	II	III	IV	V	VI
a	+	+	+	0	+	0
b	0	+	+	+	0	+
c	+	+	0	+	+	0
d	+	0	0	+	+	+
e	+	0	+	+	0	+
f	0	+	0	0	+	+

Figure 2: Schema of the process of assigning muscle activity to a string of phones. Based on Lenneberg (1967: 100).

This schema is intended as a matrix indicating which muscles are to be contracted in order to produce a given speech sound. Rows correspond to specific muscles (abstractly labeled from *a* to *f*), columns to phones; '+' means contraction of a given muscle, '0' means relaxation. For example, the schema in Figure 2 indicates that in order to produce phone IV it will be necessary to contract muscles *b*, *c*, *d*, *e*. Naturally, in actual cases of realization of phones, many more muscles are involved. The next step in transduction is to order muscular activity from Figure 2 temporally. This process is illustrated in Figure 3. A simplifying assumption is that the relevant muscles may be grouped into classes, here denoted as α through δ, ranked according to the time it takes neural impulses to travel from the brain stem and to reach the muscles in each class. Thus the α class of muscles has an activation latency that is four times greater than the δ class, three times greater than γ, and two times greater than β.

muscles grouped by activation latency

α	β	γ	δ
			a
b			
	c		
		d	
e			
		f	

temporal segments

1	2	3	4	5	6	7	8	9
			+	+	+	0	+	0
0	+	+	+	0	+			
	+	+	0	+	+	0		
		+	0	0	+	+	+	
+	0	+	+	0	+			
		0	+	0	0	+	+	

Figure 3: Schema of the process of temporal ordering of muscle activity for a given string of phones. Based on Lenneberg (1967: 101).

A further simplification is that in this schema all phones are assumed to be of equal duration.[4] Based on the classification of relevant muscles into latency groups, shown in the left table of Figure 3, the schema from Figure 2 is rearranged to obey this relative temporal order. The table on the right in Figure 3 shows that if a string of phones I to VI is to be realized correctly, then the first neuromuscular event to occur is the firing of impulses for contraction of muscle *e*; after that muscles *b* and *c* contract but *e* relaxes, and so on. Due to temporal shifting of the muscles associated with particular phones, the columns in this schema can no longer be put into one-to-one correspondence with the segments in the phonological string. It is here that the phonemic 'Easter eggs' are smashed (Hockett 1955: 210) and coarticulatory effects begin to emerge.[5] Therefore, each column in the right schema of Figure 3 corresponds to a 'temporal segment' which indicates, for a given point in time, which muscles need to be contracted or relaxed. Unfortunately, Lenneberg does not discuss the details of this temporal arrangement. For example, he leaves unresolved the question of how much time does one cell denote—5 ms, 10 ms, 20 ms? Time is represented abstractly in Figure 3, from 1 to 9, a reflection of the hypothetical and tentative nature of his discussion, that is, "merely stat[ing] what the neuronal firing order is on some given level in the brain" (Lenneberg 1967: 102).

The result of both steps in the transduction of phones into a neuromuscular schema is given in Figure 4. For each unit of time (abstractly denoted here

---

[4] This is a curious assumption/simplification on Lenneberg's behalf since four pages prior to describing the transduction of segments into neuromuscular schemata he discusses timing problems arising from differences in segmental duration (cf. Lenneberg 1967: 96–97). In fact, temporal discrepancies on various levels of phonetic organization are what initially prompted him to devise such a model of transduction.

[5] The corresponding quote referenced here is as follows:

> Imagine a row of Easter eggs carried along a moving belt; the eggs are of various sizes, and variously colored, but not boiled. At a certain point, the belt carries the row of eggs between the two rollers of a wringer, which quite effectively smash them and rub them more or less into each other. The flow of eggs before the wringer represents the series of impulses from the phoneme source; the mess that emerges from the wringer represents the output of the speech transmitter. At a subsequent point, we have an inspector [i.e., a hearer—vv & cr] whose task it is to examine the passing mess and decide, on the basis of the broken and unbroken yolks, the variously spread-out albumen, and the variously colored bits of shell, the nature of the flow of eggs which previously arrived at the wringer. Note that he does not have to try to put the eggs together again—a manifest physical impossibility—but only to identify. (Hockett 1955: 210)

as a temporal segment), the schema specifies which muscle needs to contract and across how many such units, that is, for how long. Within each column, events are assumed to be simultaneous. Notice that for example a_{I} in the fourth temporal segment, which is a muscle contraction associated with the phone ordered first in the string of Figure 2, is preceded by four muscle contractions unrelated to that phone (b_{II}, b_{III}, c_{II}, c_{III}).

The anticipation of future events emphasizes the need for a model of speech preproduction that feeds the sensorimotor system with "a hierarchic plan in which events are selected [...] as an integration of all elements within units of several seconds duration" (Lenneberg 1967: 103). For reasons discussed at length (see especially Lenneberg 1967: 102–107), Lenneberg on page 106 explains that a 'sequential chain model' that scans the surface representation from 'left to right', interpreting linearly ordered segments, is not a viable model for relating phonology to phonetics. Instead, what is needed is a 'central plan model' of speech preproduction, which Lenneberg described as follows:

temporal segments							
1	2	3	4	5	6	7	8
e_{I}	b_{II}	b_{III}	a_{I}	a_{II}	a_{III}	d_{IV}	a_{V}
	c_{I}	c_{II}	b_{IV}	c_{IV}	b_{VI}	f_{V}	d_{VI}
		d_{I}	e_{IV}		c_{V}		f_{VI}
		e_{III}	f_{II}		d_{IV}		
					e_{VI}		

Figure 4: A neuromuscular schema as a result of transduction of a string of phones into information directly interpretable by the SM system. Based on Lenneberg (1967: 102).

> On the lowest level, muscular contractions belonging to different speech sounds intermingle and therefore their sequencing cannot be programmed without considering the order of the speech sounds to which they belong. But the choice and sequencing of speech sounds cannot take place without knowledge of the sequence of morphemes to which the sounds belong. [Compare the two different pronunciations of the article *the* depending on whether the following morpheme begins with a consonant or a vowel—vv & cv] [...] On the next higher level, the level of morphemes, we encounter again the phenomenon of intermingling of elements and an impossibility to plan the sequence without insight into the syntactic structure of higher constituents. [...] On a still higher level, the level of immediate constituents, [...] syntactic elements cannot be ordered without knowledge of the entire sentence. (Lenneberg 1967: 106)

The need for a hierarchical central plan for speech production is thus just a specific example of a more general requirement for all levels of linguistic computation and behavior, a requirement that probably extends into other behavioral domains such as navigating through space.

In summary, Lenneberg (1967, chapter 3) already recognized the complexities involved in transforming a mental representation of a string of phones into a temporally coordinated sequence of muscular contractions. The result of this transduction may be understood as a neuromuscular schema such as given in Figure 4. The sequential arrangements of muscular events require preplanning with anticipation of later events. Therefore, the occurrence of some events is contingent upon other events yet to come, which may be adduced as proof that sequencing on a neuromuscular level is not accomplished by a sequential chain model (i.e., by scanning and interpreting a string of segments), but rather by a complex central plan model. The observed interdigitation of muscular correlates of a given phone is mirrored on higher levels of organization, for which a central plan model is also required. The importance of Lenneberg's work, foundational to biolinguistics, derives from his capacity to invoke and synthesize concepts and results from domains as diverse as phonology, phonetics, physiology and neurology.

3. Phonology–Phonetics Interface (PPI)

One of the points that emerged from the previous discussion is that relating phonology and phonetics is a non-trivial and complex task. Lenneberg's views were generally a step in the right direction because he understood the need to explicitly address the conceptual gap between the units and operations characteristic of these two systems. Yet, there is room for further improvement by adopting ideas and findings that were mostly unavailable in the 1960s. In particular, the discussion of the phonology–phonetics interface (PPI) can be constrained from 'both sides', that is, by strictly adopting a constrained phonological theory which feeds the interface in production (section 3.1), and by using insights from modern models of speech production which are fed by this interface (section 3.2).

3.1. Phonology

On the phonological side, we assume a generative substance-free approach (Hale & Reiss 2000a, 2000b, 2008, Reiss 2018, Bale & Reiss 2018). Phonology is understood here as a component of the language faculty that involves formal computations over discrete symbolic units such as distinctive features, syllables, feet etc. Since phonology is a part of the knowledge of language, by definition "all the work in phonology is internal to the mind/brain" (Chomsky 2012: 48). Furthermore, representations involved in phonology are abstract and symbolic, that is, devoid of articulatory, acoustic, typological, statistical etc. information; computations involved in phonology treat features and other phonological units as arbitrary symbols (Hale & Reiss 2008: 169). All representational levels of the phonological component of a generative grammar—underlying, surface, and intermediate—consist of distinctive features (and perhaps markers of other segmental and suprasegmental structure, such as syllable or foot boundaries, which need not detain us here). This means that features are part of the 'representational alphabet' of the phonological module. Representational levels are related by ordered phonological rules which serve as the computational aspect of phonology (Vaux 2008).

It is important to distinguish between computation and transduction. Computation is the formal manipulation (reordering, regrouping, deletion, addition, etc.) of representational elements *within* a module, and *without* a change in the representational alphabet. Transduction is a process of converting an element in one form into a distinct form, that is, a mapping between dissimilar formats. For example, in the process of hearing, air pressure differentials are transduced into biomechanical vibrations of the tympanic membrane and the ossicles of the middle ear, which are transduced via the oval window into fluidic movements within the cochlea, which are in turn transduced by the organ of Corti into electrical signals which are passed on for further processing in the nervous system. The distinction between computation and transduction facilitates conceptualizing the notion of modularity. A module can be thought of as a device which takes input representations and computes over them, generating thereby an output in the same representational alphabet. Modules of the mind (and of organic systems more generally) are linked by transducers which convert information in one form into a form required by the computational module fed by the conversion process. An interface between modules is therefore defined by (1) the form of the input, (2) the form of the output, and (3) a set of transformations that relate (1) to (2).

By virtue of the form of its representations and operations, each module imposes 'legibility conditions' at its interfaces: If some information is to be legible to a given module, that information must come in a specific form in which that module operates (Chomsky 2000a: 9–14). Otherwise, that information would either not be received by that module at all or would be treated as noise (perhaps as human speech is noise to dogs which lack the needed cognitive modules and transducers, even though their auditory system is far superior to that of humans).[6] The SM system imposes certain legibility conditions on phonology, the component of the grammar with which it interfaces, most notably the condition that information must have a linear arrangement (one cannot produce eleven words in parallel) with certain temporal properties (one cannot produce a polysyllabic word in three nanoseconds). Linearity is a complex notion (see Cairns & Raimy 2011, Idsardi & Raimy 2013). For example, in phonological representations, several tiers may be distinguished (segmental, moraic, prosodic, etc.), leading to a kind of multilinearity characteristic for autosegmental phonology; also, in speech, many overlapping articulatory events may be detected, as will be shown in more detail in section 4. Nonetheless, the general idea of linearity, namely, that sequential ordering and precedence relations among basic units play an important role, seems to hold for both phonology and phonetics, unlike for syntax (Chomsky 1995: 334–340, Everaert et al. 2015). Another condition, to which we will return in more detail below, is the condition of bi-directionality: If the same phonological architecture is to be employed in both language comprehension and in speaking, that is, if it is *not* the case that humans use completely different grammatical devices for each direction,[7] then the atomic representational units of phonology, features, must integrate acoustic *and* articulatory correlates.[8] If a feature were defined exclusively in terms

---

[6] As Chomsky put it:

> To be usable, the expressions of the language faculty (at least some of them), have to be legible by the outside systems. So the sensorimotor system and the conceptual-intentional system have to be able to access, to 'read' the expressions; otherwise the system wouldn't even know it is there. (Chomsky 2000b: 17)

[7] In this context, consider Lashley: "The processes of comprehension and production of speech have too much in common to depend on wholly different mechanisms" (Lashley 1951: 186).

[8] A reviewer points out that it is possible that "the articulation system relies on sensory-motoric knowledge to implement the auditory targets" and that features might not have direct articulatory correlates. Assuming non-existence of articulatory correlates of features is problematic because it leads to the conclusion that if a speaker were to suddenly lose hearing, she or he would have to become completely unable to articulate since there is no audition to guide ar-

of, say, its articulatory correlates, as the feature [CORONAL] is, then in principle such a feature could not be used in phonological decoding.

In the phonological theory we adopt, features themselves are substance-free cognitive units (see Reiss 2018: chapter 15.7 for justification), that is, they do not contain information on the temporal coordination of muscle contractions, on the spectral configuration of the acoustic target to be reached, and so on. Yet without this information, the respiratory, phonatory and articulatory systems cannot produce speech. The motor system for speech production requires information about substance and time in order to arrange the articulatory score, therefore this information has to be integrated into a representation before being fed to the motor system. The most plausible way to escape this deadlock (i.e., phonology is substance free, but the SM system needs information about substance to produce speech) is to abandon the idea of a *direct*, unmediated interface between grammar/phonology and SM system, and posit a cognitive phonetic transduction system that converts distinctive feature matrices into True Phonetic Representations that provide the SM system with legible information needed to produce speech.

In summary:

- Outputs of the phonological module, surface representations (SRs) consisting of substance-free features, do not contain substantial and temporal information.

- The SM system requires articulatory, auditory and temporal information in order to produce speech.

  ∴ SRs are not legible to the SM system and phonology cannot in principle feed speech production *directly*.

  ∴ The interface between phonology and the SM system is mediated by transduction.

Before turning to the nature of this transduction system, let us review how modern models of speech production further constrain our approach to the PPI.

3.2. Speech Production

On the side of speech production, modern models such as DIVA (Guenther 1995a, 1995b, Guenther et al. 1998, 2006, Tourville & Guenther 2011, Guenther & Vladusich 2012), HSFC (Hickok 2012), LRM (Levelt et al. 1999, Indefrey & Levelt 2004), and MAPL (Poeppel & Idsardi 2011) provide several theoretical and empirical constraints on the nature of representations that directly feed the SM system during speech. In constructing his model of transduction of phones into neuromuscular schemata, Lenneberg (1967, chapter 3) made the assumption that this process involves reaching specific articulatory targets and took into consideration only the distribution of muscle contractions in time. However, more recent research showed

---

ticulation. It is known that a substantial decline in articulation can occur in such a case, but not a complete inability to articulate (Cowie & Douglas-Cowie 1992, Lane et al. 1997). Also, healthy speakers articulate intelligibly while their hearing is blocked by loud masking noise (Lombard 1911, Lane & Tranel 1971). We therefore remain unconvinced that there are no articulatory correlates of features.

that these targets include auditory information as well. Speech production is a mechanism in which feedforward and feedback processes are tightly and intricately related, as witnessed by the general architecture of the Directions Into Velocities of Articulators (DIVA) model, currently the most elaborate and empirically validated model of speech production (see Figure 58.3 of Guenther & Hickok 2016: 728). Manipulating a speaker's auditory feedback during speech production results in substantial compensatory changes in motor speech acts compared to undisturbed speech (Yates 1963, Guenther et al. 1998, Houde & Jordan 1998, Larson et al. 2001, Purcell & Munhall 2006, Hickok & Poeppel 2016, chapter 25, section 2.2.1). For example, if a subject is asked to produce one vowel and the feedback that she or he hears is manipulated so that it sounds like another vowel, then the subject will change the vocal tract configuration so that the feedback sounds like the original vowel. In other words, speakers will readily modify their articulations to hit an auditory target, suggesting that the goal of speech production involves an intricate relation between articulatory and auditory configurations. Furthermore, although individuals who become deaf as adults can remain intelligible for years after they lose their hearing, they show some speech production impairments immediately, including the inability to adjust pitch and loudness in different listening conditions, and over time they can exhibit substantial articulatory decline (Walstein 1990, Perkell et al. 2000). The fact that speakers are able to repeat speech acts that they heard, even when given speech acts are ad hoc inventions such as "zlurb", suggests that people effortlessly map between articulatory and auditory systems (see the work on the Memory-Action-Perception Loop by Poeppel & Idsardi (2011) for further discussion).

The Hierarchical State Feedback Control (HSFC) model (Hickok 2012) provides further corroboration for the view that features integrate both articulatory and auditory information by showing that speech production involves parallel activation of both auditory and motor units corresponding to the information provided by an appropriate mental representation, and also a sensory-motor coordinate transform network mediating auditory and acoustic programs. It has been well established that surface representations of the phonological module, spelled out in terms of features, serve as both the starting point of speech production and as the end-point of speech perception (Poeppel & Idsardi 2011, Idsardi & Monahan 2016). In an indirect manner, the groundwork for these findings was already laid by the Motor Theory of Speech Perception (Liberman et al. 1967, Liberman & Mattingly 1985), which posits that speech perception involves translating acoustic signals into motor gestures that produce them, and by the Acoustic Theory of Speech Production (Fant 1960, Stevens 1998), which highlights the importance of acoustic or auditory targets in the process of speech production. It follows logically from all this that distinctive features allow for mapping from auditory input to words and from words to action, and therefore must properly be defined via abstract articulatory *and* auditory correlates.

Modern neuropsychological and neurophysiological evidence indicates that the cognitive aspect of externalizing language through speech has two distinct stages, phonological and phonetic, lending further support for the necessity of cognitive phonetics as a mediating system between phonology and the SM system. The LRM model, named after its creators Levelt, Roelofs & Meyer (1999), explicates the successive stages of spoken word production, and clearly distinguishes between cognitive phonological computation and cognitive phonetic encoding. Indefrey & Levelt (2004) reviewed data from 82 imaging experiments and found that phonological operations are independently conducted within the average time window of 205 ms, *followed by* an average of 145 ms of cognitive phonetic processing. Evidence from aphasia also supports the dichotomy between phonological and phonetic cognitive processing (Buchwald & Miozzo 2011, 2012). Consider the words *pill* and *spill* in English. Both are assumed to contain the segment /p/ in their underlying representations; in the surface representation the former has [p^{h}] and the latter [p]. It is of interest to determine what exactly happens when an aphasic patient simplifies a consonant cluster so that /s/ does not get realized in a word like *spill*. Will the resultant realization of /p/ be aspirated, consistent with the notion that the deletion of /s/ occurred within the phonological module (i.e., before motor plans for a cluster are implemented), or will it be produced without aspiration, reflecting the conception that the phonological mapping /sp/ → [sp] was left intact and that the deletion of the fricative occurred after phonological computation? Buchwald & Miozzo (2011) measured VOT productions of two aphasic patients who did not realize /s/ in /sp/, /st/, /sk/ clusters and compared these with realizations of correctly produced consonants. Results showed two different patterns of production, with one patient producing the initial stop consonant with a long VOT ([p^{h}]), and the other producing it with a short VOT ([p]). These findings have been taken to suggest that the errors of the former patient were phonologically based and the errors of the latter patient were phonetically based and "are consistent with an account of spoken production containing at least two processing levels that can be selectively impaired by brain damage: one processing stage [i.e., cognitive phonological] with context independent representations and another [i.e., cognitive phonetic] with context-specific representations" (Buchwald & Miozzo 2011: 1118). Similar results emerged in examination of durational properties of nasal consonants when deleted in /sn/ and /sm/ clusters (Buchwald & Miozzo 2012).

In summary, modern research into speech production, and to a lesser extent speech perception, constrains our approach to the PPI insofar as it shows

(1) that the target of speech production is a complex representation that integrates both articulatory and auditory information;

(2) that speech production is strongly influenced by auditory and somatosensory feedback;

(3) that features have abstract articulatory and acoustic correlates, as demanded by (1) and (2);

(4) that cognitive aspects of externalizing language through speech have two distinct stages: a substance-free computational stage (phonology) and a substantial transduction stage (cognitive phonetics).

3.3. An Interface Theory: Cognitive Phonetics

Cognitive Phonetics (CP) is a theory of the phonology–phonetics interface (PPI). It is motivated by the conceptual distance between the characteristics of phonology as

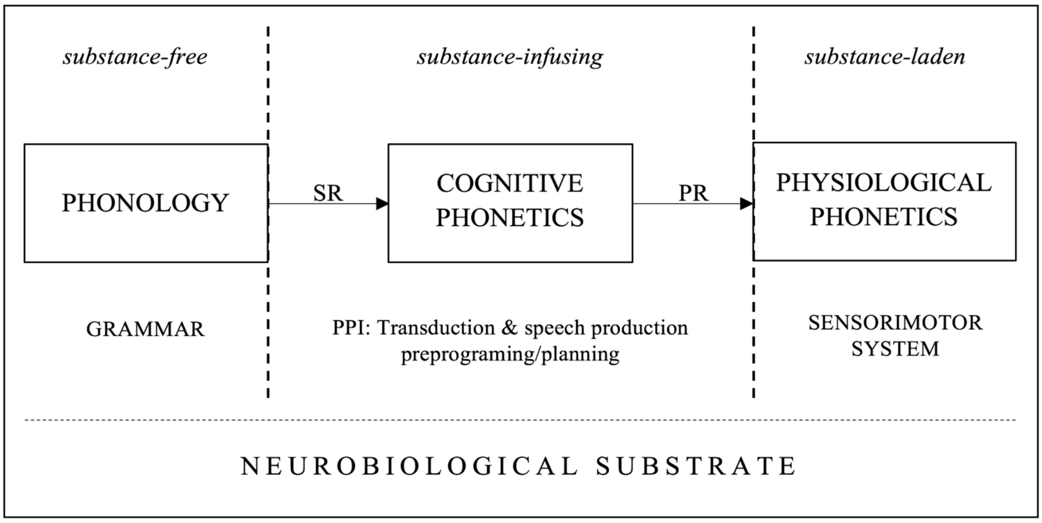


Figure 5: The architecture of the phonology–phonetics interface and the place of Cognitive Phonetics within it.

shown in section 3.1 on the one hand, and the characteristics of the speech production mechanism as shown in section 3.2 on the other. CP proposes that the output of the grammar is transduced into a representation that contains substance-related information required by the SM system in order to externalize language through speech. Figure 5 illustrates the general architecture of the PPI and the place of CP within it.

Recall that our present focus on speech externalization, without discussion of speech perception and phonological comprehension, is a matter of expository convenience, not a claim about the purview of CP. As the interface between phonology and phonetics, CP is a bi-directional system, thus also relevant for transduction in the direction of perception, that is, for decomposing, parsing, and mentally representing the sound of speech (Reiss 2007, section 2.5, Poeppel et al. 2008). Therefore, in the 'input' direction, CP serves as "the bridge from the physical to the symbolic" (Pylyshyn 1984: 152). In the 'output' direction, which is our focus here, CP is the bridge from the symbolic to the physical, relating the substance-free (phonology) to the substance-laden (physiological phonetics).

CP is fed by the output of the phonological grammar, and directly feeds the sensorimotor (SM) system associated with speech production. CP is substance-infusing in the sense that it provides the means to externalize language through speech in real time using human neurophysiological machinery. The movements of various organs and the subsequent acoustic consequences comprise the substance-laden aspect of speech traditionally associated with articulatory and acoustic phonetics. CP is a transduction system, which means it changes inputs of one ontological type into outputs of another. The input to CP is a mental representation comprised in part of abstract distinctive features. The output is a representation that contains information on the auditory target to be reached, the muscles necessary to realize a given input, and their temporal arrangement. Outputs of phonology are interchangeably called in the literature 'surface' representations and 'phonetic' representations, while representations from which these are derived are called 'underlying' or 'phonological' representations (Kenstowicz 1994: 60). Since both are *phonological* representations, that is, encoded in the primitives of the phonological module, it is misleading to call only one representational level phonological. Therefore, in line with our ideas regarding the PPI and CP, we propose a terminological clarification. Inputs to phonology, typically conceived of as strings of concatenated morphemes, we will call 'underlying phonological representations' (UPR); outputs of phonology, which are the inputs CP, will be called 'surface phonological representations' (SPR); and outputs of CP 'true phonetic representations' (TPR); or, for short, 'underlying representations' (UR), 'surface representations' (SR), and 'phonetic representations' (PR), respectively. URs and SRs are part of phonology; PRs are extragrammatical, non-phonological entities.

It is an understatement to say that progress in solving the ontological incommensurability problem in all cognitive domains has been modest. In this light, the fact that we are still talking about theoretical abstractions (e.g., PRs) and not solely in terms of neurobiological processes does not reflect a commitment to any sort of dualism. It reflects instead the position that theoretical cognitive models are crucial for understanding neurobiology of any cognitive domain, including language (Gallistel & King 2010, Poeppel 2012). However, provided that we decompose models of various aspects of cognition—language and speech programing included (Boeckx et al. 2014)—into elementary units and operations, it is a logical necessity that for these units and operations to be 'real' in any coherent sense of that word, they must have a neurobiological substrate, as reflected by Figure 5. For phonology, works like Phillips et al. (2000), Binder et al. (2000), Hickok & Poeppel (2000a, 2004, 2007), Indefrey & Levelt (2004), Obleser et al. (2004), Mesgarani et al. (2008, 2014), Idsardi & Raimy (2013), Monahan et al. (2013), Idsardi & Monahan (2016) provide information on what this substrate might be and how to look for it. For neurobiological substrate of cognitive aspects of speech perception and production see Hickok & Poeppel (2000b, 2016), Poeppel et al. (2008), Poeppel & Hackl (2008), Poeppel & Monahan (2008), Poeppel & Idsardi (2011), Blumstein & Baum (2016), Guenther & Hickok (2016), Tremblay et al. (2016). The neurobiological substrate for CP will be explored in section 4.

CP shares its name and some conceptual commitments with the theory of cognitive phonetics by Tatham (1984, 1987, 1990) and Morton (1987), although there are substantial differences. While both approaches reject the notion of a direct interface between phonology and phonetics, and argue for a cognitive approach to certain phonetic phenomena, their theory (henceforth 'CP-TM') offers a different view of what phonology is and how it works. Although CP-TM was somewhat sympathetic to contemporary developments in generative phonology (Tatham 1990, section 3.1), the most important difference from our approach is that CP-TM did not fully commit to the generative architecture of the human language faculty, and therefore did not inherit all the implications (and results) that the generative framework entails. In particular, while CP-TM acknowledges the existence and *phonological* importance of features (ibid.), as soon as the phonetic level (albeit a cognitive one) is reached, CP-TM, like most phonetic models, tacitly shift attention to the realization of segments (Tatham 1990, section 6). In contrast, we are interested in decomposing SRs into phonological primitives, features, and in exploring how these might be implemented neurobiologically in real time. A further difference is that CP-TM has no commitments to neurobiology and keeps the discussion strictly in the cognitive domain. In fact, CP-TM resolutely banishes neurobiological considerations and maintains an "extreme dualist view" (Tatham 1990: 11).

The positing of a cognitive aspect of phonetics in no way blurs the competence/performance distinction. Phonology is competence; phonetics, even its cognitive aspect, is performance by definition, since only mental grammar is defined as competence. The transduction process modeled by CP (see section 4) does not entail 'knowledge' (e.g., 'knowing how' to produce speech) in any useful sense of the word (see Chomsky (1980: 101–102) for a relevant discussion on this matter). Transduction of SRs into PRs entails a set of neuromuscular processes. Its ontogenetic development most likely follows the development of performance systems in general (Lenneberg 1967, section 4.II). These processes are most properly conceived as 'automatic synergisms', "whole trains of events that are preprogrammed and run off automatically", and that "form the basis of all motor phenomena in vertebrates" (Lenneberg 1967: 92; see also Lorenz & Tinbergen 1957, 1970 for the seminal investigation of innate egg rolling automatisms in greylag geese). That they are cognitive, at least partially, despite being part of performance should also not be controversial.[9] CP by definition has access to cognitive representations generated by phonology, as shown by the left portion of Figure 5, and it is in this respect that the epithet 'cognitive' is justified; what CP generates, phonetic representations (PFs), are instructions for the SM system on how to execute neuromuscular commands, which are no longer cognitive. One of the main characteristics of a transducer is that it changes the format of its input, and in our case the input is a cognitive entity.

4. The Inner Workings of CP: Transduction

In this section, we turn to the primary research question for Cognitive Phonetics (CP): How are phonological features related to human neurobiological structures? In other words, how can we bridge the symbolic and the physical in the domain of speech? As we have indicated, this means exploring the structure of the transducer that converts SR-type information into PR-type information. Clearly, our chances of understanding a transducer are better if we have a good understanding of the transducer's inputs and outputs. The relatively robust results of generative phonology, as compared with other domains of cognition, provide us with an anchor for such explorations—we have a fairly explicit model of the nature of SR-type information as linearly ordered strings of feature matrices. Models of comparable detail are not available for the other two aspects of CP, the transduction procedures and PR-type information, and it is to those topics that we now turn.

Marr's (1982/2010) tri-level theory, which we will adopt in further discussion, has been widely accepted as a means to gain insight into information processing systems (IPS) such as CP. Marr proposes that IPSs are best analyzed in terms of three conceptual levels, each corresponding to a specific set of questions. These levels include the 'computational level', the 'representational and algorithmic level',

---

[9] It should be noted that this is mostly a definitional matter; by 'performance' in this context we merely mean 'not grammar'.

and the ‘implementational level’ (Marr 1982/2010 22–27), defined by the following questions:

- Computational level: What does the process do? Why does the process do it?
- Representational/algorithmic level: How does the process work? In particular, what are the input and output representations and what is the algorithm for the transformation?
- Implementational level: How are the output representation and the algorithm realized physically? In particular, what is the neurobiological substrate of the mapping in question?

Before proceeding, let us clarify a confusing terminological ambiguity. The fact that we are describing transduction, as distinct from computation, and yet still can talk about the *computational level* of a transducer does not reflect an intellectual inconsistency, but rather just two different uses of a term. As was stated in section 3.1, the main difference between a computational module and a transducer is that the former is a mapping between entities in the same format (e.g., feature matrices to feature matrices), and the latter is a mapping between entities of dissimilar formats (e.g., feature matrices to muscle commands, or sound vibrations to neural impulses). However, both modules and transducers are IPSs, therefore both are amenable to Marr’s tri-level analysis, and both can be analyzed at the *computational level* in Marr’s sense.

So, what implications does Marr’s theory have for our research question? First, it calls for maximal conceptual decomposition of the representations and operations posited by linguistics. For a long time, the cognitive neuroscience of language was (and to a certain extent perhaps still is) focused on exploring the neurobiological correlates of rather complex linguistic entities or domains, such as syntax (so for example, “Broca’s area underlies syntax” would be a common assertion in such a tradition), phonology, lexical semantics, and so on (Poeppel 2012: 36–49). However, Marr (1982/2010) argued that IPSs are best studied by decomposing them into representational and computational primitives, and then by building a bottom-up understanding of them. It is partly from this method that the success of his theory of vision derives, and it is a success that has inspired much of the recent work in computational neuroscience of language. Second, Marr’s theory encourages us to seek an explanation for an IPS’s nature from several different sources (for example, linguistics, cognitive science more broadly, neurobiology, formal computational theory) and facilitates explicitly connecting cognitive primitives with neurobiological structures. Therefore, it serves as a general framework for positing linking hypotheses across the fields of linguistics and neurobiology.

4.1. The Computational Level

Let us now turn to defining transduction—the operational aspect of CP—at the phonology–phonetics interface in terms of these three levels. Firstly, we want to address the ‘what’ and ‘why’ questions of the computational level. What does transduction in CP do? It transforms a representational format that is necessary for the coding of phonological knowledge into a representational format adequate for instructing the neuromuscular system on what it must accomplish in articulatory terms. Why does CP carry out transduction? In general, the answer to this question follows directly from the theoretical and empirical considerations of section 3.1, namely, that outputs of phonology, SRs consisting of substance-free features, lack crucial substantial and temporal information and are thus not legible to the SM system; therefore, phonology cannot in principle feed speech production *directly*, but only through transduction. The very fact that phonology and phonetics constitute two distinct domains that share an interface logically implies the necessity of transduction between them. In the absence of CP, a mental expression could not be externalized through the human SM system. The transduction maps between properties of the mind—mental representations composed at the most basic level of discrete, timeless, symbolic elements—and the functioning of the motor system, which works in terms of gradual, dynamic, temporally arranged neuromuscular activity. Since we *do* speak, the existence of transduction is confirmed.

4.2. The Representational and Algorithmic Level

We now turn to the question of how the transduction process works in CP.[10] The first step at this level is to state the representations involved in transduction. The input representation, SR, is a matrix of distinctive features. Each feature is transduced and receives interpretation by the SM system. Features are elementary units of phonological computation, stored in long term memory, that represent articulatory and acoustic information in a highly abstract manner.[11] Each feature may abstractly be schematized as shown in Figure 6, which is an extension of the Memory-Action-Perception Loop of Poeppel & Idsardi (2011). The input representation thus involves a set of idealized acoustic targets at which the neuromuscular system will aim, as corroborated by studies discussed in section 3.2, and a set of idealized articulatory configurations needed to achieve these goals. It should be emphasized that these 'targets' are not precise, physically invariant acoustic measurements, as features are substance-free units; they are coarse mental representations of acoustic spaces. It is a basic finding of psychoacoustic phonetics that what a speaker deems a repetition of the same category may in fact reflect a wildly different acoustic signal (Liberman 1957). The cognitive unity between acoustic and articulatory correlates of features seems to be so strong that hearing the speech of another person excites a corresponding motor program, regardless of whether the hearer has the intention to also speak (Cooper & Lauritsen 1974, Fadiga et al. 2002).

---

[10] Here we will make two simplifying assumptions. We will assume that features within a single bundle (segment) are parts of an unordered and unstructured set and are not grouped hierarchically so as to mimic the composition of the vocal apparatus. We will also abstract away from the possibility, strongly suggested by evidence presented in Keating (1988) and Hale & Kissock (2007), that featurally underspecified segments persevere into SRs. Integrating perseverant underspecification into CP will be left aside for future research.

[11] It is doubtful that current expositions of the universal set of features in linguistic literature are either quantitatively or qualitatively adequate. Compare, for example, Kenstowicz & Kisseberth (1979: 241–253), Lass (1984: 82–93), Katamba (1989: 42–51), Carr (1993: 54–66), Gussenhoven & Jacobs (2011: 74–84), Odden (2013: 45–61), Zsiga (2013: 258–270), etc., and notice the tremendous differences in the total number of features, in the way they are classified, in the set of features that made it to the final list, in the assumptions about *n*-arity, and especially in their definitions.

The output representation, called 'True Phonetic Representation', or 'Phonetic Representation' (PR) for short, is a complex array of neural commands that activate muscles involved in speech production. As pointed out in section 2, uttering even a single syllable involves hundreds of neuromuscular connections, therefore a detailed description of every neuromuscular event for every single and interacting feature is far beyond the scope of this paper.

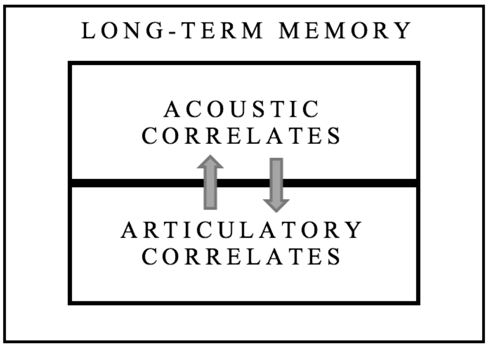


Figure 6: A schematization of a distinctive feature. Features serve as the cognitive basis of the bi-directional translation between speech production and perception, and are part of the long-term memory representation for the phonological content of morphemes, thus forming a memory-action-perception loop (Poeppel & Idsardi 2011) at the lowest conceptual level.

Our modest goal here is to sketch the fate of a transduced feature in a few simple and idealized cases. Take, for example, the feature [+ROUND]. Since lip rounding is known to have systematically varying muscular expression (due to interaction with other features, to which we will return below), the Phonetic Representation (PR) has to allow for this variation across contexts. The transduced form of [+ROUND], call it $\mathrm{PR}_{[+\text{ROUND}]}$, engages at least four muscles: *orbicularis oris*, *buccinator*, *mentalis*, *levator labii superioris*. The idealized expression, assuming no directly interfering articulatory movements (a relatively rare case in actual speech), is simultaneous contraction of the superior and inferior parts of *orbicularis oris*, contraction of *mentalis* (for protruding the lower lip) and *levator labii superioris* (for protruding the upper lip), and relaxation of *buccinator*. This is the case observed in pronouncing [u]. In [y], on the other hand, $\mathrm{PR}_{[+\text{ROUND}]}$ in addition to contracting the aforementioned muscles also involves a compressing movement (lips drawn together horizontally) caused by the contraction of the *buccinator*. The difference between protrusion and compression in $\mathrm{PR}_{[+\text{ROUND}]}$ is dependent on whether $\mathrm{PR}_{[+\text{ROUND}]}$ is interacting with $\mathrm{PR}_{[+\text{BACK}]}$ or $\mathrm{PR}_{[-\text{BACK}]}$ (Catford 1982: 172–173). Of course, various other complications exist, but this suffices to illustrate the general idea. The exact and fully detailed characterization of $\mathrm{PR}_{[+\text{ROUND}]}$ will thus be possible only after thoroughly studying various possible interactions of transduced features, no doubt a massive phonetic undertaking.

Note that PRs are still *abstractly* related to speech; they are not hi-fi encodings of speech-sound articulations, although they are less abstractly related to speech than SRs. This is because what is actually externalized is further complicated by a great number of factors. As Hale & Kissock (2007: 85) point out, transduction is followed by other performance factors that have no bearing on either grammar or transduction, factors like speech rate, loudness, interruptions due to sneezing, and many other situational effects. We will also have nothing to say here about how other aspects of SRs (e.g., prosodic elements like tone) are transduced.

The algorithm that transforms SRs into PRs has two steps, echoing Lenneberg's (1967) proposals outlined in section 2. In the first step (A_1), a feature is related to muscles which need to be contracted in order to produce an appropriate acoustic effect. Since speech occurs in real time, the second step (A_2) will entail temporal coordination of muscular activity demanded by A_1. A tremendous amount of complexity arises in relation to the second step of transduction. The main resultant phenomenon of this step is coarticulation (see Hardcastle & Hewlett 1999, Farnetani & Recasens 2013, and Volenec 2015 for surveys)—temporal overlapping of various aspects of PRs. Neurobiological studies on speech perception have uncovered that the human perceptual system consistently uses two time scales to analyze a continuous speech signal, a segmental time-frame of roughly 10–80 ms, and a syllabic time-frame of 100–500 ms (Poeppel et al. 2008, Poeppel & Idsardi 2011, Chait et al. 2015):

> There are two critically important chunk-sizes that seem universally instantiated in spoken languages: segments and syllables. Temporal coordination of distinctive features overlapping for relatively brief amounts of time (10–80 ms) comprise segments; longer coordinated movements (100–500 ms) constitute syllabic prosodies.
>
> (Poeppel & Idsardi 2011: 182)

However, transduced features often 'spill over' these temporal borders, crossing segmental and sometimes even syllabic boundaries in both directions, thus leading to coarticulation. Our decision to examine the transduction of [+ROUND] to $\text{PR}_{[+\text{ROUND}]}$ is useful since this aspect of speech relies on several muscles and is known to show great propensity for temporal overextending, especially in the anticipatory direction. Lisker (1978: 133) states that "lip-rounding and nasalization are segmental features of English that refuse to be contained within their 'proper' segmental boundaries, as these are commonly placed". (Note that Lisker's example should not be specific to English if it derives from universal transducer properties.) Likewise, according to Benguerel & Cowan (1974) $\text{PR}_{[+\text{ROUND}]}$ may be evident several consonants in advance of the rounded vowel for which it is required: In French, labial coarticulation can extend up to 6 segments in the anticipatory direction. Lubker et al. (1975) showed, using electromyography, that in Swedish $\text{PR}_{[+\text{ROUND}]}$ can start up to 600 ms ahead of a rounded vowel. Both directions of temporal overextending of $\text{PR}_{[+\text{ROUND}]}$ are observed in English, as demonstrated by Laver's (1994: 321) clever example [hʷudʷtʃʷuzʷpʷɹʷunʷdʒʷusʷ] (*Who'd choose prune juice?*).

The neurobiological mechanisms underlying transduction algorithms are universal properties of the human species, as witnessed by the fact that humans, in all non-pathological cases, use them without fail (see Dronkers 1996 for an example of a pathological case demonstrating a disruption of A_1). However, although the transduction algorithms are biologically universal in humans, CP will still show great output variability due to these two transduction steps being applied to SRs that reflect featurally distinct utterances. Here it is critical to distinguish between the status of the *output* of the transduction system (True Phonetic Representations) and the system itself (Cognitive Phonetics): The output of the system is, trivially, I-language-dependent because CP is fed by surface representations of that I-language (more precisely then, the output is surface-representation-dependent); the system itself is part of the human biological make-up and is therefore a universal property of the human species. Although this stance is somewhat controversial in phonetics, in our view it is the only biolinguistically coherent approach to the study of the PPI. The universality of CP is merely a reflection of the fact that there exists a biological object we may call 'the phonetic implementational system', of which CP is one part and the SM system another. The question of whether there are variations in individual phonetic implementational systems among humans need not detain us here, just as the fact that no two humans have identical eyes does not hinder biologists in studying a biological object called 'the human eye'. On the other hand, rejection of language-specific phonetics in no way precludes the possibility that certain sets of similar I-languages—which (sets) can roughly correspond to geosociopolitical notions 'language' and 'dialect' (see Chomsky 1986, section 2)—show recurrent (co)articulatory patterns. In our view, for example, the recurrent difference in pronunciation of English [i] and German [i] is to be attributed to representational (featural) differences present in I-languages of English and German speakers, *not* to language-specific phonetics.[12] In general, our position is that all recurrent or linguistically relevant differences in pronunciation result from representational differences in the lexicon and from differences in the phonological rule component. This position is parallel to the Minimalist idea that cross-linguistic syntactic differences arise from differences in lexicon and functional heads, and not from languages having different syntaxes (Chomsky 1995).

4.3. The Implementational Level

The implementational level is concerned with the neurobiological substrate of CP (see Figure 5). How is transduction of features at the PPI instantiated in the human brain? Many mysteries still surround this question and proposed answers are ever-changing. At a relatively gross neuroanatomical level, speech production engages a widely distributed neural network. In a meta-analysis of overt speech production, Eickhoff et al. (2009) reported consistent activation in left inferior frontal gyrus (IFG), ventral precentral gyrus (motor and premotor cortex), ventral postcentral gyrus (somatosensory cortex), superior temporal gyrus (STG; i.e., auditory cortex), supplementary motor area (SMA), anterior insula, superior paravermal cerebellum (lobules V and VI), basal ganglia and thalamus. Of particular importance for transduction is the 'dorsal stream', usually stated to have an "auditory-motor integration function" (Hickok & Poeppel 2007: 394) and to be "involved in mapping sound representations onto articulatory-based representations" (Hickok & Poeppel 2004: 72).

---

[12] A reviewer raises the question of how many features would be needed in our approach in order to describe "minute differences between neighboring dialects", for example "the differences between the English accents in the US", arguing that the twenty-something features that are usually assumed to exist are not enough. The objection is mathematically unjustified since assuming 20 features (Odden 2013) and surface underspecification (Hale & Kissock 2007) will yield 3^{20} ($\approx$ 3.5 billion) different segments that can feed CP, which seems to be more than enough not only for the description of a non-technical notion such as 'English accents in the US', but also for accounting for all possible recurrent (co)articulatory patterns. Of course, any increase in the feature set, even one that maintains the same order of magnitude as the usually assumed '20 or so', yields explosive increases in descriptive typological power: For example, 30 features yield 3^{30} which is about 206 *trillion* different segments. The reviewer's worries reflect the normal human lack of intuition with respect to combinatoric explosion. Any linear increase in what we attribute to UG results in exponential growth in descriptive capacity, clearly a welcome result (Reiss 2012).

The dorsal stream is comprised of structures in the posterior frontal lobe and the posterior dorsal-most part of the temporal lobe and parietal operculum. The dorsal stream is strongly left-dominant, which is why production deficits result predominantly from dorsal temporal and frontal lesions. The specifics of these general findings lend support for various aspects of CP.

The articulatory motor programs for executing features are coded in posterior IFG of the left hemisphere, traditionally known as Broca's area. More specifically, Hickok (2012: 138) reports that pars opercularis (BA44) and the ventral-most part of BA6 store articulatory programs needed to reach the auditory targets imposed by features. BA44 and BA6 are thus the most likely candidates for storing articulatory aspects of features (see Figure 6). The anterior insula, a cortical area beneath the frontal and temporal lobes of the left hemisphere, is reported to be involved in preparation of speech, that is, in "translating a phonetic 'concept' obtained from left IFG into articulatory motor patterns" (Blumstein & Baum 2016: 649, Eickhoff et al. 2009), roughly corresponding to our A_1. Dronkers (1996) showed that lesions to that part of the brain lead to apraxia of speech, the inability to assign muscular activity to a phonological representation. Dronkers' results are rather robust and show a clear disruption of A_1, since all 25 examined stroke patients suffering from apraxia of speech had the same lesion, while the anterior insula was spared in all 19 healthy participants. By way of the dorsal stream, information from the anterior insula is transmitted to the pre-SMA, often implicated in articulatory initiation and sequencing of neuromuscular activity (Alario et al. 2006, Guenther et al. 2006, Bohland & Guenther 2006), and then projected to the primary motor cortex. The pre-SMA also receives temporal information from the cerebellum and the basal ganglia (see below). It can therefore be hypothesized that the pre-SMA integrates information from A_1 and A_2, and forms a finalized True Phonetic Representation. From the primary motor cortex, neurons send signals to the brainstem and spinal cord that ultimately result in muscle contractions.

Important structures for the temporal organization of speech (corresponding to A_2) include the cerebellum and basal ganglia. Information from the insula (corresponding to A_1) is directly transmitted to the cerebellum and basal ganglia, structures that are well-established constituents of cortical-subcortical loops for movement preparation (Jueptner & Krukenberg 2001). More specifically, selection and sequencing of motor programs for articulation is mediated through basal ganglia, and the conversion of the discretely prepared sequences into a fluent, temporally distributed action is carried out by the cerebellum (Eickhoff et al. 2009: 2416). Cerebellar dysfunction affects temporal aspects of speech production and results in a dysarthria characterized by improper timing of cognitively discrete elements (such as feature bundles), substantial aberrations in their total and relative duration, disrupted coordination of orofacial and laryngeal movements, slowed/delayed execution of articulatory movements etc. (Ackerman et al. 2007). Information from the cerebellum and basal ganglia ties into the pre-SMA, presumably where A_1 and A_2 are integrated to form a True Phonetic Representation directly interpretable by the primary motor cortex (PMC) which sends efferent neuromuscular commands.[13]

---

[13] According to Eickhoff et al.:

> The basal ganglia and the cerebellum both forward their information to the PMC which precedes M1 in a serial fashion. The parallel engagement of the

Features also have acoustic correlates (see Figure 6) that serve as targets for articulatory movements. There is accumulating evidence and a convergence of opinion that portions of the superior temporal sulcus (STS)—bilaterally but perhaps with a mild leftward bias—are important for encoding acoustic/auditory aspects of phonological representations (Indefrey & Levelt 2004, Buchsbaum et al. 2001). In an attempt to pinpoint this region more narrowly, Hickok & Poeppel (2007: 398) suggest "that the crucial portion of the STS that is involved in phonological-level processes is bounded anteriorly by the most anterolateral aspect of Heschl's gyrus and posteriorly by the posterior-most extent of the Sylvian fissure". Mesgarani et al. (2014) showed that acoustic phonetic information is represented in the STS and is distributed along five distinct areas, each roughly corresponding to a general 'manner of articulation' class of speech sounds. By measuring the responses in implanted electrical cortical grids placed along the superior-most part of the temporal gyrus, they found that their electrode e1 responded selectively to stops, e2 to sibilant fricatives, e3 to low back vowels, e4 to high front vowels and a palatal glide, and e5 to nasals (Mesgarani et al. 2014: 1009). Similarly, Bouchard et al. (2013) constructed an auditory-based 'place of articulation' cortical map in the STG, confirming labial, coronal and dorsal 'places' with different electrodes, and cutting across various manner classifications. Scharinger et al. (2012) found, using magnetoencephalography, neural correlates of three phonologically relevant vowel variables—height, frontness and roundness spelled in terms of first three formants—again localizing them in the superior temporal gyrus.

STS and STG project auditory representations to an area in the Sylvian fissure at the boundary between the parietal and temporal lobes (called 'Spt'), where they are integrated with articulatory representations (Hickok et al. 2009, 2011, Gow 2012). Activity in Spt is highly correlated with activity in the pars opercularis (Buchsbaum et al. 2001, 2005), the posterior sector of Broca's region implicated in storage of articulatory motor programs. White matter tracts identified via diffusion tensor imaging suggest that Spt and the pars opercularis are densely connected neuroanatomically (Hickok et al. 2009). Spt therefore appears to be involved in sensorimotor integration, that is, in translation between auditory and articulatory correlates of features.

4.4. *Interim Summary*

At the beginning of this section, we stated that the main goal of this paper is to gain a better understanding of how phonological features relate to neurobiological structures. Let us summarize our proposals. Recent neuroscience evidence is consistent with the idea that Cognitive Phonetics transduces abstract features (elements of SRs) into temporally distributed neuromuscular activities (elements of PRs), relating the phonological grammar to the vastly different SM system. This is carried

---

> subcortical motor loops is thus followed by a sequentially organized common final pathway: the PMC first combines the processed information about selected movement programs and their temporal sequencing provided by the basal ganglia and the cerebellum, respectively, into a *final movement representation*. These are then forwarded to M1 for the generation of the final output to lower motor neurons and hence execution. (Eickhoff et al. 2009: 2416; emphasis added)

out by assigning each feature a specific set of muscular contractions (A_1) and by ordering them temporally (A_2). Neurolinguistic evidence outlined in section 4.3 suggests that transduction is implemented by a widely distributed neural network which engages the inferior frontal gyrus (stores articulatory correlates), the superior temporal gyrus (stores auditory correlates), the Spt (sensorimotor integration), the anterior insula (A_1), the cerebellum and basal ganglia (A_2), the supplementary motor area (integrates A_1 and A_2), and the primary motor cortex (sends efferent neural commands to the muscles).

5. Implications

We have stressed the importance of adhering to *phonological* facts in *phonetic* theorizing because decisions made on phonological grounds will have considerable impact on phonetic analysis. In particular, this means that we take serious consideration of the following notions: (1) the most basic unit of phonology is the distinctive feature; (2) features are abstract (yet real), cognitive, substance-free units; and (3) features are transduced at the phonology-phonetics interface (PPI) by being converted into temporally coordinated muscular activity. Several theoretical and empirical implications follow from Cognitive Phonetics (CP), our theory of this interface.

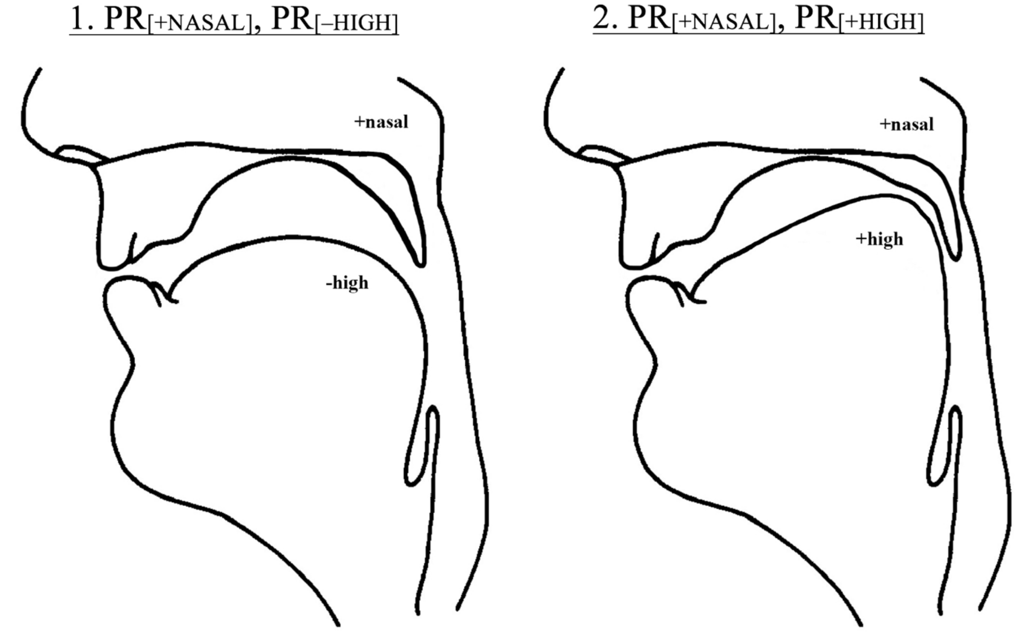
5.1. Coarticulation

The concept of coarticulation, such as the lip rounding during production of [s] before the rounded vowel of *soon*, rests upon two premises: (a) that discrete units, segments, underlie the continuous, gradient speech signal (Hammarberg 1976: 357), and (b) that these segments are converted into articulatory gestures (Farnetani & Recasens 2013: 317f).[14] The temporal overlapping of articulatory gestures pertaining to different linearly ordered segments can thus be dubbed 'intersegmental coarticulation'. However, if premise (a) is modified to be in line with much of modern phonology (see section 3.1), that is, if the phonological feature is taken as the atomic underlying unit, it follows that (c) features are converted into something more basic than segment-bound articulatory gestures (see section 4.2), and (d) that interaction in realization of features *within* a single segment is also possible, leading to what we will call 'intrasegmental coarticulation'. Here we will briefly sketch the consequence of approaching coarticulation from the framework of CP, assuming (c) and (d) instead of (just) the usual (a) and (b).

CP performs the mapping SR → PR, or, in terms of individual valued features, $[F] \rightarrow PR_{[F]}$. We will therefore take transduced features (in a general format $PR_{[F]}$, where [F] stands for an individual valued feature) to be the basic units that enter speech production. To illustrate intrasegmental coarticulation, consider the interaction of $PR_{[HIGH]}$ and $PR_{[NASAL]}$ observed, for example, in Lakhota (Boas & Deloria 1941), Yoruba (Ogunbowale 1970), and Koyra Chiini (Heath 1999), with sketches in Figure 7 based on Beddor (1983) and Ladefoged & Johnson (2010).

---

[14] Even Carol Fowler, who disagreed with Hammarberg on many issues related to coarticulation (see Fowler 1983) and who later argued for a gesture-based account of coarticulation (see Fowler & Saltzman 1993), stated that "an intuitive concept of 'segment' underlies our recog-


1. $\text{PR}_{[+\text{NASAL}]}$, $\text{PR}_{[-\text{HIGH}]}$

2. $\text{PR}_{[+\text{NASAL}]}$, $\text{PR}_{[+\text{HIGH}]}$

Figure 7: Intrasegmental coarticulation based on the interaction of $PR_{[NASAL]}$ *and* $PR_{[HIGH]}$.

In principle, $\text{PR}_{[+\text{NASAL}]}$ entails the opening of the velar port and $\text{PR}_{[+\text{HIGH}]}$ the raising of the tongue dorsum. In sketch (1) $\text{PR}_{[+\text{NASAL}]}$ can be observed in a 'default', non-coarticulated state, that is, with a substantial degree of velum lowering. The tongue dorsum is not raised due to $\text{PR}_{[-\text{HIGH}]}$, leaving more space in the oral cavity for the velum port to open. In (2) $\text{PR}_{[+\text{HIGH}]}$ pushes the tongue dorsum upward, leaving less space for the velum to lower.[15]

The velar port is still opened as the realization of $\text{PR}_{[+\text{NASAL}]}$, but to a substantially lesser extent than in (1). In other words, $\text{PR}_{[\text{NASAL}]}$ is coarticulated with $\text{PR}_{[\text{HIGH}]}$ and shows variation depending on the specification (+ or –) of $\text{PR}_{[\text{HIGH}]}$. This effect can be observed by comparing how features are transduced within different segments; $\text{PR}_{[\text{NASAL}]}$ and $\text{PR}_{[\text{HIGH}]}$ interact differently within, say, [ã] than within [ũ]. Such variation in how individual features within a segment's feature matrix are transduced depending on the specification of other features in the matrix is '*intra*segmental coarticulation', as illustrated in Figure 7. This is distinct from variation in transduction of features due to influence of features from other matrixes, which constitutes '*inter*segmental coarticulation'.

In CP, *intra*segmental coarticulation results from the workings of A_1, while *inter*segmental coarticulation arises from the effects of A_2. As defined in section 4.2, A_1 takes a feature from the phonological SR and converts it into a neuromuscu-

---

nition that there is a phenomenon of coarticulation requiring explanation." (Fowler 1980: 114)

[15] Hajek & Maeda (2000: 6) offer a different explanation as to why the velum is lowered to a lesser degree if the tongue body is elevated compared to when the tongue body is not elevated. They argue that a given velopharyngeal opening has a greater acoustic effect in high vowels because the oral tract is more constricted, and as a result, less velum lowering is required in high vowels in order to realize perceptible nasalization.

lar pattern. For each feature, this pattern is partially determined by specifications of other features within the same bundle, as shown in Figure 7. Therefore, A$_1$ will assign a different neuromuscular pattern to [+NASAL] depending on how the feature [HIGH] is specified. If one imagines a certain SR (say, [dɒg]) as a feature matrix where columns stand for segments and rows for features, then A$_1$ takes all columns (that were loaded into CP) at once, determines the specification of each feature in each column, and generates a full set of corresponding PR$_{[F]}$s. Intrasegmental coarticulation, that is, contextual variation in transduction of features, arises when different features in the same column impose conflicting demands on A$_1$. Information from A$_1$, transmitted via a pathway connecting anterior insula to cerebellum and basal ganglia, is further manipulated by A$_2$. A$_2$ arranges PR$_{[F]}$s created by A$_1$ temporally, but more importantly for this discussion, A$_2$ extends certain PR$_{[F]}$s over boundaries of their original column. This leads to *inter*segmental coarticulation. A familiar example is labial intersegmental coarticulation, where A$_2$ takes PR$_{[+\text{ROUND}]}$, typically originating from a rounded vowel, and overextends it in the regressive (anticipatory) direction. This can be observed in the word *soon*, where PR$_{[+\text{ROUND}]}$ from the vowel is overextended to produce a labialized fricative. A$_2$ can also overextend PR$_{[F]}$s in the progressive (perseverative) direction. This can be observed in the word *seek*, where the PR$_{[-\text{BACK}]}$ of [i] is overextended to influence the following [k], yielding ♪si:k̟♪, with a somewhat fronted velar stop.[16] Neurobiological studies suggest (see section 4.3) that the results of A$_1$ and A$_2$ are integrated into a final true phonetic representation in a region of the supplementary motor cortex at its boundary with the primary motor cortex, from which efferent commands are issued to the musculature of speech organs. However, it would seem that further experimentation is needed in order to establish whether A$_1$ precedes A$_2$ or whether there is overlapping in their real-time neural implementation.

A great deal of variation in the execution of PR$_{[F]}$s is of course to be expected among speakers, especially given that after transduction, various other non-linguistic and non-phonetic factors influence the actual acoustic output of the human body. The output of CP is dependent on utterance-specific SRs that feed it and on the neurophysiological structures that serve as its physical implementation. Various other situational factors are introduced after transduction, which we have put aside due to their irrelevance for the general nature of CP, but it is important to keep in mind that, if not somehow recognized, these factors *will* ‘contaminate’ all experimental results (of neural imaging techniques, for example), thus leading to the impression of even greater variation in observed speech output.

The architecture of CP opens the possibility of simultaneously exploring coarticulation along two dimensions instead of just one, which leads to interesting empirical consequences. Here we will merely state a hypothetical situation to illustrate CP’s potential empirical coverage.

Let us suppose that in some language we have detected that PR$_{[+\text{ROUND}]}$ is different in [u] than in [o] (see Linker (1982) for analogous examples from English, Cantonese, Finnish, French, and Swedish). In other words, A$_1$ assigns a slightly different configuration to [+ROUND] depending on whether it has to take into account [+HIGH] or [–HIGH] within the same bundle. This kind of intrasegmental coarticulation can clearly be observed in Figure 8.

---

[16] The symbol ♪ represents the actual acoustic output of the human body.

Figure 8: Intrasegmental labial coarticulation. Notice the difference in lip rounding corresponding to [u] on the left, and to [o] on the right.

Suppose further that A_2 temporally overextends $PR_{[+ROUND]}$ across the segmental boundary in the anticipatory direction (from 'right' to 'left'). *Intra*segmental and *inter*segmental coarticulation of the same $PR_{[F]}$ is now ineffect. Consider, for example, the tokens [lʷu:k] and [lʷo:k]. The [lʷ] of the former token and the [lʷ] of the latter token will systematically differ, since $PR_{[+ROUND]}$ of the former will carry with it the effect of intrasegmental coarticulation due to A_1, namely, the effect of $PR_{[+HIGH]}$, while the latter will carry the effect of $PR_{[-HIGH]}$. To reiterate, *inter*segmental coarticulation reflects the effects of *intra*segmental coarticulation. If we consider only SRs, then there can be no explanation for a systematic difference in the realization of the rounding on the two [l]s, since in both cases [l] precedes [+ROUND]. CP allows us to account for these subtle phonetic variations in an explicit and straightforward way—they follow naturally from its transduction algorithms. Thus, A_1 and A_2 are not just mechanisms that transduce features into information directly interpretable by the SM system, they are also mechanisms from which both types of coarticulation follow automatically, simply by adhering to the minimal architecture of CP.

Our discussion has focused on the variable neuromuscular realization of a given property, such as the rounding of the vowels [u] and [o]. It is worth remembering that such a discussion of *phonetic* variability is predicated upon acceptance of the existence of a logically prior *phonological* category of vowels containing the feature [+ROUND]—it only makes sense to talk about variable realizations of x once we accept that x is a category.[17] Why do we accept the existence of such a category? Because the two segments [o] and [u] behave alike with respect to linguistic phenomena. For example, in Turkish, a process called 'vowel harmony' generates different suffix vowels depending on the preceding root vowel. As we see in Table 1, the [+ROUND] root vowels [u] and [o] both trigger a suffix form with [u], whereas

---

[17] This is an extension to the feature level of Hammarberg's (1976) argument for phonological segments as "logically and epistemologically prior" to their phonetic correlates.

the corresponding [–ROUND] vowels [ɯ] and [ɑ] trigger a suffix form with [ɯ] (see Isac & Reiss (2013, section 6.4) for a more comprehensive analysis).

As the photographs (of a Turkish speaker) in Figure 8 show, the lip rounding on two vowels is realized differently, but we treat the vowels as members of a category [+ROUND] because of their phonological behavior. Such considerations explain why we must recognize a distinction between phonetics and phonology. Since the two domains are different but interact with each other, there must be a transduction between them. That transduction is CP.

NOM.	GEN.	gloss
pul	pulun	'stamp'
son	sonun	'end'
kɯl	kɯlɯn	'body hair'
sɑp	sɑpɯn	'stalk'

Table 1: Schematic of vowel harmony as found in Turkish.

We fully recognize that the properties of CP outlined in this paper are too general to serve immediately as a full model of coarticulation. Not only the properties of the two component transduction algorithms, A_1 and A_2, but also the basic inventory of distinctive features must be made more explicit if CP is to be an empirically testable model. In principle, however, CP offers a theoretically coherent way to account for both intra- and inter-segmental coarticulation, and their complex interactions, while maintaining theoretical and empirical insights of generative phonology.

5.2. *The (Illusory) Naturalness of Phonological Processes*

The nature of the PPI as understood in CP shows the need to strictly distinguish between phonology and phonetics. This has implications for the idea of 'naturalness' in phonology. Naturalness is an elusive notion, but it usually entails explaining linguistic phenomena in terms of directly observable empirical facts grounded in acoustics, articulation, statistics, behavior, communication etc. Donegan & Stampe (1979), proponents of Natural Phonology, suggest that the same notion of naturalness plays a role in explaining synchronic phonological patterns, diachronic phonology, as well as patterns of speech development in children:

> Natural Phonology is a modern development of the oldest explanatory theory of phonology. [. . .] Its basic thesis is that the living sound patterns of language, in their development in each individual as well as in their evolution over the centuries, are governed by forces implicit in human vocalization and perception. (Donegan & Stampe 1979: 126)

We follow Hale (2007, section 11.1) in denying any significance to apparent parallels among synchronic, diachronic and developmental 'sound patterns', therefore we will restrict our discussion to the 'naturalness' of synchronic phonology, as determined by phonetic facts. It is not difficult to find, on superficial inspection, phonological processes that seem natural in this sense. Why does [s] assimilate in voicing before adjacent [b] in a language L? Because it is easier for the human vocal system to maintain, and not to rapidly change the laryngeal configuration. Since voicing assimilation is indubitably a well-attested phonological process, and since this process receives an explanation from the efficient workings of "human vocalization" (Donegan & Stampe 1979: 126), naturalness must obviously be a part of phonology. However, this reasoning suffers from a failure of separating 'what' from 'why'. The 'what' and the 'why' do not have the same status in linguistic theory. If the goal of linguistics, phonology included, is to explicitly model the speaker's knowledge of language, that is, to model linguistic competence, then linguistics, phonology included, is to be concerned with the 'what' questions: 'What is it that a speaker knows when she or he is said to know phonology?' and 'What are the rules and representation of particular phonological grammars?' The 'why' question—'Why is phonology (or some aspect of it) the way it is?'—does not enter into discussion at this level of inquiry (but see below). Simply put, 'what' is part of competence, but 'why' is not.

Donegan & Stampe (1979), and many other phonologists more recently, proposed to offer phonetic explanations for phonological phenomena, but despite ongoing efforts in a variety of phonological frameworks (for example, see Hayes et al. (2004) for attempts within Optimality Theory), this enterprise has not been convincing:

> The attempts by those who are interested in psychological phonological grammars and in finding ways to represent phonological processes [...] in phonetically natural ways have been abysmal failures [...]. One possible solution to this is not to put more phonetic sophistication into psychological grammars but rather to abandon phonetic naturalness as a necessary feature of them. (Ohala 2003: 685)

Ohala's perspective (see also Ohala 1990) is not only that efforts to build naturalness into phonology have failed, but also that we would not want them to succeed, on grounds of scientific elegance. If certain recurrent phonological phenomena have a perfectly good phonetic explanation, then we do not get a better theory by duplicating the explanation inside phonological grammar—in science, it is not better to have two explanations than one. If naturalness (e.g., the prevalence of voicing assimilation) receives a perfectly fine phonetic explanation, then it is not better to posit another, quasi-phonological explanation, especially not if the latter explanation offers no new insight.

We suggest that phonological naturalness is an illusion that arises when inspecting *phonetic* data with the purpose of understanding *phonological* processes. In other words, 'naturalness' is introduced into data in the process of externalization (and internalization in speech perception). Since we cannot have direct access to phonological representations and computations, all of our observations are of phonetic data, that is, data from actual utterances resulting from language use, which reflects many different factors. As we argued in sections 3 and 4, CP is the first step in externalization, so understanding CP can hopefully provide insight into what is mistakenly taken as phonological naturalness. Attaining such an insight removes the need for attributing naturalness to the phonological grammar, leading to a more parsimonious and elegant phonological theory.

Once we remove the traditional 'why' questions of Natural Phonology and its derivatives from the purview of *phonology*, we will be better prepared to answer the proper 'why' questions related to the phonological domain. At this level of inquiry, we will be uncovering the biological foundations, not of speech, but of *language*, the study of which is Universal Grammar. The 'why' questions of the phonological grammar are answerable only in terms of the neurobiological substrate of the phonological faculty.

5.3. Gradience

Phonology is computation over discrete, categorical symbols. At the lowest taxonomic level, these symbols are features. However, the phonological literature is full of case studies showing the graded nature of 'phonological' units and processes (see Ernestus 2011 for an informative survey). We believe that the rejection of discreteness in phonology reflects a failure to distinguish the object of study from the data used to draw inferences about that object.

The following is a fairly standard definition of 'categoricality' vs. 'gradience', and by emphasizing certain words in it, we wish to draw the reader's attention to the conceptual level at which the definition is given:

> [C]ategorical *sounds* [...] are stable and represent clear distinct phonological categories (e.g. *sounds* showing all characteristics of voiced segments *throughout* their *realizations*) [...]; gradient *sounds* [...] may change *during* their *realization* and may simultaneously represent different phonological categories (e.g. *sounds* that *start* as voiced and *end* as voiceless). (Ernestus 2011: 2115)

While we have no objection to such a characterization of categoricality vs. gradience, from the emphasized words it is obvious that the definition is immersed in the domain of the substance-laden and temporal, that is, speech (performance), not grammar (competence). The problem arises when phonetic data is used to make inferences about phonology directly and reflexively, as if every idiosyncratic datum recorded in speech or found in a corpus is relevant for phonology, without acknowledging the distance between competence and performance. Consider another passage from Ernestus (2011: 2118):

> Ellis and Hardcastle (2002) found [by using electropalatography and electromagnetic articulography—vv & cr] that four of their eight English speakers showed categorical place assimilation of /n/ to following velars in all tokens, two speakers showed either no or categorical assimilation, and two speakers showed gradient assimilation. Together, the data show that place assimilation processes [...] may be gradient in nature. These processes cannot simply be accounted for by the categorical spreading of a phonological feature from one segment to another.

What is to be inferred from these findings that is relevant for phonology? In our view, very little (see below). The cited results, showing inter- and intra-speaker variation, as well as both discrete and gradient effects, may constitute a salient illustration of the ubiquitous lack of uniformity in the behavior of members of a speech community, but it is not in the purview of phonology to provide an explanation of such phenomena. The fact that such variation "cannot simply be accounted for by the categorical spreading of a phonological feature from one segment to another" (ibid.), a claim most certainly true, does not automatically mean there is something wrong with phonology conceived as categorical symbol manipulation. It is important to clearly distinguish between the object of study of phonology and the sources of evidence for that study. The object of phonological study is the human knowledge of externalizable aspects of I-language and the cognitive capacity required to construct that knowledge on exposure to limited experience. One of the sources of evidence, perhaps the primary one, bearing upon that object of inquiry are spoken utterances. Therefore, to a certain degree, it can be said that both phonology and phonetics draw from the same pool of evidence, namely, the analysis of speech. The point is merely that not all data from that pool is relevant for phonology, and a phonologist qua cognitive scientist needs to peel off the various complications that were introduced in the process of externalization from the underlying system of linguistic knowledge she or he is studying.

As understood here, gradience is introduced by CP's A_2, which is responsible for the temporal coordination of muscular activity specified by A_1; that is, gradience is not a phonological phenomenon. Notice the references to time highlighted in the above quote from Ernestus (2011: 2118), for example, "during" and "start as …end as". Gradience involves change over time. If we think of human phonology as involving a representational system (features and the like) that encodes the phonological portion of morphemes stored in the lexicon, and a computational system that can be thought of as a complex function of, say, composed rules (Bale & Reiss 2018), then there is no temporal aspect to phonology. (Questions about gradience in phonology are like questions about how fast a *wh*-element moves in syntax; both reflect a category error.) In this way phonology mirrors other competence modules, for the same reasons discussed at length by Chomsky (1980, 1986, 1988, 2000a), Anderson & Lightfoot (2002), and others. A fundamental property of the human language faculty is that on all analytical levels it fractionates language-related aspects of an analog signal into discrete elements to which formal operations apply.[18] Even vastly different, mostly incompatible linguistic theories have acknowledged discreteness as a defining property of language: It can be found in Martinet's (1949: 30) notion of 'Double Articulation', Hockett's (1959: 32) 'Duality of Patterning', Chomsky's (2016: 4) 'Basic Property'. Adopting such a position not only preserves a clear distinction between competence and performance, a necessity on many different grounds, but it also facilitates disentangling phonological conclusions from phonetic conclusions even though both are drawn from the same data. The only kind of conclusion a phonologist can draw from the Ellis & Hardcastle experiment cited by Ernestus is that the I-language of (some) English speakers contains a following rule: [+NASAL, CORONAL] → [+NASAL, DORSAL] / __ [DORSAL]. Phonologists can draw only this kind of conclusions because their theory both provides and determines the limits of their descriptive vocabulary. Phonological theory does not provide us with the vocabulary to describe a nasal consonant as 'kind of dorsal'. We pointed out above (section 5.1) that [o] and [u] behave phonologically the same, and that both must be analyzed as [+ROUND] vowels, despite the involvement of different muscles in realizing this feature, due to intrasegmental

---

[18] "Our mind structures the linguistic input in a digital form (as opposed to an analog form), and we call this property of language discreteness." (Boeckx 2009: 57)

coarticulation with [–HIGH] and [+HIGH], respectively. Again, phonologists do not have, and do not want, the vocabulary to describe a segment as 'kind of round'.[19]

If a featural assimilation rule correctly models a part of the implicit phonological knowledge of a speaker, a phonetician can then posit hypotheses as to why such a pattern exists, why there is variability in externalization of this knowledge, what are the limits of its variation, whether the variation is purely biomechanical or partly/mostly/solely cognitive, and so on. For example, the first of these questions might be explained by arguing that the demands of the $\text{PR}_{[+\text{DORSAL}]}$ override the demands of the $\text{PR}_{[+\text{CORONAL}]}$ because of the robustness and mechanical inertness of the relatively massive dorsal part of the tongue compared to less constrained, more mobile coronal part.[20] Therefore, the velar exerts its coarticulatory influence over the nasal. Taken this way, the relationship between assimilation and coarticulation is parallel to that of phonology and phonetics in general, that is, the former is a discretely and abstractly constructed mental *representation of* or an implicit *knowledge* of the latter (provided that the latter has been phonologized).

In brief, the data most often used in inferring about phonology comes from spoken utterances. But spoken utterances are not the object of phonological study. Therefore, it does not follow that gradience of phonetic objects automatically translates to gradience of phonological objects.

5.4. Speech Planning and the Case of the Intervocalic /j/ in Croatian

Anticipatory coarticulation is widely adduced as proof that coarticulation is not merely a reflection of biomechanical properties (e.g., inertness) of speech organs (Farnetani & Recasens 2013). In order for a coarticulatory effect of, say, labialization ([ʷ]) to influence a unit *preceding* a rounded vowel from which the effect derives, it is necessary that some cognitive planning is involved. As we see it, phonology provides the knowledge about the discretely constructed form about to be loaded into the speech production mechanism, and CP the means to plan the coarticulatory effect. An example may be drawn from findings presented by Volenec (2013).

The purpose of that study was to see whether there is a statistically significant difference between the acoustic properties of a Croatian intervocalic palatal glide [j] present in the underlying representation, as in /pijem/ → [pijem] 'I am drinking', and a (supposedly) epenthesized palatal glide that is not present underlyingly, as in /vidio/ → [vidijo] 'I saw'. In the latter case, the glide is supposed to surface only when adjacent to a front vowel (Škarić 2007: 75), therefore only intervocalic environments consisting of at least one front vowel were compared. For the comparison the study used minimal or subminimal pairs such as /gleda ix/ 'he looks at them' ∼ /gledaj ix/ 'look at them', and /priaɲati/ 'to stick (to)' ∼ /prijaʋiti/ 'to report'. The first result was that in both cases none of the typical acoustic correlates of palatal glides (lowering of F1 and heightening of F2 compared to adjacent vow-

---

[19] The idea that one's theoretical apparatus determines the range of possible observations that can be made is an old idea in the philosophy of science, discussed in particular reference to the domains of phonetics and phonology by Hammarberg (1976) and Bale & Reiss (2018).

[20] This is the main idea behind the 'degree of articulatory constraint' (DAC) model of lingual coarticulation (Recasens et al. 1997), which states that the degree of coarticulatory influence and resistance of a phonetic unit rises in proportion to the degree of tongue dorsum involvement in the production of that unit.

els, lowering of the intensity between F1 and F2; see Stevens 1998, section 9.2.1) were found in the intervocalic position. This would suggest that the correct derivations are actually /pijem/ → [piem] and /vidio/ → [vidio], that is, with deletion, not epenthesis intervocalically. However, the second result showed that in words with underlying /j/, vowels preceding the palatal glide had their F1 significantly lowered, suggesting that the glide exerted anticipatory coarticulatory influence on the vowel, despite not being otherwise present in the acoustic signal. In words with no underlying /j/, this lowering of F1 of the preceding vowel was not present.

We argue that this case shows a dissociation between three levels of analysis: phonological, cognitive phonetic, and articulatory phonetic. Since there is no incontrovertible evidence of discrete phonological alternations in any of these cases, the most plausible derivations are /pijem/ → [pijem] and /vidio/ → [vidio], despite the fact that the spectrogram corresponding to [pijem] contains no time span that independently corresponds to a segment [j]. Note that segments are abbreviations for feature bundles. The A_1 of CP receives features and transduces them into PR$_{[F]}$s. Identical adjacent PR$_{[F]}$s are fused to make a continuum; the palatal glide and front vowels share many distinctive features, and therefore many PR$_{[F]}$s. CP's A_2 temporally overextends the only PR$_{[F]}$ discriminating between the glide and front vowels—the neuromuscular command responsible for the narrowing of the palatal constriction, which results in the lowering of F1—to serve as an acoustic cue for the glide. The articulatory system then produces something like ♪piem♪, but with ♪i♪'s F1 lowered (as compared to a 'normal' /i/ that is not in the context of an underlying /j/). The hearer usually picks up this cue, which explains why native Croatian speakers consistently report vaguely hearing some sort of [j] in these cases (Škarić 2008: 206–212).

Two conclusions can be drawn from this. First, what enters the articulatory system is *not* the output of phonology (which is [pijem]); if it were, we would expect to find at least some independent glide-like acoustic properties between the vowels, but there are none. Therefore, a cognitive phonetic stage, distinct from both phonology and articulatory phonetics, is needed for transduction and planning. Second, the phonetic transformations that CP introduces target features, which correspond to a finer level of granularity than segments. The phenomenon presented here makes sense only if the input to CP consists of features, and not indivisible segments; and if the output of CP does not consist of segment-bound articulatory gestures, but PR$_{[F]}$s. This suggests that neither articulatory gestures nor segments, but transduced features (PR$_{[F]}$s) are the basic units of speech production. The apparent necessity of units at this intervening level serves as yet another justification of our CP model.

6. Conclusion

In this paper, we have argued that the interface between phonology and phonetics (PPI) consists of a transduction process that converts elementary units of phonological computation, features, into temporally specified neuromuscular patterns, which are directly interpretable by the motor system of speech production. Our inquiry is inspired by Lenneberg's magisterial book *Biological Foundations of Language* (1967), in which he discussed the transformation of phones (segments) into neuromuscular schemata. Our view of the PPI is constrained by substance-free generative phonological assumptions (section 3.1), on the one hand, and by insights gained from psycholinguistic and phonetic models of speech production (section 3.2), on the other. To distinguish transduction of abstract phonological units into planned neuromuscular patterns, arguably the very first step in speech production, from the biomechanics of speech production usually associated with physiological (or more narrowly, articulatory) phonetics, we have termed our theory 'Cognitive Phonetics' (CP). The inner workings of CP (section 4) are described in terms of Marr's (1982/2010) tri-level approach, which we used to construct a 'bridge' from a formal phonological model to activity one might plausibly find in a human nervous system. In order to connect the substance-free and timeless (phonology) with the substance-laden and temporally coordinated (the SM system used in speech), CP takes features of phonological SRs and relates them to neuromuscular activity (A_1) and arranges that activity temporally (A_2), thus generating an array of information (in a format which we call 'True Phonetic Representation') directly interpretable by the SM system. We have also presented some potential neurobiological correlates of various parts of CP (section 4.3). Finally, we have explored some of the implications of CP (section 5), showing how such an approach might inform the study of certain phonetic phenomena, most notably coarticulation, and suggesting that CP provides better explanations of some phenomena often considered to fall within the purview of phonology, such as phonetic naturalness and gradience.

Further development of CP as an explanatory model of coarticulation and other PPI phenomena will require sharpening the details of both steps of the transduction algorithm (A_1 and A_2) and of CP's output units ($PF_{[F]}$). We posit CP as a model intervening between phonology (grammar) and physiological phonetics, and it is not surprising that such ideas have implications for the nature of the adjacent systems. On the phonological side, CP calls for a reassessment of distinctive feature theory in a strict biolinguistic manner. Also, the transduction of other aspects of phonological structure (e.g., prosody) should be explored. Ideally, these further developments of CP should be driven by theoretically sound models of phonological representation and computation on the one hand, and should be grounded in neurobiological findings on the other, thus reducing the conceptual distance between formal linguistics and cognitive neuroscience.

References

Ackermann, Hermann, Klaus Mathiak & Axel Riecker. 2007. The contribution of the cerebellum to speech production and speech perception: clinical and functional imaging data. *The Cerebellum* 6(3), 202–213.

Alario, Xavier, Hanna Chainay, Stéphane Lehericy & Laurent Cohen. 2006. The role of the supplementary motor area (SMA) in word production. *Brain Research* 1076(1), 129–143.

Anderson, Stephen R. & David Lightfoot. 2002. *The Language Organ: Linguistics as Cognitive Physiology*. Cambridge: Cambridge University Press.

Bale, Alan & Charles Reiss. 2018. *Phonology: A formal introduction*. Cambridge: MIT Press.

Beddor, Patrice S. 1983. *Phonological and Phonetic Effects of Nasalization on Vowel Height*. Bloomington: Indiana University Press.

Benguerel, André-Pierre & Helen Cowan. 1974. Coarticulation of upper lip protrusion in French. *Phonetica* 30, 41–55.

Binder, Jeffrey R., Julie A. Frost, Thomas A. Hammeke, Patrick S. F. Bellgowan, Jane A. Springer, Jackie N. Kaufman & Edward T. Possing. 2000. Human temporal lobe activation by speech and nonspeech sounds. *Cerebral Cortex* 10(5), 512–528.

Blumstein, Sheila E. & Shari R. Baum. 2016. Neurobiology of speech production: Perspective from neuropsychology and neurolinguistics. In Gregory Hickok & Steven L. Small (eds.). 2016. *Neurobiology of Language*. 689–699. London: Elsevier.

Boas, Franz & Deloria, Ella. 1941. Dakota grammar. *Memoirs of the National Academy of Sciences*, 23. Washington: U.S. Government Printing Office.

Boeckx, Cedric. 2009. *Language in Cognition. Uncovering Mental Structures and the Rules Behind Them.* Malden, MA: Wiley-Blackwell.

Boeckx, Cedric, Anna Martinez-Alvarez & Evelina Leivada. 2015. The functional neuroanatomy of serial order in language. *Journal of Neurolinguistics* 32, 1–15.

Bohland, Jason W. & Frank H. Guenther. 2006. An fMRI investigation of syllable sequence production. *NeuroImage* 32(2), 821–841.

Bouchard, Kristofer E., Nima Mesgarani, Keith Johnson & Edward F. Chang. 2013. Functional organization of human sensorimotor cortex for speech articulation. *Nature* 495(7441), 327–332.

Brentari, Diane. Sign language phonology. In John A. Goldsmith, Jason Riggle & Alan C. L. Yu (eds.), *The Handbook of Phonological Theory*. 691–721. Oxford: Wiley-Blackwell.

Buchsbaum, Bradley R., Gregory Hickok & Colin Humphries. 2001. Role of left posterior superior temporal gyrus in phonological processing for speech perception and production. *Cognitive Science* 25(5), 663–678.

Buchsbaum, Bradley R., Rosanna K. Olsen, Paul Koch & Karen Faith Berman. 2005. Human dorsal and ventral auditory streams subserve rehearsal-based and echoic processes during verbal working memory. *Neuron* 48(4), 687–697.

Buchwald, Adam & Michele Miozzo. 2011. Finding levels of abstraction in speech production: Evidence from sound-production impairment. *Psychological Science* 22(9), 1113–1119.

Buchwald, Adam & Michele Miozzo. 2012. Phonological and motor errors in individuals with acquired sound production impairment. *Journal of Speech, Language, and Hearing Research* 55(5), 1573–1586.

Cairns, Charles & Eric Raimy. 2011. Precedence relations in phonology. In Oostendorp, Marc van, Colin J. Ewen, Elizabeth V. Hume & Keren Rice (eds.), *The Blackwell Companion to Phonology*. 798–823. Oxford: Blackwell.

Carr, Philip. 1993. *Phonology*. London: MacMillian.

Catford, John C. 1982. *Fundamental Problems in Phonetics*. Edinburgh: Edinburgh University Press.

Chait, Maria, Steven Greenberg, Takayuki Arai, Jonathan Z. Simon & David Poeppel. 2015. Multi-time resolution analysis of speech: Evidence from psychophysics. *Frontiers in Neuroscience* 9, 1–10.

Chomsky, Noam. 1980. *Rules and Representations*. Oxford: Blackwell.

Chomsky, Noam. 1984. *Modular Approaches to the Study of the Mind*. San Diego, CA: San Diego State University Press.

Chomsky, Noam. 1986. *Knowledge of Language: Its Nature, Origins, and Use*. New York, NY: Praeger.

Chomsky, Noam. 1988. *Language and Problems of Knowledge: The Managua Lectures*. Cambridge, MA: MIT Press.

Chomsky, Noam. 1995. *The Minimalist Program*. Cambridge: MIT Press.

Chomsky, Noam. 2000a. *New Horizons in the Study of Mind and Language*. Cambridge: Cambridge University Press.

Chomsky, Noam. 2000b. *The Architecture of Language*. Oxford: Oxford University Press.

Chomsky, Noam. 2005. Three factors in language design. *Linguistic Inquiry* 36(1), 1–22.

Chomsky, Noam. 2007. Approaching UG from below. In Uli Sauerland & Hans-Martin Gärtner (eds.), *Interfaces + recursion = language?: Chomsky's Minimalism and the View from Syntax-Semantics*. 1–24. Berlin: de Gruyter.

Chomsky, Noam. 2012. *The Science of Language*. Cambridge: Cambridge University Press.

Chomsky, Noam. 2013. Problems of projection. *Lingua* 130, 33–49.

Chomsky, Noam. 2016. *What Kind of Creatures Are We*? New York, NY: Columbia University Press.

Chomsky, Noam & Morris Halle. 1968. *The Sound Pattern of English*. New York, NY: Harper & Row.

Clements, George N. 1985. The geometry of phonological features. *Phonology* 2(1), 225–252.

Clements, George N. & Elizabeth V. Hume. 1995. The internal organization of speech sounds. In John Goldsmith (ed.), *The Handbook of Phonological Theory*. 245–306. Oxford: Blackwell.

Cooper, William E. & Marc R. Lauritsen. 1974. Feature processing in the perception and production of speech. *Nature* 252(5479), 121–123.

Cowie, Roddy & Ellen Douglas-Cowie. 1992. *Postlingually Acquired Deafness: Speech Deterioration and the Wider Consequences*. Hawthorne, NY: Mouton de Gruyter.

Curtiss, Susan. 2013. Revisiting modularity: Using language as a window to the mind. In Massimo Piattelli-Palmarini & Robert C. Berwick (eds.), *Rich Languages from Poor Inputs*. 68–90. Oxford: Oxford University Press.

Donegan, Patricia & David Stampe. 1979. The study of Natural Phonology. In Daniel A. Dinnsen (ed.), *Current Approaches to Phonological Theory*. 126–173. Bloomington: Indiana University Press.

Dronkers, Nina F. 1996. A new brain region for coordinating speech articulation. *Nature* 384(6605), 159–161.

Eickhoff, Simon B., Stefan Heim, Karl Zilles & Katrin Amunts. 2009. A systems perspective on the effective connectivity of overt speech production. *Philosophical Transactions of the Royal Society of London A: Mathematical, Physical and*

Engineering Sciences 367(1896), 2399–2421.

Ellis, Lucy & William J. Hardcastle. 2002. Categorical and gradient properties of assimilation in alveolar to velar sequences: Evidence from EPG and EMA data. *Journal of Phonetics* 30, 373–396.

Embick, David & David Poeppel. 2015. Towards a computational(ist) neurobiology of language: Correlational, integrated and explanatory neurolinguistics. *Language, Cognition and Neuroscience* 30(4), 357–366.

Ernestus, Mirjam. 2011. Gradience and Categoricality in Phonological Theory. In Marc van Oostendorp, Colin J. Ewen, Elizabeth V. Hume & Keren Rice (eds.), *The Blackwell Companion to Phonology*. 2114–2136. Oxford: Blackwell.

Everaert, Martin, Marinus Huybregts, Noam Chomsky, Robert C. Berwick & Johan J. Bolhuis. 2015. Structures, not strings: Linguistics as part of the cognitive sciences. *Trends in Cognitive Sciences* 19(12), 729–742.

Fadiga, Luciano, Laila Craighero, Giovanni Buccino & Giacomo Rizzolatti. 2002. Speech listening specifically modulates the excitability of tongue muscles: A TMS study. *European Journal of Neuroscience* 15(2), 399–402.

Fant, Gunnar. 1960. *Acoustic Theory of Speech Production*. The Hague: Mouton.

Farnetani, Edda & Recasens, Daniel. 2013. Coarticulation and connected speech processes. In William J. Hardcastle, John Laver & Fiona E. Gibbon (eds.), *The Handbook of Phonetic Sciences*. 316–351. Oxford: Wiley-Blackwell.

Fowler, Carol. 1980. Coarticulation and theories of extrinsic timing. *Journal of Phonetics* 8, 113–133.

Fowler, Carol. 1983. Realism and unrealism: A reply. *Journal of Phonetics* 11, 303–322.

Fowler, Carol & Elliot Saltzman. 1993. Coordination and coarticulation in speech production. *Language and Speech* 36, 171–195.

Gallistel, Charles R. & Adam Philip King. 2010. *Memory and the Computational Brain: Why Cognitive Science will Transform Neuroscience*. Malden: Wiley-Blackwell.

Gow, David W. 2012. The cortical organization of lexical knowledge: a dual lexicon model of spoken language processing. *Brain and Language* 121(3), 273–288.

Guenther, Frank H. 1995a. Speech sound acquisition, coarticulation, and rate effects in a neural network model of speech production. *Psychological Review* 102(3), 594–621.

Guenther, Frank H. 1995b. A modeling framework for speech motor development and kinematic articulator control. *Proceedings of the XIIIth International Congress of Phonetic Sciences.* Vol. 2. 92–99. Stockholm: KTH and Stockholm University.

Guenther, Frank H., Michelle Hampson & Dave Johnson. 1998. A theoretical investigation of reference frames for the planning of speech movements. *Psychological Review* 105(4), 611–633.

Guenther, Frank H., Satrajit S. Ghosh & Jason A. Tourville. 2006. Neural modeling and imaging of the cortical interactions underlying syllable production. *Brain and Language* 96(3): 280–301.

Guenther, Frank H. & Tony Vladusich. 2012. A neural theory of speech acquisition and production. *Journal of Neurolinguistics* 25(5), 408–422.

Guenther, Frank H. & Gregory Hickok. 2016. Neural models of motor speech control. In Gregory Hickok & Steven L. Small (eds.). *Neurobiology of Language*.

725–740. London: Elsevier.

Gussenhoven, Carlos & Haike Jacobs. 2011. *Understanding Phonology*. London: Hodder Arnold & Hachette Livre.

Hajek, John & Shinji Maeda. 2000. Investigating universals of sound change: The effect of vowel height and duration on the development of distinctive nasalization. *Papers in Laboratory Phonology V: Acquisition and the Lexicon*, 52–69.

Hale, Mark. 2007. *Historical Linguistics. Theory and Method*. Oxford: Blackwell Publishing.

Hale, Mark & Charles Reiss. 2000a. 'Substance abuse' and 'dysfunctionalism': Current trends in phonology. *Linguistic Inquiry* 31(1), 157–169.

Hale, Mark & Charles Reiss. 2000b. Phonology as cognition. In Noel Burton-Roberts, Philip Carr & Gerard Docherty (eds.), *Phonological Knowledge: Conceptual and Empirical Issues*. 161–184. Oxford: Oxford University Press.

Hale, Mark & Madelyn Kissock. 2007. The phonetics–phonology interface and the acquisition of perseverant underspecification. In Gillian Ramchand & Charles Reiss (eds.), *The Oxford Handbook of Linguistic Interfaces*. 81–101. Oxford: Oxford University Press.

Hale, Mark & Charles Reiss. 2008. *The Phonological Enterprise*. Oxford: Oxford University Press.

Halle, Morris. 1983/2002. On distinctive features and their articulatory implementation. In Morris Halle, *From Memory to Speech and Back*. 105–121. Berlin & New York: de Gruyter.

Halle, Morris & George N. Clements. 1983. *Problem Book in Phonology*. Cambridge, MA: MIT Press.

Hammarberg, Robert. 1976. The metaphysics of coarticulation. *Journal of Phonetics* 4, 353–363.

Hardcastle, William J. & Nigel Hewlett (eds.) 1999. *Coarticulation: Theory, Data and Techniques.* Cambridge: Cambridge University Press.

Hayes, Bruce, Robert Kirchner & Donca Steriade (eds.). 2004. *Phonetically Based Phonology*. Cambridge: Cambridge University Press.

Heath, Jeffrey. 1999. *A Grammar of Koyra Chiini: The Songhay of Timbuktu*. Berlin: Mouton.

Hickok, Gregory. 2012. Computational neuroanatomy of speech production. *Nature Reviews Neuroscience* 13(2), 135–145.

Hickok, Gregory & David Poeppel. 2000a. Dorsal and ventral streams: a framework for understanding aspects of the functional anatomy of language. *Cognition* 92(1), 67–99.

Hickok, Gregory & David Poeppel. 2000b. Towards a functional neuroanatomy of speech perception. *Trends in Cognitive Sciences* 4(4), 131–138.

Hickok, Gregory & David Poeppel. 2007. The cortical organization of speech processing. *Nature Reviews Neuroscience* 8(5), 393–402.

Hickok, Gregory, Kayoko Okada & John T. Serences. 2009. Area Spt in the human planum temporale supports sensory-motor integration for speech processing. *Journal of Neurophysiology* 101(5), 2725–2732.

Hickok, Gregory, John Houde & Feng Rong. 2011. Sensorimotor integration in speech processing: Computational basis and neural organization. *Neuron* 69(3), 407–422.

Hickok, Gregory & David Poeppel. 2016. Neural basis of speech perception. In Gregory Hickok & Steven L. Small (eds.), *Neurobiology of Language*. 299–310. London: Elsevier.

Hickok, Gregory & Steven L. Small (eds.). 2016. *Neurobiology of Language*. London: Elsevier.

Hockett, Charles F. 1955. *A Manual of Phonology*. Baltimore: Waverly Press.

Hockett, Charles F. 1959. Animal 'languages' and human language. *Human Biology* 31(1), 32–39.

Houde, John F. & Michael I. Jordan. 1998. Sensorimotor adaptation in speech production. *Science* 279(5354), 1213–1216.

Idsardi, William J. & Eric Raimy. 2013. Three types of linearization and the temporal aspects of speech. In Theresa M. Biberauer & Ian Roberts (eds.), *Challenges to Linearization*. 31–56. Berlin: Mouton de Gruyter.

Idsardi, William J. & Philip Monahan. 2016. Phonology. In Gregory Hickok & Steven L. Small (eds.), *Neurobiology of Language*. 141–151. London: Elsevier.

Indefrey, Peter & Willem J. M. Levelt. 2004. The spatial and temporal signatures of word production components. *Cognition* 92(1), 101–144.

Isac, Daniela & Charles Reiss. 2013. *I-Language. An Introduction to Linguistics as Cognitive Science*. 2nd edition. Oxford: Oxford University Press.

Jackendoff, Ray. 1994. *Patterns in the Mind. Language and Human Nature*. New York, NY: BasicBooks.

Jakobson, Roman. 1939. Observations sur le classement phonologique des consonnes. *Proceedings of the 3rd International Congress of Phonetic Sciences*, 34–41.

Jakobson, Roman. 1971. *Selected Writings: Phonological Studies*. The Hague: Mouton.

Jakobson, Roman, Gunnar Fant & Morris Halle. 1952. *Preliminaries to Speech Analysis.* Cambridge: MIT Press.

Jakobson, Roman & Morris Halle. 1956. *Fundamentals of Language*. The Hague: Mouton.

Jueptner, Markus & Michael Krukenberg. 2001. Motor system: Cortex, basal ganglia, and cerebellum. *Neuroimaging Clinics of North America* 11(2), 203–219.

Katamba, Francis. 1989. *An Introduction to Phonology*. London & New York: Longman.

Keating, Patricia A. 1988. Underspecification in phonetics. *Phonology* 5, 275–292.

Kenstowicz, Michael. 1994. *Phonology in Generative Grammar*. Oxford: Blackwell.

Kenstowicz, Michael & Charles Kisseberth. 1979. *Generative Phonology. Description and Theory.* New York, NY: Academic Press.

Ladefoged, Peter & Johnson, Keith. 2010. *A Course in Phonetics*. Boston, MA: Wadsworth Cengage Learning.

Lane, Harlan & Bernard Tranel. 1971. The Lombard sign and the role of hearing in speech. *Journal of Speech and Hearing Research* 14(4), 677–709.

Lane, Harlan, Jane Wozniak, Melanie Matthies, Mario Svirsky, Joseph Perkell, Michael O'Connell & Joyce Manzella. 1997. Changes in sound pressure and fundamental frequency contours following changes in hearing status. *The Journal of the Acoustical Society of America* 101(4), 2244–2252.

Larson, Charles R., Theresa A. Burnett, Jay J. Bauer, Swathi Kiran & Timothy C. Hain. 2001. Comparison of voice F0 responses to pitch-shift onset and offset conditions. *The Journal of the Acoustical Society of America* 110(6), 2845–2848.

Lashley, Karl S. 1951. *The Problem of Serial Order in Behavior*. Pasadena, CA: California Institute of Techonology.

Lass, Roger. 1984. *Phonology. An Introduction to Basic Concepts.* Cambridge: Cambridge University Press.

Laver, John. 1994. *Principles of Phonetics*. Cambridge: Cambridge University Press.

Lenneberg, Eric. 1967. *Biological Foundations of Language*. New York: Wiley.

Levelt, Willem J. M., Ardi Roelofs & Antje S. Meyer. 1999. A theory of lexical access in speech production. *Behavioral and Brain Sciences* 22(1), 1–38.

Liberman, Alvin M. 1957. Some results of research on speech perception. *The Journal of the Acoustical Society of America* 29(1), 117–123.

Liberman, Alvin M., Franklin S. Cooper, Donald P. Shankweiler & Michael Studdert-Kennedy. 1967. Perception of the speech code. *Psychological Review* 74(6), 431–461.

Liberman, Alvin M. & Ignatius G. Mattingly. 1985. The motor theory of speech perception revised. *Cognition* 21(1), 1–36.

Linker, Wendy. 1982. Articulatory and acoustic correlates of labial activity in vowels: A cross-linguistics study. *UCLA Working Papers in Phonetics* 56, 1–134.

Lisker, Leigh. 1978. Segment duration, voicing, and the syllable. In A. Bell & J. B. Hopper (eds.) *Syllables and Segments*. 133–142. Amsterdam: North-Holland.

Lombard, Etienne. 1911. Le signe de l'élévation de la voix. *Annales des Maladies de l'oreille, duLarynx du Nez et du Pharynx* 37, 101–119.

Lorenz, Konrad & Nikolaas Tinbergen. 1957. Taxis and instinct. In Claire H. Schiller (ed.), *Instinctive Behavior: The Development of a Modern Concept.* New York, NY: International Universities Press.

Lorenz, Konrad & Nikolaas Tinbergen. 1970. Taxis and instinctive behaviour pattern in egg-rolling by the Greylag goose. In *Studies in Animal and Human Behavior*. Cambridge, MA: Harvard University Press.

Lubker, James F., Robert McAllister, & Jan Carlson. 1975. Labial co-articulation in Swedish: A preliminary report. In Gunnar Fant (ed.) *Proceedings of the Speech Communication Seminar*. 55–64. Stockholm: Almqvist & Wiksell.

Maran, La Raw. 1973. Classificatory and phonetic features: A note on their relationship. In M. Kenstowicz & Kisseberth, C. (eds.), *Issues in Phonological Theory. Proceedings of the Urbana Conference on Phonology*. 61–74. The Hague: Mouton.

Marr, David. 1982/2010. *Vision. A Computational Investigation into the Human Representation and Processing of Visual Information.* Cambridge, MA: MIT Press.

Marshall, Chloë R. 2011. Sign language phonology. In Nancy C. Kula, Bert Botma & Kuniya Nasukawa (eds.), *The Continuum Companion to Phonology*. 254–277. London: Continuum.

Martinet, André. 1949. La double articulation linguistique. *Travaux du Cercle linguistique de Copenhague* 5, 30–37.

Mausfeld, Rainer. 2012. On some unwarranted tacit assumptions in cognitive neuroscience. *Frontiers in Psychology* 3, 1–13.

Mesgarani, Nima, Stephen V. David, Jonathan B. Fritz & Shihab A. Shamma. 2008. Phoneme representation and classification in primary auditory cortex. *The Journal of the Acoustical Society of America* 123(2), 899–909.

Mesgarani, Nima, Connie Cheung, Keith Johnson & Edward F. Chang. 2014. Phonetic feature encoding in human superior temporal gyrus. *Science* 343(6174), 1006–1010.

Monahan, Philip J., Ellen F. Lau & William J. Idsardi. 2103. Computational primitives in phonology and their neural correlates. In Boeckx, Cedric & Kleanthes K. Grohmann (eds.), *The Cambridge Handbook of Biolinguistics*. 233–256. Cambridge: Cambridge University Press.

Morton, Katherine. 1987. Cognitive Phonetics–some of the evidence. In Robert Channon & Linda Shockey (eds.), *In Honor of Ilse Lehiste: Ilse Lehiste Pühendusteos*. 191–194. Dordrecht: Foris Publications.

Obleser, Jonas, Aditi Lahiri & Carsten Eulitz. 2004. Magnetic brain response mirrors extraction of phonological features from spoken vowels. *Journal of Cognitive Neuroscience* 16(1), 31–39.

Odden, David. 2013. *Introducing Phonology*. Cambridge: Cambridge University Press.

Ogunbowale, Peter O. 1970. *The Essentials of the Yoruba Language*. London: University of London Press.

Ohala, John J. 1990. The Phonetics and Phonology of Aspects of Assimilation. In John Kingston & Mary Beckman (eds.), *Papers in Laboratory Phonology I: Between the Grammar and Physics of Speech*. 258–275. Cambridge: Cambridge University Press.

Ohala, John J. 2003. Phonetics and Historical Phonology. In Brian Joseph & Richard Janda (eds.), *The Handbook of Historical Linguistics*. 667–686. Oxford: Blackwell Publishing.

Öhman, Sven E. G. 1966. Coarticulation in VCV utterances: Spectrographic measurements. *Journal of the Acoustic Society of America 39*, 151–68.

Öhman, Sven E. G. 1967. Numerical model of coarticulation. *Journal of the Acoustical Society of America* 41, 310–20.

Perkell, Joseph S., Frank H. Guenther, Harlan Lane, Melanie L. Matthies, Pascal Perrier, Jennell Vick, Reiner Wilhelms-Tricarico & Majid Zandipour. 2000. A theory of speech motor control and supporting data from speakers with normal hearing and with profound hearing loss. *Journal of Phonetics* 28(3), 233–272.

Phillips, Colin, Thomas Pellathy, Alec Marantz, Elron Yellin, Kenneth Wexler, David Poeppel, Martha McGinnis & Timothy Roberts. 2000. Auditory cortex accesses phonological categories: An MEG mismatch study. *Journal of Cognitive Neuroscience* 12/6, 1038–1055.

Poeppel, David. 2012. The maps problem and the mapping problem: Two challenges for a cognitive neuroscience of speech and language. *Cognitive Neuropsychology* 29(1–2), 34–55.

Poeppel, David & David Embick. 2005. Defining the relation between linguistics and neuroscience. In Anne Cutler (ed.), *Twenty-First Century Psycholinguistics: Four Cornerstones.* 103–118. London: Psychology Press.

Poeppel, David & Martin Hackl. 2008. The functional architecture of speech perception. In James R. Pomerantz (ed.) *Topics in Integrative Neuroscience: From Cells to Cognition* 154-180. Cambridge: Cambridge University Press.

Poeppel, David & Philip J. Monahan. 2008. Speech perception: Cognitive foundations and cortical implementation. *Current Directions in Psychological Science* 17(2), 80–85.

Poeppel, David, William J. Idsardi & Virginie Van Wassenhove 2008. Speech perception at the interface of neurobiology and linguistics. *Philosophical Transactions of the Royal Society of London B: Biological Sciences* 363(1493), 1071–1086.

Poeppel, David & William J. Idsardi. 2011. Recognizing words from speech: the perception-action-memory loop. In Gareth Gaskell & Pienie Zwitserlood (eds.), *Lexical Representation: A Multidisciplinary Approach.* 171–196. New York, NY: Mouton de Gruyter.

Postal, Paul. 1968. *Aspects of Phonological Theory*. New York: Harper & Row.

Purcell, David W. & Kevin G. Munhall. 2006. Compensation following real-time manipulation of formants in isolated vowels. *The Journal of the Acoustical Society of America* 119(4), 2288–2297.

Pylyshyn, Zenon W. 1984. *Computation and Cognition: Toward a Foundation for Cognitive Science*. Cambridge, Mass.: MIT Press.

Recasens, Daniel, Maria Dolors Pallarès & Jordi Fontdevila. 1997. A model of lingual coarticulation based on articulatory constraints. *The Journal of the Acoustical Society of America* 102(1), 544–561.

Reiss, Charles. 2007. Modularity in the 'sound' domain: Implications for the purview of Universal Grammar. In Gillian Ramchand & Charles Reiss (eds.), *The Oxford Handbook of Linguistic Interfaces*. 53–79. Oxford: Oxford University Press.

Reiss, Charles. 2012. Towards a bottom-up approach to phonological typology. In Anna Maria Di Sciullo (ed.), *Towards a Biolinguistic Understanding of Grammar*. 169–191. Amsterdam: John Benjamins Publishing Company.

Reiss, Charles. 2018. Substance Free Phonology. In Stephen J. Hannahs & Anna R. K. Bosch (eds.), *The Routledge Handbook of Phonological Theory*. 425–452. New York: Routledge.

Sandler, Wendy. The phonological organization of sign languages. *Language and Linguistics Compass* 6(3), 162–182.

Scharinger, Mathias, Philip J. Monahan & William J. Idsardi. 2012. Asymmetries in the processing of vowel height. *Journal of Speech, Language, and Hearing Research* 55(3), 903–918.

Stetson, Raymond H. 1951. *Motor Phonetics. A Study of Speech Movements in Action*. Amsterdam: Oberlin College Press.

Stevens, Kenneth. 1998. *Acoustic Phonetics*. Cambridge, MA: MIT Press.

Škarić, Ivo. 2007. Fonetika hrvatskoga književnoga jezika. In Radoslav Katičić (ed.), *Glasovi i oblici hrvatskoga književnoga jezika*. 17–160. Zagreb: Nakladni zavod Globus.

Škarić, Ivo. 2008. *Hrvatski govorili!* Zagreb: Školska knjiga.

Tatham, Mark. 1984. Towards a cognitive phonetics. *Journal of Phonetics* 12(1), 37–47.

Tatham, Mark. 1987. Cognitive Phonetics–some of the theory. In Robert Channon & Linda Shockey (eds.), *In Honor of Ilse Lehiste: Ilse Lehiste Pühendusteos*. 271–276. Dordrecht: Foris Publications.

Tatham, Mark. 1990. Cognitive phonetics. *Advances in Speech, Hearing and Language Processing* 1, 193–218.

Tourville, Jason A. & Frank H. Guenther. 2011. The DIVA model: A neural theory of speech acquisition and production. *Language and Cognitive Processes* 26(7), 952–981.

Tremblay, Pascale, Isabelle Deschamps & Vincent L. Gracoo. 2016. Neurobiology of speech production: A motor control perspective. In Gregory Hickok & Steven L. Small (eds.), *Neurobiology of Language*. 741–750. London: Elsevier.

Trubetzkoy, Nikolaj Sergejevič. 1939/1969. *Principles of Phonology*. Los Angeles, CA: University of California Press.

Vaux, Bert. 2008. Why the phonological component must be serial and rule-based. In Bert Vaux & Andrew Nevins (eds.), *Rules, Constraints, and Phonological Phenomena*. 20–60. Oxford: Oxford University Press.

Vaux, Bert. 2009. The role of features in a symbolic theory of phonology. In Eric Raimy & Charles E. Cairns (eds.), *Contemporary Views on Architecture and Representations in Phonology*. 75–97. Cambridge, MA: MIT Press.

Volenec, Veno. 2013. Acoustic analysis of the intervocalic *J* in Croatian speech. *Speech* 30(2), 117–151.

Volenec, Veno. 2015. Coarticulation. In Jasmine Davis (ed.), *Phonetics: Fundamentals, Potential Applications and Role in Communicative Disorders*. 47–86. New York: Nova Science Publishers.

Waldstein, Robin S. 1990. Effects of postlingual deafness on speech production: Implications for the role of auditory feedback. *The Journal of the Acoustical Society of America* 88(5): 2099–2114.

Yates, Aubrey J. 1963. Delayed auditory feedback. *Psychological Bulletin* 60, 213–251.

Zsiga, Elizabeth C. 2013. *The Sounds of Language. An Introduction to Phonetics and Phonology.* Oxford: Wiley-Blackwell.

Veno Volenec
University of Zagreb
Faculty of Humanities and Social Sciences
Department of Linguistics
Ulica Ivana Lučića 3
10000 Zagreb
Croatia

venovolenec@gmail.com

Charles Reiss
Concordia University
Linguistics Program
FB 801.23
1250 Guy Street
Montreal H3G 1M8
Canada

charles.reiss@concordia.ca

A Program for the Genetics of Grammar

Kenneth N. Wexler

Departing from Lenneberg's biological conception of language and its development, this paper first reviews select examples from research on language development and its interface with genetics before making some specific proposals with regard to how the genetics of grammar could be investigated. The central proposal of this paper is that an important, perhaps necessary, avenue for studying the genetics of grammar is to study the genotypes corresponding to phenotypes of child (and genetically impaired) versions of the computational system of grammar, as opposed to strictly descriptive measures of a construction or standardized linguistic tests. In some cases, these phenotypes have wide explanatory ability, suggesting that they directly involve parts of the computational system of language. The primary example discussed is the phenotype of the Unique Checking Constraint (UCC). In particular, it is proposed that one could usefully start to investigate the genetic basis for he development of finiteness, object clitic omission, and related phenomena of the UCC. A second, less developed example here, corresponding to a much later developmental stage, is the Universal Phase Requirement (UPR), regulating verbal passives and many other phenomena in children.

Keywords: genetics; biolinguistics; syntax; language acquisition; Unique Checking Constraint (UCC); Universal Phase Requirement (UPR)

1. Introduction

Eric Lenneberg (1967) proposed a view of human language that situated language and its development (a particular interest of Lenneberg's, for good reason, given his overall view of language) squarely within a classical biological framework that saw at least some parts of human knowledge as being rooted in human biology. His view was very much different from the standard psychological framework that assumes learned "associations" as the fundamental basis for language. For him, language was species-specific and resulted from human biology. We might think of the capacity for sonar in bats as a comparable example. Whatever learning takes place in the bat, there is no question but that the bat is equipped for sonar by its biology. The contemporary view of human language (as in a major classical tradition) treats it in the same way, as Lenneberg saw that it should.

I would like to thank two anonymous reviewers, who made excellent suggestions, some of which I have responded to extensively. I also thank the editor, Patrick Trettenbrein, for suggesting that I write a paper and for showing a great deal of enthusiasm and patience as I worked through this. It goes without saying that all the errors are my own.

ISSN 1450–3417

One might speculate that the case of sonar is different because sonar is a sensory ability, rather than a deeper cognitive ability. I fail to see any argument for this position. Bat sonar is put to both perceptual (not purely sensory in any kind of sense that can be made of "sensory") and productive uses (as when the bat uses sonar to navigate). In this way it is like language, which is used to both comprehend and speak (or produce with other means, as in sign language). Sonar is a computational system in which the bat uses particular kinds of physical observations to compute information about properties like where objects are. This information can be used in navigation. We do not know if this is conscious computation, but given the unconscious nature of most of human linguistic computation (and other kinds of cognitive and perceptual computation), it would seem quite plausible that it is mostly, perhaps completely, unconscious. This very natural view should be extended to language: Why should we, as biological creatures, escape how biology works in general?

Lenneberg paid much attention to linguistic development as part of his argument. At the time, not too much was known about the details of the development (often called acquisition) of syntactic and semantic systems. But the simple properties that were known (the one-word stage, etc.) were enough to make Lenneberg see this development as consistent with the biological view. In the last 50 years, there has been extensive progress in results on the development of syntax and semantics, progress that allows us to strongly confirm the biological view of language. Linguistic development is a central part of the contemporary biolinguistic view, with some of its strongest results (outside of linguistic theory itself).

Can we go beyond this study and this basic claim about development and the biology of language? Can we in fact create a genetics of language and a developmental biology of language with a detailed analysis of the physical mechanisms that underlie the development of the computational system of language? The purpose of this paper is to argue that the progress that has been made in the study of language acquisition can yield the appropriate statement of the nature of the developmental phenotype that provides a natural set of hypotheses about what should be tied together (and not) in the development of language. In turn, these correlations and patterns should provide material that allows for the detailed development of a genetics and developmental biology of language. We will make some suggestions about which parts of the developmental phenotype are promising as an aid to the development of the biology. The phenotypical studies are a necessary first step. I would suggest that the results already achieved in the phenotypical studies in language acquisition are promising enough that it is worth exploring these from the detailed biological point of view.

2. Why Genetics Matters: Unification and Discovery

Unification is one of the strongest goals in science, and a strong feature of the most successful sciences. We feel that we have understood more if we have a more unified theory. Unification is strongly related to the notion of simplicity. A unified theory has principles that correctly cover a wide range of empirical phenomena. Sometimes what appear to be different levels of explanation can be unified. This results in an even stronger theory, covering a wider range of phenomena. Moreover, this unification can lead to understanding the particular levels in deeper ways than were previously understood.

Modern linguistic theory provides ample illustrations for such a move toward unification. In fact, unification may even be its most distinctive feature. It has been a constant tool of Chomsky's, since the beginning of generative work (Chomsky 2005). We are no longer satisfied with descriptions of particular constructions as an adequate theory. We want to know what the underlying grammatical computations are, the ones that result in particular constructions.

Moreover, Chomsky unified the study of linguistic description (understanding the different languages of the world) with the problem of language acquisition: the child's choice of which language she is encountering in her environment. Chomsky called this unification the problem of *explanatory adequacy*. We sometimes also refer to it as the *learnability* problem. There can be no question that focusing on this problem not only helped us to understand how language acquisition proceeds, but also had a major impact on the development of particular linguistic theories.

The contemporary science of language acquisition (i.e. linguistic development) has unified its theorizing and experimenting with the results of work coming from linguistic theory. Although it is widely known and accepted that the goal of explanatory adequacy has helped us establish the correct descriptions of linguistic phenomena, perhaps it is less widely known that the pursuit of the same types of unification has moved the study of language acquisition considerably beyond its pre-generative state. In the study of linguistic development, we are no longer content with descriptions of particular surface phenomena in a child's language at particular times, taking account of the particular language that the child is encountering. Rather, we look for the underlying principles of the grammar that are developing, unifying with linguistic theory. We do not look for the development of particular constructions. Rather, in my opinion, the major advances in the pursuit of language acquisition in the last 25 years are due to the pursuit of the same type of unification in theorizing that has characterized linguistic theory and other successful sciences.

To take an example that we will flesh out later in this paper, as we pursue ideas for genetics, there is excellent empirical research that shows that the obligatory nature of finiteness in root sentences develops slowly over time. There is also excellent empirical research that shows that the obligatory production of direct object clitics (in languages that have such) in particular semantic contexts (as opposed to the surface omission of these direct object clitics) also develops slowly over time. Moreover, the time course of development of these two very different appearing surface phenomena appears to be quite similar. There is a unified theory of development (the *Unique Checking Constraint*; UCC) that explains why these two phenomena occur and why their time courses should be similar. That is, the two phenomena are explained by the same constraints on the underlying computation, so that as the computation develops, the phenomena appear to develop in similar ways. Hence, we have a deeper sense of linguistic development than when surface phenomena were studied only in their own right.

Moreover, the unification has helped us to discover more properties of the unified constructions. For example, the UCC predicts that object clitics will be sometimes omitted by a young child, but it does not predict that the clitics will be used in the wrong semantic context, or that clitics might be placed in the wrong syntactic position, for example, after the verb when they should be before the verb. These predictions turn out to be empirically correct.

The general argument of this paper is that adding the problem of genetics to the problem of explanatory adequacy and the problem of unification to linguistic development has the potential for uncovering further unification, in this case across another level of inquiry, the genetic level, that is the causal level of development. In addition to possibly discovering more about how the genetics of language works, we might find, as we have argued, that we have discovered more about the particular developmental linguistic phenomena that we have identified, and ultimately about the nature of human language itself as understood in linguistic theory. These are possibilities when we study genetics: unification and discovery in the scientific study of language.

The central specific proposal of this paper is that an important avenue for studying the genetics of grammar is to study the genotype corresponding to the phenotype of the UCC. In particular, we can look for the genetic basis for the development of finiteness, clitic omission and related phenomena. Just as for cognitive phenomena in general, we only have extremely tentative understandings of the physiology of the brain that allows us to compute linguistic representations, for example, some locational phenomena, some time course phenomena, etc. (see, e.g., Friederici, this issue). We have no idea how neurons interact to compute the representations. Since science now knows that genetic systems not only play a major role in inheritance but also help to guide the actual workings of cells and their interaction, we might ultimately also understand the physiology of language in a much deeper way if we can make progress in the study of genetics. This is another possibility of unification and discovery. There is no reason to believe that understanding of neuroscience must precede genetics; in any particular case, the exact opposite discovery course might be the more available route to understanding the systems.

The unification of the development of finiteness and object clitics is not the only example of unification that the study of linguistic development has given us. Another prominent example is the development of many kinds of seemingly unrelated "constructions" that have been unified in the study of the *Universal Phase Requirement* (UPR) as discussed in Wexler (2004a, among many later papers). The UPR was created to apply to the development of verbal passives and raising constructions, a unification that had already been carried out in the earlier *A-Chain Delay Hypothesis* (ACDH) by Borer & Wexler (1987). The ACDH had a particular empirical problem: it predicted, contrary to fact, a late development for the movement of subjects from the VP to the Tense Phrase. The UPR solved this problem.

Once the UPR existed, it not only unified existing phenomena, but it predicted entirely new phenomena. For example, the UPR predicts that the development of specificational copula sentences (like *The winner is Mary.*) is a very late development, as late as that of verbal passives and raising constructions (around 8 years of age). This is an extremely surprising prediction, as the surface form of the specificational copula (*DP is DP.*) is very simple. However, unpublished experiments to date confirm the prediction (Hirsch & Wexler 2008). Unification, once again, can play a strong role in discovery.

Similarly, the UPR predicts that the grammar of tough-movement will be considerably delayed (Wexler 2013a), similar to the delay in the grammar of verbal passives until around age 8 (precise up to the limit of available experimental data and current methods). This too is amply confirmed; see Wexler (2013a) for a review of the experimental data.

The predictions are obvious: whatever genetic events underlie the development of verbal passives will be the same as those underlying the development of specificational copulas, or tough-movement, for example.[1] These genetic phenomena are predicted to go together. For example, if there is a genetically caused severe delay in one piece of grammar (e.g., verbal passive), the same piece of genetic machinery should cause the delay in another (e.g. specificational copulas). Of course, such an assumption could be wrong; there might be alternative genetic causes of two pieces of delay that affect two different constructions. But the default working hypothesis, the one that a scientist would always start with is that there is one cause. If it is wrong, the experiments that will show that it is wrong will hopefully add additional insight.

3. Lenneberg's View of Linguistic Development: Biological

Although Lenneberg did not have any of this knowledge of grammatical development to aid his arguments, he made the best use of the empirical material at hand and saw the general structure of how language developed in a biological framework. We can see how the arguments and proposals that we have made so far fit within his general arguments.

First, Lenneberg understood that the development of language in an individual child must be the result of biology, of maturation:[2]

> We must assume that the child's capacity to learn language is a consequence of maturation because [...] the milestones of language acquisition are normally interlocked with other milestones that are clearly attributable to physical maturation [...]. (Lenneberg 1967: 178)

Maturation only became an important researchable topic in generative studies of acquisition with the publication of Borer and Wexler's (1987) independent argument that it best explained some particular linguistic developments (and late developments). Even after that, maturation was looked upon with suspicion in much of the field of language acquisition. Lately it seems to have become more mainstream, among generative acquisitionists at least. Lenneberg saw it in 1967, without the detailed knowledge of grammatical development that went into its later appearance, the reason I called a paper on the maturation of finiteness "Lenneberg's Dream" (Wexler 2003, 2004b). See Wexler (2013b) for further discussion.

---

[1] There are complications, of course. For example, tough-movement involves not only the need for a weak phase, ruled out by UPR, but also other kinds of mechanisms (see Wexler 2013a). Some of these may have time courses of their own, further delaying tough-movement. More research is needed.

[2] I thank an anonymous reviewer for pointers to the quotations from Lenneberg in this section and for impressing on me the importance of reviewing Lenneberg's contributions to the discussion. I was taking them for granted, but it is better to lay some of them out here.

Lenneberg also saw that linguistic theory plays a central role in understanding language acquisition, and that furthermore, acquisition studies could play an important role in the development of theory:

> The problems involved in language development cannot be understood in the absence of an analysis of the structure of language; and it is quite possible that the proper understanding of language structure is dependent upon empirical investigations into the acquisition process.
>
> (Lenneberg 1967: 275)

Once again, there were very few detailed acquisition results at the time that would have confirmed this view. Nevertheless, the view has been amply confirmed, as witnessed by the actual development of detailed results in developmental linguistics, some of which we discuss in this paper. The field of generative linguistic acquisition may in fact be defined by these views, although it took years to establish them in any kind of convincing detail.

Lenneberg also had a sophisticated understanding of the importance of the connection between genes and language. He wrote:

> DNA molecules [...] probably do no more than control the protein synthesis within a cell. [...] The puzzle now is: [...] how could something like the capacity for language have a genetic foundation? [...] The puzzle is, of course, not peculiar to the problems of the genetic basis of language, but also to the relationship between genic action and the inheritance of traits in general. Although we can only speculate on this point, our speculations with regard to language are no more daring than with regard to most other structural or functional features.
>
> (Lenneberg 1967: 239–240)

On the next page, he continued by saying:

> But, as is well known, genes do not merely control the size and shape of structure but skills and capacities as well. [...] Genes can only affect ontogenesis through varying the cell's repertoire for differentiation, but this, in turn, may have secondary effects upon structure, function and capacities. (Lenneberg 1967: 241)

Against this background, the interface of genetics and linguistics is what we will discuss in the next section.

4. Genetics and Linguistics

In short, we don't know the details of how genes work to influence the computational structure of language within the human, but they must. The logic of the situation is clear. We are in the typical position of studying the indirect relation between genes and function. It is a puzzle, not a mystery. The facts tell us that genetic networks are central to linguistic development. The facts also tell us that linguistic representations are what develop at the functional level. But genetic networks and linguistic representations are on entirely different levels of analysis. How do we proceed to connect them?

Among relations between biology and language, the relation between genetics and language is not unique in this regard. The situation is quite similar to the arguments of Poeppel & Embick (2005) concerning the relation between neuroscience and language. As they point out, neuroscienctific theory and linguistic theory posit totally different entities (e.g., *neuron* for the former, *sentence* for the latter).[3] No relation has been established between these different levels. That favors neither the neural theory nor the linguistic theory; they both might be accurately describing particular levels. The problem is, can we find any relation between these levels?

Poeppel and Embick argue that in order to make progress on the issue, a theory of the brain should employ linguistic categories, an enterprise not carried out in standard cognitive neuroscience. The argument here is analogous; in order to make progress in genetics, we should attempt to study the relation between genetic networks and linguistic representations. We might start simply, by finding genes that are part of the systems responsible for particular linguistic representations, through the lens of development. That is, what genetic networks are responsible for the development of linguistic representations? This paper makes particular proposals about how we might begin to answer those questions, by including linguistic representations and computations in the vocabulary of what we have to study.

The proposal of this paper is that the detailed and explanatory understanding of grammatical development that we have achieved in the last quarter century can play an important role (obviously not the only role) in creating a wedge into the problem. The argument is that this is a much more promising approach than the traditional consideration of "language" as a general entity, without detail, measured perhaps by a general test of linguistic abilities. Can the latter be the accurate description of language for genetic purposes? No. The facts say that the specific linguistic details matter. I have heard neuroscientists (and perhaps geneticists) describe language as an "emergent" ability. I have no idea what that means. Possibly "emergent" means "mysterious," or "beyond the scope of science." But one can only hope that we can do better than this and create serious science. Indeed, there are distinguished geneticists who have agreed. See Wexler (2013b) for a discussion of the views of Salvatore Luria, a Nobel Prize winner in genetics. There is every reason to believe that Lenneberg would have agreed. As mentioned previously, we will now work out an example, as a proposal.

5. Empirical and Theoretical Tools from Language Acquisition in the Aid of Genetics

What can we use from developmental linguistics to aid us in the study of the genetics of the computational system of grammar? Broadly speaking, anything that will work. In general, given work so far, this obviously includes:

1. The timing of linguistic development of particular pieces of the computational system of grammar. Regularity in timing and particular types of delays

---

[3] Perhaps Merge would be a better example in discussing syntactic computation. Indeed, some cognitive neuroscientists have sought to operationalise concepts from theoretical linguistics in their work and at least identify their neural correlates (e.g., Friederici, this issue).

help to lump different phenomena into a particular category, which helps in creating predictions concerning which constructions are controlled by similar genetic networks. Of course, this must be done in a theoretical context. We cannot simply study any 20 pieces of development and figure out the appropriately lumped pieces, without a theory connecting them.

2. The variation in time of development of particular pieces of grammar among typically developing children. Rate of development can be used in genetics to attempt to identify responsible genes. One might ultimately find certain genes being activated at particular times, triggering the development of particular pieces of linguistic competence.
3. Developmental linguistic impairments under genetic control. These include (listing only the syndromes whose role in linguistic development has been studied with some clear results): Specific Language Impairment (SLI), Williams syndrome, autism and Down syndrome. We do not have space to consider the extensive literature on particular pieces of linguistic development in these syndromes. However, the genetic logic is clear; if a syndrome shows a delay or breakdown of a particular piece of grammar, we expect that the genes controlling that piece of grammar are implicated in the syndrome. Trying to find these genes (or networks) cross-syndrome could be a very valuable additional attempt.

None of these sources of data and theory are radically different from how genetics generally proceeds when it has been successful. The proposal of this paper is that the same methods that have worked in some other cases can work here, when the phenotypes are linguistic computations, specifically defined from the results of linguistic theory.

The particular case of finiteness and clitic omission that we will detail later in this paper (the UCC, as the affected piece of computation) has been studied in both, (1) its typical development aspect, including delay until a certain age and (3) in its genetically determined developmental impairment aspect. While the focus has been on (1) and (3), even (2) has been studied to some extent; for example, we know that IQ (measuring another piece of cognition) and particular environmental causes do not differentially affect how individual children develop the piece of cognition (Rice, Wexler & Hershberger 1998).

Another desideratum for the study of genetics is that we have a quantitative measure in the behavioral results that can be used as a phenotypical measure to correlate with genetic activity. The finiteness and clitic omission case that we have studied and will explore in more detail below has this quantitative feature. Interestingly, it is only with the development of generative approaches to language acquisition, with their attention to linguistic detail, that these measures have been intensively developed.

6. Toward a Genetics of Grammar

How should we proceed with the study of the genetics and developmental biology of language? We have several relevant sciences at our disposal, sciences that have substantial achievements. The suggestion of this paper is that these sciences can only make progress in creating a serious genetics of language if they take actual, detailed account of each other. The sciences include:

a. Linguistic theory, which is the description of human linguistic knowledge. It asks: What is the phenotype of language, Universal Grammar (UG) in Chomsky's terms?

b. Developmental linguistics (language acquisition as it is often called), which has sought to describe the changing, developing phenotype and the underlying principles of development.

c. Genetics, with all its methods for determining the physical mechanism of inheritance (and, as we now know, how this mechanism is intricately related to the control of biological systems).

d. Developmental biology of the brain (which, of course, is related to genetics).

Of course, there are other sciences, and these might have a useful role to play and should not be excluded prematurely. It is worth noting, however, that a standard suggestion that we root the study of linguistic development (and thus of a related genetics, presumably) in results from cognitive psychology cannot be right, if for no other reason than that there are very few results from cognitive psychology that seem directly relevant to what we know of language and its development, at least if we are talking about the central computational system of language.[4] There is no general model or theory of cognition that we can use that will help us, none that has any of the appropriate detail.[5]

---

[4] There are a few intriguing experimental results that might play a role. For example, the (now controversial) observation that certain Theory of Mind (TOM) abilities develop rather late in the child's development (Wimmer & Perner 1983, and a large later literature) might potentially be of some use in the study of a developmental biology/genetics of language. However, most of the development of the computational system of language does not seem to have anything to do with TOM (particular kinds of pragmatic abilities might be related). It is not clear that TOM has much more relation to the development of grammar than it does to the bat's use of sonar. Nor is there a serious understanding of why TOM develops late, if it does (see the work of de Villiers 2007 who argues that in fact TOM develops based on the development of certain linguistic categories, in particular the propositional attitude verbs). One positive argument for using TOM is that it seems to have a particular physical location in the brain (Saxe & Kanwisher 2003), so that an attempt to relate the development of TOM to the development of these particular brain areas might be a help with genetic studies. Nevertheless, we have no reason to believe that the development of TOM has anything to do with the development of grammar.

[5] Another intriguing area of psychological research (Carey 2010, and a very large literature) involves the result that the development of the natural numbers (that is, the crucial recursive step that is at the basis of the concept of natural numbers) is a surprisingly late development. It seems that the natural numbers develop in the brain at a later age than much of the computational syntax and semantics of language, a system that seems much more complicated. This comparison of the rapid development of grammar with the very slow developmental of numbers shows us something about the developmental biology underlying the two systems. But it is not clear that it can tell us something useful for the detailed development of the genetics and developmental biology of language. So far as I can tell, none of the ideas based on learning models that have been proposed explain the late development; rather they stipulate that the recursive step is greatly dispreferred, which is supposed to explain why the basic property of

In the next section, we will present results from studies of the developmental phenotype of grammar that might be central in the development of a potential genetics and developmental biology of grammar. We will point out predictions that would follow from what is known about the different types of constructions that should (and should not) share the same developmental basis. The goal is to attempt to add some small thoughts to what might actually work in the development of a new science. Unsurprisingly, this new science is best aided by starting from what is actually known in existing, related sciences.

7. Unique Checking as a Phenotype for Early Grammar

The first phenotype applies to quite early grammar, to about the age of 3 (depending on language and child). In general, a fundamental property of computational syntax is the necessity for features to check (or match in certain ways) other features. This checking process is necessary in order to carry out *Merge*, the fundamental operation of the computational system of language, called syntax. The operation puts two phrases (in the broad sense) together, forming a larger phrase, allowing not only what we think of as phrase-structure, but also merge operations that connect non-local phrases (including but not limited to movement operations). Such a checking process is a necessary part of the derivation of a sentence, allowing formal (uninterpretable) features to be eliminated so that a structure is completely interpretable. Although interpretable features might be thought of as coming from the

---

the natural number system is late. Not only is this a stipulation, but it does not account for the fact that recursion in the grammatical system is much earlier. There is no obvious reason as to why, for example, the basic property *Merge* could not recur in young children. Why should grammar be so far ahead of the natural numbers? Furthermore, accounts of the development of the natural numbers show that they need to be taught with a series of rather specific steps. The children are taught to count pointing at objects, and it is a matter of years before they realize that the number of the last counted object represents the cardinality of what is counted. Recursion in grammar, on the other hand, simply emerges, it is what humans do, including young human children.

Here is a tentative suggestion about what might underly the slow development of the natural numbers, based on another suggestion that I think throws light on the relation of integers and grammar. In an important and original paper, Fox & Hackl (2006) argue that all scales that the linguistic system uses are dense. (A scale is dense if for any two members of the scale, there is another member of the scale in between them.) Our physical world, as understood by classical physics, is dense. The system of integers, of course, is not dense (there is no integer between 6 and 7, for example). The argumentation of the paper is subtle, and we cannot go through it here. But, accepting the conclusion, we might speculate that, while grammar, including its semantic component that includes the notion of scale, easily allows the development of dense scales in children, non-dense scales like the integers are not natural—they are developed with much cultural work. This is non-intuitive, I agree; we think of integers as simple. But the developmental results say they are not human-simple, in the sense of easily developing, without instruction. We might speculate that it was natural for language to evolve using dense scales in the semantic component; after all, our cognitive systems were used to dealing with the physical world in, for example, vision, where scales are non-dense. These speculations of course remain to be tested. For our purposes, they might suggest that the development of the integers relies on a learned, in fact, *taught* process, using general purpose abilities. It might turn out that language is necessary for learning integers only because we need to use language to teach the integer system. If these speculations are correct, we may not find a component of the human genome that is distinctly related to integers, as opposed to whatever our general learning capacities are.

interface semantic component, the uninterpretable features are necessary so that merge can operate correctly.

This first phenotype in child grammar restricts the ability of a phrase to check uninterpretable features against other features (usually interpretable). In general an interpretable feature on a phrase (typically a noun phrase, a DP in current terms) can check an unlimited number of features, so long as the right configurations hold. There is no bound or limit on how often the feature can check, thereby eliminating uninterpretable features. This is the way the computational system of language works in adults. In very young children, however, this unlimited checking capacity is instead limited, to one checking relationship. Once the interpretable feature has checked one uninterpretable feature, it is frozen, in a certain sense, not allowing it to check any further features. This is what Wexler (1998a) proposed as the abovementioned UCC, defined in (1).

(1) *Unique Checking Constraint*: An uninterpretable feature u may only check one feature. Once u has checked a feature it may not check any further features.

An important example involves finiteness, the necessity for a root sentence (with rare, quite particular exceptions) to be tensed. In English and almost all European languages, for example, finite tense must show up on the verb.[6] In English or Dutch, this finite tense is *past* or *non-past*. Taking English as an example, the morpheme *-s* indicates 3rd person present tense, as in the verb *goes*:[7]

(2) a. She goes to the store every Friday.
b. * She go to the store every Friday.
(3rd person singular but does not have the *-s* morpheme)
c. * I goes to the store every Friday. (present tense but 1st not 3rd person)

Children developing English until about age 3 often say (3b) instead of (3a).

(3) a. John like Mary.
b. John likes Mary.

They omit the necessary tense marking *-s*. This phenomenon of young children using the "infinitive" instead of the tensed form is widespread in the world's languages, widespread enough that it has a name: the Optional Infinitive (OI) stage or the Root Infinitive stage, indicating the same phenomenon.[8,9]

---

[6] In some configurations of tense and agreement, the tense might not actually be audible. This is irrelevant to the point, which involves the central system of syntax, not spell-out (the phonetic realization). As we shall soon illustrate (4), the fact that the errors induced by the UCC can complicate the phonetics helps to prove the point, with acquisition evidence.

[7] In all examples that are to follow, an asterisk (*) in front of a sentence indicates that the sentence is not well-formed in the adult language. The computational system marks it as such.

[8] While widespread the OIS is not universal. In Section 9 we will discuss the well-known computational reasons (the UCC plus the positive null-subject setting) for the lack of this error in particular kinds of languages.

[9] This phenomenon was called the OI stage in Wexler's (1990, 1992, 1993) original formulation to stress the fact that finite sentences in general existed alongside the (non-adult) nonfinite sentences in the child's grammar. Rizzi (1993), accepting the facts in Wexler's paper, called

Lest one thinks that the child error in English simply reflects some kind of surface omission of tense (*go* replaces *goes* because *-s* is omitted in the production of the word), we should look at a Dutch example. In Dutch the first person singular present tense is indicated by the root form of the verb (4a). The infinitive is phonetically more complicated and adds *-(e)n* (4b). In the OI stage, children will incorrectly substitute the infinitive for the finite verb, complicating the phonetics, but showing the preference in many cases for the nonfinite form of the verb. Such a phonetic complication quite often exists in many languages in the OI stage; this error is one in the computational system of language, the syntax, not in the phonetics.

(4) a. werk
 b. werken

Schütze & Wexler (1996) investigated a generalization about the form of the subject pronoun in child grammar in English during the OI stage. Children often produce the Accusative (non-NOM) form of the subject pronoun instead of the nominative form, using *him/her* instead of *he/she* as in (5).

(5) her/her go

However, a striking generalization first discovered by Loeb & Leonard (1991) and further confirmed in Schütze and Wexler's data, as well as in Schütze (1997), is that the accusative (non-NOM) form of the subject pronoun is only used when the verb is non-finite:

(6) a. # Her goes. (# means does not occur)
 b. Her go.
 c. She goes.
 d. She go.

At the same time, children will produce the nominative form (*he, she*) with a finite or non-finite form (6c and 6d).

Schütze and Wexler explained these generalizations in the following way: Nominative is standardly assumed to be assigned/checked by agreement (between the subject and the verb, done in terms of features; we will not explain an exact implementation). Verbs have an agreement feature and a tense feature. The agreement feature of a finite verb assigns/checks Nominative case. Children in the OI stage sometimes omit AGReement and sometimes omit Tense. For simplicity we can assume that just one of these is omitted (or neither, which then results in an adult sentence such as 6c). If AGR is omitted, but Tense exists, then tense is spelled out (e.g., *went* in past tense) but the lack of AGR means that NOM is not checked, so

---

it the Root Infinitive (RI) stage in order to stress the fact that the nonfinite sentences (with infinitival verbs) existed in root sentences (main clauses), whereas they only appear in embedded (subordinate) sentences in adults. As it turns out, the effects of non-finite sentences in the child grammar can be seen also in embedded sentences where nonfinite properties replace finite ones. See Wexler (2011) for detailed evidence, especially concerning OI children's extensive omission of the non-finite Tense marker, *to*, in English. So I would conclude that OI is a less misleading term than RI. Nevertheless, either term will be understood in the field as indicating more or less the same phenomena.

that default case (one not needing checking by one of these features) is invoked. In English it is well understood that accusative (non-nominative) case is the default (it is different from language to language). Children know the default form for English, so they use accusative (non-nominative) case for the subject when AGR is not in the structure. Moreover, the lack of Tense means that the verb does not have the present or past tense feature, resulting in the root form of the verb. This yields (6b). Now, suppose that AGR exists in the child's derivation but Tense does not. NOM is now checked (by AGR) and *he/she* occurs in the subject. However, Tense is omitted, so that again, in 3rd person singular, *-s*, which demands both an agreement and tense feature, cannot appear. The root form of the verb appears with a NOM subject (6d).

Crucially, one form that occurs only very rarely is the combination of accusative (non-nominative) in the subject and *-s* on the verb (that is, a finite verb). This is because the existence of *-s* indicates that AGR exists on the verb; thus the subject must be NOM. (6a) is predicted to not exist in the child's grammar, as indicated by the # mark, which means not part of the child's grammar.[10] Schütze and Wexler argued that the child omitted either AGR or Tense. This was the AGR/Tense Omission Model (ATOM) of the OI stage. Wexler (1998a) proposed that ATOM held because there was a limitation (the UCC) on the child's computational capacity. In particular, to derive a finite English sentence, the grammatical subject has a feature that must check a feature of AGR and a feature of Tense.[11] Since the subject's feature is interpretable (as a referential feature, say, or a determiner feature), it can check AGR, eliminating this uninterpretable feature, and likewise check Tense, eliminating its uninterpretable feature.

Wexler argued that the UCC as defined in (1) prevents the child from checking both the AGR feature and the Tense feature. This would result in an uninterpretable feature remaining in the derivation. This causes a crash of the entire derivation, and the sentence simply cannot be derived by the computational system. The child omits either AGR or Tense so that the derivation does not crash. This results in the set of phenomena described by ATOM. Note that the UCC results in a smaller set of derivations being grammatical for the young child than in the adult language. This is a general property of the phenotypes that we will discuss. As biological developmental processes take place, they expand the set of computations that the computational system computes as well formed.[12]

---

[10] Pine, Rowland, Lieven & Theakston (2005) argue against the generalizations discussed here, based on data in Manchester (i.e. British) English. I don't have an explanation for this data, which goes against the data on American English that has been reported. Perhaps there is a special property of Manchester English that allows for what appear to be non-NOM pronouns in subject position with a finite verb. Some kind of pragmatic emphasis, for example, as has been suggested exists in Irish English.

[11] The feature can be thought of as a D feature or as an EPP feature, depending on the particular grammatical model.

[12] See Wexler (1998a, 2004b) for an explanation of why the child sometimes violates UCC, allowing a grammatical finite sentence.

8. Genetics of Unique Checking

At this point, we have identified the UCC as a general limitation on linguistic computations. One of the important features of this model is that it makes predictions about which structures will develop at the same time or not. We will suggest that the UCC is genetically determined; it is a piece of biological development. Biology (genetics) determines when the UCC is no longer a limitation on the child's grammar.[13]

Just like any piece of developmental biology, this development takes time to unfold. Accordingly, Table 1 (Dutch data, typically developing children) shows how the nonfiniteness of verbs that are finite in adults is a diminishing fact of child grammar.[14] Although this table shows average data for a large group of children, it is also true that when longitudinal investigations are made, it turns out that the rate of nonfiniteness within an individual child also reduces incrementally with time. This is not a one-step process that takes place in a very short time, as we might expect from a piece of learning or insight. Rather, the child's genetically determined linguistic capacities unfold over time, just as physical growth processes do (e.g., teeth do not emerge full-blown; they grow over time).

age group	% OIs
1;07–2;00	83 % (126/152)
2;01–2;06	64 % (126/198)
2;07–3;00	23 % (57/253)
3;01–3;07	7 % (29/415)

Table 1: Proportions of Dutch root infinitives by age (from Wexler, Schaeffer & Bol 2004).

The obvious claim is that this effect of the UCC—the finiteness development over time—is genetically determined. Is there evidence? Yes.

First, there is a good deal of behavioral linguistic evidence that the slow rate of finiteness development is not caused by learning, that is, by environmental factors. Standard causes of learning that affect other cognitive/linguistic capacities (e.g., the strong effect of parental education and child IQ on vocabulary growth in the young child) do not have an effect on the development of finiteness (Rice, Wexler & Hershberger 1998).

Second, standard behavioral genetic experiments on twins show that the phenotype of the OI stage (development of finiteness) is strongly inherited (Ganger, Wexler & Soderstrom 1997, Ganger 1998) and that its inheritance is independent of memory abilities, in particular of phonological working memory (Bishop, Adams & Norbury 2006). This latter twin study estimates a heritability rate (*h* squared, that is the proportion of variance in performance on finiteness that is due to genes) of about .73 on the finiteness measure.[15] That is, variations in finiteness are mostly at-

---

[13] Other models of the OI stage, for example Rizzi (1993), will make quite different predictions from the UCC, for genetics as well as for development, even if they assume maturation as the underlying cause of development. That is, most of these models will not predict the developmental or genetic correlation between finiteness and object clitics that we later discuss. It is of course a serious virtue of the generative approach to linguistic development that there are detailed models with divergent predictions. One of the advantages of the UCC is that it makes these developmental correlations explicit, finding the cause of late development to be in a computational syntactic constraint rather than in a special property of subject positions.

[14] Children with SLI also show this slowly diminishing rate of non-finite sentences, although over a much more extended period, as we discuss in Section 9.3.

[15] I have estimated this number from the graph given in Figure 2 of the referenced paper.

tributable to genetic differences. Meanwhile, other measures (e.g., vocabulary size) are *not* attributable to genetic differences.

Third, there is even some evidence concerning the physical location of genes underlying the development of finiteness. Falcaro et al. (2008) studied impaired children on a measure of finiteness in English (% correct use of past tense in obligatory past tense contexts, in an elicitation experiment), also comparing the results to a phonological working memory (non-word repetition, NWR) measure.[16] We know from Schütze & Wexler 2000 that the past tense error in English is essentially never the use of the wrong tense (in this case present tense); rather it is the use of a non-finite OI form. Falcaro et al. also took DNA from the participants, performing a linkage analysis for both measures in candidate regions. Simplifying their results, NWR linked to a region on chromosome 16 and the past tense (finiteness) measure linked to a region on chromosome 19. A major question for research is whether finiteness in general is linked to chromosome 19 and whether other grammatical impairments, developing at a much later age, are also linked to that chromosome (and the same region) or whether they are distinctly represented genetically.

An anonymous reviewer asked about whether there is evidence concerning the development of verbal passives in SLI. If SLI children are delayed on verbal passives, and if the late development of verbal passives is an independent genetic event (as suggested by the very late timing of development compared to finiteness), why should the SLI syndrome show impairment on both these constructions? This is a difficult question in genetics that often goes under the name of co-morbidity. Why are there different pieces of competence impaired if they in fact might be independently determined by different pieces of genetics? One possibility is that they are not independent, that the same genetic event is the cause of both. But then, why is their time course so different (3;6 for finiteness development versus 8 years for passives)? One answer (if the data show co-morbidity in this domain) could be that there is something in the genetic pathways that goes from the genetic basis for finiteness (the UCC) to the genetic basis for the verbal passive (Universal Phase Requirement). There is no simple a priori answer, but it falls within the realm of research.

At any rate, as the reviewer suggests, evidence from linguistic development in the syndromes is very relevant. There have been a few studies on verbal passive in children with SLI, but so far as I know there is no published study that tests psychological (subject experiencer) verbs, which are the crucial determinant of late development of passive.[17] The only data I know are from unpublished work by Perovic & Wexler (2014). They show that children with SLI (mean age 130.5 months) do have a deficit in subject experiencers when compared to a somewhat younger group of typically developing children. So there might very well be a verbal passive deficit in SLI, although we should be careful because the sample included younger children. Children with SLI were able to use an adjectival interpretation to under-

---

[16] We will discuss later why noise was introduced into their data given both the age of children studied and the particular measure that they used, and will propose a genetic experiment that might produce clearer results.

[17] See Borer & Wexler (1987) and a large subsequent literature that convincingly shows that children's performance on verbal passives of "actional" (e.g. Agent/Instrument subjects) verbs is greatly aided by a strategy that interprets these as adjectival passives.

stand “actional passives,” just as typically developing children do but, as the paper shows, children with autism and language disability cannot.

More research is needed, not only genetic but also developmental linguistic in nature, to determine whether a child who inherits genes that determine the late (if ever) growth of finiteness also inherits genes that determine that late (if ever) growth of the verbal passive (i.e. UCC implicates UPR). If so, we need a genetic model that predicts such. Not quite as straightforward as independence, but not impossible.

There is experimental biolinguistic evidence concerning verbal passives and related constructions that lends credence to the idea that this piece of maturation of the computational system is in fact genetic. In behavioral genetic research, Ganger, Dunn & Gordon (2005) showed that identical twins inherit the ability to understand the syntax of verbal passives. In particular, the development of subject experiencer passives is much closer in identical twins that in fraternal twins. The development of verbal passives of “actional” verbs does not show this effect; the adjectival strategy is a piece of learning/strategy in children, not of inheritance.

There is also research on genetically caused impairments that contributes to the biolinguistic argument for the UPR. For example, Perovic & Wexler (2006, 2010) compared the development of “actional” and subject experiencer passives in children with Williams syndrome, and concluded that these children had

> a particular difficulty with the structure of the verbal passive, not directly related to general levels of nonverbal abilities, receptive vocabulary, or general comprehension of grammar.
>
> (Perovic & Wexler 2010: 1294)

They argued that the development was due to a genetically caused delay in the development of argument-chains, now subsumed under the UPR.

9. Proposal for a Study on the Development of Finiteness[18]

Much—if not most—remains to be done in this area. We need studies that investigate in detail how the development of finiteness is genetically determined, in many languages. In this section, I will propose one such study.

English is not the best language in which to pursue the investigation of the genetics of the development of finiteness. The reason is that the “infinitive”, that is the nonfinite form, does not have a distinctive marker; rather in 3rd person singular present tense, the finiteness (tense and agreement) marker *-s* is omitted to give the nonfinite form that is the root of the verb. So first, only 3rd person singular contexts in present tense or the omission of the past tense marker *-t* in past contexts can be used as a measure of the OI stage. Moreover, omissions can sometimes occur for other reasons than finiteness. In fact, the history of developmental psycholinguistics is replete with the error of thinking that only omission errors are made, not

---

[18] In this paper I am discussing almost no genetic detail, particular genetic mechanisms. I am concentrating instead on why I think that particular phenotypes are crucial for obtaining genetic results. For some interesting ideas that attempt to suggest more particular hypotheses about regulation and timing in cells that would relate to the types of phenotypes I am discussing here, see Rice (2012).

errors of substitution of one form for another. This error was due to the unfortunate concentration of so much of the research in early developing grammar being based on English, with its impoverished morphology. We get a much better sense of how the OI stage works in a language in which there is a particular marker for the nonfinite form, like the *-n* in Dutch that we mentioned. So we should look for such a language in which to do a genetic study.

Which language should this be? Familiar genetic considerations suggest that Icelandic would be an ideal language to carry out a study of the genetic basis for the development of finiteness. First, there is a genetically much more homogeneous population in Iceland than in most other countries. Second, there are extensive records in Iceland of family histories. These two factors have combined to make Iceland a place which can offer substantial benefit for the study of human genetics.

First we have to answer a technical linguistic question about Icelandic, the language of Iceland. I mentioned that not all languages have an OI stage. Does or maybe better, *should*, Icelandic have one? The UCC actually makes a prediction about this. Wexler (1998a) argues that in languages that have "null subjects", that is the strong (prevalent) possibility of the non-pronounciation of the subject of a finite verb, there is no AGReement feature to check because the AGReement feature is interpretable, in fact interpretable as the subject. Only noninterpretable features have to be eliminated, so there is no need to eliminate the AGR feature. Therefore, there is no need for the subject to check the AGR feature. This means that the subject has to check only one feature, Tense, and the UCC is not violated, since there is not more than one feature that has to be checked. A finite sentence in a null-subject language is therefore grammatical for the child's grammar, even when the UCC holds. Thus there is no reason to omit the Tense or AGR feature and the child's grammar of a null-subject language derives finite sentences in a totally adult way. The prediction is that the phenomenological OI state (the use of nonfinite sentences when a finite sentence is derived in the adult grammar) does not exist in the development of a null-subject language. This prediction is true (see Wexler 1998a and many other references). Now, Icelandic is not a null-subject language. We therefore predict that Icelandic should have an OI stage. A study was done in Sigurjonsottir (1999) and indeed it turns out that Icelandic has a quite strong OI stage.[19]

What kind of studies would be most likely to attain results? There are several possibilities that we should explore, where Icelandic experiments would be ideal, for the reasons given, but many other languages qualify.

---

[19] This study on Icelandic actually provided a particularly important result in distinguishing between two potential generalizations concerning which languages underwent the OI stage. The null-subject property was one idea. Another idea was that the languages which contain rich agreement were the ones that did not undergo the OI stage. Mostly, the two predictions overlap; most typically, rich agreement and the possibility of null-subjects correlate. Icelandic, however, has rich agreement but is not null-subject, thereby providing a testing ground for the correct generalization. It turns out that the null-subject idea was right, thereby providing evidence for the UCC model that derives NS/OI.

9.1. *Typical Children*

One can study a population of typically developing children, who will progress out of the OI stage at different rates. Various measures of development can be used: Relative amount of finite verbs in obligatory contexts at a given age (Wexler 1990, 1992, 1993), age at which a developmental criterion of percentage production of finite verbs in obligatory contexts is reached (Ganger, Wexler & Soderstrom 1997), number of correct judgment of the non-grammaticality of non-finite sentences or a measure from signal detection theory (Rice, Wexler & Redmond 1999) among several others. These same measures can be used in the methods discussed in the next sections. One can attempt to link time of development to time of activation of particular genes. The general idea is that variation in timing of the phenotype will lead to knowledge about the genetic structures involved.

9.2. *Twins*

One can study Icelandic or other languages identical and fraternal twins against some measure of the OI stage, for example, the % of finiteness in obligatory contexts at a certain age, or a measure of when a particular criterion is reached, etc., and try to establish that identical twins are more closely related on this measure than are fraternal twins, thereby confirming the hypothesis of genetic determination for exit from the OI stage, as the studies mentioned above did in English. In linkage and other genetic studies, it might also be possible to determine actual genetic differences in fraternal twins (or siblings) that lead to differences in the OI stage.

9.3. *Specific Language Impairment*

In OI (i.e. non-null-subject) languages, SLI has a very delayed OI stage called the Extended OI Stage (EOI; Rice Wexler and Cleave 1995, Wexler 1996, and many other references). The prediction is that the Icelandic populations will include children with SLI, characterized by a very late development of finiteness (EOI). So far as I know, Icelandic has not yet been studied from this point of view.[20] It is straightforward how to accomplish such a study of SLI: One standard way is to identify (in clinical settings, or in schools) children with a language problem but no obvious sign of any of the standard physical developmental delays (Down syndrome, Williams syndrome, autism, etc. and no mental disabilities (low IQ, etc.). Experimental linguistic tests are then given to determine facts about finiteness. We expect an EOI stage, a lack of finiteness, to show up among children older than the typical age for exit from the OI stage. Since we now have a non-typical population, one might look for genetic differences between the typical and SLI populations, linkage and other studies. There might actually be such differences responsible for SLI. If we find a genetic cause of this difference in finiteness between groups of SLI and typically developing children, we can hypothesize that it is the cause of the UCC. The simplest hypothesis is that the genes responsible for causing SLI

---

[20] A reviewer points out Thordardottir (2008). So far as I can see, the relevant data are not in that paper, but one might look further into it.

are the same genes whose varying time of activation across typically developing individuals cause different development patterns of finiteness in these individuals.

The best evidence we have supports the hypothesis that children with SLI never completely develop the grammatical underpinnings of finiteness: teenagers with SLI who start to perform well on simple tests of finiteness in declarative contexts do not judge finiteness correctly in other contexts (Rice, Hoffman & Wexler 2009). The hypothesis is that they have learned or been taught in many cases to use finiteness in simple declarative contexts. The children in our studies are either in special classes or have speech and language therapy. Getting the form of the verb correctly in simple sentences is worked on in language instruction, even drilled. There is no reason for children with SLI to not be able to learn in this way, given enough time—they are intelligent. They learn to produce a form in certain contexts. But they do not actually develop the grammar, as determined by other constructions, on which they are not instructed.

In particular, Rice, Hoffman & Wexler (2009) have shown that even teenagers with SLI do not have the finiteness requirement. They asked participants to judge the grammaticality of finite and non-finite questions (e.g., *What he drinking?*), all of which omitted either a form of *be* or a form of *do*. There is no reason to think that these simple questions are used for systematic teaching of the requirement for finiteness in children with SLI. The participants with SLI very often judged the non-finite sentence as grammatical, whereas the typically developing control participants did not. There was very little advancement with age. Even at age 15, the group with SLI only judged the sentences correctly about 76 % of the time, compared to their (younger, since "language-matched") typical controls, who judged the sentences correctly about 95 % of the time. Differences between groups were even larger than this numerical mean score indicates. The best way to analyze grammaticality judgment data is, as in signal detection theory, to take account of hits and false alarms, with biases, to detect whether participants appropriately distinguish grammatical and non-grammatical sentences. Detailed analyses showed very large differences between the participants with SLI and their controls, on the level of a standard deviation or more at all age groups. Furthermore, there was another control group, participants matched by age to the group with SLI. Differences here were even larger. Finiteness in these question constructions is simply not known at age 15 (possibly longer; those were the oldest children in the study) to participants with SLI.

In summary, the UCC seems to last beyond childhood in children with SLI, perhaps indefinitely, although children at younger ages can be taught to use their general intelligence to learn to produce (and judge) simple sentences correctly. This result further suggests that one can find genetic differences between the typical and SLI populations. Since Icelandic has a clear infinitival marker and is not a null-subject language, as we have pointed out, we expect to find an OI stage in the language, and this expectation is confirmed. Likewise, we expect to find an EOI stage in Icelandic children with SLI. I propose studying such children genetically, with linkage and other studies, to attempt to find the genetic underpinnings for the slow development of finiteness and more generally for the UCC.

As I pointed out, to the best of my knowledge, there is no study of SLI in Icelandic that has asked the question of whether there is an extended finiteness delay. So pursuing such a study in young children would be an excellent idea and, assuming confirmation, doing the genetic studies on the SLI versus typically developing group comparison, using a measure of finiteness (% use of a finite form in obligatory contexts, age at which a particular % criterion is met, or similar) as the phenotype. Given the clear Icelandic pattern expected and the relative genetic homogeneity of the population, we might hope to find a clear genetic marker in this Icelandic population of the UCC. Large families with family history recorded might also be a good source for the study of the inheritance of SLI, although there will in all likelihood not be any record of measures of finiteness in childhood records of older populations. One possibility of overcoming this latter problem would be to take advantage of the finding in English that competence in finiteness is delayed for an older population of children with SLI if ones uses the proper experimental methods, namely constructions in which a child might not have been instructed during language therapy (Rice, Hoffman & Wexler 2009). In particular, we can use such methods on older children, parents and grandparents, while still using production measures on very young children, all of these methods resulting in a measure of use of finiteness in obligatory contexts. It might even be possible to establish these non-production methods on an impaired older population that might reflect SLI.

The goal would be to see if we can establish SLI or not on several generations in a family, and collect DNA to do linkage and other genetic analyses on the extended family, using the determination of SLI or typical, and/or the continuous finiteness measure as the phenotype measure. Of course, once we use alternative measures (ones that the individuals haven't been instructed on), we can study even adult twins (identical versus fraternal) in a similar manner, including genetic studies.

10. English Again

The one study that attempted to link a measure of finiteness (in this case the production of past tense in obligatory contexts) in English to a genetic basis is Falcaro et al. (2008). As hinted at above, their study had two features that provided less that optimal results: First, the measure of use of past tense included overregularizations as an error. For example, suppose the correct form to use in an elicitation of past tense for the verb *go* is (7a). There are two ways that children can be wrong; they can either use an incorrectly regularized form (7b) or use a non-finite, OI form (7c).[21]

(7) a. Mary went to the store.
b. * Mary goed to the store.
c. * Mary go to the store.

The finiteness (OI) error, caused by the UCC, is to use (7c), not (7b) which, in fact represents a (past) tensed form, though with incorrect morphology. Falcaro et al. included overregularization forms like (7b) as errors, counting as correct uses only forms like (7a). Therefore, their measure of correct use is only an approximation

---

[21] As we have already pointed out above, the wrong tense error *Mary goes to the store.* is almost non-existent.

of the rate of finiteness, and therefore, only an approximation of the phenotype measure of use of finiteness in obligatory contexts, the degree to which UCC is active in a child.[22]

Falcaro et al. do not give tables of each type of response (7a–c) so that we cannot say how much the relevant measures differ in their data. Therefore, we need a genetic study based on the English finiteness phenotype in which the use of a *finite* response in obligatory contexts is given. If there are regular verbs elicited, there is no issue. But if irregular verbs are included, we have to count the overregularized forms as finite. This produces a more accurate measure of the finiteness phenotype.

The second feature of Falcaro et al.'s study that we would like to improve is the age of participants. In general, the OI stage in typically developing English is mostly over at about age 4;0 or 4;5, with only a small number of OI errors after that.[23] The mean age for participants (selected as language-impaired in Falcaro et al.) was 14;5, range 13;1–16;2). The mean age was 44;1 for parents, 18;8 for older siblings and 12;4 for younger siblings. We know from Rice, Wexler & Hershberger (1998) that children with SLI perform near ceiling (over 90 % as a group) at age 8 on finiteness when tested on simple present and past tense elicitations. At this and later ages, they have to be tested on constructions that they are not coached on in therapy in order to once again find large differences between typically developing and SLI children. And in fact in Falcaro et al.'s data even the language-impaired group performed reasonably well on the past tense elicitation. They scored 43.09 out of 52 maximum, for an .83 score of correctness.[24] However, as we discussed

---

[22] See Rice, Wexler, Marquis & Hershberger (2000) for detailed study of overregularizations versus OI forms in the English of children with SLI. When the overregularized form (*goed*) is taken as a finite form (though with incorrect spellout), the results establish that children with SLI are significantly worse (as always) from younger language-matched typically developing children. On the other hand, when analyzed according to whether the past tense forms that were produced were produced correctly (that is, overregularized or not), children with SLI performed quite similarly to typically developing control groups. SLI shows a problem in the development of finiteness, not in problems with regularization. Redmond & Rice (2001) studied the same question by using grammaticality judgments as well as production data. They concluded that,

> the production and acceptance rates of past tense overregularizations (e.g., *he falled*) by the SLI and language-match groups were similar, and both were higher than the age-match group. (Redmond & Rice 2001: 655)

In other words, on the crucial test of comparison of the SLI and language-matched groups for overregularization, children with SLI perform at the same level as the typically developing children. Only finiteness shows the worse performance of the SLI group and the language-matched group. Overregularization reflects other skills, not the genetically determined slow growth of finiteness. An anonymous reviewer also points out that overregularizations in typically developing children do not disappear when non-finite forms disappear, around 3;6. Overregularizations also significantly occur in null-subject languages, although non-finite forms do not, for reasons we understand. Overregularization and lack of finiteness are two completely different processes. We have no particular reason to think that overregularization variations across children and types of group are genetically determined, unless it's simply genetic determination of a simple learning process.

[23] See Rice, Wexler & Hershberger (1998) and Rice & Wexler (2001) for detailed estimates of the finiteness measure by age based on large populations of typically developing children and children with SLI.

[24] The relatives scored much better, of course, but we must remember that the large majority of participants were parents, mean age over 44.

above, overregularizations were scored as incorrect, even though they had finite morphology. So we would expect the finiteness rate would be considerably larger than .83, although we cannot determine it from the paper, which does not separate out finiteness from overregularization errors.

The suggestion for a proposed genetic linkage study of finiteness in English is thus clear: First, on an elicitation task it would be good to have young children as participants (younger than 4 if typically developing, younger than 7 if SLI). Second, one might study adolescent children (and perhaps adults) using a finiteness task that adolescent children with SLI have been shown to perform poorly on, for example, the task in Rice, Hoffman & Wexler (2009) or other tasks that are first confirmed to show difficulty in an impaired population. Third, if past tense is used, the finiteness rate should be proportion finite in obligatory contexts; if irregular verbs are part of the stimuli, they should count as "wrong" for this measure only if they are OIs, the root verb, and not an overregularization. Fourth, it would be worth expanding the elicited forms to include at least 3rd person singular present tense, as in many other SLI studies, so as to obtain a wider variety of examples of finiteness. One might even include examples with auxiliaries and copulas, which are understood to be omitted as part of the OI stage. The hope is that this more precise phenotypical measure will produce a stronger and clearer genetic result, when linkage studies are attempted.

11. Object Clitics

Lastly, we now want to consider a potential genetic study that would speak to a deep and fairly often misunderstood aspect of the biological theory of the UCC (and which applies more generally as well). The UCC is a hypothesized phenotype that is more general than a particular linguistic construction or small piece of competence. Rather it is a limitation on mechanism, in particular on the computational theory of language, that applies to a variety of constructions and what look like pieces of language competence. One of the (many) arguments for this limitation on mechanism in fact is this ability to predict developmental patterns in such strikingly different pieces of phenomenology.

We may think of the UCC, then, not as the description of a low level phenomenon, a particular piece of phenomenology, but rather more like an *endophenotype*, a term coined by John & Lewis (1966) and since then widely adopted. The idea is that the proper description that relates to genetic causes is not a particular behavioral phenomenon, but something more internal and general. In the case of the UCC, it is not use of "finiteness" that should be related to a genetic cause, but rather the UCC, a limitation of a particular kind on an internal piece of the computational mechanism of language. We should expect that this piece of the computation should affect several kinds of "behaviors," in our case several kinds of linguistic constructions.

One of the more developed and striking uses of the UCC is to a construction that appears to have nothing to do with finiteness, namely to the domain of object clitics. Since direct objects do not have any particular relation to finiteness (in contrast to subjects), a more low level description of the phenomenon of the development of finiteness would not think to put it together with the development of object clitics. However, it turns out that the UCC applies to both finiteness and object clitics, with particular and different effects on each development. An object clitic is a reduced pronoun that, in many languages, appears before the verb instead of the usual position of an object (in those languages) after the verb. An example is given in (8) below.

(8) *French*

Jean	la	voit.
Jean	*CL-her*	*sees*

'Jean sees her.'

Here, the object is the clitic pronoun *la*, that appears before the verb; if instead of this clitic a proper noun (*Mary*) or definite description (*the woman*) was the object, it would appear *after* the verb. In general, pronominal clitic objects like *la* are referentially dependent; they refer to an established entity in the discourse.

The most obvious phenomenon in the development of object clitics is that children before about age 3 very often omit them, in French or Italian and other languages. One might think that this was a case of omission of a phonetically light element (for whatever reason). However, one strong argument (among several) that the explanation is more syntactic is that in many languages (e.g., Greek, Spanish, Bulgarian, and Albanian), children omit very few direct object clitics, although they are phonetically reduced just as in French or Italian. Wexler (1998b, 2014) showed that the UCC predicted clitic omission in languages like French and Italian in which children indeed omit large numbers of clitics. As predicted, the age of clitic omission corresponds to ages of the OI stage (lack of finiteness) in those languages in which children often omit finiteness. Wexler (2002), Wexler, Gavarró & Torrens (2004), Tsakali and Wexler (2004), as well as Gavarró, Torrens & Wexler (2010) showed that the UCC predicted no omission for particular kinds of languages (those in which the participle did not agree with the object clitic. Some of these papers showed that the prediction was correct for Spanish and Greek. Kapia (2011) showed that the UCC predicts no omission for Albanian, and that the prediction is correct. Radeva-Bork (2012) showed that the UCC predicts no child clitic omission in Bulgarian and that the prediction was empirically correct. The essential idea is that in languages like French and Italian, which show participle agreement with the clitic, there is a double checking (of the clitic or of an empty object) whereas in languages without this agreement, there is only one checking. Thus the UCC predicts the necessity for omitting a clitic in the former but not in the latter.

The enophenotype, that is the UCC, applies to a wide variety of constructions, predicting particular development behaviors. Often these constructions—pieces of phenomenology—appear to be extremely different from each other. Yet, the UCC is a phenotype on the computational system of language, not a piece of behavior.[25]

---

[25] Another example of a very different type of construction constrained in children by the UCC is short-form negation in Korean (Baek & Wexler 2009).

12. Proposed Genetic Study of Object Clitics

The findings discussed in the previous section immediately suggest further genetic studies related to the UCC. The obvious prediction is that the genetic substrate of finiteness is also the genetic substrate of clitic omission. So, if it turned out, for example, that Falcaro et al.'s results linking the development of obligatory use of finiteness to chromosome 19 were correct, the same region of chromosome 19 should be linked to non-omission of object clitics. This is a striking prediction; without a particular endophenotypical model, we would be in no position to make such a prediction.

Importantly, the prediction is *not* that all pieces of grammatical development should be linked to the same genetic cause. Only the pieces caused by the computational limitations of the UCC should be so linked. Thus, late development of verbal passives and all the attendant constructions are not caused by the UCC, but by other computational limitations. We do not have time or space to consider these here in any detail, but they have the same general type of theoretical and empirical base as the case that we have discussed, the UCC. That is, the predictions are particular.

What kind of studies should be done? In languages like French, Italian, and Catalan, in which object clitics are omitted (and in which we expect them to be omitted even more often and later in SLI), one can use the clitic omission phenotype (proportion of appearance rather than omission of the clitic in obligatory contexts) and link this clitic measure to genes. In the same language, say, assuming it is also an OI language, one can do the same for finiteness. The genetic substrate (genes, expression patterns, etc.) should be the same for the two measures. One could then add a measure whose development is predicted to be not related to these, that is, not caused by the UCC, and one should not expect the same genetic cause to show up. An example of a language that is both an OI language and a clitic omission language is French, so one might consider such a study in that language. It might not be ideal to use French because, for various reasons, the OI rates are somewhat smaller than in many other languages, probably because of the particular morphological patterns of the language. Some of the Germanic and Slavic languages have a kind of object clitic, not nearly so common as the Romance clitics, that might lend itself to a study of omission. Much more needs to be discovered about particular languages and clitics; it would be ideal for experimental genetic methodology to have a language in which both the OI and clitic omission rates were robust so that genetic determinations on each particular participant could be compared for the two phenotypes.

Studies could also relate a finiteness measure in one language to a clitic omission measure in another language. For example, Italian does not show an OI stage (it is null-subject). But it has a robust clitic omission pattern. (We are always speaking of children or impaired populations, e.g., SLI).[26] If we could find a language L in which the opposite is true: there is a robust OI stage but no clitic omission stage,

---

[26] For a review of data showing that clitic omission to an even greater age is a pervasive feature of SLI (for the languages that have participial agreement), see Wexler (2004b, 2014) and for very clear evidence (in Greek) that children with SLI in a language that does not show participial agreement do not omit clitics, see Manika, Varlokosta & Wexler (2011).

then we would predict that the genetic basis underlying clitic omission in Italian is the same as the genetic basis underlying the use of OIs in language L.

13. Conclusion

The clitic omission phenotype is very clear. In particular, the experimental methodology that establishes it is quite precise; the best method so far is to elicit clitics in semantic contexts that require them, that is in which there is a clear referential antecedent for the clitic. Even young children seem to know that object clitics are only possible in such contexts. Both finiteness and object clitics have a clear experimental paradigm that establishes their rates in obligatory contexts. Methodologically as well as theoretically, they make a good behavioral comparison for purposes of testing whether they are determined via the same genetic substrate.

One could think of many variations on the kinds of studies that I have suggested in this paper. The essential message is that once we have a computational endophenotype we can investigate the genetic basis for this endophenotype and make striking predictions. My own hunch—against the general direction of the field, which is to ignore the empirically and theoretically determined limitations on computational mechanisms during development, with enhanced limitations in certain impairments—is that paying serious experimental genetic attention to such phenotypes would be a major advantage in beginning the extremely important and intriguing study of the genetics of language, very much in Lenneberg's spirit.

References

Baek, Judy Yoo-Kyung & Wexler, Kenneth N. 2009. Acquisition of negation in Korean. In Chungmin Lee, Greg. B. Simpson & Youngjin Kim (eds.), *The Handbook of East Asian Psycholinguistics, Volume III: Korean*. 127–136. Cambridge, England: Cambridge University Press.

Bishop, Dorothy V. M., C.V. Adams, & Courtenay F. Norbury. 2006. Distinct genetic influences on grammar and phonological short-term memory deficits: Evidence from 6-year-old twins. *Genes, Brain & Behavior* 5, 158–169.

Borer, Hagit & Kenneth N. Wexler. 1987. The maturation of syntax. In Thomas Roeper & Edwin Williams (eds.), *Parameter Setting*. 123–172. Dordrecht, The Netherlands: Reidel.

Carey, Susan. 2010. The making of an abstract concept: Natural number. In Denis Mareschal, Paul Quinn & Stephen E. G. Lea (eds.), *The Making of Human Concepts*. Oxford, England: Oxford University Press. doi:10.1093/acprof:oso/9780199549221.003.013.

Chomsky, Noam. 2005. Three factors in language design. *Linguistic Inquiry* 36(1), 1–22.

de Villiers , Jill. 2007. The interface of language and Theory of Mind. *Lingua* 117(11), 1858–1878.

Falcaro, Milena, Andrew Pickles, D.F. Newbury, L. Addis, E. Banfield, Simon E. Fisher, A.P. Monaco, Z. Simkin, G. Conti-Ramsden, & The SLI Consortium. 2008. Genetic and phenotypic effects of phonological short-term memory and grammatical morphology in specific language impairment. *Genes, Brain & Behavior* 7, 393–402.

Fox, Danny & Martin Hackl. 2006. The universal density of measurement. *Linguistics and Philosophy* 29, 537–586.

Friederici, Angela D. 2017 [= this issue]. Neurobiology of syntax as the core of human language. *Biolinguistics*.

Ganger, Jennifer B. 1998. *Genes and Environment in Language Acquisition: A Study of Vocabulary and Syntactic Development in Twins*. Doctoral dissertation, Massachusetts Institute of Technology.

Ganger, Jennifer B., Kenneth N. Wexler & Melanie Soderstrom. 1997. The genetic basis for the development of tense: A preliminary report on a twin study. In Elizabeth Hughes, Mary Hughes & Annabel Greenhill (eds.), *BUCLD 21: Proceedings of the 21st Annual Boston University Conference on Language Development*. 224–234. Somerville, MA: Cascadilla Press.

Gavarró, Anna, Vicenç Torrens & Kennth N. Wexler. 2010. Object clitic omission: Two language types. *Language Acquisition* 17, 192–219.

Hirsch, Christopher & Kenneth N. Wexler. 2008. *Clefts, inverse copulas, and passives: Understanding their delayed acquisition*. Talk given at Generative Approaches to Language Acquisition North America Conference (GALANA), University of Connecticut, United States of America.

John, Bernard & Kenneth R. Lewis. 1966. Chromosome variability and geographic distribution in insects. *Science*. 152, 711–721.

Kapia, Enkeleida. 2011. *Acquiring clitic doubling: Syntax and pragmatics at work from early on*. Paper presented at Generative Approaches to Language Acquisition (GALA). Aristotle University of Thessaloniki, Greece.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Loeb, Diane F. & Laurence B. Leonard.1991. Subject case marking and verb morphology in normally developing and specifically language-impaired children. *Journal of Speech & Hearing Research* 34, 340–346.

Manika, Sophia, Spyridoula Varlokosta & Kenneth N. Wexler. 2011. The lack of omission of clitics in Greek children with SLI: An experimental study. In Nick Danis, Kate Mesh, & Hyunsuk Sung (eds.), *BUCLD 35: Proceedings of the 35th Annual Boston University Conference on Language Development*. 427-439. Somerville, MA: Cascadilla Press.

Perovic, Alexandra, & Kenneth N. Wexler. 2006. Knowledge of binding, raising and passives in Williams syndrome. In K. U. Deen, J. Nomura, B. Schulz, & B. D. Schwartz (eds.), *Proceedings of the Inaugural Conference on Generative Approaches to Language Acquisition–North America, Honolulu, HI* [University of Connecticut Occasional Papers in Linguistics, Vol. 4]. 273–284. Storrs, Connecticut: University of Connecticut.

Perovic, Alexandra & Kenneth N. Wexler. 2010. Development of verbal passive in Williams syndrome. *Journal of Speech, Hearing & Language Research* 53,1294–1306.

Perovic, Alexandra & Kenneth N. Wexler. 2014. Contrasting linguistic profiles of children with SLI and autism with and without language impairment. Talk presented at the International Association for the Study of Child Language, Amsterdam.

Pine, Julian M., Caroline F. Rowland, Elena V. M. Lieven & Anna L. Theakston. 2005. Testing the Agreement/Tense Omission Model: Why the data on children's use of non-nominative 3psg subjects count against the ATOM. *Journal of Child Language* 32, 269–289.

Poeppel, David & David Embick. 2005. Defining the relation between linguistics and neuroscience. In Anne Cutler (ed.), *Twenty-First Century Psycholinguistics: Four Cornerstones*. Mahwah, NJ: Lawrence Erlbaum.

Radeva-Bork, Teodora. 2012. *Single and Double Clitics in Adult and Child Grammar*. Frankfurt am Main, Germany: Peter Lang.

Redmond, Sean M. & Mabel L. Rice. 2001. Detection of irregular verb violations by children with and without SLI. *Journal of Speech, Language, & Hearing Research* 44, 665–669.

Rice, Mabel L. 2012. Toward epigenetic and gene regulation models of specific language impairment: Looking for links among growth, genes, and impairments. *Journal of Neurodevelopmental Disorders* 4(1), 27.

Rice, Mabel L., Lesa Hoffman & Kenneth N. Wexler. 2009. Judgments of omitted BE and DO in questions as extended finiteness clinical markers of specific language impairment (SLI) to 15 years: A study of growth and asymptote. *Journal of Speech, Language, & Hearing Research* 52(6), 1417–1433.

Rice, Mabel L. & Kenneth N. Wexler. 2001. *Rice/Wexler Test of Early Grammatical Impairment*. Psychological Corporation, San Antonio, TX.

Rice, Mabel L., Kenneth N. Wexler, & Patricia L. Cleave. 1995. Specific Language Impairment as a period of extended optional infinitive. *Journal of Speech, Language, & Hearing Research* 38(4), 850–863.

Rice, Mabel L., Kenneth N. Wexler & Scott Hershberger. 1998. Tense over time: The longitudinal course of tense acquisition in children with specific language impairment. *Journal of Speech, Language, & Hearing Research* 41, 1412–1431.

Rice, Mabel L., Kenneth N. Wexler, Janet Marquis, & Scott Hershberger. 2000. Acquisition of irregular past tense by children with SLI. *Journal of Speech, Language, & Hearing Research* 43, 1126–1145.

Rice, Mabel L., Kenneth N. Wexler & Shawn M. Redmond. 1999. Grammaticality judgments of an extended optional infinitive Grammar: Evidence from English-speaking children with specific language impairment. *Journal of Speech, Language, & Hearing Research* 42, 943–961.

Rizzi, Luigi. 1993. Some notes in linguistic theory and language development: The case of Root Infinitives. *Language Acquisition* 3, 371–393.

Saxe, Rebecca & Nancy Kanwisher. 2003. People thinking about thinking people: The role of the temporo-parietal junction in "theory of mind". *NeuroImage* 19, 1835–1842.

Schütze, Carson T. 1997. *INFL in Child and Adult Language: Agreement, Case and Licensing*. Doctoral dissertation, Massachusetts Institute of Technology, United States of America.

Schütze, Carson T. 2004. Why nonfinite be is not omitted while finite be is. In Alejna Brugos, Linnea Micciulla & Christine E. Smith (eds.), *Proceedings of the 28th Boston University Conference on Language Development*. 506–521. Somerville, MA: Cascadilla Press.

Schütze, Carson T. & Kenneth N. Wexler. 1996. Subject case licensing and English root infinitives. In Andy Stringfellow, Dalia Cahana-Amitay, Elizabeth Hughes & Andrea Zukowski (eds.), *Proceedings of the* 20^{th} *Boston University Conference on Language Development*. 670–681. Somerville, MA: Cascadilla Press.

Schütze, Carson T. & Kenneth N. Wexler. 2000. An elicitation study of young English children's knowledge of tense: Semantic and syntactic properties of optional infinitives. In S. Catherine Howell, Sarah A. Fish, and Thea Keith-Lucas (eds.), *Proceedings of the* 24^{th} *Boston University Conference on Language Development*. 669–683. Somerville, MA: Cascadilla Press.

Sigurjonsdottir, Sigridur. 1999. Root infinitives and null subjects in early Icelandic. In Annabel Greenhill, Heather Littlefield & Cheryl Tano (eds.), *Proceedings of the* 23^{rd} *Annual Boston University Conference on Language Development*. 630–641. Somerville, MA: Cascadilla Press.

Thordardottir, Elin. 2008. Language-specific effects of task demands on the manifestation of Specific Language Impairment: A comparison of English and Icelandic. *Journal of Speech, Language, & Hearing Research* 51(4), 914–921.

Tsakali, Vina & Kenneth N. Wexler. 2004. Why children omit clitics in some languages but not in others: New evidence from Greek. In Jaqueline van Kampen & Sergio Baauw (eds.), *Proceedings of GALA 2003 (Generative Approaches to Language Acquisition) held at Utrecht University 4-6 September 2003*. 493–504. Utrecht, The Netherlands: LOT.

Wexler, Kenneth N. 1990. *Optional Infinitives, Head Movement and the Economy of Derivations in Child Grammar*. Paper presented at the Annual Meeting of the Society of Cognitive Science, Massachusetts Institute of Technology, United States of America.

Wexler, Kenneth N. 1992. *Optional Infinitives, Head Movement and the Economy of Derivation in Child Grammar*. Occasional paper 45. Center for Cognitive Science, Massachusetts Institute of Technology, United States of America.

Wexler, Kenneth N. 1993. Optional infinitives, head movement and the economy of derivations. In David Lightfoot and Norbert Hornstein (eds.), *Verb Movement*. 305–350. Cambridge, England: Cambridge University Press.

Wexler, Kenneth N. 1996. The development of inflection in a biologically based theory of language acquisition. In Mabel L. Rice (ed.), *Toward a Genetics of Language* 113–144. Mahwah, NJ: Erlbaum.

Wexler, Kenneth N. 1998a.Very early parameter setting and the unique checking constraint: A new explanation of the optional infinitive stage. *Lingua* 106, 23–79.

Wexler, Kenneth N. 1998b. *The Unique Checking Constraint as the Explanation of Clitic Omission in SLI and Normal Development*. Unpublished Manuscript, Department of Linguistics & Philosophy, Massachusetts Institute of Technology, United States of America.

Wexler, Kenneth N. 2002. *The Theory of Clitics, a Parameter of Object Agreement, and the Extension of the Unique Checking Constraint to Massive Cross-Linguistic Differences in Object Clitic Omission: Evidence from Normal Development and SLI.* Paper presented at XXVII Incontro di Grammatica Generativa (28 February–2 March), Lecce, Italy.

Wexler, Kenneth N. 2003. Lenneberg's dream: Learning, normal language development and Specific Language Impairment. In Yonata Levi & Jeannette Schaeffer (eds.), *Language Competence across Populations: Toward a Definition of Specific Language Impairment*. 11–61. Mahwah, NJ: Lawrence Erlbaum.

Wexler, Kenneth N. 2004a. Theory of phasal development: Perfection in child grammar. *MIT Working Papers in Linguistics* 48, 159–209.

Wexler, Kenneth N. 2004b. Lenneberg's dream: Learning, normal language development and Specific Language Impairment. In Lyle Jenkins (ed.), *Variation and Universals in Biolinguistics*. 239–284. Amsterdam; Boston: Elsevier. [Reprinting with small changes of Wexler 2003.]

Wexler, Kenneth N. 2011. Grammatical computation in the optional infinitive stage. In Jill de Villiers & Tom Roeper (eds.), *Handbook of Generative Approaches to Language Acquisition* 53–118. Dordrecht, The Netherlands: Springer.

Wexler, Kenneth N. 2013a. Luria's biolinguistic suggestion and the growth of language. In Cedric Boeckx & Kleanthes K. Grohmann (eds.), *Cambridge Handbook of Biolinguistics*. 94–128. Cambridge, England: Cambridge University Press.

Wexler, Kenneth N. 2013b. Tough-movement developmental delay: Another effect of phasal computation. In Massimo Piattelli-Palmarini & Robert Berwick (eds.), *Rich Grammars from Small Inputs*. 146–167. Oxford, England: Oxford University Press.

Wexler, Kenneth N. 2014. The unique checking constraint as the explanation of clitic omission in normal and SLI development. In Theoni Neokleous & Kleanthes K. Grohmann (eds.), *Developments in the Acquisition of Clitics*. 288–344. Cambridge, England: Cambridge Scholars Publishing.

Wexler, Kenneth N., Anna Gavarró & Vicenç Torrens. 2004. Feature checking and object clitic omission in child Catalan and Spanish. In Reineke Bok-Bennema, Bart Hollebrandse, Brigitte Kampers-Manhe & Petra Sleeman (eds.), *Romance Languages and Linguistic Theory*. 253–269. Amsterdam, The Netherlands: John Benjamins.

Wexler, Kenneth N., Jeannette Schaeffer & Gerard Bol. 2004. Verbal syntax and morphology in typically developing Dutch children and children with SLI: How developmental data can play an important role in morphological theory. *Syntax* 7, 148–198.

Wimmer, Heinz,& Josef Perner. 1983. Beliefs about beliefs: Representation and constraining function of wrong beliefs in young children's understanding of deception. *Cognition* 13, 103–128.

Kenneth N. Wexler
Massachusetts Institute of Technology
Department of Brain and Cognitive Sciences &
Department of Linguistics and Philosophy
77 Massachusetts Avenue, 32-D808
Cambridge, MA 02139
United States of America

wexler@mit.edu

Neurobiology of Syntax as the Core of Human Language

Angela D. Friederici

1. Introduction

The human language capacity appears to be rooted in the ability to combine words into hierarchical structures making up phrases and sentences. There is substantial evidence that this ability is specific to humans. Other animals can use words or symbols to refer to objects and actions, and can even memorise sequences of syllables and symbols, but only humans create syntactic hierarchies to build up phrases and sentences. In humans syntactic rules and representations together with words constitute the basis of the language system which allows the construction of sentences that carry and convey meaning. The present article focuses on syntax as the hierarchy building component which is unique to humans and thought to be part of their neurobiological endowment (Friederici et al. 2017).

This view was already formulated about 50 years ago by Erich Lenneberg (1967) in *Biological Foundations of Language*. He claimed that there must be an innate biological representation of the abstract structure of language in the human nervous system, and that language was characterised by "concatenations" which obey syntactic principles. Both claims have found supportive evidence in the past 50 years. While Lenneberg formulated his views mainly on the basis of behavioural language data from patients with brain lesions, today's knowledge is based on data from functional brain imaging, measurements of the grey and white matter structures of the living brain as well the correlation of these with behavioural language measures.

2. The Computation Merge: Broca's Area

These days the syntactic principle to which all languages adhere can be described according to Noam Chomsky (Chomsky 1995; Berwick et al. 2013) as a most basic computation, called Merge. Merge is a universal language-specific combinatorial operation that takes two syntactic objects to create a new one. For example, it takes two words (a determiner *the* and a noun *ship* to create a determiner phrase *the ship*, or it takes a determiner phrase *the ship* and a verb *sinks* to create a sentence *the ship sinks*. Crucially, Merge is a recursive operation allowing the generation of the full range of hierarchical structure that is characteristic of human language distinguishing it from other human and non-human cognitive systems (Chomsky et al. 1982, Bolhuis et al. 2014).

At this point two questions arise: What kind of evidence can we find to support the claim that the syntactic operation Merge is grounded in the human brain

biolinguistics
© The Author (2017)
ISSN 1450–3417
Biolinguistics 11.SI: 325–337, 2017
http://www.biolinguistics.eu

and what is the brain basis for the ability to deal with sentence structures beyond a single Merge operation? It is clear that the generation of a sentence like *the ship sinks* requires not only the neural representation of the syntactic operation Merge but, moreover, a set of words. Together, syntax and a set of words make up the basis of the language system. In a basic model of language this language system is connected to the external world and to the internal mental world by two interface systems. The former system called external sensory-motor interface supports perception and production of speech and the latter system named internal conceptual-intentional interface relates to concepts and intentions (Berwick et al. 2013). Clear neurobiological evidence for the internal conceptual-intentional interface system is still missing. For the external interface system, however, there is ample evidence as it relates to the respective modalities in which language is realized: the auditory-articulatory system for spoken language and the visual-gesture related motor system for sign language (Levelt 1989, Guenther 2016, Zatorre et al. 1992, Emmorey et al. 2003, Petitto et al. 2001).

Here I will mainly focus on the language system and its neural representation. The language system consisting of syntax and lexical items is mainly represented in the perisylvian cortex of the left hemisphere involving the inferior frontal and temporal cortex. These brain regions are connected by white matter fibre tracts constituting dorsal and ventral pathways (see figure 1) that allow the transmission of the information from one region to the next.[1] The brain areas which are connected by the ventral pathway are involved in processing words and semantic information (Binder et al. 2009, Patterson et al. 2007, Thompson-Schill et al. 1997, Newman et al. 2010, Weiller et al. 2009, 2011, Turken & Dronkers 2011). The processing of syntactic information, in contrast, is related to the dorsal pathway that targets BA 44 in Broca's area. Note that the figure displays two dorsal fibre tracts. One is terminating in BA 44 and discussed in detail here. A second fibre tract terminating in the premotor cortex (PMC) is involved in sensory-motor mapping and assumed to be essential for the sensory-motor interface system briefly mentioned above, but not discussed further in this article.

Since we learned that single words as such do not make up language, I will not report neuroscientific studies on the processing of single words, but only discuss those studies in which words become part of a hierarchical structure.

Traditionally, the processing of syntax has been investigated in the context of sentences with varying syntactic complexity (for a review see Zaccarella & Friederici 2015a). These studies systematically reported Broca's area in the left inferior frontal gyrus to support syntactic processes. Another approach chose to compare the processing of a 'possible' language that followed natural grammar rules to the processing of an 'impossible' language that did not follow such rules. Only the processing of 'possible' languages activated Broca's area, whereas the processing of languages which did not follow natural language rules did not (Musso et al. 2003, Tettamanti et al. 2002). Interestingly, the human brain uses Broca's area, in particular its posterior part BA 44, to process syntactic rules even in sequences that follow natural

---

[1] The identification of fibre tracts is based on diffusion tensor imaging (DTI) allowing the measurement of the fractional anisotropy reflecting among other parameters the myelination in white matter which surrounds the fibre. It serves as an electrically isolating layer surrounding the fibre thereby increasing the propagation speed of the electrical signal and thus the information transfer between neurons and neuronal ensembles (Turner 2015).

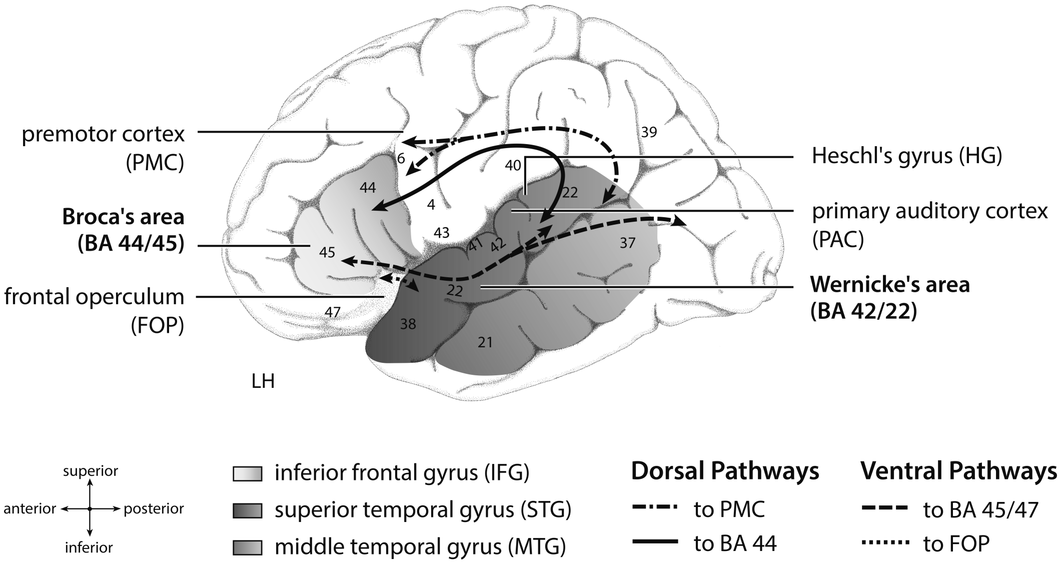


Figure 1: ***Neuroanatomy of language****. Anatomical details of the left hemisphere (LH). Top: Major language relevant gyri (inferior frontal gyrus (IFG), superior temporal gyrus (STG), middle temporal gyrus (MTG)) are shaded in grey. Numbers indicate language-relevant Brodmann Areas (BA) which Brodmann (1909) defined on the basis of cytoarchitectonic characteristics. The vertical coordinate labelled (see bottom left) superior/inferior indicates the position of the gyrus within a lobe or within a BA. The horizontal coordinate labelled anterior/posterior indicates the position within a gyrus. Broca's area consists of a posterior part (BA 44) and an anterior part (BA 45). Located anterior to Broca's area is area BA 47. The frontal operculum (FOP) is located ventrally and medially to BA 44, BA 45. The premotor cortex (PMC) is located in BA 6. Wernicke's area is defined as BA 42 and BA 22. The primary auditory cortex (PAC) and Heschl's gyrus (HG) are located in a lateral to medial orientation in the temporal lobe. White matter fibre tracts, i.e. the dorsal and ventral pathways connecting the language-relevant brain regions, are indicated by arrows. There are two dorsal and two ventral fibre tracts with respective different termination regions. (Adapted from Friederici 2011.)*

grammar rules, even when the 'words' are not real (Opitz & Friederici 2003, 2007, Goucha & Friederici 2015). These data provide suggestive evidence that BA 44 as part of Broca's area (see figure 1) is responsible for the processing of syntax in sentences as well as in sentence-like sequences.

The question remains, however, whether this brain region should be viewed as the neural basis of the most basic linguistic operation Merge. It has been reasoned that if the neural substrate of Merge is the same independent of the number of recursive applications, the single application of Merge should also recruit BA 44 as part of Broca's area. Thus this brain region should be activated not only for the processing of sentences, but also for a single Merge operation. A recent study was able to provide support for this assumption. This study investigated the computation Merge of a determiner phrase using a semantic-free determiner (*the*) and a semantic-free noun (*bish*) in an fMRI experiment and found activation in the most ventral portion of BA 44 (Zaccarella & Friederici 2015b). This stood in clear contrast to the processing of two-word sequences without-syntactic hierarchy (*cloud*, *pish*) which activated the frontal operculum/anterior insula (Zaccarella & Friederici 2015c)—a phylogenetically older brain region than BA 44 itself (Sanides

1962, Friederici 2004, Amunts & Zilles 2012). These results suggest that the processing of syntactic hierarchy selectively involves a phylogenetically more recent cortical region, namely BA 44, independent of the number of recursive applications.

3. Beyond Merge: The Neural Syntactic Network

Although responsible for the syntactic operation Merge, Broca's area is not the only player when it comes to processing sentences. There is ample neuroscientific evidence that Broca's area in the inferior frontal gyrus and Wernicke's area in the posterior superior temporal cortex together constitute a fronto-temporal network that serves sentence comprehension (Friederici 2011, see figure 1). Within this network, BA 44 as the posterior part of Broca's area is responsible for syntactic processes whereas the posterior temporal cortex appears to support the integration of semantic and syntactic information (Friederici et al. 2009, den Ouden et al. 2012, Makuuchi & Friederici 2013, Ding et al. 2015). This conclusion is based on the observation that posterior temporal cortex is seen inactive when artificial grammar sequences lacking semantic information are processed (Friederici et al. 2006) but active when natural sentences are processed (for a review see Friederici 2011). It has been proposed that the posterior temporal cortex particularly comes into play for thematic role assignment, crucial for sentence comprehension (Bornkessel et al. 2005). In addition functional connectivity analyses observing a coactivation of Broca's area and the posterior temporal cortex revealed that these two regions work closely together when sentences are processed (den Ouden et al. 2012, Makuuchi & Friederici 2013).

Structurally, these two brain regions are connected by a white matter fibre tract relating the posterior temporal cortex and BA 44 in Broca's area via the arcuate fascicle and the superior longitudinal fascicle (Catani et al. 2005, Anwander et al. 2007). Empirical data from patients with deficiencies of this fibre tract (Wilson et al. 2010) and from young children in whom this fibre tract is still immature (Skeide et al. 2016) indicate that this dorsally located fibre tract is crucial for the processing of syntactically complex sentences. During development the function of this dorsal fibre tract becomes particularly obvious. It was shown that children's behavioural performance on processing syntactically complex sentences improves as the strength of this fibre tract increases (Skeide et al. 2016). The strength of a fibre tract is indicated by the status of the myelination of the fibres which in turn is essential for the transmission of electrical impulses to be sent from one brain region to another (Wake et al. 2011, Nave & Werner 2014).

This dorsal fibre tract is not yet myelinated at birth (Perani et al. 2011) and only matures slowly throughout childhood (Skeide et al. 2016), reaching its adult stage after puberty. This developmental trajectory is interesting in the context of Lenneberg's (1967) claim of a critical period of language acquisition whose window is thought to close in early puberty. He already drew suggestive parallels between the time course of language acquisition and the maturation of certain features of the human brain. Today we know that the maturation of the white matter of the dorsal fibre tract predicts processing of syntactically complex sentences (Skeide et al. 2016). Moreover, there is also evidence that the maturation of the grey matter is crucial for language development. It has been shown that performance on syntactically complex sentences during development is predicted by the maturation of the grey matter of BA 44 in particular, and the posterior temporal cortex (Fengler et al. 2016). These findings advance the view that the dorsal fibre tract together with its termination regions, namely BA 44 in Broca's area and the posterior temporal cortex, constitute the neural basis of the human syntactic capacity.

4. Comparing Human and Non-Human Primates

When considering syntax as a unique human ability, a comparison between human and non-human primates can add important aspects. Central to the discussion on sequence processing in human and non-human primates is not whether sequences can be learned, but rather what type of syntactic sequence can be learned. In this context a fundamental distinction is made between two grammar types, namely finite state grammars following an $(AB)^n$ rule and phrase structure grammars following an A^nB^n rule (Hauser et al. 2002, Fitch & Hauser 2004). The important difference between these two types of grammars is that sequences based on the $(AB)^n$ rule contain adjacent dependencies between an A-element and a B-element, whereas sequences based on the A^nB^n lead to non-adjacent dependencies. While it is open whether non-adjacent dependencies in artificial grammars necessitate the build up of hierarchies, it is clear that non-adjacent dependencies in a natural grammar require the build-up of syntactic hierarchies which is guaranteed by the computation Merge (Chomsky 1995).[2]

Fitch & Hauser (2004) were the first to investigate artificial grammar learning in human and non-human primates using such a finite state grammar $(AB)^n$ and phrase structure grammar (A^nB^n). Testing cotton-top tamarins and human adults in a behavioural grammar learning study, they found that humans could learn both grammar types easily, whereas monkeys were only able to learn the finite state grammar with its adjacent dependencies. More recently it has been shown that macaques can even learn non-adjacent dependencies in auditory syllable sequences of the $A \times B$ type (Milne et al. 2016). But note that the recognition of the dependency between the A-element and the B-element in such sequences does not necessarily require hierarchy building. Thus it appears that a crucial difference between human and non-human primates lies in the ability to process syntactic hierarchies.

Concerning the evolutionary aspect of a recent study investigated artificial grammar learning in two types of monkeys who differ in their evolutionary distance to humans: marmosets with a further distance and macaques with a closer distance to humans (Wilson et al. 2013). In the study both species had to learn an artificial grammar with non-deterministic word transitions. Marmosets showed sensitivity to simple violations in the sequence, whereas macaques showed sensitivity to violations of a higher complexity. This suggests an evolutionary interesting result with monkeys, namely that those that are closer relatives to us demonstrate a more advanced artificial grammar processing ability than those that are more distant.

---

[2] It has been claimed, however, that A^nB^n artificial grammar sequences can in principle be processed by simpler cognitive processes such as counting and memorising. For a detailed discussion see Friederici 2017.

Human and non-human primates clearly differ in their abilities to process complex rule-based sequences. And so far there is no evidence that any other species except humans can process hierarchically structured sequences as they appear in syntactic structures of natural languages. This is interesting as the genetic difference between human and non-human primates is less than 2 % (The Chimpanzee Sequencing and Analysis Consortium 2005, Scally et al. 2012, Meyer et al. 2012), but there are differences in the basic neuroanatomy. These differences may be crucial and, therefore, deserve a closer look, both with respect to brain structure and brain function. A focused across-species look at the language-related brain structures as defined for humans may be of special interest here. These language-related brain regions in humans—as discussed above—are the inferior frontal gyrus and the posterior temporal cortex.

In humans, the language network with its posterior temporal region and Broca's area is lateralized to the left hemisphere. Neuroanatomically, it has long been reported that in the human brain the posterior temporal cortex is larger in the left than in the right hemisphere (Witelson 1982). For the planum temporale, a region that lies posterior to Heschl's gyrus and encompasses Wernicke's area, which has long been identified to support speech and language processing, a hemispheric asymmetry is consistently reported for humans (Geschwind & Levitsky 1968, Steinmetz et al. 1989, Watkins et al. 2001). It was shown in a recent meta-analysis that the anatomical asymmetry of the posterior temporal cortex is necessary for optimal verbal performances (Tzourio-Mazoyer & Mazoyer 2017). A cross-species comparison involving chimpanzees and three other non-human primate species, including macaques, focused on the grey matter asymmetry of the planum temporale. Analyses revealed that only chimpanzees as our closest relatives demonstrate an asymmetry of the planum temporale similar to humans (Lyn et al. 2011). The other crucial language-related brain region, Broca's area, known to be essentially involved in the human ability to process syntax, also deserves a detailed neuroanatomical evaluation. It has been demonstrated that a leftward asymmetry of Broca's area evidenced by a cytoarchitectonic analysis exists in the adult brain (Amunts et al. 2003). No such asymmetry can be found in the homologue of Broca's area in adult chimpanzees (Schenker et al. 2010). The observed neurobiological difference of these brain regions between the human and the non-human primate may be viewed as a crucial parameter for the evolution of language.

Moreover, the white matter connections between these brain regions should be of particular interest as they guarantee the information transfer between regions. There are a number of structural imaging studies on long-range white matter connections in macaques, chimpanzees, and humans which suggest interesting differences between human and non-human primates (Catani et al. 2002, Anwander et al. 2007, Rilling et al. 2008, Saur et al. 2008, Makris & Pandya 2009, Petrides & Pandya 2009). These studies indicate differences in the strength of the fibre bundles connecting the frontal and temporal regions known to be involved in language processing in humans. In these studies two major white matter pathways were analysed: the dorsal pathway connecting Broca's area to the posterior superior temporal gyrus/superior temporal sulcus and the ventral pathway connecting the most ventral part of the frontal cortex to the temporal cortex (Catani et al. 2005, Rilling et al. 2008). In humans this dorsal pathway is much stronger than in non-human primates. A direct comparison revealed that macaques and chimpanzees display a weak dorsal pathway but a strong ventral pathway, whereas humans display a strong dorsal pathway and a weaker though well-developed ventral pathway. The dorsal pathway was therefore discussed as the crucial pathway for the language ability in humans (Rilling et al. 2008; see also Rilling et al. 2012).

The difference in the strength of these fibre tracts is of particular interest in light of a combined functional and structural imaging study in humans (Friederici et al. 2006) which investigated that processing of artificial grammar types similar to those used in the behavioural study by Fitch & Hauser (2004). In humans, processing the $(AB)^n$ grammar, with its adjacent dependencies, activated the frontal operculum, whereas processing the more complex A^nB^n grammar, however, additionally recruited the phylogenetically younger Broca's area (Friederici et al. 2006). Furthermore, the structural imaging analyses conducted in this study found that the frontal operculum processing the $(AB)^n$ grammar was connected to the temporal cortex via a ventral pathway, whereas the posterior part of Broca's area computing the A^nB^n grammar was connected to the posterior temporal cortex via a dorsal pathway (Friederici et al. 2006). These data were taken to suggest that the posterior part of Broca's area (BA 44) and its dorsal connection to the temporal cortex, in particular, support the processing of higher-order hierarchically structured sequences relevant to language.[3]

5. Conclusion

The present review on neuroscientific studies of syntax processing revealed an intriguing overlap concerning the brain basis of syntactic processes. They culminate in the view that the human-specific ability to build syntactic hierarchies is neurobiologically anchored in BA 44 as part of Broca's area and the dorsally-located fibre tract connecting this brain region to the posterior temporal cortex. The empirical evidence for this view comes from neurofunctional and neuroanatomical observations in adult humans, in developing children and from cross-species comparisons of human and non-human primates.

The data show that within the language domain the posterior part of Broca's area, BA 44 is functionally unique in its involvement in the basic syntactic operation Merge. It can be functionally differentiated from BA 45 as the more anterior part of Broca's area involved in semantic processes and from the more ventrally located frontal operculum responsible for simple combinatory processes without building a syntactic hierarchy. Phylogenetically, BA 44 appears to be a more recent brain region than the frontal operculum, and this more recently evolved BA 44 reveals a structural difference between human and non-human primates (Sanides 1962, Friederici 2004, Amunts & Zilles 2012). Only in humans is BA 44 larger in the left than in the right hemisphere. Moreover, there are structural cross-species differences with respect to BA 44's connectivity in the larger language network. In

---

[3] There is an ongoing debate whether Broca's area is involved in other cognitive processes. Broca's area has been shown to be activated during the processing of syntactic structure and hierarchies in music (Maess et al. 2001, Koelsch et al. 2013) and in mathematics (Makuuchi, Bahlmann & Friederici 2012). I have discussed this in several other publications (Friederici 2002, Jeon & Friederici 2013, Goucha, Zaccarella & Friederici, in press).

humans, BA 44 in Broca's area is connected via a dorsal fibre tract to the posterior temporal cortex including Wernicke's area, and its integrity is related to the ability to process syntax. This dorsal fibre tract is strong in human primates, but weak in non-human primates.

These observations support the view that BA 44 in the posterior part of Broca's area and its white matter connection to the temporal cortex is fundamental for the human language faculty with syntax as its core.

References

Amunts, Katrin & Karl Zilles. 2012. Architecture and organizational principles of Broca's region. *Trends in Cognitive Sciences* 16(8), 418–426.

Amunts, Katrin, Axel Schleicher, Annette Ditterich & Karl Zilles. 2003. Broca's region: Cytoarchitectonic asymmetry and developmental changes. *Journal of Comparative Neurology* 465(1), 72–89.

Anwander, Alfred, Marc Tittgemeyer, Detlef Yves von Cramon, Angela D. Friederici & Thomas R. Knösche. 2007. Connectivity-based parcellation of Broca's area. *Cerebral Cortex* 17(4), 816–825.

Berwick, Robert C., Angela D. Friederici, Noam Chomsky & Johan J. Bolhuis. 2013. Evolution, brain and the nature of language. *Trends in Cognitive Sciences* 17(2), 89–98.

Binder, Jeffrey R., Rutvik H. Desai, William W. Graves & Lisa L. Conant. 2009. Where is the semantic system? A critical review and meta-analysis of 120 functional neuroimaging studies. *Cerebral Cortex* 19, 2767–2796.

Bolhuis, Johan J., Ian Tattersall, Noam Chomsky & Robert C. Berwick. 2014. How could language have evolved? *Trends in PLoS Biology* 12, e1001934.

Bornkessel, Ina, Stefan Zyssett, Angela D. Friederici, D. Yves von Cramon & Matthias Schlesewsky. 2005. Who did what to whom? The neural basis of argument hierarchies during language comprehension. *NeuroImage* 26, 221–233.

Brodmann, Korbinian. 1909. Beiträge zur histologischen Lokalisation der Grosshirnrinde: VI. Die Cortexgliederung des Menschen. *Journal für Psychologie und Neurologie* 10, 231–246.

Catani, Marco, Robert J. Howard, Sinisa Pajevic & Derek K. Jones. 2002. Virtual in vivo interactive dissection of white matter fasciculi in the human brain. *NeuroImage* 17(1), 77–94.

Catani, Marco, Derek K. Jones & Dominic H. Ffytche. 2005. Perisylvian language networks of the human brain. *Annals of Neurology* 57(1), 8–16.

Chomsky, Noam. 1995. *The Minimalist Program*. Cambridge, MA: MIT Press.

Chomsky, Noam, Riny Huybregts & Henk C. van Riemsdijk. 1982. *Noam Chomsky on the Generative Enterprise*. Dordrecht: Foris.

den Ouden, Dirk-Bart, Dorothee Saur, Wolfgang Mader, Bjoern Schelter, Sladjana Lukic, Eisha Wali, Jens Timmer & Cynthia K. Thompson. 2012. Network modulation during complex syntactic processing. *NeuroImage* 59, 815–823.

Ding, Nai, Lucia Melloni, Hang Zhang, Xing Tian & David Poeppel. 2015. Cortical tracking of hierarchical linguistic structures in connected speech. *Nature Neuroscience* 19, 158–164.

Emmorey, Karen, John S. Allen, Joel Bruss, Natalie Schenker & Hanna Damasio. 2003. A morphornetric analysis of auditory brain regions in congenitally deaf adults. *Proceedings of the National Academy of Sciences of the United States of America* 100, 10049–10054.

Fengler, Anja, Lars Meyer & Angela D. Friederici. 2016. How the brain attunes to sentence processing: Relating behavior, structure, and function. *NeuroImage* 129, 268–278.

Fitch, W. Tecumseh & Marc D. Hauser. 2004. Computational constraints on syntactic processing in a nonhuman primate. *Science* 303(5656), 377–380.

Friederici, Angela D. 2002. Towards a neural basis of auditory sentence processing. *Trends in Cognitive Sciences* 6, 78–84.

Friederici, Angela D. 2004. Processing local transitions versus long-distance syntactic hierarchies. *Trends in Cognitive Sciences* 8(6), 245–247.

Friederici, Angela D. 2011. The brain basis of language processing: From structure to function. *Physiological Reviews* 91, 1357–1392. doi:10.1152/physrev.00006.2011.

Friederici, Angela D. 2017. Evolution of the neural language network. *Psychonomic Bulletin and Review* 24(1), 41–47.

Friederici, Angela D., Jörg Bahlmann, Stefan Heim, Riccardo I. Schubotz & Alfred Anwander. 2006. The brain differentiates human and non-human grammars: Functional localization and structural connectivity. *Proceedings of the National Academy of Sciences of the United States of America* 103(7), 2458–2463.

Friederici, Angela D., Michiru Makuuchi & Jörg Bahlmann 2009. The role of the posterior superior temporal cortex in sentence comprehension. *Neuroreport*, 20(6), 563–568.

Friederici, Angela D., Noam Chomsky, Robert C. Berwick, Andrea Moro, & Johan J. Bolhuis. 2017. Language, mind and brain. *Nature Human Behaviour* 1, 713–722. doi:10.1038/s41562-017-0184-4.

Geschwind, Norman & Walter Levitsky. 1968. Human brain: left-right asymmetries in temporal speech region. *Science* 161(3837), 186–187.

Goucha, Tomás B. & Angela D. Friederici. 2015. The language skeleton after dissecting meaning: a functional segregation within Broca's area. *NeuroImage* 114(6), 294–302.

Goucha, Tomás, Emiliano Zaccarella & Angela D. Friederici. In press. A revival of the Homo loquens as a builder of labeled structures: neurocognitive considerations. *Neuroscience & Biobehavioral Reviews*. doi:10.1016/j.neubiorev.2017.01.036.

Guenther, Frank H. 2016. *Neural Control of Speech*. Cambridge, MA: MIT Press

Hauser, Marc D., Noam Chomsky & W. Tecumseh Fitch. 2002. The faculty of language: what is it, who has it, and how did it evolve? *Science* 298(5598), 1569–1579.

Jeon, Hyeon-Ae & Angela D. Friederici. 2013. Two principles of organization in the prefrontal cortex are cognitive hierarchy and degree of automaticity. *Nature Communications*, 4. doi:10.1038/ncomms3041.

Koelsch, Stefan, Martin Rohrmeier, Renzo Torrecuso & Sebastian Jentschke. 2013. Processing of hierarchical syntactic structure in music. *Proceedings of the National Academy of Sciences of the United States of America* 110, 15443–15448.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Levelt, William J.M. 1989. *Speaking: From intention to articulation*. Cambridge, MA: MIT Press.

Lyn, Heidi, Peter Pierre, Allyson J. Bennett, Scott Fears, Roger Woods & William D. Hopkins. 2011. Planum temporale grey matter asymmetries in chimpanzees (Pan troglodytes), vervet (Chlorocebus aethiops sabaeus), rhesus (Macaca mulatta) and bonnet (Macaca radiata) monkeys. *Neuropsychologia* 49(7), 2004–2012.

Maess, Burkhard, Stefan Koelsch, Thomas C. Gunter & Angela D. Friederici. 2001. Musical syntax is processed in Broca's area: An MEG study. *Nature Neuroscience* 4, 540–545.

Makris, Nikos & Deepak N. Pandya. 2009. The extreme capsule in humans and rethinking of the language circuitry. *Brain Structure and Function*N 213(3): 343–358.

Makuuchi, Michiru, Jörg Bahlmann & Angela D. Friederici, A.D. 2012. An approach to separating the levels of hierarchical structure building in language and mathematics. *Philosophical Transactions Of The Royal Society Of London. Series B, Biological Sciences* 367, 2033–2045.

Makuuchi, Michiru & Angela D. Friederici. 2013. Hierarchical functional connectivity between the core language system and the working memory system. *Cortex* 49, 2416–2423.

Meyer, Matthias, Martin Kircher, Marie-Theres Gansauge, Heng Li, Fernando Racimo, Swapan Mallick, et al. 2012. A high-coverage genome sequence from an archaic denisovan individual. *Science* 338, 222–226.

Milne, Alice E., Jutta L. Mueller, Claudia Männel, Adam Attaheri, Angela D. Friederici & Christopher I. Petkov. 2016. Evolutionary origins of non-adjacent sequence processing in primate brain potentials. *Scientific Reports* 6, 36259. doi:10.1038/ srep36259.

Musso, Mariacristina, Andrea Moro, Volkmar Glauche, Michel Rijntjes, Jürgen Reichenbach, Christian Büchel & Cornelius Weiller, C. 2003. Broca's area and the language instinct. *Nature Neuroscience* 6(7), 774–781.

Nave, Klaus-Armin & Hauke B. Werner. 2014. Myelination of the nervous system: mechanisms and functions. *Annual Review of Cell and Developmental Biology* 30, 503–533.

Newman, Sharlene D., Toshikazu Ikuta & Thomas, Jr. Burns. 2010. The effect of semantic relatedness on syntactic analysis: An fMRI study. *Brain & Language* 113(2), 51–58.

Opitz, Bertram & Angela D. Friederici. 2003. Interactions of the hippocampal system and the prefrontal cortex in learning language-like rules. *NeuroImage* 19, 1730–1737.

Opitz, Bertram & Angela D. Friederici. 2007. Neural basis of processing sequential and hierarchical syntactic structures. *Human Brain Mapping* 28, 585–592.

Perani, Daniela, Maria C. Saccuman, Paola Scifo, Alfred Anwander, Danilo Spada, Cristina Baldoli, Antonella Poloniato, Gabriele Lohmann & Angela D. Friederici. 2011. The neural language networks at birth. *Proceedings of the National Academy of Sciences of the United States of America* 108(38), 16056–16061.

Patterson, Karalyn, Peter J. Nestor & Timothy T. Rogers. 2007. Where do you know what you know? The representation of semantic knowledge in the human brain. *Nature Reviews Neuroscience* 8, 976–987.

Petitto, Laura Ann, Siobhan Holowka, Lauren E. Sergio & David Ostry. 2001. Language rhythms in baby hand movements. *Nature* 413, 35–36.

Petrides, Michael & Deepak N. Pandya. 2009. Distinct parietal and temporal pathways to the homologues of Broca's area in the monkey. *PLoS Biology* 7(8): e1000170. doi:10.1371/ journal.pbio.1000170.

Rilling, James K., Matthew F. Glasser, Saad Jbabdi, Jesper Andersson & Todd M. Preuss. 2012. Continuity, divergence, and the evolution of brain language pathways. *Frontiers in Evolutionary Neuroscience* 3:11. doi:10.3389/ fnevo.2011. 00011.

Rilling, James K., Matthew F. Glasser, Todd M. Preuss, Xiangyang Ma, Tiejun Zhao, Xiaoping Hu & Timothy E. J. Behrens. 2008. The evolution of the arcuate fasciculus revealed with comparative DTI. *Nature Neuroscience* 11(4), 426–428.

Sanides, Friedrich. 1962. The architecture of the human frontal lobe and the relation to its functional differentiation. *International Journal of Neurology* 5, 247–261.

Saur, Dorothee, Björn W. Kreher, Susanne Schnell, Dorothee Kümmerer, Philipp Kellmeyer, Magnus-Sebastian Vry, Roza Umarova, Mariacristina Musso, Volkmar Glauche, Stefanie Abel, Walter Huber, Michel Rijntjes, Jürgen Hennig & Cornelius Weiller. 2008. Ventral and dorsal pathways for language. *Proceedings of the National Academy of Sciences of the United States of America* 105(46), 18035–18040.

Scally, Aylwyn, Julien Y. Dutheil, LaDeana W. Hillier, Gregory E. Jordan, Ian Goodhead, Javier Herrero, et al. 2012. Insights into hominid evolution from the gorilla genome sequence. *Nature*, 483, 169–175.

Schenker, Natalie M., William D. Hopkins, Muhammad A. Spocter, Amy R. Garrison, Cheryl D. Stimpson, Joseph M. Erwin, Patrick R. Hof & Cher C. Sherwood. 2010. Broca's area homologue in chimpanzees (pan troglodytes): Probabilistic mapping, asymmetry, and comparison to humans. *Cerebral Cortex* 20(3), 730–742.

Skeide, Michael A., Jens Brauer & Angela D. Friederici. 2016. Brain functional and structural predictors of language performance. *Cerebral Cortex* 26, 2127–2139.

Steinmetz, Helmuth, J—"org Rademacher, Yanxiong Huang, Harald Hefter, Karl Zilles, Armin Thron & Hans-Joachim Freund. 1989. Cerebral asymmetry - MR planimetry of the human planum temporale. *Journal of Computer Assisted Tomography* 13(6), 996–1005.

Tettamanti, Marco, Hatem Alkadhi, Andrea Moro, Daniela Perani, Spyros Kollias & Dorothea Weniger. 2002. Neural correlates for the acquisition of natural language syntax. *NeuroImage* 17, 700–709.

The Chimpanzee Sequencing and Analysis Consortium. 2005. Initial sequence of the chimpanzee genome and comparison with the human genome. *Nature* 437, 69–87.

Thompson-Schill, Sharon L., Mark D'Esposito, Geoffrey K. Aguirre & Martha J. Farahs. 1997. Role of left inferior prefrontal cortex in retrieval of semantic knowledge: A reevaluation. *Proceedings of the National Academy of Sciences of the United States of America* 94, 14792–14797.

Turner, Robert. 2015. Myelin imaging. In Arthur W. Toga (ed.), *Brain Mapping: An*

Encyclopedic Reference, Volume 1, 137–142. Elsevier.

Turken, And U. & Nina F. Dronkers. 2011. The neural architecture of the language comprehension network: converging evidence from lesion and connectivity analyses. *Frontiers in Systems Neuroscience* 5:1. doi:10.3389/ fnsys.2011.00001.

Tzourio-Mazoyer, Nathalie & Bernard Mazoyer. 2017. Variations of planum temporale asymmetries with Heschl's Gyri duplications and association with cognitive abilities: MRI investigation of 428 healthy volunteers. *Brain Structure and Function*. doi:10.1007/ s00429-017-1367-5.

Wake, Hiroaki, Philip R. Lee & Douglas Fields. 2011. Control of local protein synthesis and initial events in myelination by action potentials. *Science* 333(6049), 1647–1651.

Watkins, Kate E., Tomas Paus, Jason P. Lerch, Alex Zijdenbos, D. Louis Collins, Peter Neelin, John C. Taylor, Keith J. Worsley & Alan C. Evans. 2001. Structural asymmetries in the human brain: a voxel-based statistical analysis of 142 MRI scans. *Cerebral Cortex* 11(9) 868–877.

Weiller, Cornelius, Mariachristina Musso, Michel Rijntjes & Dorothee Saur. 2009. Please don't underestimate the ventral pathway in language. *Trends in Cognitive Sciences* 13(9), 369–370.

Weiller, Cornelius, Tobias Bormann, Dorothee Saur, Mariachristina Musso & Michel Rijntjes. 2011. How the ventral pathway got lost: and what its recovery might mean. *Brain and Language* 118(1–2), 29–39.

Wilson, Benjamin, Heather Slater, Yukiko Kikuchi, Alice E. Milne, William D. Marslen-Wilson, Kenny Smith & Christopher I. Petkov. 2013. Auditory artificial grammar learning in macaque and marmoset monkeys. *Journal of Neuroscience* 33(48), 18825–18835.

Wilson, Stephen M., Nina F. Dronkers, Jennifer M. Ogar, Jung Jang, Matthew E. Growdon, Federica Agosta, Maya L. Henry, Bruce L. Miller & Maria Luisa Gorno-Tempini. 2010. Neural correlates of syntactic processing in the nonfluent variant of primary progressive aphasia. *Journal of Neuroscience* 30(50), 16845–16854.

Witelson, Sandra F. 1982. Hemisphere specialization from birth. *International Journal of Neuroscience* 17(1), 54–55.

Zaccarella, Emiliano & Angela D. Friederici. 2015a. Syntax in the Brain. In Arthur W. Toga (ed.), *Brain Mapping: An Encyclopedic Reference*, Volume 3, 461–468. Waltham: Academic Press.

Zaccarella, Emiliano & Angela D. Friederici. 2015b. Merge in the human brain: a sub-region based functional investigation in the left pars opercularis. *Frontiers in Psychology* 6, 1818.

Zaccarella, Emiliano & Angela D. Friederici. 2015c. Reflections of word processing in the insular cortex: A sub-regional parcellation based functional assessment. *Brain and Language* 142, 1–7.

Zatorre, Robert J., Alan C. Evans, Ernst Meyer & Albert Gjedde. 1992. Lateralization of phonetic and pitch discrimination in speech processing. *Science*, 256, 846–849.

Angela D. Friederici
Max Planck Institute for Human
Cognitive & Brain Sciences
Department of Neuropsychology
Stephanstraße 1a
04103 Leipzig
Germany

friederici@cbs.mpg.de

The Neurobiology of Language: Looking Beyond Monolinguals

Ellen Bialystok & Judith F. Kroll

1. Introduction

The publication of *Biological Foundations of Language* in 1967 by Eric Lenneberg fundamentally changed the way we think about language. Chomsky brought language from the abstract realm of philosophy into the more grounded world of mind, and Lenneberg completed the process by rooting that mental view of language firmly in the brain. Without Lenneberg, it is difficult to imagine the immense amount of research over the past 50 years that has revealed its structure and function, its social and cognitive dimensions, and obviously, its neurobiology. For Chomsky, the biological basis of language was static, based on innate concepts that unfolded with experience and the reference to biology was largely metaphoric: "mental organ". For Lenneberg, the biological basis of language was real and dynamic. He was the first thinker to seriously understand language as part of human cognition: "[Words] stand for a cognitive process, that is, the *act* of categorization or the *formation* of concepts" (Lenneberg, 1967: 365, emphasis in original). This conception of language blossomed over the subsequent decades, leading to more sophisticated accounts of human language that were based on the use of new methodologies that Lenneberg was unlikely to even imagine. The expansion of technology for observing the brain, the explosion in the sheer amount of knowledge that was accumulated about the brain and its function, and the widespread access to these technologies that became available irrevocably changed the way that language research was conducted (Friederici 2017, Kemmerer 2015). Lenneberg's visionary ideas about the neurobiology of language set the stage for 50 years of exciting and productive study.

In parallel with Lenneberg's developing understanding of language as a biological system, another field began to emerge around the same time. There was growing interest in the process of learning a second language, particularly in adulthood, spawning the field of second-language acquisition (SLA). Much of this research was generated in response to practical needs. A salient example comes from the post-war efforts of The British Council to teach English in various corners of the British Empire by recruiting graduates from the top schools such as Oxford and sending them to distant lands. Armed with little more than intelligence and intuition, many of these teachers thought deeply about their experiences and began

---

Preparation of this manuscript was supported by in part by NIH Grants HD052523 and AG048431 and NSERC Grant A2559 to EB and NSF Grants BCS-1535124 and OISE- 1545900 and NIH Grant HD082796 to JFK.

ISSN 1450–3417

to uncover how to best teach foreign languages and by extension, how languages were learned. One leader in this effort was Pit Corder who had been teaching language and developing syllabus design in various countries for many years. His seminal paper proposed a new set of ideas about language teaching that freed SLA from the behaviorist roots by which it had long been constrained (Corder 1967). However, the predominant model for language subsequently adopted in SLA research was the nativist view of Chomsky, thereby limiting the dynamic component that connected language to cognitive structures and environmental constraints.

Both fields have matured over the past 50 years and yet, somewhat remarkably, remain largely distinct. The fiftieth anniversary of founding works in these fields is an opportunity to consider how more cross-fertilization might benefit our understanding of language. Language science is now deeply embedded in biological models and brain research (Kutas & Van Petten 1994). SLA has evolved through research in such areas as psycholinguistics and bilingualism that explore language-mind-brain connections when more than one language is involved (Kroll, Dussias, Bice, & Perrotti 2015). But Lenneberg had access to none of these insights; his theories and arguments were based on monolingual characterizations, with additional languages representing special cases that did not challenge the central theoretical claims. Our discussion will explore the implications of research with second-language learners and bilinguals for some of Lenneberg's most important claims.

Lenneberg did address the issue of foreign language learning to some extent. He acknowledged that second languages can be learned at any time, even after puberty and even at 40 years old, but asserted that there was no longer access to "automatic acquisition from mere exposure". Moreover, he noted that foreign accents were almost inevitable for languages learned after puberty. This observation is related to his claims for a critical period for (first) language acquisition, described below, but he saw no contradiction with the notion of a critical period because no further cerebral organization is required

> since natural languages tend to resemble one another in many fundamental aspects [. . .], the matrix for language skills is present.
>
> (Lenneberg 1967: 176)

Our view is that recent research in SLA, bilingualism, and psycholinguistics provides crucial evidence that requires a revision of these assumptions.

A central implication of situating language in the biology of the human brain is the acceptance of a framework based on maturation, leading irrevocably to the discussion of critical periods for language learning:

> Language cannot begin to develop until a certain level of physical maturation and growth has been attained. (Lenneberg 1967: 158)

He goes on to state that the years between 2-years old and the early teens are optimal for acquiring language and that language skills acquired after puberty remain "deficient for life". Before 2, there is inadequate brain development, and after puberty the brain loses its ability for reorganization. His argument that the critical period for language learning occurred in this window were based on his description of the structural, biochemical, and electrophysiological development of the brain during this period, all of which he believed were essential to support language, although the precise relations between these developments and language learning were not explained. His view is summarized as follows:

> The disequilibrium state called language-readiness is of limited duration. It *begins around two* and *declines with cerebral maturation* in the early teens. At this time, apparently a steady state is reached, and the cognitive processes are firmly structured, the capacity for primary language synthesis is lost, and *cerebral reorganization of functions is no longer possible*. (Lenneberg 1967: 376–377, emphasis added).

Evidence from SLA and bilingualism challenges each of these main points indicated in italics. We will address them by describing evidence from preverbal infants who are less than 2-years old, adults learning a foreign language, and reorganization of first language representations from second languages learned after the close of the critical period.

2. Language Learning Before Two

Biological developments in the form of critical periods are involved in aspects of the complex set of processes leading to language acquisition, but much has changed since Lenneberg's original description so the nature of that involvement is unlikely to be exactly as envisaged by him. Much of the revision of his ideas can be traced to the dramatic increase in our knowledge of brain structure and the neurobiological mechanisms that underlie human behavior, including language.

In a comprehensive review of speech perception in infancy, Werker & Hensch (2015) describe the multiple developments in the first two years of life and the biological mechanisms that provide the basis for language discrimination, phoneme perception, and audiovisual integration, all essential for language development. All these component developments are traced to specific critical periods that have clear onset and offset windows and in many cases, known biological bases. Crucially, however, they also examine factors that serve to maintain plasticity and avoid closing the critical period, even for these highly circumscribed abilities. One factor they discuss in this regard is bilingualism.

Despite being controlled by critical periods, several pre-linguistic landmarks in the first year of life evolve differently for infants being raised in monolingual or bilingual homes. Thus, even before the onset of Lenneberg's critical period for language learning at 2 years, monolingual and bilingual children are developing a different substrate for language acquisition, setting a different neural foundation for this process in the two language groups. An early example of this effect of experience was in infant phoneme perception. From birth, infants can discriminate between phonemic contrasts in consonants relevant to all (known) natural languages, making them in effect universal language learners (although the trajectory is different for vowels). By 10- to 12-months old, distinctions can only be perceived in the language they are learning, narrowing their perceptual focus and probably improving their ability to learn the environmental language. However, for infants being raised in bilingual environments, the ability to discriminate among all phonemic contrasts remains open, even after the critical period has closed for monolingual infants (Aslin, Pisoni, & Perey 1981, Best 2001, Werker & Tees 1984). Therefore, by the time of the first word, around one year old, the phonemic representation system is different for infants raised in different kinds of language environments.

A more dramatic example comes from studies showing the ability of infants in the first year to distinguish between languages being spoken on the basis of visual information alone. As with phonemic contrasts, monolingual infants can detect such changes until about 7 months old but then fail to make the discrimination. This is not the case for infants raised in bilingual environments. Infants were shown silent videos of a woman speaking French and then, after the infant has habituated, switching to English (or the reverse order). Monolingual infants older than 8-months old did not notice the change, but bilingual infants raised in French-English bilingual homes (Weikum et al. 2007) or Spanish-Catalan homes where they have never encountered either French or English (Sebastian-Galles, Albareda-Castellot, Weikum, & Werker 2012), were able to detect the switch from French to English.

Related to this ability to identify visual language is the way infants look at faces. For newborns, attention naturally focuses on the eyes, but by the end of the first year, preferential attention to faces shifts to the mouth. This new preference presumably supports their growing interest in language by focusing on the most relevant source of information. The shift is earlier for bilingual infants; by 8-months old, infants being raised in bilingual environments are more interested in looking at the mouth whereas infants in monolingual homes continue to focus on the eyes (Ayneto & Sebastian-Galles 2017, Pons, Bosch, & Lewkowicz 2015). By 12-months old, only bilingual infants continue to look at the mouth when both the native and non-native languages are spoken. By 12-months old, monolingual babies look at the mouth only for non-native speech.

These studies used behavioral methods. What happens when we look more directly at brain activity in young infants? Pettito et al. (2012) used functional near infrared spectroscopy (fNIRS) to investigate changes in brain activation for younger (4–6 months) and older (10–12 months) infants exposed to one language alone or more than one language. All the babies had been exposed to English but the bilingual babies had also been exposed to another language. Pettito et al. presented them with the sounds of Hindi, a language equally unfamiliar to the monolingual and bilingual infants. Critically, there was a difference in the pattern of brain activity between the older monolingual and bilingual babies. Consistent with the behavioral pattern reported by Werker & Tees (1984), at 10- to 12-months old the bilingually exposed babies continued to reveal brain activity in response to non-native phonetic contrasts while monolingual babies had lost that ability. They termed this phenomenon for bilinguals the "perceptual wedge" to suggest that bilingually exposed brains maintain greater openness to new language input. Other recent studies using the tools of cognitive neuroscience have examined the consequence of this special openness to speech in bilingual babies' developing brains. Using methods such as magnetoencephalography, they have demonstrated that there is not only increased openness for bilingually exposed babies, but also that there are consequences for the development of brain regions associated with cognitive control and executive function (e.g., Ferjan Ramírez, Ramírez, Clarke, Taulu, & Kuhl 2017). What is clear is that these are not simple effects of maturation but rather evidence for the powerful influence of environmental exposure on language development and the brain, even before the beginning of the critical period defined by Lenneberg.

Two generalizations from these findings challenge aspects of Lenneberg's original theory. First, language acquisition does not begin at 2 years when physical and neurological development has passed some maturational threshold but rather emerges from processes begun at least at birth and possibly in utero. In that sense, there is essentially no lower bound on the window for language acquisition. Second, the modifications found in these developments for children raised in bilingual homes demonstrate the plasticity of language learning even for simple perceptual processes such as phoneme discrimination and even during the crucial early stages of language acquisition. Although Lenneberg was open to a limited notion of plasticity and environmental influence, the evidence from infants in dual language environments speaks to a far greater interaction between biological and experiential contingencies than he imagined.

3. Too Old to Learn?

Critical periods are a common mechanism across species in which maturation requires receiving specific input or experience during a window of maximum sensitivity so that development can proceed. These critical periods are generally used to describe low-level processes such as perception that are part of the foundation for higher-level processes, such as visual interpretation or cognition. This distinction applies as well to critical periods in humans where such low-level maturationally-timed developments in vision (Lewis & Maurer 2005) underlie higher-level visual processing, and low-level developments in speech perception (Werker & Hensch 2015) set the stage for language acquisition. But is there also a maturational restriction on the higher-level processes involved in language acquisition, including mastery of syntax and morphology?

Lenneberg was careful to restrict his deterministic notions of a critical period to primary language acquisition, acknowledging that foreign language learning could take place later in life although it would proceed through different mechanisms. He also limited the degree of proficiency that could be expected for older second-language learners and noted that a foreign accent was likely to occur. Others, however, have made broader claims and essentially argued that all language learning was curtailed after the close of the critical period (e.g., Johnson & Newport 1989). The time that marks the close of the critical period is also different in various accounts: for Lenneberg it was puberty, for Penfield & Roberts (1959) it was 9 years old, and for Johnson & Newport (1989) it was late teens. Throughout these views, however, there is consensus that the close of the critical period is a turning point that either ends the possibility of learning a second language (e.g., Johnson & Newport 1989) or changes its learning mechanism and reduces its expected outcome (e.g., Lenneberg). Therefore, it is important to establish what the evidence is for a biological restriction on this high-level process and whether it applies to all language learning after the close of the critical period or only to acquisition of the first language or only to aspects of the first language.

The challenge in evaluating the role of a critical period for a high-level process such as language acquisition is to determine what evidence is appropriate to test the hypothesis. With the exception of rare cases of abused or feral children, children are unlikely to be completely deprived of language until the close of the critical period. Lenneberg's approach was to investigate children's language acquisition following a brain lesion or acquired aphasia from brain injury and compare the prognosis as a function of the age at which the trauma occurred. His observation was that for children less than 3-years old, when language acquisition resumed it followed the usual stages, possibly proceeding more rapidly than usual. The older children were when the aphasia occurred, the more effortful the recovery, up to puberty which Lenneberg called "a turning point", after which language impairments from aphasia never completely clear up. A similar pattern was noted for patients undergoing the removal of the entire left cerebral hemisphere because, as Lenneberg states,

> language learning can take place, at least in the right hemisphere, only between the age of two to about thirteen. (Lenneberg 1967: 153)

These data are necessarily fragmentary and brain lesions are never identical for different individuals, so comparisons are difficult. However, the pattern is that injury to language acquisition with increasing age is increasingly disruptive, and that injury after puberty cannot restore the language system.

Newport and her colleagues have taken a different approach to examining the possibility for a critical period in first language acquisition and studied the acquisition of American Sign Language (ASL) by congenitally deaf children (summary in Newport 1990). These children can be first exposed to ASL at different ages, providing a natural manipulation to test the hypothesis. In practice, however, children are not first exposed at any time but rather at specific points that mark experiential landmarks, such as starting school. Therefore, their studies typically compared children whose first exposure to ASL was native (from birth), early (4- to 6-years old), or late (after 12 years old). Across studies and measures, outcomes were different for the three groups, with earlier exposure leading to the best mastery of ASL.

In both the investigations of language acquisition following brain lesion and acquisition of ASL at different ages, the evidence shows that older ages are associated with poorer outcomes. In both cases as well, the interpretation is that puberty is a juncture after which language acquisition will be compromised, leading the researchers to conclude that this is caused by the close of the critical period. However, in both cases, the interpretation of a critical period with a qualitative change at puberty is inferential because the data do not include a continuous sampling of ages.

A more direct test of the critical period hypothesis comes from examining second-language acquisition but here, too the evidence is mixed. In a comprehensive review of children acquiring a second-language between the ages of one and 3-years old or between 4- and 7-years old, Unsworth (1916) reported no significant difference between these groups in several aspects of language proficiency, including vocabulary and morphosyntax. However, Newport examined this question and arrived at a different conclusion. In a study comparing English proficiency in second-language learners who began using English at different ages, Johnson & Newport (1989) reported a relation between age and proficiency with better outcomes for those who began learning at a younger age. The relation was shown as a significant linear function across all ages, with a stronger correlation for those who began learning English before the age of 17 years than for those who began after 17. Their conclusion was that the pattern in which a critical period marked the close of a capacity to learn language extended to the acquisition of a second language.

Johnson & Newport's (1989) study was based on data from 46 individuals. In a substantially larger-scale investigation, Hakuta, Bialystok, & Wiley (2003) examined census data from 2.3 million immigrants to the U.S. whose first language was Spanish or Chinese. The census asked respondents to provide a self-rating of their English proficiency, so these scores were analyzed in terms of the number of years they had lived in the U.S. on the assumption that on average that would indicate the age at which they began learning English. The ages of initial acquisition ranged from birth until around 80 years old, and the results showed a significant linear relation between age and proficiency across the entire spectrum, similar to the pattern reported by Johnson and Newport.

There are two problems with the conclusion that evidence for a linear relation between age of acquisition and proficiency supports a critical period for language acquisition. First, if the critical period defines the optimal window for learning to occur, then the variation in learning outcomes within that window should be relatively minor and certainly less than variation in outcomes when comparing learning within and outside of the critical period. Johnson & Newport (1989) argued that their data did show that pattern in that the correlation between age and proficiency was not significant considering only the 23 learners who were more than 17 years old at the time of acquisition, but the overall correlation was significant and the sample was very small. Hakuta et al. (2003) demonstrated that by removing only one participant from the Johnson & Newport data who did not line up on the regression curve, the correlation between age and proficiency for the learners who were over 17 years old became significant. Using the much larger data reported by Hakuta et al. (2003), the relation between these variables was statistically equivalent inside and outside the critical period.

Second is the related point that the definition of a critical period presumes an abrupt change in learning potential following the close of that window. This abrupt change was clearly not found in the Hakuta et al. data, but to confirm that interpretation, the authors compared the correlation before and after specific juncture points of 15 and 20 years old. No change in slope was detected. Importantly as well, the Hakuta et al. study included participants at every age along the continuum, whereas the other studies sampled from specific points making the interpretation of a linear function more inferential than real. Similarly, if the critical period begins at 2-years old, then there should be no difference in outcomes for those who begin language learning at 2 years, 3 years, or 4 years. Yet in the lesion data, the ASL data, and the second-language acquisition data, these onset ages are all associated with declining success.

There is no doubt that age is a crucial factor in determining language learning outcomes for both a first and second language. The question is whether these age-related patterns support the interpretation of a critical period for overall proficiency. Evidence from second-language acquisition is more in line with a gradual decline in the success of language learning than a biological barrier that interferes with its potential. Regarding first language acquisition, there is simply inadequate evidence to conclude there is a critical period. The most compelling evidence is that reported by Newport and colleagues regarding the acquisition of ASL at different ages, but again the pattern is inferential. The late learners certainly had poorer outcomes than the early learners, but the early learners also had poorer outcomes than the native learners. That should not happen if both those groups were within the critical period for language acquisition. Instead, it may be that this pattern again follows the lifelong linear relation reported for second-language acquisition in which older acquisition ages are associated with poorer outcomes in a continuous function. This relation still needs to be explained, but if it is indeed continuous across the lifespan then it is more likely a reflection of gradual changes in cognition, learning and memory. These are important changes, but they are not captured by critical periods.

4. Language Learning, Reorganization, and Processing Beyond the Critical Period

Two predictions drawn from a strict interpretation of the critical period hypothesis have been widely tested. One is that second language learning past the critical period necessarily relies on mechanisms that differ from those that had been available initially for the first language. The second is that past the critical period, the native language is largely stable, remaining unchanged when adult learners acquire and use a second language. In each case, recent findings require a revision of the idea that hard constraints determine the trajectory and outcome of late second language learning.

The question of whether late second language learners can fully acquire the nuances of the second language grammar beyond any putative critical period has been the focus of a great deal of research. To account for the reduced ability for adult to acquire native-like sensitivity to second language grammar, some have argued that late learners exploit semantic and pragmatic information rather than strictly syntactic or morpho-syntactic information, relying on mechanisms available only via explicit learning (e.g., Clahsen & Felser 2006, Ullman 2001). Although a full consideration of the evidence on this issue is beyond the scope of the present discussion, we note that recent studies using neuroscience methods have shown that it is possible for late learners to acquire native-like sensitivity to a range of grammatical structures in the second language. A critical observation concerning the previous behavioral research on this issue is that it suffered from an inevitable confounding between age of acquisition (AoA), length of time spent learning the second language, and second language proficiency (Steinhauer 2014). However, when the performance of highly proficient late second language learners is examined, the neural networks that are activated when processing even subtle aspects of the second language grammar are largely the same as those that are activated by native speakers of the language (e.g., Berken et al. 2015, Caffarra, Molinaro, Davidson, & Carreiras 2015; Morgan-Short et al. 2012, Roncaglia-Denissen & Kotz 2016). These similar patterns suggest common processes and underlying mechanisms.

The evidence on late learners does not refute the observation central to Lenneberg's claim that there may be an effect of AoA for the grammar. The circumstances of learning for adults clearly differ from those for young children and adult second language learning is not as reliably successful as child language learning. Critically, what the new data do show is that when that process is successful, either because learners have been immersed in a second language context or because they have acquired the control mechanisms that enable them to regulate the native language, it reflects the same underlying networks used by native speakers of the language (e.g., Perani & Abutalebi 2005). In all cases, there appears to be much greater plasticity for adult learners than was known at the time of Lenneberg's original claims about the critical period.

The research on acquiring the second-language grammar past the critical period focuses primarily on acquired language abilities in the second language itself. A more recent line of investigation has asked how the native language changes in the process of acquiring and using a second language as an adult learner. Evidence for such changes would indicate reorganization of the first language. Contrary to the view that the native language is stable past an early critical period for language learning, the recent research demonstrates that the native language is both more variable than previously understood, even for monolingual speakers (e.g., Pakulak & Neville 2010), and that the process of learning and using a second language proficiently comes to have profound influences on the native language. Those changes can be seen at the level of the phonology (Chang 2013), the lexicon (Ameel et al. 2005), and the grammar (Dussias & Sagarra 2007). The bilingual's two languages are influenced by each other, with changes that not only reflect transfer from the native language to the second language, the direction of influence that characterized most early research on this topic, but also from the second language to the first. The consequence is that the native language of proficient bilingual speakers is not precisely like the native language of monolingual speakers of the same language. That observation itself requires a reassessment of the native speaker model that has characterized research on second language learning and bilingualism.

Changes to the native language can be observed in long-term studies of language attrition (Schmid 2010) but they can also be seen during briefer periods of immersion in the second language (Linck et al. 2009), and in the laboratory when speakers use the native language after a very brief exposure to the second language (Misra et al. 2012). Contrary to the view that maturation alone determines the presence of sensitivity to the syntax of the second language, recent studies show that the form of language usage, such as whether bilinguals code switch across their two languages, comes to affect the way they process each language and influences the observed patterns of brain activity in both comprehension and production (Beatty-Martinez & Dussias 2017, Green & Wei 2016). The brain networks that support cognitive control are engaged by these language processes and come to shape the relationship between language and cognition.

The plasticity revealed by the new research is evident not only in proficient bilinguals but also in adult learners at early stages of acquiring the second language. The second language quickly comes to affect the native language (e.g., Bice & Kroll 2015) and what is not yet well understood is what these changes might predict about success in second-language learning. Studies using electrophysiological methods have shown remarkable sensitivity to emerging learning, with the brain outpacing behavior and suggesting important sources of individual variation in the process (e.g., McLaughlin et al. 2004, Tanner et al. 2014). Given the deterministic nature of critical periods, such evidence for lifelong plasticity is a significant challenge to explanations in which language acquisition is constrained by a critical period.

5. Conclusion

The research since Lenneberg (1967), illustrated by the findings we have discussed, shows that there is variation in how constrained or plastic different aspects of language acquisition may be. Infants tune to the speech of the language or languages to which they are exposed within the first year of life. In a sense, that process is more constraining than Lenneberg imagined. At the same time, studies of dual language exposure show that early exposure to two or more languages has profound consequences for creating a broader bandwidth for new language learning for young infants exposed to more than one language. The process of tuning to speech happens quickly and much earlier than one might expect from Lenneberg's account. If any aspect of language learning is open to hard constraints, it may be speech, with rapid changes in the first year of life and late exposure producing accents that be difficult or impossible to overcome. The evidence we have considered on acquiring grammar suggests that there may be soft constraints that are modulated by the context in which language is learned and used but that the hard constraints that were thought to be associated with a critical period can be overcome. A crucial point in our review is that it is only by examining how a second language comes into play that these features of language learning can be identified.

In 2005, on the occasion of the 125th anniversary of *Science*, Kennedy & Norman wrote an editorial in which they identified the top 125 questions to be answered in all of science in the following 25 years. One of these questions was the biological basis of second-language learning. In the time since 2005, there has been an explosion of studies on this topic, reflecting many of the themes we have discussed in this paper. The intensive effort to uncover the neural mechanisms engaged by language learning across the lifespan is an enduring tribute to Lenneberg. While the findings since Lenneberg largely refute the notion of a strict critical period for language, and the new evidence for plasticity fails to match the junctures in development that he first identified, the spirit of this new work is congenial with his visionary commitment to a biology of language.

References

Ameel, Eef, Gert Storms, Barbara C. Malt, Steven A. Sloman. 2005. How bilinguals solve the naming problem. *Journal of Memory and Language* 53, 60–80.

Aslin, Richard N., David B. Pisoni, Beth L. Hennessy, Alan J. Percy. 1981. Discrimination of voice onset time by human infants: New findings and implications for the effects of early experience. *Child Development* 52, 1135–1145.

Ayneto, Alba & Nuria Sebastian-Galles. 2017. The influence of bilingualism on the preference for the mouth region of dynamic faces. *Developmental Science* 20, e12446.

Beatty-Martínez, Anne L. & Paola E. Dussias. 2017. Bilingual experience shapes language processing: Evidence from codeswitching. *Journal of Memory and*

Language 95, 173–189.

Berken, Jonathan A., Xiaoqian Chai, Jen-Kai Chen, Vincent L. Gracco, & Denise Klein. 2016. Effects of early and late bilingualism on resting-state functional connectivity. *Journal of Neuroscience* 36, 1165–1172.

Best, Catherine T. 2001. Discrimination of non-native consonant contrasts varying in perceptual assimilation to the listener's native phonological system. *Journal of the Acoustical Society of America* 109, 775.

Bice, Kinsey & Judith F. Kroll. 2015. Native language change during early stages of second language learning. *NeuroReport* 26, 966–971.

Caffarra, Sendy, Nicola Molinaro, Doug Davidson, Manuel Carreiras. 2015. Second language syntactic processing revealed through event-related potentials: An empirical review. *Neuroscience & Biobehavioral Reviews* 51, 31-47.

Chang, Charles B. 2013. A novelty effect in phonetic drift of the native language. *Journal of Phonetics* 41, 520–533.

Clahsen, Harald & Claudia Felser. 2006. Grammatical processing in language learners. *Applied Psycholinguistics* 27, 3–42.

Corder, S. Pit 1967. The significance of learner's errors. *International Review of Applied Linguistics in Language Teaching* 5, 161–170.

Dussias, Paola E. & Nuria Sagarra. 2007. The effect of exposure on syntactic parsing in Spanish-English bilinguals. *Bilingualism: Language and Cognition* 10, 101–116.

Friederici, Angela D. 2017. *Language in Our Brain: The Origins of a Uniquely Human Capacity*. Cambridge, MA: MIT Press.

Ferjan Ramírez, Naja, Rey R. Ramírez, Maggie Clarke, Samu Taulu, & Patrica K. Kuhl. 2017. Speech discrimination in 11-month-old bilingual and monolingual infants: A magnetoencephalography study. *Developmental Science* 20, e12427.

Green, David W. & Li Wei. 2016. Code-switching and language control. *Bilingualism: Language and Cognition* 19(5), 883–884.

Hakuta, Kenji, Ellen Bialystok, & Edward Wiley. 2003. Critical evidence: A test of the critical-period hypothesis for second-language acquisition. *Psychological Science* 14(1), 31–38.

Johnson, Jacqueline S. & Elissa L. Newport. 1989. Critical period effects in second language learning: The influence of maturational state on the acquisition of English as a second language. *Cognitive Psychology* 21, 60–99.

Kemmerer, David (2015). *Cognitive Neuroscience of Language*. New York, NY: Psychology Press.

Kennedy, Donald & Colin Norman. 2005. What don't we know? *Science* 309, 75–75.

Kroll, Judith F., Paola E. Dussias, Kinsey Bice, & Lauren Perrotti. 2015. Bilingualism, mind, and brain. *Annual Review of Linguistics* 1, 377–394.

Kutas, Marta & Cyma K. Van Petten. 1994. Psycholinguistics electrified. In Morton Ann Gernsbacher (ed.), *Handbook of Psycholinguistics* 83–143. San Diego, CA: Academic Press.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Lewis, Terri L. & Daphne Maurer. 2005. Multiple sensitive periods in human visual development: Evidence from visually deprived children. *Developmental Psychobiology* 46, 163–183.

Linck, Jared A., Judith F. Kroll, & Gretchen Sunderman. 2009. Losing access to the native language while immersed in a second language: Evidence for the role of inhibition in second language learning. *Psychological Science* 20, 1507–1515.

McLaughlin, Judith, Lee Osterhout, & Albert Kim. 2004. Neural correlates of second-language word learning: Minimal instruction produces rapid change. *Nature Neuroscience* 7, 703–704.

Misra, Maya, Taomei Guo, Susan C. Bobb, & Judith F. Kroll. 2012. When bilinguals choose a single word to speak: Electrophysiological evidence for inhibition of the native language. *Journal of Memory and Language* 67, 224–237.

Morgan-Short, Kara, Karsten Steinhauer, Cristina Sanz, & Michael T. Ullman. 2012. Explicit and implicit second language training differentially affect the achievement of native-like brain activation patterns. *Journal of Cognitive Neuroscience* 24, 933–947.

Newport, Elissa L. 1990. Maturational constraints on language learning. *Cognitive Science* 14, 11–28.

Pakulak, Eric & Neville, Helen J. 2010. Proficiency differences in syntactic processing of monolingual native speakers indexed by event-related potentials. *Journal of Cognitive Neuroscience* 22, 2728–2744.

Penfield, Wilder & Lamar Roberts. 1959. *Speech and Brain Mechanisms*. Princeton, NJ: Princeton University Press.

Perani, Daniela & Jubin Abutalebi. 2005. The neural basis of first and second language processing. *Current Opinion in Neurobiology* 15, 202–206.

Petitto, Laura-Ann, Melody S. Berens, Ioulia Kovelman, Matt H. Dubins, Kaja K. Jasińska, & Mark H. Shalinsky. 2012. The "Perceptual Wedge Hypothesis" as the basis for bilingual babies' phonetic processing advantage: New insights from fNIRS brain imaging. *Brain and Language* 121, 130–143.

Pons, Ferran, Laura Bosch, David J. Lewkowicz. 2015. Bilingualism modulates infants' selective attention to the mouth of a talking face. *Psychological Science* 26, 490–498.

Roncaglia-Denissen, M. Paula & Sonja A. Kotz. 2016. What does neuroimaging tell us about morphosyntactic processing in the brain of second language learners? *Bilingualism: Language and Cognition* 19, 665–673.

Schmid, Monika S. 2010. Languages at play: The relevance of L1 attrition to the study of bilingualism. *Bilingualism: Language and Cognition* 13, 1–7.

Sebastián-Gallés, Núria, Bàrbara Albareda-Castellot, Whitney M. Weikum, Janet F. Werker. 2012. A bilingual advantage in visual language discrimination in infancy. *Psychological Science* 23, 994–999.

Steinhauer, Karsten. 2014. Event-related potentials (ERPs) in second language research: a brief introduction to the technique, a selected review, and an invitation to reconsider critical periods in L2. *Applied Linguistics* 35, 393–417.

Tanner, Darren, Kayo Inoue, & Lee Osterhout. 2014. Brain-based individual differences in online L2 grammatical comprehension. *Bilingualism: Language and Cognition* 17, 277–293.

Ullman, Michael T. 2001. The neural basis of lexicon and grammar in first and second language: the declarative/procedural model. *Bilingualism: Language and Cognition* 4, 105–122.

Unsworth, Sharon. 2016. Early child L2 acquisition: Age or input effects? Neither, or both? *Journal of Child Language* 43, 608–634.

Weikum, Whitney M. , Athena Vouloumanos, Jordi Navarra, Salvador Soto-Faraco, Núria Sebastián-Gallés, Janet F. Werker. 2007. Visual language discrimination in infancy. *Science* 316, 1159.

Werker, Janet F. & Takao K. Hensch. 2015. Critical periods in speech perception: New directions. *Annual Review of Psychology* 66, 173–196.

Werker, Janet F. & Richard C. Tees. 1984. Phonemic and phonetic factors in adult cross-language speech perception. *The Journal of the Acoustical Society of America* 75, 1866–1878.

Ellen Bialystok
York University
Department of Psychology
4700 Keele Street
Toronto, Ontario M3J 1P3
Canada

ellenb@yorku.ca

Judith F. Kroll
University of California, Riverside
Department of Psychology
Riverside, CA 92521
United States of America

judith.kroll@ucr.edu

Grammar as a Maturationally Controlled Behavior: Minimality in Development and Impairment

Maria Garraffa

Much is already present in the organism, only needing to be activated.
—Noam Chomsky

1. Introduction

In his seminal book on the *Biological Foundations of Language*, Eric Lenneberg proposed that a critical period similar to the one necessary for maturational controlled behaviors applies also to language acquisition (Lenneberg 1967). The notion of a critical period, a maturational stage during which the nervous system is sensitive to specific aspects of the environment, has been considered crucial for language acquisition theories based on the assumption of a biologically predetermined language faculty that needs to be activated by favourable internal and environmental circumstances. Chomsky wrote:

> A consideration of the character of the grammar that is acquired, the degenerate quality and narrowly limited extent of the available data, the striking uniformity of the resulting grammars, and their independence of intelligence, motivation and emotion state, over wide ranges of variation, leave little hope that much of the structure of the language can be learned by an organism initially uninformed as to its general character.
>
> (Chomsky 1965: 58)

The crucial assumption of the critical period hypothesis, as originally proposed, was that language acquisition has an immediate onset for its natural acquisition, and this onset is as early as birth, if not before. Language acquisition has also a predetermined offset, an ideal final state that needs to be completed/activated for reaching a full competence (see Meisel 2013 for a review). This makes the language acquisition process tailored to a restricted time window in which our cognitive development is sensitive to capture human language properties. Already in the 1980s, those who actually studied language development in children as young as newborns found that children quickly began to use a wide variety of cues during their critical period, including syntactic, semantic, and prosodic information. Many studies have investigated the timing and the nature of these cues implicated in the activation of the process of language acquisition in typical developing children, and there is general consensus on a very early onset of the critical period and

ISSN 1450–3417

benefit for early exposure to a rich linguistic environment, including during simultaneous acquisition of more than one language (see for example Mehler et al. 1988, Dehaene-Lambert et al. 2006; Nazzi & Ramus 2003).

A different issue is posed by the study of the other side of the critical period hypothesis, the biological timing for an offset of the language acquisition process. This offset, in other words, involves the occurrence of a biological stage for optimal acquisition linked to early maturational factors that ended early, favouring learning of increasingly complex skills at sequent stages.

A very early offset for the acquisition of language was recently proposed by Friedmann and Rusou in a paper reviewing data on syntax—in particular derived sentences with long distance dependencies—in children with hearing loss from birth and adults with a special case of malnutrition, a thiamine deficiency occurring during the first year of life (Friedmann & Rusou 2015, Friedmann & Szterman 2006, Fattal, Friedmann & Fattal-Velevsky 2011). The fundamental argument in these studies is that some aspects of syntax are affected by lack of language input (as in the case of hearing loss) or by a neurocognitive deficiency (as in the thiamine deficiency group) as early as one year old. This very early offset for language advocates for an urgent requirement to activate the acquisition process. Furthermore, the idea is that selective aspects of the language system require early acquisition and that this is particularly true for syntax. Aspects of language acquisition, such as lexical knowledge, could be acquired after the critical period offset, while syntactic knowledge crucially requires optimal internal and external conditions very early. The consequence of the proposal of an early offset for syntax is that specific and well-known aspects of sentence structures, such as movement-derived sentences—or grammatically based intervention structures that will be described below—although occurring later in life (many crosslinguistic studies reported above chance comprehension and production for object moved sentences after 7 years old) require appropriate input and typical development of the neurological substrate of our receptive system during the first year of life.

Pursing the hypothesis of an early offset for syntax has the main logical consequence of a *strictu senso* neurobiological maturation approach for language, where we could suppose that the sentence has evolved because the brain has evolved. The opposite occurs with no exposure to a rich set of linguistic input or with a lack of a proper neurological condition, both being necessary for the complex process of the acquisition of syntax. Early cases of non-appropriate circumstances for language acquisition, such as the one discussed above, together with evidence of selective developmental disorders in specific areas of the language faculty, as the syntactic Specific Language Impairment (SLI) that will be discussed below, are often presented in support of the crucial role of a biological predetermined knowledge for language acquisition that needs to be activated by environmentally and biologically driven principles.

As linguists, we should reflect on the description of linguistic knowledge and its integration with a plausible developmental process compatible with the critical period hypothesis. This is still not clearly defined by theoretical models and criticism coming from neurodevelopmental psychologists on the insufficient level of integration of descriptive language models with biological principles of language acquisition needs to be taken in consideration, aiming at convergent approaches for the study of language maturation in children:

> The problem is that the theories are derived from a consideration of adult language, and take no account of the process of development. There is a fundamental problem with an essential premise about what is learned that has led to years of confusion and sterile theorising.
>
> (Bishop 2012)

Many questions around how grammar is acquired, and what are the possible routes for understanding late emergence of some aspect of grammar, are still at the center of the debate in developmental language with little productive collaborations between linguists and developmental psychologists and many unresolved problems. Although the definition of the knowledge of language acquisition milestones has now reached incredible levels of detail, due to the amplification of crosslinguistic studies in different populations (see Guasti 2017 for an overview of the growth of grammar and Friedmann & Rizzi 2000 on the acquisition of syntax), the questions of which are the units for language acquisition and which are the developmental stages are still not clearly addressed.[1]

The main purpose of this paper is to point out that although the learning task for the emergence of grammatical knowledge remains a largely unaddressed issue, with no systematic or longitudinal studies on the acquisition of specific aspects of the grammar, a biological perspective, similar to the one proposed in Lenneberg's seminal work in the chapter on "Language in the context of growth and maturation," should be adopted to discuss some recent evidence coming from the studies of sentences with long distance dependencies. In particular, although difficulties with these kinds of structures were reported in diverse populations, the atypical production or comprehension in different populations can have different biological sources of differentiations. This is the case of children with syntactic SLI, a selective disturbance in syntactic dependencies and adults with aphasia, an acquired language disorder. Superficial similarities in the grammatical behaviours in these two atypical populations are based on different sources of impairment, fundamentally representational in children with syntactic SLI due to a disturbed critical period and caused by a lack of more general resources necessary for grammatical processing in adults with aphasia.

The biological perspective on the acquisition of language, like the one proposed in Lenneberg's seminal book, considers language as a maturationally controlled behavior (MCB). Any MCB is defined by a set of properties:

(A) a regular sequence of milestones correlated with age and other developmental factors;

(B) environmental stimulation as an opportunity for use;

---

[1] The lack of dialogue between linguistic theory and the neurobiological development of language, the so called Linking problem (see Fodor 2001) was not limited to the parameter-setting account, which described language acquisition as a process of "setting a switch" for a number of innately-determined parameters. Evidence, though, that children's grammars actually changes in discrete steps, is lacking and it is not clear which aspects of syntactic knowledge should be considered parametric and which is not part of the setting process.

(C) the emergence of the behavior before its use; and

(D) the evidence that this is not a sign of a goal-directed practice.

Points (A) and (B) are accounted for in any approach on language development, with obvious consequences and with several studies reporting data on cases of language deprivation (see Curtiss 1977, Crain 1991). More important for the syntax early offset hypothesis is to address the issue of precursors for the emergence of a specific grammatical behavior. What kind of potentialities of a given grammatical behavior need to be activated during language acquisition (point C) and as a consequence, are these "protogrammatical behaviors" used in diverse contexts, beyond imitation (point D)?

Starting from point (C), in the field of developmental psychology, the ability often reported as a precursor for grammatical learning is the one to extract regularity from the input. This is called statistical learning (see Obeid et al. 2016 for a recent review). Statistical learning accounts have been proposed as optimal tools to study the process beyond grammatical learning. Sensitivities to regularities of the input is considered an asset for learning, but not specific for language learning.

The main idea beyond grammar as MCB is that the properties to be acquired are internal to the grammatical system and are not based on frequency of exposure or cognitive general abilities of extracting regularities. The antithetic argument proposed in statistical learning approaches compared to an approach based on the maturation of grammatical knowledge is that instead of assuming that children start with knowledge of linguistic categories, categories are abstracted from statistical regularities in the input. An obvious argument in favour of the grammatically based maturational approach comes from study of discontinuous behaviours in grammatical development. Data on discontinuous development in the acquisition of grammar are scarce, but the few that are available support of emergence of grammar where exposure alone cannot account for the growth and development of grammar as an organic system (see for example Riches & Garraffa 2017 for a study on intervention effects in children).

This brief contribution illustrates the relation between the acquisition of grammatical knowledge, in particular long distance dependencies subject to intervention, and the accompanying deployment systems required to develop this grammatical knowledge.

The first section is devoted to the illustration of the syntactic phenomenon under investigation. The following two sections discuss special circumstances of acquisition and loss of competence with intervention structures, such as in the case of children with language impairment and adults with aphasia. Both cases are of considerable interest for theoretical models, given that both populations are far more sensitive to grammatically-based intervention effects compared to expert speakers due to their atypical computational system (see Garraffa & Grillo 2008 for language disorders in adults and Friedmann et al. 2009 for language acquisition, as well as Tsimpli et al. 2017 for an overview on language pathology in linguistics).

2. Grammatically-Based Intervention Structures

The last ten years have seen a proliferation of research on intervention effects in language impairment, processing and acquisition benefiting from the application of the minimality framework, as originally proposed in linguistic theory studies (Rizzi 1990, 2004, Chomsky 1995). Focusing on language acquisition, minimality assumes that the nonadultlike behaviours during comprehension of Object Relative clauses or Object-extracted wh-elements can be described as a case of immature knowledge predicted by an immature grammatical system (see Friedmann, Belletti & Rizzi 2009, Belletti 2017). Children with poor production and comprehension of sentences based on intervention, and in particular sentence with structural similarity between the moved object and the intervening subject (as in 1a), are adopting a restricted version of locality and they are more sensitive to locality constraints. In particular, the model makes clear predictions for the syntactic context not fully developed in child systems, any representations similar to the one described in (1a) and (1b).

(1) a. +A ... +A ... <+A> (identity)
    b. +A, +B ... +A ... <+A, +B> (inclusion)
    c. +A ... +B ... <+A> (disjunction)

Between-group differences in both comprehension and production of sentences where minimality can induce grammatically-based interference effects have been found to be greater in conditions where there is overlap of features between lexically-restricted NPs (see Friedmann, Belletti & Rizzi 2009). This is fully represented in the identity condition, (1a), where there is no feature distinction between positions, making this structure highly complex. Subject dependencies are excluded in this system, since they do not include an intervener between the target and the trace and they do not induce intervention effects. Object dependencies may be a challenging structure to acquire, depending on the internal structure of the moved constituent and of the intervening element, the subject. The generalisation which emerges is that if the target of the movement and the intervening subject are sufficiently different in their internal structure, the configuration is unproblematic, where the critical differential element appears to be the presence or absence of a lexical NP restriction. This model assumes that the source of difficulties in children's grammatical development is based on a partial encoding of the grammatical information, not sufficient to parse (1a). Children adhere to a literal version, or stricter version of the locality principle, requiring distinct feature specifications for the target and for its intervener, and imposing a disjoint specification.

In these immature grammatical systems, an internal grammatical pressure of coping with the next level of the configuration, such as the one manifesting the hardest intervention configuration, could end with production of sentences barely used in adults and not attested in standard languages. This is the case for example of the avoidance strategies reported in children's production of passives in Italian and its substitution with a set of unattested forms in the matched adult competence (see Belletti 2017 for details).

Another important aspect of the acquisition of the grammatically-based intervention structures is the assumption of a genuine operation that needs to be acquired, or in biological terms, the assumption of a dissociation between general language abilities and performance on a specific operation. Dissociations between aspects of language competence are consistent with maturational accounts of language acquisition (Borer & Wexler 1987, Wexler 2003). For example, Wexler

(2003) notes that the disappearance of optional infinitives is completely uncorrelated with maternal education, IQ, or vocabulary scores, factors which are likely to be closely associated with general language learning abilities. This dissociation arises, according to Wexler, because the relevant linguistic knowledge (expressed as a parameter) adheres to a genetically-determined time frame. A similar argument could be put forward to explain the data reported in a recent study on intervention structures in children, where the performance on Object which-questions was reported as not correlated with aspects of performance on sentence comprehension in different grammatical conditions, such as subject-extracted questions or binding (Riches & Garraffa 2017). Intervention effects may be subject to a maturational constraint based on intervention and consequently they are divorced from other domain-general language abilities. By contrast, non-intervention structures which are not governed by such a constraint should show a stronger relationship with overall language abilities.

If this is the case, we should aspect to find in nature a developmental disorder selective for this operation, a dissociation in the process of acquisition of grammatical based intervention structures compared to the acquisition of other linguistic operations. We will now discuss some evidence in support of a selective impairment in grammatically-based intervention structures in children with developmental language disorders.

3. Developmental Language Disorders (DLD): The Case of Syntactic SLI

Lenneberg wrote:

> The development of children with various abnormalities provides the most convincing demonstration that the onset of language is regulated by a maturational process, much the way the onset of gait is dependent upon such a process but at the same time the maturational process is independent of motor-skeletal maturation. (Lenneberg 1967: 131)

According to this approach, internally to language mechanisms we can find a natural extension of basic principles of organization of behavior, which are adapted to the specific function of language. This function under natural conditions evolves to a richer level of organization of the stimuli surrounding a child.

> Both the perceived patterns and the self-produced patterns become organized or grouped in functional categories and hierarchies of category. Members of a particular category are functionally equivalent because they either elicit an identical response or they serve one and the same function within the over-all structure of a particular behavioral pattern. It is the general principles of differentiation and categorization that appear in specialized form in verbal behavior. (Lenneberg 1967: 325)

Interesting evidence for an impairment in linguistic knowledge as an effect of a divergent maturation of the grammatical system is the case of syntactic SLI children (Friedmann & Novogrodsky 2007). While typically-developing children with immature linguistic knowledge may nonetheless display appropriate interpretive behavior because of the way this partial knowledge is deployed (as presented in section 2), children with syntactic SLI reveal a selective impairment of grammatical competence with no sign of immature behavior and no opportunities for natural development of the operation. These children have severe difficulties in understanding and producing movement-derived sentences subject to interferences, such as Object Relative clauses and Object Questions. Their problem is not related to the syntactic structure but to the mechanism to assign thematic roles in highly complex configurations as (1a). It is interesting to note that there is a consensus in the literature in identifying subgroups of language disorders in the large spectrum of developmental language impairment, with many studies reporting dissociations between different linguistic capacities (children with lexical retrieval deficit and intact syntax and children with poor phonological processing and no impairment of syntactic structures).

More important for the biological argument proposed here is the lack of evidence of development of the grammatical behavior in children with syntactic SLI, with no instances of grammatical strategies in their speech, for example to avoid complex sentences, and no indication of an immature propositional attitude as a source of delay. There is no syntactic adaptation in children with syntactic SLI and syntactic knowledge cannot develop independently to specific structure-frames. Similar evidence was reported in clinical studies: after training with a particular construction type, fluent automatic comprehension was not achieved in impaired children (Bishop, Adams & Rosen 2006).

More research on the mechanisms of grammatical learning and the role of effective input is needed in these children, to better define the optimal condition for input exposure, measuring the interaction between grammatical competence and exposure.

Recently, syntactic priming has been adopted to track the learning of a grammatical behavior in children with syntactic SLI (Garraffa, Coco & Branigan 2015). Syntactic priming, the tendency of reusing a structure previously used, has been proposed as an optimal tool for investigating children development of grammar, making it possible to track both the learning of a structure and the grammatical behavior beyond the attempted production in a predicted setting (Leonard 2011). Being primed by a recently heard structure is evidence of sensitivity for a grammatical pattern, but current studies on structural priming are not guided by theoretically-based predictions (though see Oltra-Massuet, Sharpe, Neophytou & Marantz 2017).

Preliminary results on the acquisition of subject relative clauses suggest that the impairment in children with syntactic SLI involves reduced initial learning from each syntactic experience, rather than atypically rapid decay following unimpaired initial learning. This result makes it necessary to better define the source of poor learning rates attested in these children, with in-depth investigations of their grammatical competence. Some studies found a deficit in children with syntactic SLI selective for comprehension of intervention structures, where children are avoiding structure with intervention, but it is not clear if the source of this lack of competence lays in a specific syntactic operation or if for some of them the deficit goes deeper in the tree-structure (Friedmann, Yachini & Szterman 2015). Language competence in children with syntactic SLI should be investigated within a richer model of the grammatical development, including sentences not attested in adult speech but grammaticalised in an immature system. The model of grammar should consider grammar as a maturational behaviour and describe which grammatical behaviours are considered benchmarks for typical language acquisition, framing the research on language development with targets mirroring the children's biological system and not the adult competence.

4. Asyntactic Comprehension in Aphasia

Adults with non-fluent aphasic speech were extensively studied in their comprehension of syntactic structures, reporting a well-attested deficit in sentence comprehension of intervention-based effects (see Grillo 2008, Friedmann & Shapiro 2003, as well as Druks 2017 for a review of the theories on agrammatic aphasia).

While neurolinguistic studies have investigated the operational nature of Broca's area, aphasiology has not reached a level of integration with the theoretical models for the investigation and design of therapeutic programs. The lack of integration between linguistic theory and the neurobiology of language is not unique to aphasiology. It is a pervasive problem that still persists in the cognitive neuroscience of language at large, also in the absence of pathology (Poeppel & Embick 2005/2013). The role of Broca's area in language processing is still contested, whereas some researchers have attempted to link it to the neural network correlated with the computation Merge (Zaccarella & Friederici, 2015), while yet other researchers argue for an understanding of Broca's area as a kind of memory buffer.

Degrees of the activation of Broca's area were reported to depend on the number of interveners in a function magnetic resonance imaging (fMRI) study (see Santi & Grodzinsky 2007), supporting the idea that Broca's area is involved specifically in the processing of syntactic operations and that modulation of the activation is visible by manipulating the number of interveners in the sentence. Other recent evidence of a modular language-specific role of this area comes from research using transcranial direct current stimulation (tDCS; Garraffa & Sedda 2017). In this study, Broca's area (Brodmann areas [BA] 44/45) was temporarily inhibited in a group of healthy participants, while a sentence comprehension task was carried out. A second group of participants received the same stimulation in the temporal area (BA 22) of the left hemisphere, during the same comprehension task. An effect in term of reduction in the sentence comprehension was reported only for the group with the inhibition of Broca's area, supporting the hypothesis for a core functional engagement for syntactic processing with no involvement of the left temporal area for core processing of syntax.

The loss of (syntactic) processing abilities has been proposed to be at the core of the deficit in individuals with non-fluent aphasia, with some models aiming at integrating linguistic theory and syntactic processing. A first attempt to integrate both processing-based and representationally-based accounts was recently proposed in the investigation of the poor comprehension of long-distance dependencies. The hypothesis proposed that the processing deficit at the core of the acquired language deficit is what compromises the representation of the full array of morphosyntactic features normally associated with syntactic elements, thereby giving rise to Minimality Effects in specific syntactic configurations (see Garraffa &

Grillo 2008).[2] The cause of aphasics' limited processing capacities with non-local dependencies is in their impairment in retaining the activation of the representation of all the morphosyntactic features associated with syntactic nodes. Thus some of the features needed for the computation of structures involving dependencies over possible interveners may be absent with a consequent underspecification of the structure; compare (2a) with (2b). While expert speakers can differentiate the element due to their structural position and composition, those individuals with an acquired deficit do not have enough resources for maintaining active the full representation. A crucial consequence of the model presented below is that the deficit is not selective to people with agrammatic aphasia, but it can be adapted to any instance of poor processing resources.

(2) a. . . . X . . . Z . . . Y *Expert speakers*
$[\dots]_{\mathrm{Q}}$ $[\dots]_{\mathrm{A}}$ $[\dots]_{\mathrm{Q}}$
b. . . . X . . . Z . . . Y *Impaired speakers*
$[\dots]_{\mathrm{A}}$ $[\dots]_{\mathrm{A}}$ $[\dots]_{\mathrm{A}}$

This leads us to expect an asymmetry in aphasic treatment of dependencies: those involving one NP crossing over another similar one are reported to be more problematic than those that do not involve such a configuration. The asymmetry reported in many studies between aphasic comprehension of object movement (which implies crossing of the subject NP) versus subject movement (which does not imply crossing of any NP) supports the intervention hypothesis (Sheppard, Walenski, Love & Shapiro 2015).

This approach to aphasic comprehension and to grammatical deficit in general has interesting consequences for the definition of a theory of the grammatical nature of linguistic processing. For example, in much work on sentence processing there is an explicit reference to the concept of complexity (see Just, Carpenter & Keller 1996). Their model is based on the assumption that a working memory for language functionally separated from the representation of linguistic knowledge is at the core of the deficit reported in people with non-fluent aphasia. To define more clearly what complexity is, linguistic knowledge is required. Without a clear description of complexity and a theoretically motivated set of predictions, the best we could hope to achieve would be a list of structures ordered by their complexity judged according to empirical measures. We will thus know which structures are easier and which are more difficult to process, but we will not know why this is so nor will we have a definition of complexity, merely a descriptive tool. Underspecification in individuals with aphasia would be caused by any (permanent or temporal) reduction of syntactic processing capacities. General slowing of lexical activation and a concomitant delay in the formation of syntactic dependencies involving "moved" constituents and empty elements was reported in many studies with people with aphasia (Love, Swinney, Walenski & Zurif 2007).

On the basis of minimality, it is possible to provide a definition of complexity unfolding the composition of the structure and its number and quality of morphosyntactic features which need to be maintained active in the syntactic representation in order for the computation not to crash. A clear set of predictions can be

---

[2] The reader is referred to Starke (2001), Rizzi (2004), and Grillo (2008), for the original formulation of Minimality Effects in term of classes of features.

generated on the basis on the nature of the comprehension deficit in people with aphasia with specific crosslinguistically predicted differences.

The rationale behind this approach is that the full representation of the morphosyntactic feature structure of a syntactic constituent has a processing cost and that a computational system driven by (among other factors) economy considerations might try to avoid those costs. Delaying the representation of required feature structure implies delaying the representation of required positions in the structure; for example, a delay in the representation of a feature on the head of a relative clause implies that the chain member in CP will not immediately be represented, with predictable consequences once the parser encounters evidence for the necessity of the postulation of those features/members.

An advantage of the formulation of the acquired impairment in terms of feature structure is that not only we can predict the nature of the source of a preferred structure, on the basis of minimality, we can also predict in what kind of environments this preference for less costly structures will turn into a mistake. Furthermore, this approach has the clear advantage of treating mistakes as structures ruled out by grammatical principles. Generation of featurally impoverished syntactic structures allows us to rule them out on a par with other more standard cases of syntactic violation.

5. Conclusion

As Lenneberg pointed out in his monograph, accounts of language should consider it as an MCB—a maturationally-controlled behavior—and investigate the evolution of coherent developmental stages predicted by both neurodevelopmental growth and a rich language environment as a trigger for acquisition. In the case of developmental language impairment, we should be conducting research to find out what kinds of input are most effective for children who are at genetic risk. It is possible that rather than more language input, they may do best with a different kind of language input, specifically tailored to take into account children's cognitive strengths and weaknesses.

For adults with aphasia, there is a need for developing training based on language exposure to specific grammatical properties, aiming at generating effects of short-term cortical plasticity after specific language learning tasks. It seems that short training of less than an hour of high cognitive demand can induce microstructural changes in the cortex, suggesting a rapid time scale of neuroplasticity (Hofstetter, Friedmann & Assaf 2017). At the moment there are no intervention programs for people with aphasia designed with insights from linguistic theory and the neurobiology of language. The absence of theoretically-motivated programs is a lack of evidence for informing both language recovery and language retention.

References

Belletti, A. 2017. Internal grammar and children's grammatical creativity against poor Inputs. *Frontiers in Psychology* 8, 2074. doi:10.3389/fpsyg.2017.02074.

Bishop, D. V. M. 2006. What causes specific language impairment in children? *Current Directions in Psychological Science* 15, 217–221.

Bishop, D. V. M. 2012. What Chomsky doesn't get about child language. http://deevybee.blogspot.co.at/2012/09/what-chomsky-didnt-get-about-child.html (11 December 2017).

Bishop, D. V. M., C. V. Adams, & S. Rosen. 2006. Resistance of grammatical impairment to computerized comprehension training in children with specific and non-specific language impairments. *International Journal of Language and Communication Disorders* 41(1), 19–40.

Borer, H. & K. Wexler. 1987. The maturation of syntax. *Parameter Setting*. 123–172.

Chomsky, N. 1965. *Aspects of the Theory of Syntax*. Cambridge, MA: MIT Press.

Chomsky, N. 1980. On cognitive structures and their development: A reply to Piaget. In M. Piattelli-Palmarini (Ed.) *Language and Learning: The Debate Between Jean Piaget and Noam Chomsky*. Cambridge, MA: Harvard University Press. 35–52.

Chomsky, N. 1986. *Knowledge of Language: Its Nature, Origin, and Use*. Westport, CT: Praeger.

Chomsky, N. 1995. *The Minimalist Program*. Cambridge, MA: MIT Press.

Crain, S. 1991. Language acquisition in the absence of experience. *Behavioural and Brain Science* 14(4), 597–612.

Curtiss, S. R. 1977. *Genie: A Linguistic Study of a Modern Day "Wild-Child"*. New York, NY: Academic Press.

Dehaene-Lambertz, G., L. Hertz-Pannier, & J. Dubois. 2006. Nature and nurture in language acquisition: Anatomical and functional brain-imaging studies in infants. *Trends in Neurosciences* 29(7), 367–373.

Druks, J. 2017. *Contemporary and Emergent Theories of Agrammatism. A Neurolinguistics Approach*. New York, NY: Routledge.

Evans, J., J. Saffran, & K. Robe-Torres. 2009. Statistical learning in children with specific language impairment. *Journal of Speech, Language and Hearing Research* 52, 321–335.

Fattal, I., N. Friedmann, & A. Fattal-Valevski. 2011. The crucial role of thiamine in the development of syntax and lexical retrieval: A study of infantile thiamine deficiency. *Brain* 134(6), 1720–1739.

Fodor, J. D. 2001. Setting syntactic parameters. In M. Baltin and C. Collins (Eds.), *The Handbook of Contemporary Syntactic Theory*. Oxford, Endland: Blackwell. 730–767.

Friedmann, N., & R. Szterman. 2006. Syntactic movement in orally trained children with hearing impairment. *Journal of Deaf Studies and Deaf Education* 11(1), 56–75.

Friedmann, N. & R. Novogrodsky. 2007. Is the movement deficit in syntactic SLI related to traces or to thematic role transfer? *Brain and Language* 101(1), 50–63.

Friedmann, N., & D. Rusou. 2015. Critical period for first language: The crucial role of language input during the first year of life. *Current Opinion in Neurobiology*. doi:10.1016/j.conb.2015.06.003.

Friedmann, N., A. Belletti, & L. Rizzi. 2009. Relativized relatives: Types of intervention in the acquisition of A-bar dependencies. *Lingua* 119, 67–88. doi:10.1016/j.lingua.2008.09.002.

Friedmann, N., M. Yachini, & R. Szterman. 2015. Relatively easy relatives: Children with syntactic SLI avoid intervention. In E. Di Domenico, C. Hamann, & S. Matteini (Ed.), *Structures, Strategies and Beyond. Studies in Honour of Adriana Belletti*. Amsterdam, The Netherlands: John Benjamins. 303–320.

Friedmann, N., & L. P. Shapiro. 2003. Agrammatic comprehension of simple active sentences with moved constituents: Hebrew OSV and OVS structures. *Journal of Speech Language and Hearing Research* 46, 288–297.

Friedemann, M.A. & L. Rizzi (Eds.). 2000. *The Acquisition of Syntax: Studies in Comparative and Developmental Linguistics*. London, England: Longman.

Garraffa, M. & N. Grillo. 2008. Canonicity effects as a grammatical phenomenon. *Journal of Neurolinguistics* 21(2), 177–197.

Garraffa, M., M. Coco, & H. Branigan. 2015. Effects of immediate and cumulative syntactic experience in language impairment: Evidence from priming of subject relatives in children with SLI. *Language Learning and Development* 11(1), 18–40.

Garraffa, M. & Sedda, A. (2017). Core regions for syntactic processing? A tDCS study on the language network. *Stem-, Spraak- en Taalpathologie* 22(2), 47–49.

Jacques, G. & J. Mehler. 2010. Speech perception and language acquisition in the first year of life. *Annual Review of Psychology* 61, 191–218.

Grillo, N. 2008. *Generalized Minimality: Syntactic Underspecification in Broca's Aphasia*. Doctoral dissertation, University of Utrecht, The Netherlands.

Guasti, M.T. 2017. *The Growth of Grammar*. Cambridge, MA: MIT Press.

Hofstetter, S., N. Friedmann, & Y. Assaf. 2017. Rapid language-related plasticity: Microstructural changes in the cortex after a short session of new word learning. *Brain Structure and Function* 222(3), 1231–1241. doi:10.1007/s00429-016-1273-2.

Just, M. A., P. A. Carpenter, & T. A. Keller. 1996. The capacity theory of comprehension: New frontiers of evidence and arguments. *Psychological Review* 103, 773–780.

Lenneberg, E. H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Leonard, L. 2011. The primacy of priming in grammatical learning and intervention: A tutorial. *Journal of Speech, Language, and Hearing Research* 54, 608–621.

Levy, H. & N. Friedmann. 2009. Treatment of syntactic movement in syntactic SLI: A case study. *First Language* 29, 15–50. doi:10.1177/0142723708097815.

Love, T., D. Swinney, M. Walenski, & E. Zurif. 2007. How left inferior frontal cortex participates in syntactic processing: Evidence from aphasia. *Brain and Language*. doi:10.1016/j.bandl.2007.11.004.

Mehler, J., P. Jusczyk, G. Lambertz, N. Halsted, J. Bertoncini, & C. Amiel-Tison. 1988. A precursor of language acquisition in young infants. *Cognition* 29(2), 143–178.

Meisel, J. M. 2013. Sensitive phases in successive language acquisition: The critical period hypothesis revisited. In C. Boeckx & K. K. Grohmann (Eds.), *The Cambridge Handbook of Biolinguistics*. Cambridge, UK: Cambridge University Press. 69–85.

Nazzi, T. & F. Ramus. 2003. Perception and acquisition of linguistic rhythm by infants. *Speech Communication* 41(1), 233–243.

Obeid, R., P. Brooks, K. Powers, K. Gillepsie-Lynch, & J. Lum. 2016. Statistical learning in Specific Language Impairment and Autism Spectrum Disorder: A meta-analysis. *Frontiers in Psychology*. doi:10.3389/fpsyg.2016.01245.

Oltra-Massuet, I., V. Sharpe, K. Neophytou, & A. Marantz. 2017. Syntactic priming as a test of argument structure: A self-paced reading experiment. *Frontiers in Psychology* 8, 1311. doi:10.3389/fpsyg.2017.01311.

Poeppel, D. & D. Embick. 2005/2013. Defining the relation between linguistics and neuroscience. In A. Cutler (Ed.), *Twenty-First Century Psycholinguistics: Four Cornerstones*. New York, NY: Psychology Press. 103–118.

Riches, N. G. & M. Garraffa. 2017. A discourse account of intervention phenomena: An investigation of interrogatives. *Glossa: A Journal of General Linguistics* 2(1), 74. doi:10.5334/gjgl.100.

Rizzi, L. 1990. *Relativized Minimality*. Cambridge, MA: MIT Press.

Rizzi, L. 2004. Locality and left periphery. In A. Belletti (Ed.), *Structures and Beyond: The Cartography of Syntactic Structures*. New York, NY: Oxford University Press. 223–251.

Santi, A. & Y. Grodzinsky. 2007. Working memory and syntax interact in Broca's area. *NeuroImage* 37(1), 8–17.

Sheppard, S. M., M. Walenski, T. Love, & L. P. Shapiro. 2015. The auditory comprehension of wh-questions in aphasia: Support for the intervener hypothesis. *Journal of Speech, Language and Hearing Research* 58, 781–797.

Starke, M. 2001. *Move Dissolves into Merge: A Theory of Locality*. Doctoral dissertation, University of Geneva, Switzerland.

Tsimpli, I. M., M. Kambanaros, & K. K. Grohmann. 2017. Language pathology. In I. Roberts (Ed.), *The Oxford Handbook of Universal Grammar*. Oxford, Endland: Oxford University Press. 486–508.

Wexler, K. 2003. Lenneberg's dream: Learning, normal language development, and specific language impairment. In Yonata Levy & Jeannette C. Schaeffer (Eds.), *Language Competence Across Populations: Toward a Definition of Specific Language Impairment*. Mahwah, NJ: Lawrence Earlbaum. 11–61

Wright, H., R. Downey, M. Gravier, T. Love, & L. Shapiro. 2007. Processing distinct linguistic information types in working memory in aphasia. *Aphasiology* 21, 802–813.

Zaccarella, E. & A. D. Friederici. 2015. Merge in the human brain: A sub-region based functional investigation in the left pars opercularis. *Frontiers in Psychology* 6, 1818. doi:10.3389/fpsyg.2015.01818.

Zurif, E., D. Swinney, & M. Garrett. 1990. Lexical processing and sentence comprehension in aphasia. In A. Caramazza (Ed.), *Cognitive Neuropsychology and Neurolinguistics: Advances in Models of Cognitive Function and Impairment*. Mahwah, NJ: Lawrence Earlbaum. 123–136.

Maria Garraffa
Heriot-Watt University
Department of Psychology
Edinburgh Campus
EH14 4AS Edinburgh
United Kingdom

m.garraffa@hw.ac.uk

Can a Morphological Feature of Dendritic Structure be Linked to Language Acquisition?

Harvey M. Sussman

1. Introduction

Eric Lennenberg (1967) popularized the notion of a critical period for language acquisition, an ideal developmental time window, from approximately age two to puberty, beyond which achieving native-speaker like competence is greatly diminished. The critical period hypothesis (CPH) has been and continues to be a much discussed and controversial topic, particularly in the context of second language acquisition (for a review see Birdsong, in press). My contribution to this discussion is very limited and focused on a specific issue—that is, can an enhanced, developmentally-based feature, empirically documented within a neuron's dendritic arborization, play a role in language acquisition?

A reasonable expectation is that in a normal postnatal environment, a functional enrichment of neuronal circuitry interconnecting brain regions engaged in speech and language processing should parallel and underlie the emergence of a natural language in a child. From initial vocalic-like cries and squeals, to canonical and variegated babbling, to first words, to two word utterances, and culminating in the production of sentences, one would expect a concomitant maturation of the complex neural infrastructure mediating this genetically and experientially driven, but poorly understood, cognitive achievement.

What may be unreasonable, however, is an expectation of linking neuroanatomical features of micro-level structure to cognitive function. Fifty years ago, Lennenberg cautioned against making such claims:

> As biologists we cannot discern meaning or purpose of specific anatomical developments. (Lenneberg 1967: 33)
>
> Thus the microscopic anatomical detail does not contribute to our search for histological correlates of speech and language. (Lenneberg 1967: 56)
>
> Broca's area consists of large cells in the third and fifth cortical layer, but it is doubtful that this is relevant to language. (Lenneberg 1967: 62)

If Lenneberg were alive today, with our assortment of brain imaging techniques, would he still make such claims? Lenneberg readily admitted that "anatomy is a descriptive science" and as such, "[...] does not imply knowledge of causality [...]" (1967: 33). My guess is that Lenneberg would still hold fast to his 1967 convictions.

biolinguistics

ISSN 1450–3417

Arnold Scheibel and colleagues (Scheibel et al. 1985, Scheibel 1990, 1992), employing sophisticated techniques for microscopic scrutiny of postmortem human tissue samples, have cautiously suggested that the basilar dendritic branching pattern of cortical pyramidal neurons in left opercular cortex, might reveal a viable, underlying neuronal correlate of speech/language development during early childhood. Scheibel dubbed this dendritic branching network a "fossilized record" and an "organic autobiography" reflecting the neuro-ontogenetic development of an individual's cognitive experiential interactions with the environment.

The goal of this paper is to (1) review Scheibel's studies and the evidence for this claim; and (2) critically evaluate whether the neurodevelopmental processes underlying language acquisition can ever be, with confidence, causally related to specific morphological features of the cortical neuropil.

2. Neurophysiological Evidence for Age-Related Developmental Plasticity

The interaction of inherent genetic programs with environmental exposures and experiences directly determines the course of neurogenesis (growth and subsequent pruning of axons, dendritic arbors, and synaptic interfaces) of the brain (Tau & Peterson 2010). Both nonhuman and human studies have convincingly shown that the plasticity of neurosensory systems is greater during early development than in a mature organism (Harrison et al. 2004). Numerous animal studies have demonstrated the critical, time-sensitive, importance of early sensory input in brain development. Most famously, Wiesel & Hubel (1963, 1965) documented the detrimental effects of neonatal monocular deprivation on visual cortical wiring responsible for the development of ocular dominance columns in kittens. Critical periods in the human visual system have also been well documented—for example, failure to correct for congenital cataracts in infants by 6 months of age or strabismus by 7 years of age produces irreversible impairments in the visual system (Levi & Li 2009, Maurer et al. 1999). Tau & Peterson in their review of normal and abnormal development of neural circuitry conclude:

> Thus, certain epochs in the maturation of neural circuits for vision appear to constitute critical periods of developmental vulnerability, times when experiential input is necessary for the normal development of specific neural circuits and their functional capacities, and without which the potential for development of those functional capacities is lost forever. (Tau & Peterson 2010: 154)

Similar studies have shown critical periods for thalamocortical connectivity in the auditory system. Zhou & Merzenich (2008) and Barkat et al. (2011) exposed mice to narrowly restricted sound inputs and showed subsequent developmental alterations in tonotopic maps in primary auditory cortex. Harrison et al. (2004) examined the issue of an age-related sensitivity period in human language development based on age of cochlear implantation in congenitally deaf children. Age at implantation is the equivalent of "duration of auditory deprivation." Eighty-two children, who received implants ranging from 1 to 15 years, were tested with a battery of auditory and speech comprehension tests prior to implantation, and at regular intervals up to 8 years post-implantation. Children implanted at 5 years or younger revealed the highest post-implant test scores across the battery of tests. For all age-at-implant groups, "a point in time is reached after which gains are limited or absent" (Harrison et al. 2004: 255). The authors, however, were very explicit in concluding that it is highly unlikely there is one critical period. The language-related behavioral outcomes have many underlying neural mechanisms, each with their own developmental timetable. The preferred conclusion was that there is a robust age- related plasticity effect, and the earlier the implant, the better the overall outcome.

The next section provides a rudimentary review of early developmental milestones in the infant brain that will provide a backdrop for subsequent sections.

3. Primer on Normal Development of Brain Structure in Infancy

By the end of the first year of life the human brain is approximately 70 % of its adult size, and by age 2, about 80 % of adult size, with a hierarchical progression of cerebellum growth first, followed by subcortical areas, and culminating in the cerebral cortex. Cortical synaptogenesis across the first two years of life is characterized by the elaboration of dendrites, spines, and synapses, within both pyramidal cells and GABAergic inhibitory interneurons (Mrzljak et al. 1992). Progressive and regressive developmental events that began in utero, continue in postnatal life, fine-tuning local synaptic connections spreading across long range cortical circuits (Knickmeyer et al. 2008).

The overall time course of synaptogenesis occurs at different rates across cortical regions (Huttenlocher & Dabholkar 1997). The time course for fine tuning neural circuitry, as well as myelination, follows a path of sensory and motor cortices first, followed by association cortices and the corpus callosum, and lastly cortical areas sub-serving higher cognitive functions (Levitt 2003, Paus et al. 2001). Diffusion tensor imaging studies have shown that the highest rates of increase of white matter tracts occur before the age of 10, proceeding at different rates in different brain regions, and continuing well into adulthood (Ashtari et al. 2007). Gogtay et al. (2004) analyzed magnetic resonance imaging (MRI) data from 13 individuals spanning the age range of 4–21 years of age, each scanned four times at 2-year intervals. Cortical gray matter volume began to decline in late childhood/adolescence, proceeding in a "back-to-front" direction, occurring first in sensorimotor areas, followed by association areas, and lastly in higher-order cortical areas (superior prefrontal cortex and posterior parietal cortex).

The consensus view of cortical thinning (pruning of synapses, axons, and dendrites) is that it is a marker of cortical maturation. Such developmental events refine connectivity within local and widely distributed networks, and enhance the efficiency and fidelity of signal transmission (Tau & Peterson 2010). Supportive evidence for this hypothesis comes from MRI studies examining overall brain metabolism showing that it declines at ages 9–10 years to reach adult levels by age 16–18 years (Chugani 1994). This fine-tuning of neural circuits is believed to be primarily "activity-dependent," that is affected by environmental influences, as well as by genetic factors (Rakic et al. 1994).

Of relevance to a "critical window" for cognitive development, Kostovic et al. (1995) reported that in prefrontal cortex the number of dendritic spines on layer IIIc pyramidal neurons, reflecting associative and commissural projections, peaks at 29 months and declines after 6 years of age. Also, of particular relevance to a critical period hypothesis, is the fact that during the "plateau phase" of brain development (i.e., brain size only grows from 80-to-90 % of adult size between the ages of 2-to-5 years), both myelination expansion and synaptic shaping are particularly active.

Travis et al. (2004) examined the basilar dendritic systems of pyramidal neurons across hierarchically arranged regions of human infant cortex to compare regional differences in neonate brains, as well as comparing infant regional dendritic patterns to well established adult patterns. Tissue blocks were removed from four areas in the left hemisphere—primary (Brodmann area [BA] 4, BA 3-1-2), unimodal (BA 18), and supramodal (BA 10) from four neurologically normal neonates (aged 1 to 41 days). Their findings revealed that the relative regional pattern of dendritic complexity in the neonate was basically the inverse of adult patterns. In infants BA 4 exceeded BA 10 in total dendritic length (52 % greater) and dendritic spine number (67 % greater). The inverse is true in adult brains, with BA 10 > BA 4. Thus, the developmental time course of basilar dendritic systems is heterochronous and more protracted for supramodal cortex regions (BA 10) than for primary or unimodal regions. The more complex pyramidal neurons in the adult are relatively immature at birth, develop more slowly than those in primary cortical regions, but ultimately exhibit more complex dendritic/spine systems with aging.

4. Dendritic Branching Patterns in Language Areas of Postmortem Human Brain Tissue

Arnold Scheibel was one of the earliest to speculate that the pattern and complexity of a neuron's dendritic arborization, specifically the number and length of furcating branches, could be a biological marker reflecting the level of computational processing of those neurons. The dendritic branching patterns emanating, in three-dimensional space, from a neuron's soma, were thought to reflect a "fossilized record" of the ontogenetic growth pattern of that neuron, from gestation to maturation during the individual's life span (Scheibel et al. 1985, Jacobs & Scheibel 1993, Jacobs et al. 1993).

In the original seminal study (Scheibel et al. 1985), human brain tissue was obtained following autopsies of 20 cadavers (10 male and 10 female) with mean ages of 52.2 and 47.8 years respectively. Tissue blocks (1–2 cm along the long axis of the gyrus) were removed from similar areas of both hemispheres (prefrontal cortex and Broca's area). Staining and empirical analyses were restricted to the (easier to trace) basilar dendrites of supragranular pyramidal cells in lamina III of the cortex. Apical dendrite branches proved too difficult to confidently identify and measure individual branches due to the high "neighborhood density" of nearby branches from other cells (Jacobs, personal communication).

The entirety of each neuron's dendritic arborizations were traced by means of a camera lucida, entered to a computer via a digitizing tablet, where lengths of the branching orders were meticulously measured in micrometers. Dendritic branches arising from the soma of a neuron were labeled in sequence such that the initial branch was the first-order segment until it furcates into second-order segments, which then furcate into third-order segments, etc. Segments 1–3 were labeled as proximal, and segments 4–6 as distal branches, which appear with developing age. The lengths of the 3D dendritic segments were labeled as $x_1\ y_1\ z_1$; $x_2\ y_2\ z_2$; $x_3\ y_3\ z_3$; $x_4\ y_4\ z_4$; etc.

The total dendritic length (tdl) of right and left hemisphere neurons (adding the cumulative lengths of all six branch segments) were comparable, but interestingly, the higher order branches 4–6 in the left hemisphere contributed a significantly greater percentage to the tdl relative to the right hemisphere's higher order distal branches. Stated in another way, even though the total dendritic lengths were basically identical, the higher order distal branches were distinctively and relatively longer in left language areas relative to their homologue in the right hemisphere. A relatively longer dendritic branch allows for a greater number of dendritic spines to establish functional synapses onto that neuron, and thus provide a richer synaptic input pattern onto that neuron. Proximal segments are known to develop early ontogenetically and appear, despite continued growth and resorptions, to be relatively stable across epigenetic influences in animals (Carughi et al. 1989). Distal branches develop later ontogenetically, and are remarkably sensitive to environmental and activity-dependent influences.

In a similar study, Jacobs et al. (1993) analyzed basilar dendrites of supragranular pyramidal neurons from blocks of tissue extracted from left hemisphere superior temporal cortex (so-called "Wernicke's area"). Postmortem tissue samples were obtained from 20 neurologically normal individuals (10 males and 10 females ranging in age from 13 to 47). Education level (less than high school, high school, and university) was available. Significant inter-individual variation in dendritic measurements (total dendritic length, mean dendritic length, and dendritic segment count) were reported that roughly reflected individuals' personal backgrounds. The most pronounced finding was in total dendritic length of 3rd and 4th order branches as education levels increased. Distal branches were 76 % longer than proximal branches, and also exhibited greater variability. The effect of environmental enrichment on shaping the neuronal cortical micro-structure of lab animals (e.g., Greenough et al. 1973, Green & Greenough 1986) is well established. The results of Jacobs et al. (1993) seem to extend this finding to morphological growth patterns in human brain tissue.

5. Dendritic Morphological Development in Infants and Early Childhood

Scheibel (1992) examined postmortem tissue samples of infants ($n = 17$) spanning the ages of 3 to 72 months. The number of brains examined in each age grouping was four at 3 months, three at 5–6 months, two at 12–15 months, four at 24–36 months, and four at 42–72 months. The analysis was limited to infragranular pyramidal cells of layer V. As in all previous studies by the Scheibel group, the dendritic number and length measures were taken along the 3D branching sequence as 1st order, 2nd order, 3rd order, etc. A total of 276 neurons were analyzed, yielding 941 1st order dendrites, and several 1000's of higher order branches. Specimen tissue samples were obtained from four cortical sites: left and right pars opercularis and triangularis (Broca's area), and the most inferior gyri of the precentral gyrus, an area destined to mediate motor control of the orofacial musculature.

Figure 1 presents stained images of neurons analyzed at 3 months, 24 months, and 72 months, obtained from Broca's and oro-motor cortical regions, from right and left hemispheres. One developmental factor that can be ascertained from this image is that higher order (4th and 5th) distal dendritic branches have already formed by 24 months in both hemispheres, but appearing somewhat more dense in the left hemisphere in this isolated cross-sectional sample at 72 months.

The major conclusions from this study were: (1) histological development of the right hemisphere greatly exceeded that of the left at birth, but was gradually overtaken and surpassed by the left hemisphere as language capacity developed; (2) the rate of dendritic development, as measured by number and length of branches, was initially greater in motor speech areas controlling orofacial and laryngopharyngeal musculature relative to BA 44 and 45, thought to mediate more language-related activity; (3) though there was no significant difference in total dendritic length across the two hemispheres, the more proximal branches (1st, 2nd, 3rd) tended to be shorter, and the longer more distal branches (4th, 5th, 6th) were more numerous in the left hemisphere. Proximal branches develop during the prenatal period, while distal branches emerge and grow appreciably later during the postnatal period. Furthermore, the more distal branches are the only segments likely to respond to variations of environmental sensory input (Diamond 1988). Complicating this issue, however, is the more recent findings of Travis et al. (2004), also examining basilar dendritic neuropils of pyramidal neurons, that showed the order of synaptogenesis maturation in infants was 180 degrees out of phase with adult brains when relative cortical regions were compared.

6. Caveats in Attempting to Identify Micro-Level Neural Correlates Underlying Language Acquisition

The basic premise of the Scheibel group (particularly Scheibel et al. 1985 and Jacobs et al. 1993), was that (1) greater relative lengths of distal dendritic branches of cortical pyramidal neurons (in left hemisphere language areas) provide a larger surface area, thus allowing for greater proliferation of dendritic spines, and hence higher densities of potential synaptic inputs to that neuron. This section now provides neuroanatomically-based arguments that dilute the Scheibel hypothesis.

Measuring specific features of morphological cell structure from postmortem tissue specimens has serious limitations. At the core of the limitations are sampling issues. After staining a small "snapshot" of brain tissue, it is microscopically examined, searching for visible and structurally intact dendritic arborizations. In the Scheibel studies ten neurons (per hemisphere) were randomly selected and analyzed from each individual brain, following standard criteria. To put these numbers into proper perspective, it has recently been estimated that the human brain contains approximately 86 billion neurons (Herculano-Houzel & Lent 2005). The numerology of estimating synapses is not as precise, with a range of 10^{14} to 10^{15} (100 trillion to a quadrillion). In more manageable numbers, a cubic millimeter of brain tissue has been estimated to contain approximately 100,000 neurons and 1 billion synapses (Hubel 1979). Whatever the exact figure, do the math. More importantly, there is no way of knowing whether the few neurons measured by the Scheibel lab were actually engaged in language-related processing during the

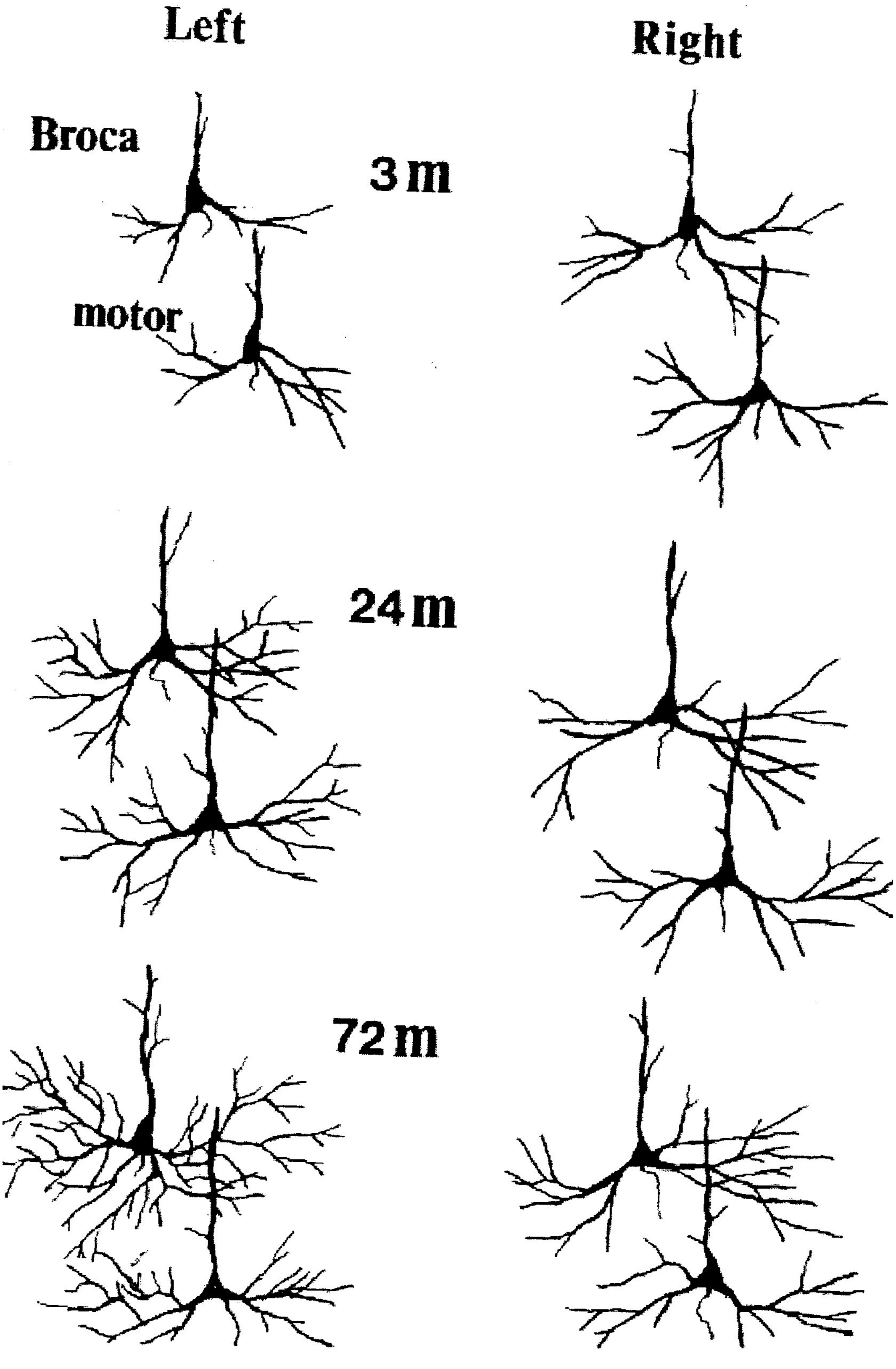


Figure 1: Stained images of neurons analyzed at 3 months, 24 months, and 72 months, obtained from Broca's and oro-motor cortical regions, from right and left hemispheres.

life span of the deceased, and moreover, nothing can be ascertained about where afferent inputs were coming from, or where axonal outputs were going.

Another complicating issue is that neurons are likely to operate in a functionally shared manner—neuron X may be functionally active during a particular language function, say syntax, and also active in concert with different networks when performing mathematical operations, or accessing memory/attentional circuitry at another moment in time (Fuster 2006). The newly recognized relevance of a "temporal view" in speech perception, namely how different oscillating brain rhythms may be scaled for the encoding of varying, linguistic-based, information chunks (e.g., 500–2000 msec for intonational contours, 150–300 msec for syllable structure, 20–80 msec for rapidly changing featural information), suggests that the same underlying neural substrate can be operative for different encoding/decoding operations across different temporal processing windows (Poeppel 2014, Giraud & Poeppel, 2012, Luo & Poeppel, 2007). Thus, a specific morphological feature of the neuropil may play different roles relative to the changing time constants in play at the moment. However, a counter argument can also be made that the greater overall complexity of synaptic connections (due to longer distal dendritic branches) can provide the means for efficient "multi-memberships" across the various time constants of functional cognitive processing.

Another issue is the simple fact that language is not a single, stand alone, cognitive function. Neural circuitry underlying memory and attention must also co-develop and interactively support language-based operations. So, when a single layer III pyramidal neuron is selected to examine its dendritic/spine morphology, that neuron's specific cognitive role is unknown and, moreover, indeed unknowable.

Another sampling-based caveat is that all the tissue samples used in investigations measuring dendritic branching/spine structure were from the cerebral cortex. The cerebral cortex receives all of its inputs directly from the thalamus, and is constantly in reciprocal communication with the thalamus, which is, understandably, rarely analyzed in human postmortem anatomical studies (Sherman & Guillery 2001). In language functioning, both the cerebellum and basal ganglia have crucial roles in both planning and selecting aspects of speech motor activations. These subcortical structures reach developmental maturation before synaptogenesis is fine-tuned in the cerebral cortex (Tau & Peterson 2010), but do not usually get discussed when language acquisition in the context of a critical period is discussed.

Additional sampling alternatives of what and where to measure can also influence outcomes. Which is more relevant — the *postsynaptic matrix* or the *presynaptic matrix*? The Scheibel group has settled exclusively on the postsynaptic matrix, the graded potentials arriving onto the dendritic branching arbors of the receiving neuron's cell body. What about the presynaptic matrix? How does density of axon boutons affect emerging circuits, or stage of myelin development, or number of collateral branches off an axon?

Compounding the overabundance of possible morphological features to analyze, another concern is the continued growth of neurons. Elston & Fujita, in their extensive staining studies across sensory, association, and executive human cortex, concluded that

> dendrites and spines of pyramidal cells continue to grow throughout the entire life cycle, including infancy, childhood, adolescence, adulthood and senescence, as a normal process [. . .].
>
> (Elston & Fujita 2014: 6)

They go on to say that

> [i]n essence, though there may be a net reduction in spine density in the dendritic trees of cells from 3.5 months of age into adulthood, new spines are continually grown through this period.
>
> (Elston & Fujita 2014: 6)

Perhaps sheer numbers of dendritic spines is not really that important. Regressive neurogenesis prunes away millions of non-functional synapses throughout development. A relatively smaller number of highly functional synapses is what gets the job done, not the over abundances characterizing earlier stages of development.

Yet another problem in selecting brain tissue samples is the vast differences encountered as a function of cortex region. Jacobs et al. (2001), explored differences in dendritic/spine extent across several human cortical regions. They limited their sampling to basilar dendrites/spines of supragranular pyramidal cells across eight different Brodmann areas that spanned a functional hierarchy from primary cortex (somatosensory BA 3-1-2; motor, BA 4); unimodal cortex (Wernicke's area, BA 22; Broca's area (BA 44); heteromodal cortex (supplementary motor area, BA 6; angular gyrus, BA 39); and supramodal cortex (superior frontopolar zone, BA 10; inferior frontopolar zone, BA 11). Primary and unimodal cortices were designated as low integrative regions relative to heteromodal and supramodal areas which were designated as high integrative areas. Tissue samples were only obtained from the left hemisphere, from 10 neurologically normal individuals, five male and five female, spanning the age interval of 13–47. Ten neurons were sampled from each region, and evaluated by total dendritic length, mean segment length, dendritic segment count, dendritic spine number, and dendritic spine density. Dendritic systems in primary and unimodal regions were consistently less complex than in heteromodal and supramodal areas. For example, total dendritic length in BA 10 (frontal cortex) was 31 % greater than in somatosensory cortex (BA 3-1-2), while dendritic spine number was 69 % greater. Jacobs et al. conclude:

> These findings demonstrate that cortical regions involved in the early stages of processing (e.g. primary sensory areas) generally exhibit less complex dendritic/spine systems than those regions involved in the later stages of information processing (e.g., prefrontal cortex).
>
> (Jacobs et al. 2001: 558)

A complex cognitive function such as language spans the entire range of this cortical hierarchy—primary, unimodal, heteromodal, and supramodal, and in most instances, in ways not even remotely understood.

7. Conclusions

The human brain is the most complex biological structure to ever evolve, and language is perhaps the most miraculous emergent property emanating from this "alien" structure. The relative dendritic lengths of a miniscule number of randomly analyzed neurons cannot, unfortunately, provide the necessary insights to understand how a child develops language in the first decade of life. What is inescapable is the fact that a child's self-generated oral movements and reception of speech sounds, shape their developing connectomes when it is extremely ripe for input, learning and eventual fine tuning, just like Eric Lenneberg said fifty years ago.

Perhaps the only realistic methodological approach in seeking potential neural correlates of language acquisition is to (i) enlarge one's observational lens and (ii) restrict your search to post mortem tissue samples from brains of extraordinarily (linguistically-speaking) gifted individuals.

With regard to (ii), Amunts et al. (2004) performed cytoarchitectonic scrutiny (morphometry and multivariate statistical analyses) of random cortical tissue samples within BA 44 and 45, from the brain of Emil Krebs (1867–1930), a diplomat who was known to have fluently spoken more than 60 languages. Histological sections (20 μm), taken from both hemispheres, were stained to see only cell bodies. Visual area BA 18 was also included as a comparison to language-related neocortex. The fraction of cortical volume occupied by cell bodies was estimated by measuring the the gray level index (GLI), using an image analyzer. The laminar changes in the volume of cell bodies, from the pial surface to the white matter, were quantified and displayed as GLI profiles, consisting of 10 shape-describing features. The 10 features were subsequently used in a canonical principal component analysis that clearly showed the separation distances of Krebs' cytoarchitecture features from control brains. The cytoarchitecture of Krebs' brain differed significantly from the controls in both left and right hemisphere BA 44, but was most pronounced in right hemisphere BA 45. Of the 10 statistical features of the GLI profile, the maximal difference concerned the feature "dsk," which is an index of the degree of lamination between supra-granular layers (I to III) and infragranular layers (V and VI). Another profile feature (dmGLI), which reflects variations in volume fraction of cell bodies between cortical layers and sublayers across the whole cortex, revealed striking differences between Krebs and the control brains. Krebs' brain had different arrangements of cell bodies across the cortical lamina relative to controls, but not in the absolute amount of neuropil. There were no differences found in cytoarchitecture laminations in visual area BA 18, and thus Krebs' brain exhibited "a local microstructural specialization" (Aumnts et al. 2004: 350). Interestingly, Krebs' brain showed a highly unusual pattern of hemispheric asymmetries—maximal cytoarchitectonic symmetry for BA 44, and maximal hemispheric asymmetry for BA 45, which may have enhanced his multi-lingual talents.

So, in one respect Eric Lennenberg has been proven wrong, as he once said:

> There is no cytoarchitectural peculiarity of the cortical areas involved in language. (Lenneberg 1967: 61)

But Lennenberg was most likely talking about normal human beings, not Emil Krebs. Unfortunately, opportunities to examine brain tissue from individuals like Emil Krebs do not come along too often.

References

Amunts, Katrin, Axel Schleicher & Karl Zilles. 2004. Outstanding language competence and cytoarchitecture in Broca's speech region. *Brain and Language* 89, 346–353. doi:10.1016/S0093-934X(03)00360-2.

Ashtari, Manzar, Kelly Cervellione, Khader Hasan et al. 2007. White matter development during late adolescence in healthy males: A cross-sectional diffusion tensor imaging study. *NeuroImage* 35(2), 501–510. doi:10.1016/j.neuroimage.2006.10.047.

Barkat, Tania, Daniel Polley & Takao Hensch. 2011. A critical period for auditory thalamocortical connectivity. *Nature Neuroscience* 14(9), 1189–1196. doi:10.1038/nn.2882.

Birdsong, David. In press. Critical periods. In M. Aronoff (ed.), *Oxford Bibliographies in Linguistics*. New York, NY: Oxford University Press.

Carughi, Arianna, Kenneth Carpenter, & Marian Diamond. 1989. Effect of environmental enrichment during nutritional rehabilitation on body growth, blood parameters and cerebral cortical development of rats. *Journal of Nutrition* 119, 2005–2016.

Chugani, Harry. 1994. Development of regional brain glucose metabolism in relation to behavior and plasticity. In G. Dawson & K. W. Fischer (eds.), *Human Behavior and the Developing Brain*. New York, NY: Guilford,153–175.

Diamond, Marian. 1988. *Enriching heredity: The Impact of the Environment on the Anatomy of the Brain*. New York, NY: Free Press.

Elston, Guy & Ichiro Fujita. 2014. Pyramidal cell development: postnatal spinogenesis, dendritic growth, axon growth, and electrophysiology. *Frontiers in Neuroanatomy* 8, 78. doi:10.3389/fnana,2014.00078.

Fuster, Joachim. 2006. The Cognit: A network model of cortical representation. *International Journal of Psychophysiology* 60, 125–132. doi:10.1038/nn.3063.

Giraud, Anne-Lise & Poeppel, David. 2012. Cortical oscillations and speech processing: Emerging computational principles and operations. *Nature Neuroscience* 15, 511–517. doi:10.1016/j.ijpsycho2005. 12. 015.

Gogtay, Nitin, Jay Giedd, L. Lusk et al. 2004. Dynamic mapping of human cortical development during childhood through early adulthood. *Proceedings of the National Academy of Science of the United States of America* 101, 8174–8179. doi:10.1073/pnas.0402680101.

Green, Edward & William Greenough. 1986. Altered synaptic transmission in dentate gyrus of rats reared in complex environments: Evidence from hippocampal slices maintained in vitro. *Journal of Neurophysiology* 55, 739–749.

Greenough, William, A. Yuwiler & M. Dollinger. 1973. Effects of posttrial serine administration on learning in "enriched" and "impoverished" reared rats. *Behavioral Biology* 8, 261–272.

Harrison, Robert, Karen Gordon & Richard Mount. 2004. Is there a critical period for cochlear implantation in congenitally deaf children? Analyses of hearing and speech performance after implantation. *Developmental Psychobiology* 46(3), 252–261. doi:10.1002/dev.20052.

Herculano-Houzel, Suzana & Roberto Lent. 2005. Isotropic fractionator: A simple rapid method for the quantification of total cell and neuron numbers in the brain. *Journal of Neuroscience* 25, 2518–2521. doi:10.1523/JNEUROSCI.4526-04.2005.

Hubel, David. 1979. The brain. *Scientific American* 241, 44–65.

Huttenlocher, Peter & Arun S. Dabholkar. 1997. Regional differences in synaptogenesis in human cerebral cortex. *Journal of Comparative Neurology* 387, 167–178. doi:10.1002/(SICI)1096-9861(19971020)387:2<167::AID-CNE1>3.0.CO;2-Z.

Jacobs, Bob, Matthew Schall, Melissa Prather, Elisa Kapler et al. 2001. Regional dendritic and spine variation in human cerebral cortex: A quantitative Golgi study. *Cerebral Cortex* 11, 558–571. PMID:11375917.

Jacobs, Bob & Arnold Scheibel. 1993. A quantitative dendritic analysis of Wernicke's area in humans. I. Life span changes. *Journal of Comparative Neurology* 327, 83–96. doi:10.1002/cne.903270107.

Jabobs, Bob, Matthew Schall, & Arnold Scheibel. 1993. A quantitative dendritic analysis of Wernicke's area in humans. II. Gender, hemispheric, and environmental factors. *Journal of Comparative Neurology* 327, 97–111. doi:10.1002/cne.903270108.

Knickmeyer, Rebecca, Sylvain Gouttard, Chaeryon Kang et al. 2008. A structural MRI study of human brain development from birth to 2 years. *Journal of Neuroscience* 28, 12176–12182. doi:10.1523/JNEUROSCI.3479-08.2008.

Kostovic, Ivica, Milos Judas, Zagreb Petanjek & Goran Simic. 1995. Ontogenesis of goal-directed behavior: Anatomo-functional considerations. *International Journal of Psycho-physiology* 19, 517–527. doi:10.1016/0167-8760(94)00081-O.

Lenneberg, Eric H. 1967. *Biological foundations of language*. New York, NY: Wiley.

Levitt, Pat. 2003. Structural and functional maturation of the developing primate brain. *Journal of Pediatrics* 143, S35–S45. doi:10.1067/S0022-3476(03)00400-1.

Levi, Dennis & Roger W. Li. 2009. Improving the performance of the amblyopic visual system. *Philosophical Transactions of the Royal Society of London B: Biological Sciences* 364, 399–407. doi:10.1098/rstb.2008.0203.

Luo, Huan & Poeppel, David. 2007. Phase patterns of neuronal responses reliably discriminate speech in human auditory cortex. *Neuron* 54, 1001–1010. doi:10.1016/j.neuron.2007.06.004.

Maurer, Daphne, Terri Lewis, Henry Brent & Alex Levin. 1999. Rapid improvement in the acuity of infants after visual input. *Science* 286, 108–110. doi:10.1126/science.286.5437.108.

Mrzljak, Ladislav, Harry Uylings, Ivica Kostovic & Corbert van Eden, 1992. Prenatal development of neurons in the human prefrontal cortex. I. A quantitative Golgi study. *Journal of Comparative Neurology* 316, 485–496.

Paus, Tomas, Louis Collins, A. C. Evans, G. Leonard et al. 2001. Maturation of white matter in the human brain: A review of magnetic resonance studies. *Brain Research Bulletin* 54, 255–266. doi:10.1016/S0361-9230(00)00434-2.

Poeppel, David. 2014. The neuroanatomic and neurophysiological infrastructure for speech and language. *Current Opinion in Neurobiology* 28,142-149. doi:10.1016/j.conb.2014.07.005.

Rakic, Pasko, Joan Bourgeois, Patricia Goldman-Rakic. 1994. Synaptic development of the cerebral cortex: implications for learning, memory, and mental illness. *Progress in Brain Research* 102, 227–243.

Scheibel, Arnold. 1990. Dendritic correlates of higher cognitive function. In A. Scheibel & A. Wechsler (eds.), *Neurobiology of higher cognitive function*, New York, NY: Guilford, 239–270.

Scheibel, Arnold. 1992. Dendritic structure and language development. In B. de Boysson-Bardies, S. de Schonen, P. Jusczyk, P. MacNeilage, & J. Morton (eds.), *Developmental neurocognition: Speech and face processing in the first year of life*. Dortrecht, The Netherlands: Springer, 51–62.

Scheibel, Arnold, L. A. Paul, I. Fried et al. 1985. Dendritic organization of the anterior speech area. *Experimental Neurology*, 87, 109–117. PMID:3967694.

Sherman, Murray & Ray Guillery. 2001. *Exploring the Thalamus*. San Diego, CA: Academic Press.

Tau, Gregory & Bradley S. Peterson. 2010. Normal development of brain circuits. *Neuropsychopharmacology Reviews* 35, 147–168. doi:10.1038/npp.2009.115.

Travis, Katie, Kevin Ford & Bob Jacobs. 2004. Regional dendritic variation in neonatal human cortex: A quantitative Golgi study. *Developmental Neuroscience* 27(5), 277–287. doi:10.1159/000086707.

Wiesel, Thorton & David Hubel. 1963. Single-cell responses in striate cortex of kittens deprived of vision in one eye. *Journal of Neurophysiology* 26, 1003–1017. PMID:14084161.

Wiesel, Thorton & David Hubel. 1965. Comparison of the effects of unilateral and bilateral eye closure on cortical unit responses in kittens. *Journal of Neurophysiology*, 28, 1029–1040. PMID:5883730.

Zhou, Xiaolin & Michael Merzenich. 2008. Enduring effects of early structured noise exposure on temporal modulation in the primary auditory cortex. *Proceedings of the National Academy of Sciences of the United States of America* 105(11), 4423–4428. doi:10.1073/pnas.0800009105.

Harvey M. Sussman
University of Texas at Austin
Departments of Linguistics and
Communication Sciences & Disorders
305 E. 23rd St. (B5100)
Austin, TX 78712
United States of America

sussman@austin.utexas.edu

Innate Mechanisms for Acquiring Syntactic Displacement

Misha K. Becker

1. Introduction

The central arguments within Lenneberg's thesis of a biological basis for language are the species-specific nature of the physiological and neurological structures that make language possible, the cross-species uniformity of language development (the fact of its acquisition as well as its developmental path, irrespective of culture, race, etc.; excepting cases of pathology), and the transformational nature of syntax. Transformational syntax forms an important piece of support for Lenneberg's *discontinuity* theory of the evolution of language, meaning that human language has not descended directly from communication systems found in non-human animals (i.e., our shared ancestors). This is because transformational syntax is also species-specific, i.e. not found in the communication systems of other animals.

Transformational syntax allows us to convey complex and abstract meanings, rather than being limited to the here-and-now (e.g. alarm calls) or to simple semantic relations, and it enables us to transform our expressions through syntactic displacement, or movement. In this short paper I will address some questions about how human children come to acquire the meanings of semantically abstract predicates, how they figure out which strings of words are generated by displacing operations, and the sense in which the tools that allow children to acquire both these things are innate. The inspiration for this research can be traced to some of the central themes in Lenneberg's important work.

2. Displacement

The examples Lenneberg drew on to illustrate the transformational nature of human language syntax involved syntactic ambiguity: the surface form *They are boring students* is compatible with two underlying structures, neither of which is given ostensively to the child learner. Such types of sentences present an interesting puzzle and a demonstration of the necessity for learners to determine the underlying structure of a sentence which may not be straightforward to determine from its surface properties. Another example of this puzzle, and one that intersects with the puzzle of children's word learning (in particular, predicate learning), concerns what Lees called "multiply ambiguous adjectival constructions," such as *John is tough to please* (Lees 1960). It is not that the sentence itself is structurally ambiguous, but rather it has a "constructional homonym" (Chomsky 1964) in surface-similar sentences like *John is eager to please*. Thus, while not ambiguous in the adult grammar, the string of words cannot be mapped unambiguously onto its underlying structure without

biolinguistics

ISSN 1450–3417

prior knowledge of what the main predicate means. In the case of children, however, we can't assume that the lexical meaning of such abstract predicates is known a priori.

Within movement-based approaches to syntax, such as Government–Binding Theory or Minimalism, phrases are generated in one position, as determined by their argument structure relations, and then they can move to a different position in the sentence (driven and constrained by structural requirements) where they are pronounced on the surface. When NPs are displaced like this there is a difference between where the phrase is spoken and where it is interpreted. In order to represent these sentences correctly, children have to figure out where the displaced NP is interpreted.

I will limit the discussion here mostly to raising (1) and *tough*-constructions (2).

(1) Donald$_i$ seems [t_i to be lying]

(2) Melania$_i$ is tough [PRO$_i$ to please t]

The well-known structure in (1) presents a case in which the matrix subject *Donald* has raised into the subject position of the main clause from the subject position of the embedded clause. Thus, although *Donald* is local to *seem* on the surface, its selecting predicate (the predicate it is semantically related to) is the non-local predicate *be lying*. *Tough*-constructions have evaded a clear syntactic analysis within the GB and Minimalist frameworks (Chomsky 1977, Lasnik & Uriagereka 1994; but see Hicks 2009 for one recent Minimalist account), but they likewise involve a structure in which the matrix subject is not interpreted in its surface position. Instead, it is interpreted as the object of the embedded verb (in this case, the theme of *please*). Thus, in both of these constructions the subject of the main clause is not an argument of the (surface-local) matrix predicate (*seem* or *be tough*).

The question for acquisition, then, is how learners figure out the underlying structure and interpretation of these sentences. A possible answer comes from the Semantic and Syntactic Bootstrapping hypotheses, two accounts of how children use inductive biases about language structure in order to "bootstrap" themselves into the grammar. In a nutshell, Semantic Bootstrapping is the idea that since there is a regular and (mostly) reliable mapping between semantics and the syntactic category or function of constituents (nouns often label objects (DP in VP), verbs (vP/VP) often label events and states, subjects (DP in vP) are often agents), children could use the expectation of this mapping to figure out some basic aspects of syntax, such as word order (Grimshaw 1981, Pinker 1982). If you hear the sentence *The dog is chasing the cat* while viewing a scene in which a dog is chasing a cat, and if you know which objects the nouns *dog* and *cat* map onto and that the agent (the chaser) should be the syntactic subject, and if you can infer that *chase* means something like "pursue," then you could determine that your language has Subject-Verb-Object (SVO) word order. In this way, the child uses expectations about semantic patterns to bootstrap into syntax. The expectations themselves are taken to be innate (not learned from experience) and specific to language (that is, domain specific).

Evidence for the bias to map animate or agentive NPs to the most prominent syntactic position comes not only from psycholinguistic studies with children and adults (Clark & Begun 1971, Trueswell & Tanenhaus 1994, Mak et al. 2002, Traxler et al. 2005, Becker 2005, Becker & Estigarribia 2013), but also from argument hierarchies in language that are based on broad typological patterns. Well-known hierarchies such as the Animacy Hierarchy (Silverstein 1976, Dixon 1979) and the Thematic Hierarchy (Jackendoff 1972), as well as the Promotion to Subject Hierarchy (Keenan 1976) show how widespread the preference is in human language to associate animacy and agency with syntactic prominence, and inanimacy with lower positions. While this does not in itself necessitate an appeal to innateness, many linguists believe that typological universals are good candidates for innate components of language. Furthermore, there is recent neurolinguistic evidence for anatomically distinct cortical areas for representing agents and patients in sentences (Frankland & Greene 2015). Once again, by itself this does not necessarily point to innate knowledge of agents vs. patients, but combined with typological universals and psycholingusitic evidence, it is strongly suggestive of a biological basis or predisposition for this knowledge—as Lenneberg already recognized (see Piattelli-Palmarini, this issue for discussion).

There is wide support for the biases underlying Semantic Bootstrapping aiding early parsing of what Keenan (1976) called "basic sentences." One problem in applying this type of approach to sentences like (2) and (1), however, is that, as mentioned above, these sentences have "constructional homonyms," surface-similar sentences that are generated by very different underlying structures. They are exemplified in (3) and (4), respectively.

(3) Donald$_i$ claims [PRO$_i$ to be tremendous]

(4) Melania$_i$ is eager [PRO$_i$ to please e]

Thus, these constructions provide another example of the challenge Lenneberg alluded to with structurally ambiguous sentences. Prior to knowing what *claim* or *seem* means, the sentence is structurally ambiguous. A second shortcoming of Semantic Bootstrapping is that this procedure depends upon learners' ability to infer the meanings of individual words from observing what is going on in the world when words are uttered. Rarely are individual words used to label events or properties in isolation, outside of the context of a sentence. Thus, while it may be reasonble to assume that children could figure out the meanings of some concrete nouns (*dog, cat*) from observing the world, most words, verbs in particular, are not learned that way (parents might say "Look at the *dog!*" but probably not "Look at *chasing!*"). If this criticism holds with verbs like *chase*, consider how much more problematic it would be to have to figure out the meaning of *seem* based on observation of the world.

Gleitman (1990) proposed a solution that exploited the predictable relationships between the argument structure frames a verb may participate in (that is, the number and category of "arguments," or phrases a verb selects as the participants in the verb's event) and the semantics of that verb. Her idea was that if children anticipated such relationships, they could use argument structure to bootstrap into verb meanings. Argument structure frames do not tell you exactly what an individual verb means (certainly, transitive sentence frames admit thousands of verb meanings), but they can help discriminate verbs that denote an individual action or change of state (1 argument; 5a) from verbs of contact or causation (2 arguments; 5b) from verbs of transfer (3 arguments; 5c; Landau & Gleitman 1985, Gleitman

1990). Crucially, this procedure works particularly well for certain verbs that denote unobservable states such as mental states and beliefs, like (5d; Gleitman et al. 2005.

(5) a. Jeff slept.
(1 NP argument: intransitive, individual action/change of state)
b. Jim hugged Melissa. (2 NP arguments: transitive; contact, causation)
c. Tommie offered [his constituents] [options]
(3 arguments: ditransitive; transfer)
d. Theresa regrets [that she called for an election]
(NP subject and clausal complement; mental verb)

According to this procedure, known as Syntactic Bootstrapping, learners use expectations about syntax to map whole sentences onto events in the world and thereby bootstrap into verb meanings. While Semantic and Syntactic Bootstrapping aim to explain different aspects of how children learn syntax and argument structure, they both rely on the fact that argument structure relations must be discernable from the surface forms of sentences; that is, they are surface-local. More must be said, however, when thematic relations among words in a sentence span long distances. What happens when the noun next to a verb is not the verb's argument? Precisely this situation arises in raising and *tough*-constructions.

In brief, both Semantic and Syntactic Bootstrapping employ inductive bias to help children determine the syntactic and semantic properties of "basic sentences." But both approaches fall short when we consider displacing constructions, which illustrate one of the core properties of human language and present a similar puzzle to what Lenneberg cited as evidence for the transformational nature of syntax. The solution to how children figure out these constructions will rest on a number of innate biases.

3. Toward a Solution

3.1. Inanimate Subjects as Cues to Structure

We just saw that both Semantic and Syntactic Bootstrapping rely on inductive biases to associate certain semantic roles with certain syntactic roles or grammatical categories, and to associate certain argument structure patterns with certain subcategories of predicate meanings. Both the semantic roles and the argument structure relations are projected locally and, in the canonical case, computed locally. These biases are taken to be innate in the sense that they are specific to language (which in turn is species-specific), and apply cross-linguistically (and thus species-wide; see Lee & Naigles 2008 for cross-linguistic evidence).

With these biases in place, the learner could identify sentences that break with the canonical pattern if there is some feature of these non-canonical sentences that could serve as a trigger. In my previous work I have argued that hearing an inanimate subject in sentences like (1) and (2) can provide such a trigger by signaling that the subject and main predicate are not thematically related (i.e. not in an argument structure relationship), and therefore that the subject is displaced from elsewhere in the sentence (Becker 2005, 2006, 2009, 2014, 2015, Mitchener & Becker 2011, Becker

& Estigarribia 2013). Inanimate subjects provide a helpful clue because inanimate NPs cannot be agents or experiencers and therefore can't be the subject argument of a control predicate (the ones found in (3) and (4)). All languages which contain both raising and *tough* constructions, to my knowledge, allow inanimate NPs as subjects of raising and *tough* predicates and disallow inanimate NPs as subjects of control predicates (various Indo-European languages, plus Tongan, Samoan, Niuean, Chamorro and Maori for raising, and Finnish, Mandarin, Labrador Inuttut, Niuean and Bahasa Indonesian for *tough* constructions). Moreover, inanimate subjects are more generally restricted: there are a number of languages that simply ban inanimate subjects in monoclausal transitive contexts (Japanese, Jacaltec, Navajo, Tlapanec, Blackfoot), and in languages that allow inanimate subjects in such contexts there are restrictions on their distribution that do not apply to animate subjects (Chung 1983, Comrie 1989, Dahl & Fraurud 1996).

In short, inanimate subjects are well tolerated as NPs that have undergone the kind of displacement in raising and *tough*-movement, but not as well tolerated as NPs that are underlyingly generated as external arguments—that is, as underlying subjects.[1]

In terms of the learning procedure, if children are biased to expect sentence subjects to be agents of the main predicate, and if they further assume that inanimate things cannot be agents (supported by both research on conceptual development, e.g., Woodward et al. 1993, Poulin-Dubois et al. 1996, and linguistic development, e.g., Corrigan 1988, Scott & Fisher 2009), then an inanimate NP in subject position should indicate that there is something out of the ordinary going on with the sentence. It is either a syntactic object, implying an alternative word order in just these sentences, or it is a displaced subject. My contention is that learners take it to be a displaced subject. In my experimental work with adults and children, I have found that by manipulating the animacy of subjects of sentences with infinitive complements we can see asymmetries in how people parse these sentences: an animate subject tends to lead people to interpret the subject as an argument of the main predicate, as it would be in a control structure, and an inanimate subject tends to lead people to interpret the subject as being an argument only of the lower predicate, as it would be in a raising or *tough* construction.

It is important to note that *tough*- and raising constructions have been argued to be acquired relatively late in development. Chomsky (1969), Cromer (1970) and, more recently, Anderson (2005) have shown that children go through a long stage of misinterpreting the subject of a *tough*-construction as coreferent with the subject of the embedded clause rather than the object (i.e., they take the matrix subject to refer to the "pleaser" in (4)—note that this doesn't mean children cannot interpret the subject as displaced, rather they haven't figured out exactly where it is displaced *from*); Hirsch & Wexler (2007), Orfitelli (2012) and others have argued that certain types of raising constructions are acquired late in development, as late as age 7.

---

[1] There is an interesting question about what this would mean for languages that are considered topic-prominent, meaning that they contain topics instead of subjects. As discussed in Becker (2014:p. 290ff), although topics, unlike subjects, are not selected by predicates, and therefore are not as much semantically limited as true subjects are, there are nevertheless strong tendencies both for topics to be associated with agenthood (Li & Thompson 1976, Givón 1976, Schachter 1976) and for topic-prominent languages to also admit subjects when no topic is available, e.g. in Lisu (Li & Thompson 1976).

Although my own empirical work has shown that children may comprehend these structures as early as age 4 it is clear that their knowledge of them is not fully adult-like until later. The relatively late acquisition of these constructions is not in itself problematic for a biological account of language acquisition. Lenneberg notes that the gradual development of language along a specific trajectory is in fact to be expected on the view that it is biologically based; biological structures typically mature along a developmental timeline.

3.2. *Other Displacing Constructions*

I've focused here on inanimate subjects as a cue to raising and *tough*-constructions, but there are other constructions involving displacement, as well as other cues to displacement.[2] I'll touch on some of these constructions briefly.

There are several other types of A(rgument)-movement, the type of displacement I've discussed above, such as passivization, Raising-to-Object, and subject raising with unaccusative verbs (intransitive verbs whose sole argument is internal, rather than external, e.g. *fall, arrive*).[3] There is some evidence that inanimate subjects can serve as a cue to distinguishing these subcategories of verbs as well. In adult grammar unaccusative verbs (*fall, arrive*) freely permit inanimate subjects (see 6), while unergative verbs (*laugh, jump*) typically require animate subjects (see 7).

(6) a. The tree$_i$ fell t_i.
b. The package$_i$ arrived t_i.

(7) a. # The tree laughed.
b. # The package jumped.

There is room for debate about how strictly unergative verbs require an animate subject; Folli & Harley (2008) in fact argue that the relevant feature here is not animacy or agency, but "teleological capability," and that this accounts for the fact that (8a) is well-formed (since trains are capable of emitting a whistling sound) but (8b) is not, unless you add the directional PP *into the room*, since bullets are not capable of sound emission except through their movement.

(8) a. The train whistled.
b. The bullet whistled *(into the room).

There are some other cases in which unergative verbs allow inanimate subjects (e.g. *The machine ran for hours*). Nevertheless, when we look at the distribution of unaccusative and unergative verbs with respect to subject animacy, we see a striking asymmetry: while children produce unergative verbs almost exclusively

---

[2] Space limitations prevent taking up the issue of expletive subjects, which serve as another cue to learners that a given predicate is a raising or *tough* type of predicate (e.g. *It's tough to please John* vs. **It's eager to please John*). Expletives undoubtedly serve as an important cue to a predicate's selectional properties in languages that have them.

[3] I am putting aside constructions involving what is known as A-bar movement (e.g. wh-movement, scrambling, topicalization), which does not move a phrase into a canonical argument position.

with animate subjects (over 93 %), they produce unaccusative verbs with both animate and inanimate subjects in roughly equal proportions (Becker & Schaeffer 2013, Becker 2014). A similar asymmetry is found in parental speech to children (Becker 2014). In addition, experimental work has shown that children interpret an inanimate subject of an intransitive verb as a patient (and the verb as an unaccusative verb), but they interpret an animate subject as an agent (and the verb as unergative; Bunger & Lidz 2008, Scott & Fisher 2009).

The passive construction, like unaccusatives, involves displacement of a verb's internal argument to the subject position on the surface. Like unaccusatives and the other displacing constructions discussed, passives are quite compatible with inanimate subjects. Languages that disallow inanimate subjects of active transitive verbs (noted above, e.g., Japanese, Navajo) freely allow inanimate subjects under passivization. Thus, inanimate subjects could in theory be used as a cue for acquiring, or at least identifying passive constructions. Unfortunately, it is not so clear that inanimate subjects are used by children as cues to passives. To give just one example, Lempert (1989) trained children who were not yet producing passives spontaneously to imitate passive sentences that had either animate or inanimate patients. She then tested these children a few days later to see how many passives they would produce on their own in a picture description task. Lempert found that children who had been trained on sentences with *animate* patients produced the most passives, while children trained with *inanimate* patients produced more active sentences.

An important consideration about passives is that the lexical meaning of a verb does not change according to whether it is used with passive or active voice (*X ate the sandwich* and *The sandwich was eaten* both denote a sandwich-eating event), but the subcategories of predicates that participate in either raising vs. control, *tough* vs. control or unaccusative vs. unergative structures have very different *kinds* of lexical meanings. Table 1 shows a partial list of the meanings of raising and control predicates found in various languages that contain these constructions.

Perlmutter (1978) defined unergative verbs as "predicates describing willed or volitional acts" and unaccusative verbs as denoting "non-voluntary emission of stimuli that impinge on the senses" as well as verbs denoting aspectual and in-

Language	Raising meanings	Control meanings
Chamorro	begin, stop	be afraid
English	seem, appear, turn-out, tend, used-to, gonna	want, try, decide, claim
German	seem, used-to, must	try, forget, forbid
Italian	seem, turn-out	try, claim, pretend
Maori	not, don't	want, decide, go, be able, agree, prepare
Niuean	be possible, begin, not, usual, almost, nearly	try, want, choose
Samoan	be able, be necessary, begin, be (done) thus	want, try, encourage, go, think, be tired of, (dis)like
Tongan	be able, be possible	go, come, stand up, turn, hit

Table 1: A partial list of raising and control predicate meanings in different languages (in alphabetical order top to bottom).

choative properties of events. In languages that have *tough*-adjectives (e.g. English, Finnish, Mandarin, Labrador Inuttut, Niuean and Bahasa Indonesian) the lexical meanings of these predicates display a remarkably tight range of meanings exclusively revolving around ease and difficulty. If an inanimate subject serves as a cue not only for identifying a displaced subject but also for narrowing down the lexical meaning of the predicate in these constructions, then the claim about inanimate subjects should not in fact extend to the case of passives. Space limitations prevent a more thorough exploration of this issue.

3.3. *Animacy*

The ability to use cues from animacy in the service of decoding syntax, whether for canonical or non-canonical structures, depends upon young children's ability to distinguish conceptually between animate and inanimate entities, and to know that animate entities can be agents while inanimate entities cannot. There is a wealth of research on cognitive development showing children's very early ability to make these distinctions (Spelke 1991, Woodward et al. 1993, Poulin-Dubois et al. 1996, Woodward 1998). Much of this research reports infants' and toddlers' ability to distinguish between inanimate objects and humans, but there is strong evidence that preschoolers reason quite differently about the internal properties and potentialities of inanimate objects and non-human animals as well (Carey 1985, Massey & Gelman 1988, Keil 1989, Rakison & Poulin-Dubois 2001), and that they have a sophisticated understanding of the propensity for animals, but not inanimate objects, to have agentive properties (contrary to the common wisdom, due to Piaget, that children are "animistic" in their beliefs). Thus, there is good evidence that young children have reliable knowledge of the animate–inanimate distinction that is not limited to human vs. inanimate entities.

Could knowledge of the animate–inanimate distinction be innate? It is difficult to answer this question, but I'll offer some ways to think about how to approach it. First, there is evidence that from the earliest moments of life, infants attend to human faces but not to rearranged components of a human face (Johnson et al. 1991). This suggests a hard-wired ability to distinguish humans via a salient and prototypically animate property. Cognitive systems for recognizing faces vs. objects are functionally and anatomically distinct (Farah 1995), as are the systems for recognizing animate vs. inanimate objects (Warrington & Shallice 1984), suggesting a differentiation of these categories on a neurological level. The way people reason about animals vs. objects, according to a folk biology or folk taxonomy, is remarkably uniform across the world (Atran 1998), speaking to the species-wide nature of this distinction. Taken together, these facts suggest that the animate–inanimate distinction is profound and fundamental to our conceptualization of the world, and they are consistent with a view that this distinction, or the basis for it, is innate.

Second, there is evidence that the ability to discriminate animates from inanimates and to attribute agency only to animates is found *across* species, not only among primates but also distantly related species such as dogs and birds (Hare et al. 2000, Flombaum & Santos 2005, Carey 2009), suggesting a shared trait with our common ancestors. Here I depart from Lenneberg's focus on species-specific traits as evidence for biological sources of abilities or behaviors. However, using cross-species evidence for the ability to distinguish animates from inanimates as a rationale for its biological roots is not at odds with Lenneberg's overarching thesis; rather, if we view an innate ability to distinguish animates from inanimates as *external* to language, but used in the service of decoding grammar by individuals who *have* language (i.e. humans), this should not run counter to any of Lenneberg's arguments. Indeed, he noted that "No biological phenomenon is without antecedents," (Lenneberg 1967: 234). Surely, a great many physiological and cognitive traits are shared with our ancestors, encoded in our genetic makeup, and a shared ability to distinguish animates from inanimates could be one of these. It seems reasonable to acknowledge that this ability is likely outside of language and merely used within the process of acquiring language, as many other cognitive, motoric and perceptual abilities are. In the case of the constructions discussed here this conceptual distinction, in combination with inductive biases about grammatical structure and syntax–semantics mappings, provides language learners with a crucial means of achieving what no other communication system permits and is one of the hallmarks of human language: computation of non-local semantic dependencies.

4. Conclusion

In this brief article I extended some of Lenneberg's claims about the biological basis for language by exploring the acquisition of some syntactic constructions that epitomize the transformational nature of syntax, namely raising and *tough*-constructions. The primary claim is that in order to acquire the structure of these types of sentences children must rely on biases specific to language as well as an innate (but not necessarily specific to language) ability to distinguish animate from inanimate entities and to associate animates with (some degree of) agency. The biases specific to language include the expectation that subjects will tend to be animate and/or agentive (as in Semantic Bootstrapping), that argument structure frames are projected locally prior to any syntactic displacements and that these frames restrict the lexical meanings of predicates (as in Syntactic Bootstrapping), and that deviations from the canonical alignment of semantic properties with syntactic roles and categories within a sentence provides a clue to learners that semantic relations may need to be computed non-locally.

References

Anderson, Deborah. 2005. *The Acquisition of* tough *Movement in English.* Doctoral Dissertation, University of Cambridge.

Atran, Scott. 1998. Folk biology and the anthropology of science. *Behavioral & Brain Sciences* 21, 597–611.

Becker, Misha. 2005. Learning verbs without arguments: The problem of raising verbs. *Journal of Psycholinguistic Research* 34, 165–191.

Becker, Misha. 2006. There began to be a learnability puzzle. *Linguistic Inquiry* 37, 441–456.

Becker, Misha. 2009. The role of animacy and expletives in verb learning. *Language Acquisition* 16, 283–296.

Becker, Misha. 2014. *The Acquisition of Syntactic Structure: Animacy and Thematic Alignment*. Cambridge: Cambridge University Press.

Becker, Misha. 2015. Animacy and the acquisition of *tough*-adjectives. *Language Acquisition* 22, 68–103. doi:10.1080/10489223.2014.928298.

Becker, Misha, & Bruno Estigarribia. 2013. Harder words: How children learn abstract verbs with opaque syntax. *Language Learning and Development* 9, 211–244. doi:10.1080/15475441.2013.753798.

Becker, Misha, & Jeannette Schaeffer. 2013. Animacy, argument structure and unaccusatives. In *Generative Linguistics and Acquisition: Studies in Honor of Nina M. Hyams*, ed. Misha Becker, John Grinstead, & Jason Rothman, 13–33. Amsterdam: John Benjamins.

Bunger, Anne, & Jeffrey Lidz. 2008. Thematic relations as a cue to verb class: 2-year-olds distinguish unaccusatives from unergatives. In *Proceedings of the 31st Penn Linguistics Colloquium*. Philadelphia: Penn Working Papers in Linguistics.

Carey, Susan. 1985. *Conceptual Change in Childhood*. Cambridge, MA: MIT Press.

Carey, Susan. 2009. *The Origin of Concepts*. New York: Oxford University Press.

Chomsky, Carol. 1969. *The Acquisition of Syntax in Children from 5 to 10*. Research Monograph 57. Cambridge, MA: MIT Press.

Chomsky, Noam. 1964. *Current Issues in Linguistic Theory*. The Hague: Mouton.

Chomsky, Noam. 1977. On *wh*-movement. In *Formal Syntax*, ed. Peter Culicover, Thomas Wasow, and Adrian Akmajian. New York: Academic Press.

Chung, Sandra. 1983. ECP and government in Chamorro. *Natural Language & Linguistic Theory* 1, 207–244.

Clark, Herbert H., & Jeffrey S. Begun. 1971. The semantics of sentence subjects. *Language & Speech* 14, 34–46.

Comrie, Bernard. 1989. *Language Universals and Linguistic Typology*. Chicago: University of Chicago Press.

Corrigan, Roberta. 1988. Children's identification of actors and patients in prototypical and nonprototypical sentence types. *Cognitive Development* 3, 285–297.

Cromer, Richard. 1970. "Children are nice to understand": Surface structure clues for the recovery of deep structure. *British Journal of Psychology* 61, 395–408.

Dahl, Östen, & Kari Fraurud. 1996. Animacy in grammar and discourse. In *Referent and Feferent Accessibility*, ed. Thorstein Fretheim & Jeanette K. Gundel, 47–64. Amsterdam: Benjamins.

Dixon, R.M.W. 1979. Ergativity. *Language* 55, 59–138.

Farah, Martha. 1995. Dissociable systems for visual recognition: A cognitive neuropsychology approach. In *An Invitation to Cognitive Science, vol. 2: Visual cognition*, ed. Stephen M. Kosslyn & Daniel N. Osherson, 101–119. Cambridge, MA: MIT Press.

Flombaum, Jonathan, & Laurie Santos. 2005. Rhesus monkeys attribute perceptions to others. *Current Biology* 15, 447–452.

Folli, Raffaella, & Heidi Harley. 2008. Teleology and animacy in external arguments. *Lingua* 118, 190–202.

Frankland, Steven M., & Joshua D. Greene. 2015. An architecture for encoding sentence meaning in left mid-superior temporal cortex. *Proceedings of the National Academy of Sciences of the United States of America* 112, 11732–11737.

Givón, Talmy. 1976. Topic, pronoun, and grammatical agreement. In *Subject and Topic*, ed. Charles Li, 267–280. New York: Academic Press.

Gleitman, Lila. 1990. The structural sources of verb meanings. *Language Acquisition* 1, 3–55.

Gleitman, Lila, Kimberly Cassidy, Rebecca Nappa, Anna Papafragou, & John C. Trueswell. 2005. Hard words. *Language Learning & Development* 1, 23–64.

Grimshaw, Jane. 1981. Form, function, and the language acquisition device. In *The Logical Problem of Language Acquisition*, ed. C.L. Baker & J.J. McCarthy, 165–182. Cambridge, MA: MIT Press.

Hare, Brian, Josep Call, Bryan Agnetta, & Michael Tomasello. 2000. Chimpanzees know what conspecifics do and do not see. *Animal Behaviour* 59, 771–786.

Hicks, Glyn. 2009. *Tough*-constructions and their derivation. *Linguistic Inquiry* 40, 535–566.

Hirsch, Christopher, & Kenneth Wexler. 2007. The late development of raising: What children seem to think about *seem*. In *New Horizons in the Analysis of Control and Raising*, ed. William D. Davies & Stanley Dubinsky, 35–70. Dordrecht: Springer.

Jackendoff, Ray. 1972. *Semantic Interpretation in Generative Grammar*. Cambridge, MA: MIT Press.

Johnson, Mark H., Suzanne Dziurawiec, Hadyn Ellis, & John Morton. 1991. Newborns' prefential tracking of face-like stimuli and its subsequent decline. *Cognition* 40, 1–19.

Keenan, Edward. 1976. Toward a universal definition of "subject". In *Subject and Topic*, ed. Charles Li, 303–334. New York: Academic Press.

Keil, Frank C. 1989. *Concepts, Kinds and Cognitive Development*. Cambridge, MA: MIT Press.

Landau, Barbara, & Lila R. Gleitman. 1985. *Language and Experience: Evidence from the Blind Child*. Cambridge, MA: Harvard University Press.

Lasnik, Howard, & Juan Uriagereka. 1994. *A Course in GB Syntax*. Cambridge, MA: MIT Press.

Lee, Joanne N., & Letitia R. Naigles. 2008. Mandarin learners use syntactic bootstrapping in verb acquisition. *Cognition* 106, 1028–1037.

Lees, Robert B. 1960. A multiply ambiguous adjectival construction in English. *Language* 36, 207–221.

Lempert, Henrietta. 1989. Animacy constraints on preschool children's acquisition of syntax. *Child Development* 60, 237–245.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York: John Wiley & Sons.

Li, Charles, & Sandra Thompson. 1976. Subject and topic: A new typology of language. In *Subject and Topic*, ed. Charles Li. New York: Academic Press.

Mak, Willem M., Wietske Vonk, & Herbert Schriefers. 2002. The influence of animacy on relative clause processing. *Journal of Memory & Language* 47, 50–68.

Massey, Christine, & Rochel Gelman. 1988. Preschooler's ability to decide whether a photographed unfamiliar object can move itself. *Developmental Psychology* 24, 307–317.

Mitchener, Garrett, & Misha Becker. 2011. A computational model of learning the raising-control distinction. *Research on Language & Computation* 8, 169–207. doi:10/1007/s11168-011-9073-6.

Orfitelli, Robyn. 2012. Argument intervention in the acquisition of A-movement. Doctoral Dissertation, UCLA.

Perlmutter, David M. 1978. Impersonal passives and the unaccusative hypothesis. In *Proceedings of the Berkeley Linguistic Society 4*, 157–189. Berkeley.

Piattelli-Palmarini, Massimo. 2017 [= this issue]. From zero to fifty: Considerations on Eric Lenneberg's *Biological Foundations of Language* and updates. *Biolinguistics*.

Pinker, Steven. 1982. A theory of the acquisition of lexical interpretive grammars. In *The Mental Representation of Grammatical Relations*, ed. Joan Bresnan. Cambridge, MA: MIT Press.

Poulin-Dubois, Diane, Anouk Lepage, & Doreen Ferland. 1996. Infants' concept of animacy. *Cognitive Development* 11, 19–36.

Rakison, David H., & Diane Poulin-Dubois. 2001. Developmental origin of the animate–inanimate distinction. *Psychological Bulletin* 127, 209–228.

Schachter, Paul. 1976. The subject in Philippine languages: Topic, actor, actor-topic, or none of the above? In *Subject and Topic*, ed. Charles Li, 491–518. New York: Academic Press.

Scott, Rose M., & Cynthia Fisher. 2009. Two-year-olds use distributional cues to interpret transitivity-alternating verbs. *Language & Cognitive Processes* 24, 777–803.

Silverstein, M. 1976. Hierarchy of features and ergativity. In *Grammatical Categories in Australian Languages*, ed. R.M.W. Dixon. Canberra: Australian Institute for Aboriginal Studies.

Spelke, Elizabeth. 1991. Physical knowledge in infancy: Reflections on Piaget's theory. In *The Epigenesis of Mind: Essays on Biology and Cognition*, ed. Susan Carey & Rochel Gelman, 133–170. Hillsdale, NJ: Lawrence Erlbaum Associates.

Traxler, Matthew J., Rihana S. Williams, Shelley A. Blozis, & Robin K. Morris. 2005. Working memory, animacy, and verb class in the processing of relative clauses. *Journal of Memory & Language* 53, 204–224.

Trueswell, John C., & Michael K. Tanenhaus. 1994. Toward a lexicalist framework for constraint-based syntactic ambiguity resolution. In *Perspectives on Sentence Processing*, ed. C. Clifton, L. Frazier, & K. Rayner, 155–179. Mahwah, NJ: Lawrence Erlbaum Associates.

Warrington, Elizabeth K., & T. Shallice. 1984. Category specific semantic impairments. *Brain* 107, 829–853.

Woodward, Amanda. 1998. Infants selectively encode the goal object of an actor's reach. *Cognition* 69, 1–34.

Woodward, Amanda, Ann Phillips, & Elizabeth Spelke. 1993. Infants' expectations about the motion of animate versus inanimate objects. In *Proceedings of the Fifteenth Annual Meeting of the Cognitive Science Society, Boulder, CO*, 1087–1091. Hillsdale, NJ: Lawrence Erlbaum.

Misha K. Becker
University of North Carolina
Department of Linguistics
301 Smith Building, CB#3155
Chapel Hill, NC 27599-3155
United States of America

mbecker@email.unc.edu

Language, Reading, and Motor Control: Get Rhythm!

Maria Teresa Guasti, Elena Pagliarini & Natale Stucchi

1. Introduction

Biological Foundations of Language already included a discussion of the role of temporal structural regularities and rhythm as organizing principle in language (see chapter 3 in Lenneberg 1967). In this article, we rely on Lenneberg's biological notion of language and related ideas like rhythmicity and temporal structural regularities in order to show that individuals with developmental dyslexia (DD) are less efficient than control individuals in using structural regularities during handwriting and some language activities. Consequently, they cannot fully exploit (temporal) structural representations to process oral language, to execute handwriting movement and to read, although they have developed compensatory mechanisms to understand language, perform motor activities, and read. This proposal is based on findings collected in a varieties of studies conducted in our lab.

First, we show that children with DD, who do not fail standardized language tests, but do not process oral language in the same ways as age-matched peers, as evident through ERP measures. They are also less efficient than control peers in morphologically manipulating non-words. Second, we show that children with DD are impaired in complying with two rhythmic principles governing handwriting considered in its motor dimension (not spelling), although they do not meet the criteria for disgraphia. Thus, children with DD have subtle oral language problems and motor disorders, beyond clear reading difficulties.

Although we are aware of the great genetic and phenotypic heterogeneity of dyslexia and language disorders, we would like to conjecture that there might be a common source to language, motor and reading difficulties. This lies in the efficient use of the temporal structural regularities underlying these three behaviors. The possibility of a common source does not preclude the existence of different phenotypic manifestations, as the way to compensate for the difficulties may vary across individuals.

2. Heterogeneity in Development Dyslexia

DD is usually defined as a specific difficulty in learning to read accurately and fluently. Many researchers agree on the notion that DD is the expression of a core deficit in phonology (Ramus, Pidgeon & Frith 2003, Snowling 2000) rather than in visual processing or in attention (Vellutino et al. 2004). As such, it is a language-related disorder (Vellutino 1977). The complex and variable neuropsychological profiles of individuals with DD have promoted other hypotheses, and have led

ISSN 1450–3417

to the notion that there exist different types of DD. For example, Wolf & Bowers (1999) suggested that there are three types of DD. One whose core deficit is phonology, a second one caused by a timing deficit that impairs temporal integration of the letters' forming words (Bowers & Wolf 1993) and prevents children with DD to recognize and represent orthographic patterns and a third one caused by the combination of the preceding two types. Other subtypes of DD, resulting from the breakdown of component processes involved in reading, are discussed in Zoccolotti & Friedmann (2010) and Friedmann & Coltheart (in press).

Beyond the heterogeneity in the profiles of individuals with DD, another dimension of variation is the fact that often children with DD display difficulties with language before their exposure to literacy, which are expressed in delay of language onset and of development. Among others, McArthur et al. (2000) reported that 55% of the children with DD scored more than one standard deviation below the mean on CELF-R, a test measuring oral syntactic and morphosyntactic skills. In other words, DD often co-occurs with language difficulties.

In addition to language problems, some studies revealed that children with DD experience fine and gross motor problems (Cappellini, Coppede & Valle 2010, Cheng-Lai et al. 2011, Thompson et al. 2015). It should be pointed out that, with few exceptions, motor skills in children with DD have rarely been investigated. These exceptions include Nicolson & Fawcett 1990 (see also Nicolson & Fawcett 2011), who formulated the Automatisation Deficit Hypothesis, according to which children with DD have difficulties in automatizing skills, including reading and motor skills. This hypothesis has been further developed into the Cerebellar Deficit Hypothesis. In this theory, the co-occurrence of reading and motor problems finds its root in a dysfunction at the level of the cerebellum, that blocks the automatisation of skills (e.g., reading, motoric skills, rules of language; see e.g., Stoodley & Stein 2013, Stoodley 2014 for a critical view of this hypothesis). Other exceptions are studies in which the tapping activity in synchrony with an isochronic-pacing metronome was investigated. In particular, Wolff (2002) found that, in a tapping task, students with DD anticipated an isochronic-pacing metronome signal much more than control children did and had some difficulties in reproducing patterned rhythms. Extending this line of research, Thomson & Goswami (2008) asked 10-year-old children with DD to tap in time/synchrony with a metronome beat. They found variability within individuals in the paced inter-tap intervals, suggesting that these children were not constant in their tapping rate within a given condition. In addition, association between motoric and auditory rhythmic skills, on the one hand, and literacy measures, on the other, were found. Other studies supported the relevance of the link between rhythm perception and literacy (Flaugnacco et al. 2014) and between rhythmic skills and grammar (see Gordon et al. 2015) and led to the conclusion that rhythmic timing may be crucial for language, motor skills and reading. Overall, these findings suggest that there is a lot of heterogeneity in individuals with DD and that DD often co-occurs with language problems and with motor deficits. They also revealed a special role for rhythmic timing.

3. Morphological Rules in Children with Developmental Dyslexia

As outlined in the previous section, several studies have demonstrated that children with DD have problems with oral language. For example, Joanisse et al.

(2000) reported that English-speaking children with DD have difficulties in the use of past verb marking (*-ed*); Bar-Shalom, Crain & Shankweiler (1993) showed that 7- to 8-year-old English-speaking children with DD have severe difficulties in comprehending relative clauses (see also Robertson & Joanisse 2010). In addition, individuals with DD, who do not fail on standardized oral language measures or behave as controls in behavioural tasks, may display subtle morphosyntactic problems evidenced through the event related potential (ERP) technique. Rispens, Been & Zwarts (2006) showed that Dutch-speaking adults with DD exhibit ERP anomalies during the processing of oral sentences including a subject-verb agreement violation (equivalent to *the child speak* or *the children speaks*; see also Rispens, Roeleven & Koster 2004).

In a similar vein, Cantiani et al. (2015) found that 8–10 year old Italian-speaking children with DD, who did not differ from controls on an Italian standardized test for grammatical comprehension, manifested an atypical ERP response during the processing of oral sentences including a subject-verb agreement violation. Specifically, whilst in control children the subject-verb agreement violation elicited the expected P600, in children with DD the same violation provoked a N400, suggesting that the two groups did not process this morphosyntactic violation in the same way (see also Rispens, Roelevan & Koster 2004). Interestingly, the two groups did not differ in the behavioural task consisting of judging the grammaticality of the sentences.[1] However, the two groups differed on a behavioural task requiring the morphological manipulation of non-words. In this task, children were given a legal non-word and were asked to produce the diminutive, the augmentative, or they had to derive the infinitive from when given a finite invented verb, or derive the noun from a given verb. In other words, children had to use their implicit knowledge of various morphological rules, whose application is very common in Italian. The results showed that children with DD scored significantly lower than control children.

Cantiani et al. (2013) interpreted these findings in the light of the Declarative/Procedural model (Ullman 2001, Ullman & Pullman 2015) and suggested that the N400 response found in children with DD reflected the use of a lexical-semantic compensatory strategy or the use of an explicit rule. Children with DD have difficulties in forming or using implicit morphological rules stored in the procedural memory and rely on explicit rules or on lexical forms stored in the declarative memory. Otherwise put, children with DD process subject-verb agreement violations through a lexical-semantic route, rather than a structural syntactic dependency applied at the hierarchical level between a constituent and an inflectional head hosting the inflected verb (Guasti 2017). Consistent with this view, children with DD were unable to use implicit morphological rules to modify non-words and to the extent that they succeed, they likely used processes like analogy to existent words memorized in the declarative memory. This conjecture is supported by the fact that the higher the negativity of the N400 in the ERP task, the greater the accuracy in the morphological manipulation of non-words.

---

[1] Similar behavioural and ERP evidence was gathered by Cantiani et al. (2013) with well-compensated adults affected by DD, who were followed in the same clinic from their childhood and never received a diagnosis of specific oral language impairments.

All in all, these findings suggest that children with DD exhibit subtle problems in oral language, which are not necessarily evident in standardized behavioural task and in everyday life, because they are very well compensated. However, these problems are very relevant to understand the nature of the difficulties, which affect individuals with DD. Whether oral language problems in children with DD results from comorbidity with Specific Language Impairment (SLI) or are the sign of a deeper relation between the two disorders is an open question. What we can conclude is that these language deficits seem to be located in the rule-governed component of language and are compensated through lexical-semantic processes, whose nature requires further investigation.

4. Motor Rhythmic Difficulties in Children with Developmental Dyslexia

While the association between DD and motor skills deficit is generally seen as a case of comorbidity, the works by Wolff (2002) and Thomson & Goswami (2008) put forward the possibility that rhythmicity plays a crucial role in these two disorders. According to Llinás (1993) "rhythmicity is the ability to generate a sequence of rhythmic events that are time-locked to each other" (Pagliarini et al. 2015: 162). Furthermore, Llinás (1993) noticed that individuals with DD, beyond linguistic deficits, had difficulties in generating fast sequential movements. Inspired by these observations, and based on the hypothesis that rhythmicity may be impaired in individuals with DD, Pagliarini et al. (2015) studied handwriting in children with DD and in control children.

Handwriting is a motor activity which requires the generation of a rapid sequence of movements governed by two rhythmic principles: Isochrony and homothety. These two principles, which govern different types of movement, are here explained in relation to handwriting. According to the principle of isochrony, the absolute writing duration of a word (or generally of a movement) remains more or less invariant irrespective of its size (Freeman 1914, Lacquaniti, Terzuolo & Viviani 1983, Viviani & Terzuolo 1983). This invariance is guaranteed by the existence of a compensatory mechanism that guides the writer in modulating writing speed as a function of the size of the word being written (Binet & Courtier 1893, Stetson & McDill 1923, Viviani & Terzuolo 1982). For example, if the size of a word increases, the speed of handwriting will also increase, that is, one writes faster. The principle of homothety (Lashley 1951, Viviani & Terzuolo 1982) asserts that the relative duration of the writing of the single letters of a word will remain the same across changes in the writing duration of the whole word. In order to exemplify, let us consider an example. When writing the word *cat*, if the time spent on writing the letter 'c' is 50%, the letter 'a' is 20% and the letter 't' is 30% of the total writing duration of the entire word, the same relative durations are maintained across different total writing durations (as a consequence of writing faster or slower).

Thus, the isochrony principle rules the absolute duration of the whole word (which tends to remain more or less constant under different writing conditions) and the principle of homothety rules the relative duration of single letters, which does not vary despite the variations of the duration of the whole word. Notice that the two principles are independent and a violation of one does not requires a violation of the other.

As we said, Llinás (1993) pointed out that individuals with DD experience difficulties in generating fast sequential movements. Pagliarini et al. (2015) went further and hypothesized that children with DD struggle in complying with the two principles of rhythmic organization of handwriting. Furthermore, the authors conjectured that if rhythmicity is problematic for these children, correlations between handwriting, language, and reading measures should be found, as all these activities involve a rhythmic component. To test these hypotheses, Pagliarini et al. (2015) asked 9-year-old children with DD (scoring -2 SD below the mean on accuracy or speed in a standardized reading task) and same age typically developing (TD) children to write the Italian word *burle* (English: *jokes*) with a wireless pen on a digitizing tablet. Children were asked to write the target word in both cursive and all-capital block scripts, and for each script, children were asked to write the target word in three conditions: Spontaneous, that is, as they usually write, bigger, and faster with respect to the spontaneous condition. A number of kinematic measures (duration, velocity, disfluency) of writing were collected and analyzed.

The results showed that TD children complied with the principle of isochrony, meaning that they wrote faster when they had to write the word bigger (so that the absolute duration of the word remained constant). They also complied with the principle of homothety, meaning that they were able to keep constant the relative writing duration of individual letters across changes in the total word duration. By contrast, children with DD struggled in obeying the two principles. Although children with DD somewhat changed their handwriting behavior across conditions, they did not increase the writing speed adequately when they had to write bigger. In other words, these children had difficulties in modulating their handwriting movement as a function of the size of the word, thus failing to fully comply with the principle of isochrony. They tried, but they did not increase their handwriting movement enough. Children with DD were also less able to keep the relative writing duration of single letters constant across changes in size: For example, they were poor in rescaling the duration of single letters when they had to write bigger. Again, they tried and did some rescaling, but they did not maintain the relative duration constant. These results support the hypothesis that children with DD have difficulties in keeping the rhythm of writing across changes in the size of words.

Furthermore, Pagliarini et al. (2015) also demonstrated that there were correlations between reading, writing, and language (vocabulary and non-word repetition) measures, thus supporting the hypothesis that reading, writing, and language are mediated by rhythmic competence. In a following study, Pagliarini and colleagues (Pagliarini et al. 2017) investigated the development of the rhythmic principles of handwriting by testing typically developing (TD) children from the first to fifth grade of primary school (i.e. from age 6 to 10). The authors found that children with TD were already perfectly able to comply with the principle of isochrony and homothety in their first grade of primary school (i.e., at age 6) and only after a few months of formal education (and of learning to write).[2] They also found that, from age 6 to 10, children complied equally well with these two principles. In other words, TD children do not need a lot of training to comply with the rhythmic principles of handwriting, a fact that is compatible with the idea that

---

[2] Notice that these children were on average 2–3 years younger than children with DD in Pagliarini et al. (2015).

the two principles are already available before schooling. As a consequence, the difficulties experienced by children with DD in obeying the rhythmic principles of handwriting cannot be due to a lack of training, as already at age 6 TD children comply with them as well as 10-year-old children do.

5. Language, Reading and Motor Skills: What do they Have in Common?

The data reviewed on handwriting suggest that children with DD have troubles with the rhythmic principles of writing movements, that is, with the temporal structure that underlies the production of sequential movements. Let us consider more thoroughly what it means to comply with the principles of isochrony and homothety, which govern the rhythm of writing. To keep the absolute duration of the whole word and the relative duration of single letters constant across changes in size, one needs to have an abstract temporal representation (independent of the specific script), that is, the temporal structure of the whole handwriting movement (of the word) and of its components before starting to write. In this way one can modulate the velocity appropriately for writing the whole word and for rescaling the relative duration of single letters when the total duration changes. This representation is hierarchical in that the time allocated to each component unit (letters and, below letters, strokes) of the word depends on the duration of the whole word. There are two key aspects that are worth to be emphasized further. One is that to behave adequately and efficiently during handwriting, one needs to have an abstract hierarchical representation of the movements; the second is that one employs this temporal representation to anticipate changes in velocity and in rescaling the duration of components movements. Based on the findings we discussed, different options are open to characterize the difficulties experienced by children with DD. We could say that children with DD do not have the abstract representation underlying the execution of movement or have a different representation (e.g., with less details) than TD children. They could also have the same representation as TD children, but cannot exploit it as efficiently as TD children to modulate their handwriting movements.

Let us now consider the data reviewed about oral language problems in children with DD. These also point toward a problem having to do with the abstract representations or with the rules underlying word formation or sentence formation. Children with DD are less efficient than TD children in applying morphological rules to non-words to obtain other non-words with for example specific suffixes. They also do not process the subject-inflected verb agreement dependency via an abstract morphosyntactic rule, according to which agreement holds between a constituent located in a certain hierarchical configuration in the clausal structure and the verbal inflection. In this case, it was found that at the behavioural level children with DD were as good as TD children in judging the grammatical violations.

The ERP data shed light on this aspect by revealing that they did so in a different way than TD children. What can this different way be? One possibility is to assume that, on the basis of a morphosyntactic rule, children can predict the inflection on the verb by having extracted the inflection of the subject constituent. Another way consists in processing the subject constituent, processing the inflected verb and verify that their agreement suffixes match. The first mechanism is more efficient than the second one, because it is based on prediction and consists in temporally anticipating something that has still to come. If the prediction is met, as happens in most of the cases, nothing has to be done. The second mechanism does not predict: It checks that the linguistic features of two items match. It is obvious that children who use the second mechanism to process language are less efficient in reading or read slowly, since reading also takes advantage of the ability to predict. Based on this, two alternatives can be drawn: Children with DD perform language tasks and process language by basing themselves on different representations or rules than TD children. Alternatively, the mechanisms that they use are less efficient than the mechanisms used by TD children. Cantiani et al. (2015) conjectured that children with DD used lexical-semantic rules stored in declarative memory rather than morphosyntactic rules stored in the procedural memory.[3] If this is so, we have to conclude that the former types of rules may lead to less efficient linguistic behaviours than the latter ones, e.g., in the case of the morphological manipulation of non-words, but not in judging grammatical and ungrammatical sentences.

Our data on handwriting (Pagliarini et al., 2015, Pagliarini et al., 2017) and oral language processing (Cantiani et al., 2015) in children with DD leave open some options concerning what may get awry, but somehow these options turn around the notion of abstract hierarchical representation, abstract temporal representation and prediction. In fact, having an abstract representation or a rule is used for predicting incoming input. Children with DD might be able to extract some sort of abstract representation, but this is less detailed than that of TD children or is of a different nature and its use is less efficient than that of the representation that TD children can avail themselves of. Alternatively, the abstract representation can be the same in children with DD and in children with TD, but the mechanisms that use this representation are less efficient in children with DD.

How are these conjectures related to the defining problems of DD, that is, troubles in reading? First, reading is grounded on oral language (see Kovelman et al. 2008), both at the level of decoding and of comprehending written texts.[4] In fact, one hypothesis attributes the problems of children with DD to phonology. Second, reading also involves forming abstract orthographic representations (i.e., not specified in a particular script) of words or of units larger than graphemes (e.g., morphemes) that can be used to predict future words (for example on the basis of the first morpheme of a word, one can predict the rest of the word). It is known that the number of fixations (on a text) is higher for children with DD than for TD children. This fact suggests that children with TD can predict and thus skip frequent words or highly expected words, that is, to recognize words, they do not need to perceptually process them all; children with DD, instead, do not predict as efficiently as TD children and have to process all (or almost all) words to recognize them. This is precisely what we have conjectured above when we suggested that

---

[3] Ullmann & Pierpoint (2005) advance a similar hypothesis for children with SLI. They proposed the Procedural deficit hypothesis, whereby children with SLI suffer from atypical development of brain structures that subserve procedural memory. See also van der Lely & Ullman (2001) and van der Lely (2005).

[4] Kovelmann et al. (2008) showed that 8-year-old children exposed to English after age 3 are weaker than monolingual and children exposed before age 3 on word reading and phonological awareness tasks.

there can be two mechanisms to perform linguistic tasks, one based on prediction and one not based on predictions. In ongoing research, we are exploring the possibility that the predictive mechanism is not efficient or is impaired in individuals with DD. Preliminary results show that individuals with DD are poor in anticipating future events, that is, they are not as efficient as TD children/adults in using an abstract representation to predict future events.

Although we left some options open, our proposal amounts to say that there is a common core underlying language (and reading being a language-related activity) and motor actions. It is possible that this common core consists in making efficient use of supra-modal abstract hierarchical representations to predict future events. The idea that the shape of linguistic rules is not domain-specific, but is shared, at least in part, with other cognitive systems converges with data from a fMRI study carried out by Tettamanti et al. (2009) on healthy adults. In this study, the authors extended to the visuo-spatial domain a previous fMRI study by Tettamanti and colleagues (Tettamanti et al. 2002) carried out with language stimuli (see also Tettamanti & Weniger 2002). In Tettamanti et al. (2002), the processing of non-rigid syntactic dependencies (NRSD; e.g., drop the subject of finite clauses) in the language domain was contrasted with the processing of rigid syntactic dependencies (RSD), that is, dependencies between items at fixed position (e.g., negation is the third element of a sentence). In Tettamanti et al. (2009), the same two types of rules (NRSD and RSD) were tested in the visuo-spatial domain (instantiated by sequences of symbols). During the training phase, participants were exposed to strings that obeyed either NRSD or RSD and had to discover the rules. During the testing phase, they had to judge whether given strings conformed or not to the learned rules. Tettamanti et al. (2009) hypothesized that "non-rigid" syntactic dependencies (NRSD)–that is, syntactic rules established between words at various positions, which "are the core type of dependencies found in all human languages", are not domain-specific, but are present in various cognitive domains (beyond language). The authors found that participants were able to learn both types of rules (NRSD and RSD) with great accuracy when presented in the visuo-spatial domain. However, learning RSD was slower than learning NRSD. In addition, the processing of NRSD in the visuo-spatial domain, as previously established for the language domain, activated Broadmann area 44 of the left inferior frontal gyrus.

The authors conclude that "in the human brain, one single 'grammar without words' [expressed by NRSD rules] serves different higher cognitive functions" (Tettamanti et al. 2009: 825). Our data based on language and motor skills in children with DD are compatible with the idea that this grammar may be less efficiently used by children with DD compared to children with TD and this may cause subtle deficits across different cognitive domains that may be very well compensated and hard to single out in everyday activities.

6. Conclusion

In this short article, we have seen that children with DD have difficulties in maintaining the rhythm of handwriting and have difficulties in manipulating words and sentences. On this basis, we have proposed that they exhibit deficits in making efficient use of abstract temporal representations or rules. Alternatively, we could suggest that they have difficulties in extracting abstract temporal representations or rules, or have less detailed abstract representations than TD children. At present, the data discussed do not allow us to decide between these hypotheses. Our conjectures converge with extant data showing that there are links between rhythm perception and literacy (Flaugnacco et al. 2014), between rhythmic skills and grammar (see Gordon et al. 2015). It is also consistent with the findings that there are non-rigid syntactic dependencies that are operative in different cognitive domains (Tettamanti et al. 2009). Although there are various pieces of data that go in the same direction, nevertheless our conjectures open a series of questions: Why is there heterogeneity among individuals with DD? Is the deficit we have uncovered present in populations with other language problems, like individuals with SLI? Since children with SLI and DD differ from children with a sole diagnosis of DD (even if language impairments are evident in these individuals; see Cantiani et al. 2015 and reference cited there), what makes them different?

References

Bar-Shalom, Eva G., Stephen Crain, & Donald Shankweiler. 1993. A comparison of comprehension and production abilities of good and poor readers. *Applied Psycholinguistics* 14, 197–227.

Binet, Alfred & Jules Courtier. 1893. Sur la vitesse des mouvements graphiques. *Revue Philosophique de la France et de l'Étranger* 35, 664–671.

Bowers, Patricia G. & Maryanne Wolf. 1993. Theoretical links among naming speed, precise timing mechanisms and orthographic skill in dyslexia. *Reading and Writing* 5(1), 69–85.

Cantiani, Chiara, Maria Luisa Lorusso, Paolo Perego, Massimo Molteni, Maria Teresa Guasti. 2013. ERPs reveal anomalous morphosyntactic processing in developmental dyslexia. *Applied Psycholinguistics* 34, 1135–1162.

Capellini, Simone Aparecida, Aline Cirelli Coppede, & Talita Regina Valle. 2010. Fine motor function of school-aged children with dyslexia, learning disability and learning difficulties. *Pró-Fono Revista de Atualizaçāo Científica* 23(3), 201–208.

Chang, Shao-Hsia & Nan-Ying Yu. 2013. Handwriting movement analyses comparing first and second graders with normal or dysgraphic characteristics. *Research in Developmental Disabilities* 34(9), 2433–2441.

Cheng-Lai, Alice, Cecilia W. P. Li-Tsang, Alan H. L. Chan, Amy G. W. Lo. 2013. Writing to dictation and handwriting performance among Chinese children with dyslexia: Relationships with orthographic knowledge and perceptual-motor skills. *Research in Developmental Disabilities* 34(10), 3372–3383.

Gordon, Reyna L., Magdalene S. Jacobs, C. Melanie Schuele, & J. Devin McAuley. 2015. Perspectives on the rhythm-grammar link and its implications for typical and atypical language development. *Annals of the New York Academy of Sciences* 1337, 16–25.

Friedmann, Naama & Max Coltheart. In press. Types of developmental dyslexia. In Amalia Bar-On and Dorit Ravid (Eds.), *Handbook of Communication Disorders: Theoretical, Empirical and Applied Linguistics Perspectives*. Boston, MA: De Gruyter.

Freeman, Frank N. 1914. Experimental analysis of the writing movements. *Psychological Monographs: General and Applied* 17(4), 1–57.

Guasti, Maria Teresa. 2017. *Language Acquisition: The Growth of Grammar* (2nd ed.). Cambridge, MA: MIT Press.

Joanisse, Marc F., Franklin R. Manis, Patricia Keating, & Mark S. Seidenberg. 2000. Language deficits in dyslexic children: Speech perception, phonology, and morphology. *Journal of Experimental Child Psychology* 77, 30–60.

Kovelman, Ioulia, Stephanie A. Baker, & Laura-Ann Petitto. 2008. Age of first bilingual language exposure as a new window into bilingual reading development. *Bilingualism: Language and Cognition* 11(2), 203–223.

Lam, Sutie S. T., Ricky K. C. Au, Howard W. H. Leung, Cecilia W. P. Li-Tsang. 2011. Chinese handwriting performance of primary school children with dyslexia. *Research in Developmental Disabilities* 32(5), 1745–1756.

Lacquaniti, Francesco, Carlo Terzuolo, Paolo Viviani. 1983. The law relating the kinematic and figural aspects of drawing movements. *Acta Psychologica* 54(1), 115–130.

Lashley, Karl S. 1951. The problem of serial order in behavior. In Lloyd A. Jeffress (Ed.), *Cerebral Mechanisms in Behavior*. New York, NY: Wiley. 112–146.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Llinás, Rodolfo 1993. Is dyslexia a dyschronia?. *Annals of the New York Academy of Sciences* 682(1), 48–56.

Nicolson, Roderick I. & Angela J. Fawcett, A. J. 1990. Automaticity: A new framework for dyslexia research? *Cognition* 35, 159–182.

Nicolson, Roderick I. & Angela J. Fawcett. 2011. Dyslexia, dysgraphia, procedural learning and the cerebellum. *Cortex* 47(1), 117–127.

McArthur, G. M., J. H. Hogben, V. T. Edwards, S. M. Heath, & E. D. Mengler. 2000. On the "specifics" of specific reading disability and specific language impairment. *The Journal of Child Psychology and Psychiatry* 41, 869–874.

Pagliarini, Elena, Maria Teresa Guasti, Carlo Toneatto, Elisa Granocchio, . . . , & Natale Stucchi. 2015. Dyslexic children fail in complying with rhythmic constraints of handwriting. *Human Movement Science* 42, 161–182.

Pagliarini, Elena, Lisa Scocchia, Mirta Vernice, Marina Zoppello, . . . , & Natale Stucchi. 2017. Children's first handwriting productions show a rhythmic structure. *Scientific Reports*. 7: 5516. doi:10.1038/s41598-017-05105-6.

Ramus, Franck, Elizabeth Pidgeon, & Uta Frith. 2003. The relationship between motor control and phonology in dyslexic children. *Journal of Child Psychology and Psychiatry* 44(5), 712–722.

Rispens, Judith E., Pieter H. Been, Frans Zwarts. 2006. Brain responses to subject-verb agreement violations in spoken language in developmental dyslexia: An ERP study. *Dyslexia* 12, 134–149.

Rispens, Judith E., Susan Roeleven, & Charlotte Koster. 2004. Sensitivity to subject-verb agreement in spoken language in children with developmental dyslexia. *Journal of Neurolinguistics* 17, 333–347.

Robertson, Erin K. & Marc F. Joanisse. 2010. Spoken sentence comprehension in children with dyslexia and language impairment: The role of syntax and working memory. *Applied Psycholinguistics* 31, 141–165.

Stetson, Raymond H., & James A. McDill. 1923. Mechanism of the different types of movement. *Psychological Monographs: General and Applied* 32(3), 18–40.

Stoodley, Catherine J. & John F. Stein. 2013. Cerebellar function in developmental dyslexia. *The Cerebellum* 12, 267–276.

Stoodley, Catherine J. 2014. Distinct regions of the cerebellum show gray matter decreases in autism, ADHD, and developmental dyslexia. *Frontiers in Systems Neuroscience* 8: 92. doi:10.3389/fnsys.2014.00092.

Tettamanti, Marco, Irene Rotondi, Daniela Perani, Giuseppe Scotti, …, & Andrea Moro. 2009. Syntax without language: Neurobiological evidence for cross-domain syntactic computations. *Cortex* 45(7), 825–838.

Tettamanti, Marco, Hatem Alkadhi, Andrea Moro, Daniela Perani, Spyros Kollias, & Dorothea Weniger. 2002. Neural correlates for the acquisition of natural language syntax. *NeuroImage* 17(2), 700–709.

Tettamanti, Marco & Dorothea Weniger. 2006. Broca's area: A supramodal hierarchical processor? *Cortex* 42(4), 491–494.

Thomson, Jennifer M. & Usha Goswami. 2008. Rhythmic processing in children with developmental dyslexia: Auditory and motor rhythms link to reading and spelling. *Journal of Physiology-Paris* 102(1), 120–129.

Thompson, Paul A., Charles Hulme, Hannah M. Nash, Debbie Gooch, Emma Hayiou-Thomas, & Margaret J. Snowling. 2015. Developmental dyslexia: Predicting individual risk. *Journal of Child Psychology and Psychiatry* 56, 976–987.

Ullman, Michael T. 2001. A neurocognitive perspective on language: The declarative/procedural model. *Nature Reviews Neuroscience* 2, 717–726.

Ullman, Michael T. & Elizabeth I. Pierpont. 2005. Specific language impairment is not specific to language: The procedural deficit hypothesis. *Cortex* 41(3), 399–433.

Ullman, M. T., & Pullman, M. Y. (2015). A compensatory role for declarative memory in neurodevelopmental disorders. *Neuroscience & Biobehavioral Reviews* 51, 205–222.

van der Lely, Heather K. J. 2005. Grammatical-specific language impairment (G-SLI): Identifying and characterising the G-SLI subgroup. *Fréquences* 17, 13–20.

van der Lely, Heather K. J. & Michael T. Ullman. 2001. Past tense morphology in specifically language impaired and normally developing children. *Language and Cognitive Processes* 16, 177–217.

Vellutino, Frank R. 1977. Alternative conceptualizations of dyslexia: Evidence in support of a verbal-deficit hypothesis. *Harvard Educational Review* 47(3), 334–354.

Vellutino, Frank R., Jack M. Fletcher, Margaret J. Snowling, & Donna M. Scanlon. 2004. Specific reading disability (dyslexia): What have we learned in the past four decades? *Journal of Child Psychology and Psychiatry* 45(1), 2–40.

Viviani, Paolo & Carlo Terzuolo. 1982. Trajectory determines movement dynamics. *Neuroscience* 7(2), 431–437.

Viviani, P. & C. Terzuolo. (1983). The organization of movement in handwriting and typing. In Brian Butterworth (Eds.), *Language Production, Vol. II: Development, Writing and Other Language Processes*. New York, NY: Academic Press. 103–146.

Wolf, Maryanne & Patricia G. Bowers. 1999. The double-deficit hypothesis for the developmental dyslexia. *Journal of Educational Psychology* 91, 415–438.

Wolff, Peter H. 2002. Timing precision and rhythm in developmental dyslexia. *Reading and Writing* 15(1–2), 179–206.

Zoccolotti, Pierluigi & Naama Friedmann. 2010. From dyslexia to dyslexias, from dysgraphia to dysgraphias, from a cause to causes: A look at current research on developmental dyslexia and dysgraphia. *Cortex* 46(10), 1211–1215.

Maria Teresa Guasti
Università di Milano-Bicocca
Dipartimento di Psicologia
P.zza Ateneo Nuivo 1
20126 Milano
Italy

mariateresa.guasti@unimib.it

Elena Pagliarini
Universitat Pompeu Fabra
Center for Brain and Cognition
C/ Ramon Trias Fargas 25–27
08005 Barcelona
Spain

elena.pagliarini@upf.edu

Natale Stucchi
Università di Milano-Bicocca
Dipartimento di Psicologia
P.zza Ateneo Nuivo 1
20126 Milano
Italy

natale.stucchi@unimib.it

Revisiting Lenneberg's Hypotheses About Early Developmental Plasticity: Language Organization After Left-Hemisphere Perinatal Stroke

Elissa L. Newport, Barbara Landau, Anna Seydell-Greenwald,
Peter E. Turkeltaub, Catherine E. Chambers, Alexander W.
Dromerick, Jessica Carpenter, Madison M. Berl, &
William D. Gaillard

1. Introduction

A prominent theme in the literature on brain injury and recovery has been the notion of early developmental plasticity (Kennard 1940, Kolb et al. 2000). This has been a particular focus in work on language. In healthy adults, language is virtually always lateralized to the left hemisphere (LH; Broca 1861, Gazzaniga & Sperry 1967). However, Basser (1962) and Lenneberg (1967) compiled published case studies, their own patient histories, and available medical records of children and adults with left and right hemisphere lesions or hemispherectomy to determine whether there were systematic effects of hemisphere and age of insult on the development or recovery of language. From these data, Lenneberg (1967) concluded that, when even massive injuries to one hemisphere occurred before age 2, most children developed language normally or with only some delay; and these outcomes were the same regardless of which hemisphere was affected. This led him to argue that initially, before cerebral dominance was fully established, the two hemispheres were equipotential for language. This was less true for older children and was definitively no longer true for adults, who showed strong LH specificity for language interference and some recovery from mild aphasias, but did not recover completely from severe aphasias or left hemispherectomies. Using the Wada test (briefly anesthetizing one hemisphere and then the other; see Loring et al. 1992) to determine which hemisphere controls speech, Rasmussen & Milner (1977) showed that in children, depending on the age at injury, speech that is ordinarily in the left hemisphere could be controlled successfully by the right hemisphere or by an alternate region of the damaged left hemisphere. Similar reorganization was not observed in adults,

---

This work is supported by funds from Georgetown University, MedStar Health, the Solomon James Rodan Pediatric Stroke Research Fund and the Feldstein Veron Innovation Fund to the Center for Brain Plasticity and Recovery; by NIH grants K18DC014558 to ELN, K23NS065121 to MMB, and R01NS244280 to WDG; by American Heart Association grant 17GRNT33650054 to ELN; by U10 NS086513 to the SCANR node of StrokeNet at MedStar Health and Georgetown University; and by M01RR020359 and P30HD040677 to the IDDRC U54 HD090257 at Children's National Health System and Georgetown University.

biolinguistics

ISSN 1450–3417
Biolinguistics 11.SI: 407–421, 2017
http://www.biolinguistics.eu

even decades after injury. These generalizations have long formed the classic picture of recovery of language function.

However, recent research on organization after early injury in children has not always found such consistent outcomes. Some studies have found good language abilities after focal brain injury in children, but others have not (Banich et al. 1990, Ballantyne et al. 2007, Levine et al. 2005, Moesch, Max, & Tranel 2005, Montour-Proulx et al. 2004, Stiles et al. 2012, Westmacott et al. 2010). Relatively few studies of neural reorganization have been done with children, also with somewhat inconsistent outcomes (see, e.g., Mbwana et al. 2009, Rosenberger et al. 2009, and You et al. 2011 for language reorganization with epilepsy, and Booth et al. 2000, Dick et al. 2013, Fair et al. 2006, 2010, Jacola et al. 2006, Liégeois et al. 2004, Raja et al. 2010, Staudt et al. 2002, 2007, and Tillema et al. 2008 on perinatal stroke). This variation of outcomes may be due to true variation among children, or to the inclusion of children with a variety of types and causes of focal brain injuries (e.g., periventricular leukomalacia, moya moya, vasculitis, tumors, and hemorrhagic or arterial ischemic strokes) or the effects of other medical problems that are often comorbid with stroke in children (e.g., seizures and seizure medications, heart disease and reduced cortical perfusion, or sickle cell anemia). It might also be due to variation in the ages at which participants were evaluated (see Bates et al. 2001, showing that children with focal brain injuries may show developmental delays but later reach normal levels of performance).

There has also been little consistency in investigators' views of the principles governing developmental plasticity for language. Only a few researchers have proposed hypotheses about what areas or networks in the brain are capable of subserving language in the face of early brain injury, and these proposals are in sharp conflict. Vargha-Khadem et al. (1985) suggested that the left hemisphere is uniquely suited for language and that successful reorganization of language is limited to LH brain areas. (See also Raja et al. 2010, who have argued that the remaining left hemisphere voxel activity correlates best with language proficiency after left hemisphere perinatal stroke). Staudt (2002) and Gaillard and colleagues (Gaillard et al. 2007, Berl et al. 2014, Mbwana et al. 2009) have argued that left hemisphere areas or their precise right hemisphere homologues can subserve language when there are early left hemisphere abnormalities. In contrast, Bates et al. (1997) have suggested that the young brain is highly plastic; they argue that "the human capacity for language is not localized at birth," implying that reasonably normal language skills might be able to develop in numerous other brain regions. Bedny et al. (2011) have argued that congenitally blind individuals utilize even occipital cortex (including V1) during spoken language processing. Can this wide range of brain areas indeed support language? In our ongoing work we seek to understand the forces that lead language to develop in only certain brain areas in the healthy child and also to understand what areas can support language after early brain injury.

An important literature is the work of the Gaillard lab (Gaillard et al. 2007, Berl et al. 2014, Mbwana et al. 2009) using functional magnetic resonance imaging (fMRI) to examine the organization of language over development and how it is affected by early and continuing epilepsy (and the brain abnormalities that cause them). In response to chronic epilepsy, cortical processing of language is frequently restructured, with some or all language function shifted to the right hemisphere.

Their work has shown a limited number of ways in which language is organized across a very large number of children: in the usual left hemisphere areas, in the precisely homotopic right hemisphere areas, or in the usual left hemisphere temporal areas combined with the homotopic right hemisphere frontal areas. No other patterns of language organization appear in their subjects.

However, while chronic seizures can be clinically devastating for children, they apparently exert relatively mild effects on cortical organization: 75 % of children with early chronic seizures retain the typical left hemisphere pattern of language organization. To examine language after very early damage to the brain, we are focusing on perinatal arterial ischemic stroke, a relatively rare neurological event but one whose characteristics may provide an excellent model for examining the neural organization of language after early brain injury and for gaining insight into important principles of neural plasticity for language. In perinatal stroke, the injuries are typically much larger than in pediatric epilepsy but are relatively stereotyped in anatomy; approximate time of onset is clear; and in most patients there are not continuing seizures or long periods of time on antiepileptic medications. This makes our perinatal population an important contrast to Gaillard et al.'s work on epilepsy.

2. The Perinatal Stroke Project

Until recently, distinctions among the types of stroke that occur in children were not well understood. The availability of new imaging techniques and the establishment of the International Pediatric Stroke Study (deVeber 2005), with investigators around the world contributing case histories and data to a large repository, has only recently made it possible for investigators and physicians to establish a typology of arterial ischemic stroke (AIS) in children (Sébire, Fullerton, Riou, & deVeber 2004). Fortunately, stroke in children is uncommon; and it often occurs from different mechanisms than stroke in adults, including congenital heart disease, sickle cell anemia, or other disorders that can affect stroke outcomes in complex ways. In contrast, perinatal AIS has become a focus of research due to its occurrence often without other health problems. Many children with perinatal strokes are born after a healthy, full-term pregnancy, without birth complications, and without subsequent disease. They suffer from a sudden ischemic event whose causes are not well understood (thought perhaps to be a clot from the placenta or clots formed during changes from fetal to neonatal circulation) and then will often go on to develop without continuing seizures (some infants may have an early seizure, but many have none or only one).

Following Lenneberg's lead, our *Perinatal Stroke Project* re-examines the important issues he raised by studying language in teenagers and young adults who had such a perinatal stroke, many years before. Thanks to our collaboration with some of the largest and best known pediatric stroke programs in the United States (at Children's National Medical Center and Children's Hospital of Philadelphia), our research project is able to focus on this highly selected and uniform population: those who had a perinatal arterial ischemic stroke (defined as onset between 28 weeks gestation to 28 days postnatal (Lynch 2009), though most of our participants had their stroke within a few days of birth). Perinatal stroke to the middle cerebral artery (MCA) provides an excellent model for this work: lesions are well defined, damaging LH language areas or their right hemisphere (RH) homologues, and often occur without other medical problems. While perinatal stroke occurs in only one out of 4,000 live births (Lynch 2009), we have been able to recruit a good number of participants with very similar injuries and fairly clean medical histories (born after full-term healthy pregnancies; no significant additional disease, such as sickle cell anemia, congenital heart defects, or multiple strokes; no medically refractory seizure disorders). We are not following our participants longitudinally (though see Stiles et al. 2012, Bates & Roe 2001, and Bates et al. 1997, for information about the course of language development in infants and toddlers after focal brain injuries). Rather, our question is how language abilities and their neural organization turn out, many years after the stroke, when they are teenagers or young adults. This is many years after most assessments have been conducted—an important question since other research has shown that they may develop language abilities more slowly than healthy controls. As Lenneberg asked, does their language develop successfully, despite their injuries to the left hemisphere brain areas normally dedicated to language, and does it do this by successfully reorganizing to healthy brain areas? Lenneberg suggested that the right and left hemispheres in very young children were equipotential for language and that, after left hemisphere injury, the right hemisphere could support normal language development. However, others since that time have argued that the left hemisphere is specialized and privileged for language and that the right hemisphere cannot support complex syntax (e.g., Dennis & Whitaker 1976, Raja Beharelle et al. 2010). Our project aims at addressing these important questions once more.

There are also important clinical questions that our research can address. While many children perform well after perinatal strokes, 25–45 % have some long-term impairments (often to motor or executive functions) that restrict their success in academic and everyday life (Lynch 2009). The common clinical picture for perinatal stroke is usually a mild hemiparesis—many of our participants walk with a slight limp and may have limited control of their hand and fingers on the affected side; but for most there is good cognitive and language development, even with very large infarcts. Our participants are at grade level in school, and some are honors students, though many require extra time on tests in school. Recent studies have demonstrated some language impairments, particularly for high-level language (Ballantyne et al. 2007), and some evidence that remaining LH areas (rather than homotopic areas of the RH) may be crucial for these skills (Raja et al. 2010). A better understanding of outcomes and the variables that correlate with outcome variations can provide a foundation for developing improved treatments.

We address these questions—Is the young brain successfully plastic? Can language be successful acquired by the RH if the LH is damaged?—by testing a group of teenagers and young adults who have had a large perinatal stroke to the LH MCA territory; and, for comparison, teenagers and young adults with comparable infarcts to the RH, and healthy controls (including their siblings) who are matched to these groups in age and socio-economic status. Each participant and their families spend 3–4 days with us and are given a large battery of behavioral tests (verbal and performance IQ tests, tests of executive function and ADHD, and carefully selected tests of processing and producing linguistic syntax, morphology, and prosody) and are also given a battery of fMRI tasks we have developed to examine neural activation for these same skills. In the next section we provide an overview of our results thus far for language.

3. Our Participants' Injuries and Their Neural Activations for Language

All participants undergo an anatomic MRI scan (an MP-RAGE), which provides a picture of the structure of their brain, and a functional scan examining their activation for language using a task called the Auditory Definition Decision Task (ADDT), along with other fMRI tasks. The ADDT was developed by Gaillard and colleagues (Gaillard et al. 2004, 2007, Berl et al. 2014) as part of an fMRI battery used to examine the neural activation for language in healthy children and in children with chronic seizure disorders. The ADDT involves a block design in which sentences like *A large gray animal is an elephant.* (the forward speech condition) are contrasted with the same sentences played backwards (the backward speech condition) and with blocks of silence. In the forward condition, participants push a button if the sentence is true; in the backwards condition they push a button when they hear a beep (which are matched in distribution and frequency to the button pushes required in the forward condition). The similarities between these conditions thus control for auditory and motor activation; the activation differences between conditions are thus due to processing and understanding the sentence. Task difficulty is kept constant across groups and individuals, at 90 % correct or better for all participants, by selecting one of 4 levels of word frequency for the target words (e.g. elephant). We administer other fMRI language tasks to our participants as well, but this task has the advantage of activating virtually all of the LH language network in healthy controls and therefore also reveals where this network is localized after a LH or RH stroke.

The activation pattern from a group of healthy children (Gaillard et al. 2007, Berl et al. 2014) is shown in Figure 1. Voxels with significantly greater activation for forward speech over backward speech in the ADDT are indicated in yellow/orange. The left panel shows a side view of the LH, with activation throughout the frontal and temporal lobe language areas; the right panel shows the complementary view of the RH, where there is minimal activation. This is the typical pattern of strong LH lateralization for language in healthy individuals.

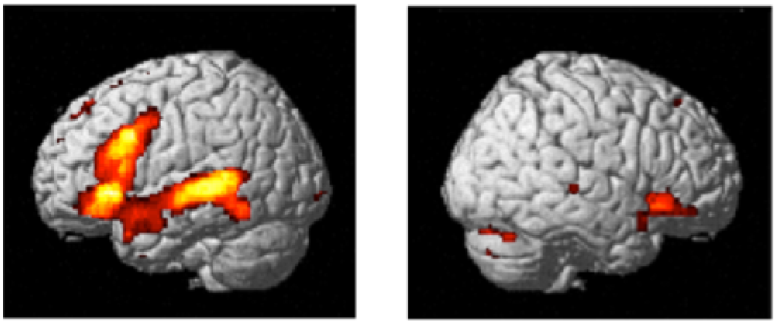


Figure 1: Activation for forward > backward speech in the ADDT for healthy children ages 4–12 (n = 68). (Based on Berl et al. 2014.)

The ADDT elicits such robust and reliable activation that we can also examine patterns of activation in individual participants, which is important for understanding neural plasticity and patterns of language organization after stroke. Figure 2 shows some example ADDT scans from 6 individuals tested in our ongoing research. These are axial scans (horizontal slices, with the front of the head at the top, back of the head at the bottom) and are in neurological orientation (the LH is on the left). Voxels with significantly greater activation for forward speech over backward speech are colored in yellow/orange. On the left top and bottom are two participants who are healthy controls (siblings of the patients); on the right top and bottom are two participants who have had a RH perinatal stroke. Both the healthy controls and the participants with RH strokes show the expected activation in LH temporal and frontal language areas. (The blob of activation in the LH toward the front is in the frontal region; the blob toward the back is in the LH temporal region.)[1] In contrast, the participants with LH strokes (in the middle) both show their language activation in the *right hemisphere homotopic areas*. Thus far we find this pattern of activation in the RH areas homotopic to the normal LH language network for all of the participants who have sizeable LH infarcts. (Only those with very small LH infarcts retain language activation in the typical areas of the LH). No other patterns of language activation appear across the 12 participants we have tested to date.

These results for language activation accord with what Lenneberg suggested on the basis of the clinical literature, well before imaging was available: after major left-hemisphere injury during very early infancy, language apparently ‘shifts’ to the right hemisphere.[2] (It is important to note, however, that this may not actually be a ‘shift’ but rather the maintenance and enhancement of early bilateral language, as Lenneberg also suggested; see a brief discussion of this hypothesis at the end of this paper, and see Berl et al. 2014a and Olulade et al., in preparation, for evidence.) This ‘shift’ of language to the right hemisphere does not successfully occur after stroke in adults (Turkeltaub et al 2011).

An important follow-up question, then, is whether the RH can fully support language processing and do so as well as—or almost as well as—the left hemi-

---

[1] The box around the frontal regions in the RH stroke patients indicate that we have combined a slice showing frontal activation with a different slice showing temporal activation. This is not an important feature of the imaging; in many individuals the strongest activations for frontal and temporal regions do not appear on the same brain slice.

[2] An important question is whether atypical fMRI activations reflect atypical neural organization for language, or rather whether activations arise from compensatory strategies, errorful performance (Fair et al. 2010), increased difficulty of the tasks, or feed forward/feedback (Price & Crinion 2005). For example, Raja Beharelle et al. (2010) showed that LH voxel activation best predicts language performance, even when the main activations are in the RH. Unfortunately, other techniques for testing language localization (e.g., using TMS to temporarily inactivate areas hypothesized to be crucial for language) are not safe for participants at higher seizure risk. However, several findings suggest that our fMRI activation patterns do reveal cortical language organization. First, we have selected participants for this research who have relatively large LH MCA infarcts with little or no healthy tissue in relevant LH areas for supporting language. Many of our patients have complete LH MCA infarcts (one has an infarct that encompasses the entire LH), and yet all show normal conversational language abilities and test scores. We have also designed our fMRI tasks to reduce such problems. We adjust task difficulty across participants to achieve over 90 % correct performance, reducing the likelihood that RH activation arises from task difficulty differences across groups.

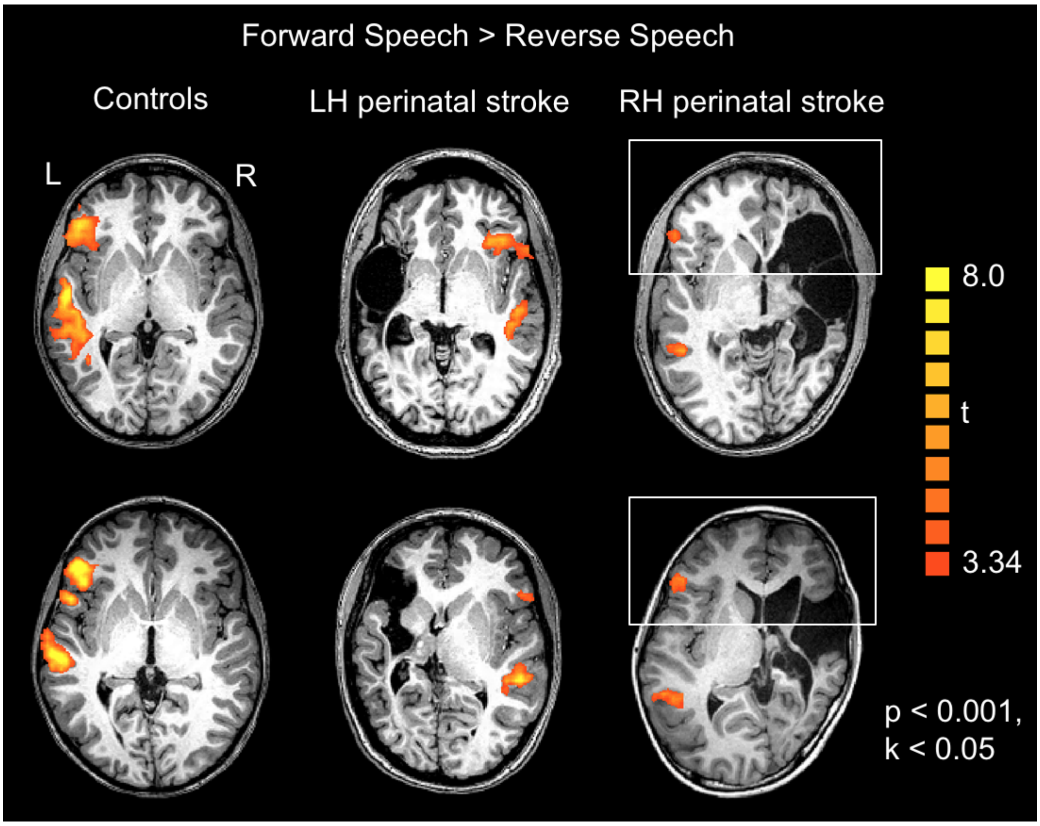
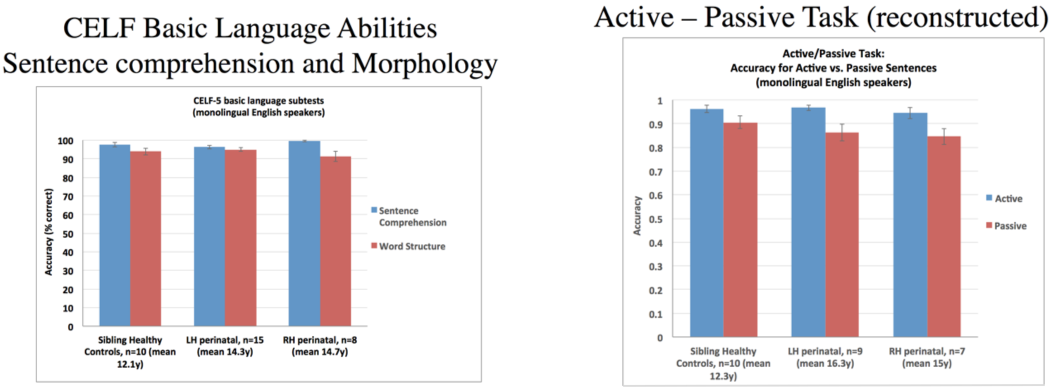


Figure 2: Example individual scans from the ADDT task, with orange/yellow showing voxels that are activated significantly more for forward than for backward speech. The leftmost scans are from two healthy controls; the rightmost scans are from two participants who had a RH perinatal stroke; and the scans in the middle are from two participants who had a LH perinatal stroke. All participants were teenagers at the time of testing.

sphere. Research on patients with a hemispherectomy during infancy (Dennis & Whitaker 1976) has argued that the LH is privileged for processing syntax and that patients who had LH surgery (even early in life) are less able to process complex syntax than those who had RH surgery. Lidzba et al. (2013) have suggested that there is a comparable result for children with perinatal stroke, although they have compared children with LH stroke only to healthy controls. In contrast, others have found no difference in language skills after early LH versus RH injury (Bates et al. 2001, Ballantyne et al. 2007, Feldman et al. 2002, Liégeois et al. 2008).

Dennis & Whitaker's findings predict that teenagers and young adults with RH language (LH stroke) will score comparably on simple sentences but will show greater deficits on complex syntax than those with LH language (RH stroke). Figure 3 shows our participants' performance on two different types of language tasks: on the left, in simple sentence comprehension and use of English morphology (two subtests from the Clinical Evaluation of Language Fundamentals; CELF-5), and on the right, in a more complex syntax task testing the comprehension of affirmative and negative active versus passive sentences (a task developed in our lab as a reconstruction of the Active-Passive task used by Dennis & Whitaker (1976) for testing hemispherectomy patients). It is important to note that these tasks, and others we are using, have been chosen carefully to assess linguistic skills through tasks that are as free as possible of extraneous executive function demands (which we know are impaired in individuals who have had a stroke). While our research is still in progress, thus far the LH perinatal stroke group (with RH language) scores almost exactly as well as their healthy siblings (with LH language) and the RH perinatal stroke group (also with LH language, but with brain injuries comparable in size to those of the LH stroke group). These results do suggest, then, that the left hemisphere and the right hemisphere may be relatively equipotential for language early in life and that either one can successfully support language development after very early injuries to the opposite hemisphere.


CELF Basic Language Abilities
Sentence comprehension and Morphology

Active – Passive Task (reconstructed)

Figure 3: Language abilities of our three participant groups. On the left, language abilities in two subtests from the CELF; on the right, in the Active-Passive Test

4. Discussion and Conclusions of our Results to Date

Taken together, these results fully support Lenneberg's original suggestions about language and its representation in the brain after early injury. First, virtually all of the participants we have studied—those with fairly large perinatal strokes to left hemisphere frontal and temporal lobes—show their activation for sentence processing in the *right hemisphere homotopic regions* as teenagers or young adults. (See the same result also in 7 participants tested on the same task after perinatal stroke, included in a larger study of language laterality in individuals with chronic epilepsy by Berl et al. 2014b.) Second, their performance on both simple and complex language processing tasks, testing syntax and morphology, is very good and even equal to their healthy siblings and to matched participants with damage to the right hemisphere, as long as we test them with fairly natural language production or comprehension tasks that do not require extensive executive function demands. Overall, then, these results suggest that the infant brain does have a high degree of plasticity for supporting language in either the left or the right hemisphere, as Lenneberg suggested.

However, it is also extremely important to emphasize that the plasticity we see for language is highly constrained. After left hemisphere injuries, language does not develop in a wide range of alternative locations. Rather, as suggested by Gaillard and colleagues for atypical language after early chronic seizures and by

Booth and colleagues, Feldman and colleagues, Holland and colleagues, Szaflarski and colleagues, as well as Staudt, Lidzba, Wilke and colleagues for perinatal stroke, in the face of severe early left hemisphere injuries to the normal language areas, language virtually always and only develops in the right hemisphere homologues.

Why might atypical neural organization for language be restricted to these right hemisphere areas? Many researchers have referred generally to the high degree of plasticity apparent in the young brain and have talked about 'reorganizing' language to the right hemisphere. While a mechanism for such 'reorganization' is not often articulated, the implication is that, in the young brain, areas not ordinarily subserving language can *take on new functions* in the face of injury. (See, for example, Finger 2009 for a discussion of *vicariation*.) In contrast, our own hypothesis, again following Lenneberg's original suggestions, is what we call the *Developmental Origins Hypothesis*. This hypothesis suggests that much of what appears to be reorganization of cognitive functions may actually be the outcome of more distributed functional representations in the healthy young brain. In particular, we and others have found that early language abilities are more bilateral than those of older children and adults (Berl et al 2014a; Szaflarski et al 2006; Newport et al in preparation; Olulade et al in preparation). We believe that this initially bilateral representation of language permits the *maintenance and enhancement of right hemisphere language development* when the left hemisphere is injured. But again, our research is still ongoing and investigates other accounts as well.

4.1. *Questions for the Future*

Our initial results also serve as an entrée to research on a number of further questions: What are the consequences of atypical organization of language in children? In particular, what are the effects on typically right hemisphere cognitive functions when language is also controlled in part or in whole by the RH?

4.2. *Potential Effects on Right Hemisphere Language Functions*

While we often say that language is left lateralized in the healthy brain, we usually mean to refer in this statement to only certain aspects of language. In reality, of course, 'language' is comprised of a number of coordinated functions, not all of which are ordinarily lateralized to the left hemisphere. The most well studied of these—naming and word recognition, sentence production and comprehension—are indeed ordinarily lateralized to the left hemisphere in healthy adults (Broca 1861, Gazzaniga & Sperry 1967), as noted above. But there are also some linguistic functions that are ordinarily lateralized to the homotopic regions of the right hemisphere, including the processing of vocal emotion and of linguistic intonation (e.g., tonal contrasts indicating statement vs. question; Ross & Monot 2008, Wildgruber et al. 2004, 2005). If early left hemisphere injury results in left-hemisphere language functions being reorganized to the right hemisphere, how are they integrated with the other functions of the right hemisphere? Does one set of functions dominate, resulting in savings to those but impairments to the others? Do the functions mix, with impairments to all? Or do they each find their own distinct territories in the right hemisphere, through normal Hebbian processes of competition, and peacefully coexist? Yet another possibility is that there are individual differences in neural outcomes, leading to the individual differences in functional outcomes described in the clinical literature. We are still in the process of investigating these questions.

4.3. *Potential Effects on Other Right Hemisphere Functions*

The Crowding Hypothesis (Teuber 1974) posed a similar question regarding the effects of atypical language organization on right-lateralized visual-spatial functions. This hypothesis suggests that, due to its importance for human cognition, language will take priority in either hemisphere that is available, but then potentially leaves less neural territory or computational power for visual-spatial functions if crowded into the same hemisphere. Since visual-spatial functions are generally attributed to the parietal lobe (not the frontal and temporal areas involved in language processing), it is unclear why there would necessarily be such an interaction, but available evidence has suggested impaired visual-spatial cognitive abilities after LH perinatal injury (Lidzba et al. 2006, Stiles et al. 2012). However, only a few tasks have previously been used to assess visual-spatial functions in the context of the Crowding Hypothesis, so our research is re-examining these questions as well.

4.4. *Principles and Mechanisms of Developmental Plasticity*

Finally, the overarching questions that arise from Lenneberg's original suggestions and our own and others' evidence on these hypotheses concern the principles and mechanisms underlying developmental plasticity. Is the young brain endlessly plastic, with the capability for drastic reorganization of function, or are there important constraints and principles of developmental plasticity that have not been extensively addressed in the literature? Our results, combined with those already in the clinical literature, suggest that there are very limited and patterned ways in which language develops in the human brain. In the healthy brain, virtually everyone (approximately 99 % of right handers and 75 % of left handers; altogether approximately 95 % of the population) develops language in the same frontal and temporal lobe regions of the left hemisphere. When there is early left hemisphere injury, language develops in these regions if the lesion is very small, or in the homotopic regions of the right hemisphere if the injury is large. In Gaillard and Berl's work (Berl et al. 2014b, Mbwana et al. 2009), chronic seizure in middle childhood may result in an unusual combination of these two patterns (left hemisphere temporal activation; right hemisphere frontal activation). No other patterns of atypical language organization have been well documented.

Again following Lenneberg, we have also suggested a reason why the right hemisphere regions homotopic to the normal language network are capable of supporting language after early left hemisphere injury. Very young children show more bilateral representation of language than is seen in older children and adults (Lenneberg 1967, Szaflarski et al. 2006, Holland et al. 2001, Berl et al. 2014a, Newport et al., in preparation, Olulade et al., in preparation); that is, early in life, the right hemisphere homotopic regions are heavily involved in language processing even in the healthy brain. We hypothesize, then, that this forms the basis for the enhancement of these regions' involvement in language processing in the face of early injury.

It is important to note that these findings and hypotheses require further evidence. Even 50 years after Lenneberg's suggestions, these issues regarding developmental plasticity remain unresolved. But, with gratitude to Lenneberg for his remarkable insights and for his stimulation of 50 years of fascinating discussion and controversy, these questions continue to be a hot topic on the cutting edge of the science of neural plasticity.

References

Ballantyne, Angela O., Amy M. Spilkin, & Doris A. Trauner. 2007. Language outcome after perinatal stroke: Does side matter? *Child Neuropsychology* 13, 494–509.

Banich, Marie T., Susan C. Levine, Hongkeun Kim, & Peter Huttenlocher. 1990. The effects of developmental factors on IQ in hemiplegic children. *Neuropsvchology* 28, 35–47.

Basser, L. S. 1962. Hemiplegia of early onset and the faculty of speech with special reference to the effects of hemispherectomy. *Brain* 85, 427–460.

Bates, Elizabeth, Donna Thal, Doris Trauner, Judi Fenson, Dorothy Aram, Julie Eisele, & Ruth Nass. 1997. From first words to grammar in children with focal brain injury. *Developmental Neuropsychology* 13, 447–476.

Bates, Elizabeth, Judy S. Reilly, Beverly Wulfeck, Nina Dronkers, Meiti Opie, Judi Fenson, Sarah Kriz, Rita Jeffries, LaRae Miller, & Kathryn Herbst. 2001. Differential effects of unilateral lesions on language production in children and adults. *Brain and Language* 79, 223–265.

Bates, Elizabeth & Katherine Roe. 2001. Language development in children with unilateral brain injury. In Charles A. Nelson & Monica Luciana (eds.), *Handbook of Developmental Cognitive Neuroscience*, 281–307. Cambridge, MA: MIT Press.

Bedny, Marina, Alvaron Pascual-Leone, David Dodell-Feder, Evelina Fedorenko, & Rebecca Saxe. 2011. Language processing in the occipital cortex of congenitally blind adults. *Proceedings of the National Academy of Sciences of the United States of America* 108, 4429–4434.

Berl, Madison M., Jessica Mayo, Erin N. Parks, Lisa R. Rosenberger, John VanMeter, Nan Bernstein Ratner, Chandan J. Vaidya, & William D. Gaillard. 2014a. Regional differences in the developmental trajectory of lateralization of the language network. *Human Brain Mapping* 35, 270–284.

Berl, Madison M., Lauren A. Zimmaro, Omar I. Khan, Irene Dustin, Eva Ritzl, Elizabeth S. Duke, Leigh N. Sepeta, Susumu Sato, William H. Theodore, & William Gaillard. 2014b. Characterization of atypical language activation patterns in focal epilepsy. *Annals of Neurology* 75, 33–42.

Booth, James R., Brian MacWhinney, Keith R. Thulborn, Kelley Sacco, James T. Voyvodic, & Heidi M. Feldman. 2000. Developmental and lesion effects in brain activation during sentence comprehension and mental rotation. *Developmental Neuropsychology* 18, 139–169.

Broca, Paul. 1861. Remarques sur le siége de la faculté du langage articulé, suivies d'une obsevation d'aphémie (perte de la parole) [Remarks on the seat of the faculty of articulated language, following an observation of Aphemia (loss of speech)]. *Bulletin de la Société Anatomique* 6, 330–357.

Dennis, Maureen& Harry A. Whitaker. 1976. Language acquisition following hemidecortication: Linguistic superiority of the left over the right hemisphere. *Brain and Language* 3, 404–433.

DeVeber, Gabrielle. 2005. In pursuit of evidence-based treatments for paediatric stroke: The UK and Chest guidelines. *The Lancet: Neurology* 4, 432–436.

Dick, Anthony S., Anjali Raja Beharelle, Ana Solodkin, & Steven L. Small. 2013. Interhemispheric functional connectivity following prenatal or perinatal brain injury predicts receptive language outcome. *Journal of Neuroscience* 33, 5612–5625.

Fair, Damien A., Timothy T. Brown, Steven E. Peterson, & Bradley L. Schlaggar. 2006. fMRI reveals novel functional neuroanatomy in a child with perinatal stroke. *Neurology* 67, 2246–2249.

Fair, Damien A., Alexander H. Choi, Yannic B. L. Dosenbach, Rebecca S. Coalson., Francis M. Miezin, Steven E. Petersen, & Bradley L. Schlaggar. 2010. The functional organization of trial-related activity in lexical processing after early left hemispheric brain lesions: An event-related fMRI study. *Brain and Language* 114, 135–146.

Feldman, Heidi M., Brian MacWhinney, & Kelley Sacco. 2002. Sentence processing in children with early unilateral brain injury. *Brain and Language* 83, 335–352.

Finger, Stanley. 2009. Recovery of function: Redundancy and vicariation theories. In Stanley Finger, François Boller, Kenneth L. Tyler (eds.), *Handbook of Clinical Neurology* (vol. 95), 833–841. New York, NY: Elsevier.

Gaillard, William D., L. Balsamo, B. Xu, C. McKinney, P. H. Papero, S. Weinstein, J. Conry, P. L. Pearl, B. Sachs, S. Sato, L. G. Venzina, C. Frattali, & W. H. Theodore. 2004. fMRI language task panel improves determination of language dominance. *Neurology* 63, 1403–1408.

Gaillard, William D., Madison M. Berl, E. N. Moore, E. K. Ritzl, L. R. Rosenberger, . . . , & W. H. Theodore. 2007. Atypical language in lesional and nonlesional complex partial epilepsy. *Neurology* 69, 1761–1771.

Gazzaniga, Michael S. & Roger W. Sperry. 1967. Language after section of the cerebral commissures. *Brain* 90, 131–148.

Holland, Scott K., Elana Plante, Anna Weber Byars, Richard H. Strawsburg, Vince J. Schmithorst, William S. Ball, Jr. 2001. Normal fMRI brain activation patterns in children performing a verb generation task. *NeuroImage* 14, 837–843.

Jacola, Lisa M., M. B. Schapiro, Vince J. Schmithorst, Anna Weber Byars, Richard H. Strawsburg, J. P. Szaflarski, Elana Plante, & Scott K. Holland. 2006. Functional magnetic resonance imaging reveals atypical language organization in children following perinatal left middle cerebral artery stroke. *Neuropediatrics* 37, 46–52.

Kennard, Margaret A. 1940. Relation of age to motor impairment in man and in subhuman primates. *Archives of Neurology and Psychiatry* 44, 377–397.

Kolb, Bryan, Robin Gibb, & Grazyna Gorny. 2000. Cortical plasticity and the development of behavior after early frontal cortical injury. *Developmental Neuropsychology* 18, 423–444.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Levine, Susan C., Ruth Kraus, Erin Alexander, Linda W. Suriyakham, & Peter R. Huttenlocher. 2005. IQ decline following early unilateral brain injury: A longitudinal study. *Brain and Cognition* 59, 114–123.

Lidzba, Karen, Andreas Konietzko, Eleonore Schwilling, Inge Krägeloh-Mann, & Susanne Winkler. 2013. Processing of non-canonical word-order: A case-series on lesion-induced reorganized language and age-effects in typical development. *Brain and Language* 127, 377–387.

Lidzba, Karen, Martin Staudt, Marko Wilke, & Ingeborg Krageloh-Mann. 2006. Visuospatial deficits in patients with early left hemispheric lesions and functional reorganization of language: Consequence of lesion or reorganization? *Neuropsychologia* 44, 1088–1094.

Liégeois, Frédérique, A. Connolly, J. Helen Cross, S. G. Boyd, D. G. Gadian, Faraneh Vargha-Khadem, & T. Baldeweg. 2004. Language reorganization in children with early-onset lesions of the left hemisphere: An fMRI study. *Brain* 127, 1229–1236.

Loring, David W., Kimford J. Meador, Gregory P. Lee, & Don W. King. 1992. Amobarbital effects and lateralized brain function: The Wada Test. New York, NY: Springer.

Lynch, John K. 2009. Epidemiology and classification of perinatal stroke. *Seminars in Fetal & Neonatal Medicine* 14, 245–249.

Mbwana, J., M. M. Berl, E. K. Ritzl, L. Rosenberger, J. Mayo, …, & W. D. Gaillard. 2009. Limitations to plasticity of language network reorganization in localization related epilepsy. *Brain* 132, 347–356.

Moesch, Sonja C., Jeffrey E. Max, & Daniel Tranel. 2005. A matched lesion analysis of childhood versus adult-onset brain injury due to unilateral stroke: Another perspective on neural plasticity and recovery of social functioning. *Cognitive and Behavioral Neurology* 18, 5–17.

Montour-Proulx, Isabelle, Claude M J. Braun, Sylvie Daigneault, Isabelle Rouleau, Sally Kuehn, & Jean Bégin. 2004. Predictors of intellectual function after a unilateral cortical lesion: Study of 635 patients from infancy to adulthood. *Journal of Child Neurology* 19, 935–943.

Raja Beharelle, Anjali, Anthony Steven Dick, Goulven Josse, Ana Solodkin, Peter R. Huttenlocher, Susan C. Levine, & Steven L. Small. 2010. Left hemisphere regions are critical for language in the face of early left focal brain injury. *Brain* 133, 1707–1716.

Rasmussen, Theodore & Brenda Milner. 1977. The role of early left-brain injury in determining lateralization of cerebral speech functions. *Annals of the New York Academy of Sciences* 299, 355–369.

Rosenberger, L. R., J. Zeck, M. M. Berl, E. N. Moore, E. K. Ritzl, …, & W. D. Gaillard. 2009. Interhemispheric and intrahemispheric language reorganization in complex partial epilepsy. *Neurology* 72, 1830–1836.

Ross, Elliott D. & Marilee Monot. 2008. Neurology of affective prosody and its functional-anatomic organization in right hemisphere. *Brain & Language* 104, 51–74.

Sébire, Guillaume, Heather Fullerton, Emilie Riou, & Gabrielle deVeber. 2004. Toward the definition of cerebral arteriopathies of childhood. *Current Opinion* *in Pediatrics* 16, 617–622.

Stiles, Joan, Judy S. Reilly, Susan C. Levine, Doris A. Trauner, & Ruth Nass. 2012. *Neural Plasticity and Cognitive Development: Insights from Children With Perinatal Brain Injury*. New York, NY: Oxford University Press.

Staudt, Martin, Karen Lidzba, Wolfgang Grodd, Dirk Wildgruber, Michael Erb, & Ingeborg Krägeloh-Mann. 2002. Right-hemispheric organization of language following early leftsided brain lesions: Functional MRI topography. *NeuroImage* 16, 954–967.

Staudt, Martin. 2007. (Re-)organization of the developing human brain following periventricular white matter lesions. *Neuroscience and Biobehavioral Reviews* 31,1150–1156.

Szaflarski, Jerzy P., Scott K. Holland, Vincent J. Schmithorst, & Anna W. Byars. 2006. fMRI study of language lateralization in children and adults. *Human Brain Mapping* 27, 202–212.

Teuber, Hans L. 1974. Why two brains? In F. O. Schmitt & F. G. Worden (eds.), *The Neurosciences: Third Study Program*, 71–74. Cambridge, MA: MIT Press.

Tillema, Jan-Mendelt, Anna W. Byars, Lisa M. Jacola, Mark B. Schapiro, Vince J. Schmithorst, Jerzy P. Szaflarski, & Scott K. Holland. 2008. Cortical reorganization of language functioning following perinatal left MCA stroke. *Brain and Language*105, 99–111.

Turkeltaub, Peter E., Samuel Messing, Catherine Norise, & Roy H. Hamilton. 2011. Are networks for residual language function and recovery consistent across aphasic patients? *Neurology* 76, 1726–1734.

Vargha-Khadem, Faraneh, A. M. O'Gorman & G. V. Watters. 1985. Aphasia and handedness in relation to hemispheric side, age at injury, and severity of cerebral lesion during childhood. *Brain* 108, 677–696.

Westmacott, Robyn, Rand Askalan, Daune Macgregor, Peter Anderson, & Gabriele Deveber. 2010. Cognitive outcome following unilateral arterial ischaemic stroke in childhood: effects of age at stroke and lesion location. *Developmental Medicine & Child Neurology* 52, 386–393.

Wildgruber, D., I. Hertrich, A. Riecker, M. Erb, S. Anders, W. Grodd, W., & H. Ackermann. 2004. Distinct frontal regions subserve evaluation of linguistic and emotional aspects of speech intonation. *Cerebral Cortex* 14, 1384–1389.

Wildgruber D, A. Riecker, I. Hertrich, M. Erb, W. Grodd, T. Ethofer, & H. Ackermann. 2005. Identification of emotional intonation evaluated by fMRI. *NeuroImage* 24, 1233–1241.

You, Xiaozhen, Malek Adjouadi, Magno R. Guillen, Melvin Ayala, Armando Barreto, …, & William D. Gaillard. 2011. Sub-patterns of language network reorganization in pediatric localization related epilepsy: A multisite study. *Human Brain Mapping* 32, 784–799.

Elissa L. Newport
Georgetown University
Center for Brain Plasticity and Recovery
4000 Reservoir Road, N.W.
Washington D.C. 20057
United States of America

eln10@georgetown.edu

Anna Seydell-Greenwald
Georgetown University
Center for Brain Plasticity and Recovery
4000 Reservoir Road, N.W.
Washington D.C. 20057
United States of America

as2266@georgetown.edu

Catherine E. Chambers
Georgetown University
Center for Brain Plasticity and Recovery
4000 Reservoir Road, N.W.
Washington D.C. 20057
United States of America

catherine.chambers@georgetown.edu

Jessica Carpenter
Children's National Medical Center
111 Michigan Ave N.W.
Washington D.C. 20010
United States of America

jcarpent@childrensnational.org

William D. Gaillard
Children's National Medical Center
111 Michigan Ave N.W.
Washington D.C. 20010
United States of America

wgaillar@childrensnational.org

Barbara Landau
Johns Hopkins University
Department of Cognitive Science
241 Krieger Hall
Baltimore, MD. 21218
United States of America

landau@jhu.edu

Peter E. Turkeltaub
Georgetown University
Center for Brain Plasticity and Recovery
4000 Reservoir Road, N.W.
Washington D.C. 20057
United States of America

turkeltp@georgetown.edu

Alexander W. Dromerick
MedStar National Rehabilitation Hospital &
Georgetown University
Center for Brain Plasticity and Recovery
4000 Reservoir Road, N.W.
Washington D.C. 20057
United States of America

alexander.w.dromerick@medstar.net

Madison M. Berl
Children's National Medical Center
111 Michigan Ave N.W.
Washington D.C. 20010
United States of America

mberl@childrensnational.org

★ FORUM ★

From Zero to Fifty: Considerations on Eric Lenneberg's *Biological Foundations of Language* and Updates

Massimo Piattelli-Palmarini

1. Preamble

Had I been teaching a graduate course in biolinguistics in the years 1968–1975, I would have had the perfect textbook: Eric H. Lenneberg's *Biological Foundations of Language*. Everything was right in it: general considerations, updated expositions of neuroscience, genetics, developmental biology and, of course, language, beautifully complemented by an appendix by Noam Chomsky. The prudence with which extrapolations are suggested and Lenneberg's unwavering honesty in pointing out the tentativeness of some suggestions, are a model for us all. I am teaching biolinguistics now, but so many things have happened in the intervening fifty years that I could not use it as a textbook, possibly with the exception of the last chapter "Toward a biological theory of language development (general summary)," with only some minor additions and clarifications.

2. The Road Ahead

Lenneberg's (1967) intuitions about what lay ahead in the future are remarkable, some offer almost superhuman prescience. A brief sample:

> The evidence is strong that speech and language are not confined to the cerebral cortex.[1] (1967: 64)
>
> Cortical projection areas do not contain percepts nor are any other cortical areas the depository of thoughts; whatever the nature of the signals that travel through transcortical fibers, they cannot be identified with the phenomenal content of experience.[2] (1967: 213)

His approach to lexical semantics, being non-referential and entirely intensional, is unquestionably right, foreshadowing later work by Noam Chomsky, but also Paul

---

I am indebted to Noam Chomsky for suggestions on a previous draft.

[1] For an update and confirmations see (Piattelli-Palmarini, in press) and references therein.

[2] A detailed and cogent explanation of why it's so is to be found in the (alas poorly known and insufficiently appreciated) book by C. R. Gallistel and A. P. King (Gallistel & King 2011).

ISSN 1450–3417

Biolinguistics 11.SI: 423–444, 2017
http://www.biolinguistics.eu

Pietroski, and James McGilvray (Pietroski 2003, 2005, McGilvray 1998). On page 333 we read:

> Words are not the labels of concepts completed earlier and stored away; they are the labels of a *categorization process or family of such processes*.
>
> (1967: 333; emphasis in original)

Then, on the following page, italicized in the original, he says:

> Words tag the processes by which the species deals cognitively with its environment.[3] (1967: 334)

On page 366, he adds:

> Natural languages differ in the particular conceptualization processes that are reflected in their vocabulary. However, since speakers use words freely to label *their own conceptualization processes* [emphasis in original], the static dictionary meaning of words does not appear to restrict speakers in their cognitive activities: thus it is not appropriate to use the vocabulary meanings as the basis for an estimation of cognitive capacities.
>
> (1967: 366)

Finally, we read:

> Until rigorous proof is submitted to the contrary, it is more reasonable to assume that all natural languages are of equal complexity and versatility and the choice of this assumption detracts much from the so-called relativity theory. (1967: 364)

The above is part of Lenneberg's cogent critique of cognitive relativism and of the Sapir-Whorf hypothesis. He adds that there are: "enormous similarities between the cognitive functioning of all individuals" (p. 336). Later work by Lila Gleitman, Anna Papafragou and collaborators (Li, Abarbanell, Gleitman, & Papafragou 2011, Li & Gleitman 2002, Papafragou, Cassidy, & Gleitman 2007) and Charles Randy Gallistel (Gallistel 2002) have dispelled all remnants of plausibility of this hypothesis.[4]

The separation between semantics, pragmatics and communication is clearly outlined. Chomsky's distinction between competence and performance is adopted and corroborated by data and arguments. On page 355, Lenneberg says:

> Efficiency of communication is mostly dependent upon such extra-semantic factors as the number of and perceptual distance between discriminanda.
>
> (1967: 355)

---

[3] Giuseppe Vitiello and myself have suggested (what we think are) interesting validations of an internalist semantics from quantum field theory (Piattelli-Palmarini & Vitiello 2015, 2017).

[4] Of special cogency is the uniformity of understanding of belief-verbs in children who speak a language with morphemic evidentials (as in Korean and Turkish) and children who are speakers of languages without them (English). A difference would have been clear evidence in favor of cognitive relativism, because there is nothing parents can "show" when conveying degrees of reliability of assertions and the available kind of evidence for that assertion. It's all morpho-syntactic and semantic, therefore only internal and intensional.

The prevalence of syntactic structure over communication is also a centerpiece of his counters to an adaptationist neo-Darwinian account of language evolution, an important topic on which I will return shortly.

Lenneberg's defense of innate predispositions for language acquisition and of the central role of maturation, on the basis of genetic and general biological processes, is unparalleled.

On page 221, after an insightful discussion on humans, animals and machines, we read:

> There is, then, nothing unscientific about the claim that a species-specific behavior pattern, such as language, may well be determined by innate mechanisms. (1967: 221)

Then, on page 393, we read:

> There was a time when "innateness" was on the index of forbidden concepts. Much has changed in the official censorship of technical terms, but there are still many scientists who regard the postulation of anything innate as a clever parlor trick that alleviates the proponent from performing "truly scientific" investigations. (1967: 393)

This caveat is, alas, still applicable today.

He concludes the whole book by stating, quite correctly, that:

> No features that are characteristic of only certain natural languages, either particulars of syntax, or phonology, or semantics, are assumed here to be innate. However, there are many reasons to believe that the *processes* [emphasis in original] by which the realized, outer structure of a natural language comes about are deeply-rooted, species-specific, innate properties of man's biological nature. (1967: 394)

Not a word needs to be changed today.

Let's now embark on a summary exposition of important updates, of how and why knowledge has grown in later years, much in line with what Lenneberg had insightfully anticipated.

3. The Brain

The quest of what, in the human brain, makes us unique has been relentless. Overall size, relative size with respect to body size, volume and density of the cerebral cortex, size of neurons, degree of interconnectivity, have all been painstakingly examined. A number of Lenneberg's analyses and graphs are still valid, but there have been new discoveries. Brain evolution must today be framed in the rich new domain of evo-devo, the booming recent revolution integrating the study of evolution with that of ontogenesis. The motto is: "evolution is the evolution of ontogenies" (Laubichler & Maienschein 2007, Raff 2000, Carroll 2005). In this huge literature, special mention is due to Sprecher and Reichert (2003) as well as Striedter (2006), where the remarkable differences between the nervous system of vertebrates (dorsal) and invertebrates (ventral) is reconstructed as an initial mirror inversion of morphogenetic gradients piloted by, essentially, equivalent genes.

In tune with evo-devo, and confirming several of Lenneberg's intuitions, the key to the differences between animal species are the developmental morphogenetic routes regulated by the patterned activation/inactivation of genes along the ontogenetic timing. The complexity of gene regulatory networks defies our imagination and needs elaborate computer graphs to be analyzed (Davidson 2006, 2010, Davidson & Erwin 2006). In spite of this, the remarkable conservation of genes all along biological evolution has been a major discovery. In her Nobel lecture the German geneticist and embryologist Christiane Nüsslein-Volhard says:

> Many Drosophila genes have been shown to have homologs in vertebrates. This homology is not restricted to amino acid sequence and to their biochemical function, but extends to the biological role played in development. *This remarkable conservation came as a great surprise. It had been neither predicted nor imagined.* (1995: 295; emphasis added)

The discovery of such conservation, perfected and deepened since 1995, has suggested to the Boston University biochemist and geneticist Michael Sherman the hypothesis of a "universal genome" (Sherman 2007). Possibly exaggerated, but revealing.[5]

The most updated comparative analysis of human brains and primate brains is due to the Brazilian neuroscientist and evolutionary biologist Suzana Herculano-Houzel (2016). The issue of neuronal density does not reveal significant differences. She says:

> Neuronal density does not decrease significantly across primates, as the rest of brain gains neurons [...] [and] neurons in the rest of brain on average become larger with increasing body mass across all species alike.
>
> (Herculano-Houzel 2016)

Herculano-Houzel offers a detailed and persuasive hypothesis about the main factor in human brain evolution: the practice of transforming food (cooking, drying, marinating etc.). This would explain the sudden change in caloric intake, the expansion of the cortex, increased manual dexterity and the relatively small volume of the digestive tract. Not much is said (wisely) about the emergence of language, attributed to:

> Cortical abilities that rely heavily on the associative functions of a prefrontal cortex. Through making more energy available, becoming hunter-gatherers probably put our ancestors on the path toward both benefiting from and being able to afford greater number of neurons in the brain.
>
> (Herculano-Houzel 2016)

The most interesting recent suggestion about brain evolution, human brain ontogenesis and the emergence of language circuits is due to the German neuroscientist Angela Friederici and the Italian neuroscientist Daniela Perani and collaborators (Berwick et al. 2013, Friederici 2012, Friederici & Singer 2015, Perani et al.

---

[5] It should not surprise us that Noam Chomsky likes and cites this hypothesis. Unbeknownst one to the other, Chomsky and Sherman had been using the acronym UG in different contexts. Now they are mutually aware of this coincidence and possibly of a convergence.

2011, Friederici, this issue). In *Why Only Us*, Robert Berwick and Noam Chomsky offer this finding, suggesting, in conformity with the above cited authors, that:

> There are two dorsal pathways, one connecting the mid-to-posterior superior temporal cortex with the premotor cortex and one connecting the temporal cortex with Broca's area. It has been suggested that these two may serve different functions, with the former supporting auditory-to-motor mapping [. . .] and the latter supporting the processing of sentence syntax. There are also two ventral pathways that connect from the region where the "lexicon" is presumed to be, to the front dorsal region. The idea is that these dorsal and ventral fiber tracts together form a complete "ring" that moves information from the lexicon to the areas on the dorsal side where it is used by Merge. The key idea is that this fiber-tract "ring" must be in place in order that syntactic processing work. (Berwick & Chomsky 2016)

Evidence from the lack of a complete formation of this "ring" in nonhuman primates and in infants suggests that this may be a valid explanation of the ontogenetic and phylogenetic emergence of language. Anyway, as Lenneberg had warned us, there is no single, isolated brain region correlated with language, nor is there a "language gene."

4. Genetics

Starting with the pioneering studies of Dorothea McCarthy (1930) and Ella Day (1932) in the Thirties, the original evidence in favor of a genetics of language comes from twins, in particular comparing identical twins and fraternal twins. As well summarized by Lenneberg, identical twins manifest closer similarity in patterns and milestones of language acquisition than fraternal twins.

The awesome development of human genetics in recent years has resulted in a wealth of data on genetic predispositions to various diseases, but has also revealed the fiendish difficulty in lawfully connecting genotypes and phenotypes. Geneticists alert us that:

> Even seemingly simple traits like height are controlled by more than 180 separate genes. Imagine the complexity of the genetic network that determines the structure of the human brain: Billions of neurons connected to one another by at least as many axons. Variations in these links lead to differences among us, and sometimes to disability, but picking out the main connections is not easy. [. . .] Two versions of a protein that guides growth of the prefrontal cortex one of which is known to confer risk of autism generate distinct neural circuits in this region of the brain, possibly explaining the increased risk of autism and other intellectual disabilities in carriers. (Scott-Van Zeeland 2010)

In a recent (June 15, 2017) review of all these studies (Boyle, Li & Pritchard 2017) the authors say:

> Intuitively, one might expect disease-causing variants to cluster into key pathways that drive disease etiology. But for complex traits, association

> signals tend to be spread across most of the genome—including near many genes without an obvious connection to disease. We propose that gene regulatory networks are sufficiently interconnected such that all genes expressed in disease-relevant cells are liable to affect the functions of core disease-related genes and that most heritability can be explained by effects on genes outside core pathways. We refer to this hypothesis as an "omnigenic" model. (Boyle, Li & Pritchard 2017)

The notion of an omnigenic model of how genes affect disease (or, for that matter, also any cognitive trait) is intriguing but also depressing. No wonder that the identification of the genetic bases of language remains elusive.

The identification of the regulatory gene FOXP2 as a speech-relevant gene was followed by much (unjustified) fanfare. One single mutation in a specific locus of one allele of the gene appears to cause dysarthria and other linguistic inadequacies. The precision with which this genetic defect was determined was combined with rather generic, linguistically un-informed, tests. This is why Juan Uriagereka and I said that FOXP2 is a geneticist's dream and a linguist's nightmare (Piattelli-Palmarini & Uriagereka 2011). The enormous diffusion of the GWAS technology (Genome-Wide Association Studies) has quite tentatively identified genes related to dyslexia, language deficits and other intellectual disabilities (Christoforou et al. 2014) with premature assertions as to the heritability of intelligence (Davies et al. 2011).

The recent boom of epigenetic studies (Allis, Jenuwein, Reinberg & Caparros 2006, 2007, Halfmann & Lindquist 2010, Vercelli 2004) fails, to this day, to connect to language. There is, in my opinion, little doubt that epigenetic processes can explain some differences in rates of maturation and language acquisition (one child reaching a syntactic milestone at age, say, 3 years-old, while another child reaches that milestone at, say, 3 years and 6 months) but we will have to wait. Finally, on this topic, mention must be made of the other booming sector: the study of individual differences in the microbiome (Martinez 2014). Less clear is whether this "new kind of biological causality" (sic), important as it is for the child's susceptibility to a variety of diseases (Ober & Nicolae 2011, von Mutius & Vercelli, 2010), will reveal some effect on human brain maturation and language acquisition. The role of microbiota in modulating behavior and neurodevelopmental disorders in the mouse has been shown (Hsiao et al. 2013).[6] Still a far cry from language in humans, but time will tell.

5. Language deficits

Lenneberg's review of cases of aphasia was impressive for his time, but there have been considerable new developments in diagnosis (notably the successive refine-

---

[6] These authors say:

> Gut bacterial effects on the host metabolome impact behavior. Taken together, [our] findings support a gut-microbiome-brain connection in a mouse model of ASD [Autism Spectrum Disorders] and identify a potential probiotic therapy for GI [Gastro Intestinal abnormalities] and particular behavioral symptoms in human neurodevelopmental disorders. (Hsiao et al. 2013)

ments of the Boston Test of Aphasia, now adapted to many languages) and treatment. Other specific language deficits have confirmed the modularity of language and mind (for a recent review, see Curtiss, 2013). Better integration between clinical examination and linguistic theory has allowed for considerable progress, sometimes confirming the posits of syntactic theory by accurate diagnoses and patterns of recovery (Friedmann 2006, Friedmann, Belletti, & Rizzi 2009, Friedmann & Grodzinsky 1997).

Special mention must be made of the identification of SLI (Specific Language Impairment), unknown at the time of Lenneberg's book. After it was precisely defined and accurately diagnosed, cases of SLI have been reported in more and more languages (from German to Italian, from Japanese to Inuktitut, including British Sign Language—see the special issue of *Lingua* in January 2011, edited by Petra Schulz and Naama Friedman). The heritability of the deficit leaves little doubt (Barry, Yasin, & Bishop 2007, Bishop & Norbury 2002, Bishop, Adams, & Norbury 2006, Van der Lely 2005, Van der Lely & Stollwerck 1996). Ken Wexler and collaborators have suggested quite precise and deep explanations of the core of the deficit based on refined syntactic processes (Rice, Wexler, & Cleave 1995, Wexler 1994, 2013).

These explanations are based on the special difficulty encountered by the SLI-affected child with non-actional passives, unaccusative versus unergative verbs, the unique checking constraint, universal phase requirement, deriving object to subject, the formation of chains. These are quite subtle elements of linguistic theory, some posited only recently. These lexico-syntactic and semantic operations are rather late milestones even for normal children, but SLI children mature these significantly later.

Lenneberg's treatment of congenital deafness, especially the case of deaf children of normally hearing parents, and of hearing impairments, are very illuminating. Explicitly or implicitly, the remarkable final success of these children in acquiring language is evidence for the poverty of the stimulus (to which I will return). In his times, grave mistakes were made by educators in the schools for the deaf (including the insistence on lip-reading and the suggestion to hearing parents not to gesture to their deaf children) and Lenneberg, respectfully, but unambiguously, laments these.

Ever since, studies of the structure of sign languages have revealed the perfect equivalence, not only in efficacy of communication, but in syntactic structure, with spoken languages (Klima & Bellugi 1979, Bellugi et al., this issue). The case of the Nicaraguan Sign Language, when special schools for deaf children were belatedly created, revealed the richness of the child's spontaneous creativity in actualizing fundamental language structures and that the earlier was the exposure to a full sign language, the greater was the linguistic quality of the final language stage (Senghas, Kita, & Özyürek 2004).

The next item, poverty of the stimulus, gains from mention of the studies of the late Carol Chomsky on deaf and blind children. (Chomsky 1986; reprinted in Piattelli-Palmarini & Berwick 2013). Adopting the Tadoma method, which consists in the deaf and blind child positing his/her fingers in specific points of the cheeks and throat of the speaker, language development attains all the milestones of normal children, sometimes with only a small delay.

Lenneberg is adamant in stressing the importance of the poverty of the stimulus. He reports that child-directed speech by adults is full of semi-sentences, ungrammatical but interpretable sentences, hesitations, ellipses and restarts. The fundamental principle in language acquisition is, in his words: "What is acquired are patterns and structures, not constituent elements" (p. 281). Lenneberg calls this "a fundamental principle of language acquisition". This appears to contradict the classic PoS arguments, but in my opinion it does not.[7] Lexical items are surely learned, while patterns and structures are supplied by the genetic endowment. My reading of this sentence is that Lenneberg wants to draw a distinction between the peculiarities of a language (the sound pattern, the sound of lexical items) and fundamental patterns and structures. In fact, it is preceded by this sentence: "the infant's initial lack of concern for phonetic accuracy is by no means a trivial or logically necessary phenomenon" (p. 281). In hindsight, Lenneberg should probably have used a different wording, but many other passages in the book make it clear that he considers PoS an indubitable thesis.

Poverty of the stimulus is a topic that is still controversial, for strange reasons, today, in some corners of the academia.

6. Poverty of the Stimulus

The existence of critical periods in language acquisition, the cases of "feral children," of deep deafness and other cases of insufficient exposure to language, as expounded in Lenneberg's book and the rich bibliography it contains, are evidence of the importance of linguistic external input. But the role of this input is not one of shaping language, it's more akin to the role of food in bodily growth. This is an insightful parallel made explicitly by Lenneberg and then endorsed by Chomsky. Raw materials need to be supplied, but the organism breaks them down chemically and internally re-builds the blocks according to its constitution. Language-readiness and the child's maturational path (in Lenneberg's terms) are actualized over time in virtue of internal, innate, species-specific predispositions, not shaped by the linguistic input coming from the outside. Poverty of the stimulus is manifest in some extreme cases, like the deaf and blind children studied by Carol Chomsky, but is no less real in normal children.[8]

Over all the intervening years, various authors have tried to deny or belittle the poverty of the stimulus. Extra-linguistic factors (gestures, indexicality, ostension, facial expressions, generic induction, manifest approval or disapproval) and some marginal linguistic phenomena (intonation, repetitions, explicit corrections) have been suggested as "enriching" the stimulus in crucial ways. In recent years, other suggestions have been made, based on the frequency of bigrams (Reali & Christiansen 2005) and on the subtlety of Bayesian statistical generalizations by the child (Perfors, Tenenbaum & Regier 2011a, 2011b). In essence, resuming a very old expository example by Chomsky (dating back to the Royaumont debate with Jean Piaget (Piattelli-Palmarini 1994, 1980), from the declarative sentence:

---

[7] I am grateful to a *Biolinguistics* reviewer for pointing out this possible discrepancy.

[8] I have heard Chomsky, in his lectures and in conversation, stress this point: extreme cases are very interesting, but should not induce us to think that poverty of the stimulus is only present in these.

(1) The man is happy.

The interrogative is formed:

(2) Is the man happy?

One hypothesis is that the interrogative is formed mechanically, in a structure-independent way, by ante-posing the auxiliary is. But this does not work with the sentence:

(3) The man who is tall is happy.

This simplest hypothesis would give:

(4) * Is the man who tall is happy?

An error that no child makes. The other, correct, explanation is that the child masters a more complex rule, a structure-dependent one: move to the front the auxiliary that follows the whole constituent "the man who is tall." Giving the correct interrogative:

(5) Is the man who is tall happy?

Nothing physical marks that constituent, therefore the child masters the invisible syntactic constituency in the sentence.

Lenneberg had already offered germane considerations:

> This[, an essentially "transformational" process,] is most clearly seen where the constituents of a single [syntactic] category lack any common physical dimension and where the commonality is thus an abstract pattern or structure. In these cases, the physically given, sensory "reality" is *transformed* [emphasis in original] into abstract structure, and similarity between the two physically different patterns is established through the possibility of transforming the abstracted structures back to either of the physically given patterns. (1967: 325)

Reali and Christiansen (2005) cannibalize this very elementary expository example (Chomsky, in the Royaoumont debate, clearly stressed that the issue of struc-ture-dependence goes well beyond such simple examples and requires an integrated theory). Their claim is, in essence, that the child is sensitive to the frequency of bigrams in the language corpus they have received. In essence, the bigram *who-tall* is exceedingly rare, while the bigram *who-is* has high frequency. This is the explanation. No internal invisible structures, no poverty of the stimulus, no structure-dependent rules. Why this suggestion fails immediately, with clear counterexamples and for a number of reasons, is explained in (Berwick, Chomsky, & Piattelli-Palmarini 2013), in part on the basis of previous data by Janet Fodor and collaborators (Kam & Fodor 2013 and references therein).

Next, comes the suggestion by Perfors, Tenenbaum, and Regier (2011a, 2011b): the child is equipped by nature, not with a universal grammar, but with general Bayesian statistical generalizations. Exposed to the standard linguistic input a child is exposed to, a grammar that has internal organization is the best guess in virtue of these statistical generalizations. Other kinds of grammar (the simpler, mechanical ones) cannot emerge with equal success. The received language corpus prompts the child to the more complex induction. No special innate language structures are needed. This is a "rational approach" (sic!) to the poverty of the stimulus. It's that simple!

The falsification of this explanation, slightly subtler than that of Reali and Christiansen, is also detailed in my chapter with Chomsky and Berwick and need not detain us here, for reasons of space. The inadequacy even of Phrase Structure Grammars (assuming that this is what the Bayesian induction gives) to explain the child's tacit knowledge of language was demonstrated long ago by Chomsky and is put to task in our chapter.

Lenneberg was right in stressing the resistance to innate linguistic predispositions and the poverty of the stimulus. I had a confirmation of this a few years ago. In casual conversation, a colleague, a distinguished philosopher (the name will remain unspecified), who was teaching introduction to cognitive science to undergraduates, told me with emphasis "poverty of the stimulus has been falsified, no one believes it any more." I reacted and asked why he was saying that. He cited the papers mentioned above by Reali and Christiansen, and Perfors, Tenenbaum and Regiers. I told him that they were totally off the mark and promised to send him the chapter by Chomsky, Berwick and myself. He asked me to give a lecture in his class, which I gladly did. I do not know whether I managed to persuade his students, but he was not convinced. Since I was to teach that course later, he sent me the syllabus of his course for the following year (please note, the following year). For his one lecture on the poverty of the stimulus, he still gave the students two readings only (Reali and Christiansen, and Perfors, Tenenbaum and Regiers). Not a mention of our chapter.

So be it. Innatism and poverty of the stimulus are still hard to be accepted, unfortunately.

7. Language Evolution

In privileging internal constraints, internal computations and the internal development of access to rules, Lenneberg was, once more, right. Also, his perplexities regarding a neo-Darwinian adaptationist account of language evolution were perfectly justified.

Lenneberg criticizes the legitimacy of data (then and still now) brought to support a progressive, step by step, continuous evolution of human language from animal communication. Allegedly, a story of quantitative, not qualitative, progression. Purported evidence is, in fact, chosen from a scatter of orders and species, in total disregard for phylogenetic continuity with humans. He says:

> Frequently, only one species within a given genus or family even possesses the trait, indicating clearly that we are dealing with species-specificities, probably all of comparatively recent date. The reason the examples are so disparate is that parallels are rare. This suggests accidental convergence (if, indeed, it is even that) rather than milestones within one continuous phylogeny. (1967: 232)

He rightly insists that evolutionary discontinuity is not equivalent to special creation, anticipating a line of inquiry based on "punctuated equilibria" cogently pursued in later years by Stephen Jay Gould and Niles Eldredge, raising fierce criticism (Sterelny 2002). In a footnote we read:

> The emergence of celestial navigation in birds or the diving abilities of whales are no less mysterious than the emergence of a language-enabling cognition. (1967: 374)

These traits are rightly indicated by Lenneberg as having emerged discontinuously, like language. For germane consideration in the domain of insect navigation, see (Gallistel 1998, 1999).

In the following years, however, most of publications on language evolution focused on the advantage allegedly given by communication, not internal structures. In spite of radical critiques of standard neo-Darwinian selectionism and several authoritative statements that natural selection is not considered any more the main factor in evolution (see my book with Jerry Fodor for relevant quotes and data and arguments: Fodor & Piattelli-Palmarini 2011) the suggestions of selective factors in the evolution of language have proliferated. The critique of these hypotheses goes well beyond language and humans, extending to all species and most biological traits. The evo-devo revolution has produced further perplexities, marginalizing the role of natural selection in speciation.

Few in number, but equally authoritative, have been the exceptions to the prevailing trend in the approaches to language evolution (Berwick & Chomsky 2016, Berwick et al. 2013, Bolhuis et al. 2014, Bolhuis et al. 2015; Bolhuis & Everaert 2013, Everaert et al 2015).

Stressing the relatively recent (between 150,000 and 75,000 years, a blink of an eye in evolutionary time) and sudden appearance of the language faculty and showing that communication is ancillary to language,[9] Berwick and Chomsky have cogently tried to redress the issue of language evolution. Rini Huybregts summarizes his detailed work, and work on the genetic prehistory of southern Africa by Pickrell et al. (2012) supporting the hypothesis of an ancient link between southern African Khoisan (northwestern and southeastern Kalahari groups, who separated only within the last 30,000 years) and eastern African Hadza and Sandawe. He says:

> Language must have existed before human populations became separated [...] but language did not emerge until long after these population divergences occurred. Distinguishing capacity for language from externalized language resolves the apparent paradox. Speech emerged only after the capacity for language became fixated. This accords well with a fundamental property of human language. Rules mapping to meaning rely on structural properties only, while rules mapping to sound are (also) sensitive to linear order, reflecting properties of sensorimotor modalities. The asymmetry suggests (i) *language as a system of thought takes primacy over language as communication* [emphasis added], and (ii) evolution of the language capacity preceded emergence of speech. Click phonemes with their unique genealogical, genetic and geographical distribution may be relevant here. Separation followed possession of inter-

---

[9] Noam Chomsky pointed me to this important article: Huybregts (in press).

> nal language but preceded externalized language. Clicks were recruited for externalization in San populations only after deepest separation.
>
> (Huybregts, in press)

Unfortunately, I doubt that these approaches have persuaded the die-hard Darwinians.[10]

A recent and important development, with expected future impact on brain evolution, is the detailed study of the very complex processes taking place inside the neurons. Pioneered by the University of Arizona anesthesiologist Stuart Hameroff and the British mathematician and physicist Roger Penrose with the study of microtubules (Craddock, Tuszynski, & Hameroff 2012; Hameroff, 1998; Woolf & Hameroff 2001), it has blossomed in recent years thanks to extremely small electric probes, capable of recording signals from inside neurons, without affecting the functioning of the neuron as a whole. In essence, the formidable equipe of biophysicists and molecular neuroscientists in Tsukuba (Japan), under the guidance of Anirban Bandyopadhyay, has discovered very complex patterns of impulses at different resonating frequencies, from a few hertz to terahertz, mostly grouped in triplets and presenting a fractal distribution (Ghosh et al. 2014, Ghosh, Dutta, Sahu, Fujita, & Bandyopadhyay 2013). Solid connections with memory storage in the long term have been established[11] and some mentions are made, maybe a bit prematurely, to basic syntactic processes.[12]

The importance of physical laws in the explanation of fundamental linguistic structures and computations, correctly stressed by Lenneberg by citing and commenting work by D'Arcy Thompson, has found significant developments, well summarized in the first international conference on The Physics of Language (Sophia University, Tokyo, March 4–5 2016), sequels to which are now planned. This leads to the final segments of this paper, the ones dearest to me these days.

7.1. *Optimization in Biology*

Lenneberg insists on the notion of canalization and borrows from Waddington's work the picture of epigenetic landscapes (see figure 1 reproduced below). Germane to, nay almost indistinguishable from this, is Ivan Ivanovich Schmalhausen's idea of "coordination" (for a biography of this illustrious Russian evolutionist, embryologist and geneticist—harassed by the Soviets—and a complete bibliography

---

[10] A revealing anecdote. At the IX EVOLANG, the biannual big conference on language evolution, held in Kyoto in 2012, Noam Chomsky had initially accepted to be the keynote speaker. Then he declined, (he told me he had no interest in sitting for days listening to people talking about a topic no one understands). The organizers asked him for a substitute. He suggested Robert Berwick, who also declined, and he then suggested me. I accepted and had the undeserved role of starting the opening session of the conference. I did my best to present cogent data and arguments against a neo-Darwinian explanation of language evolution. There were some contrarian questions, which I did my best to answer. Then, for three days, lots of papers were presented totally ignoring what I had suggested. Some older and "classic" proponents of a selectivist account were treated like royalty and received special prizes.

[11] This vindicates Gallistel's intuition that we have to explore "room at the bottom" (sic), lower than neurons and synapses, to find molecular traces for memory (Gallistel and King 2011).

[12] The richness and subtlety of processes occurring inside single neurons may soon render irrelevant the overly celebrated use of neural networks in understanding brain functions. In these models, each neuron is assimilated to a single, unstructured node.

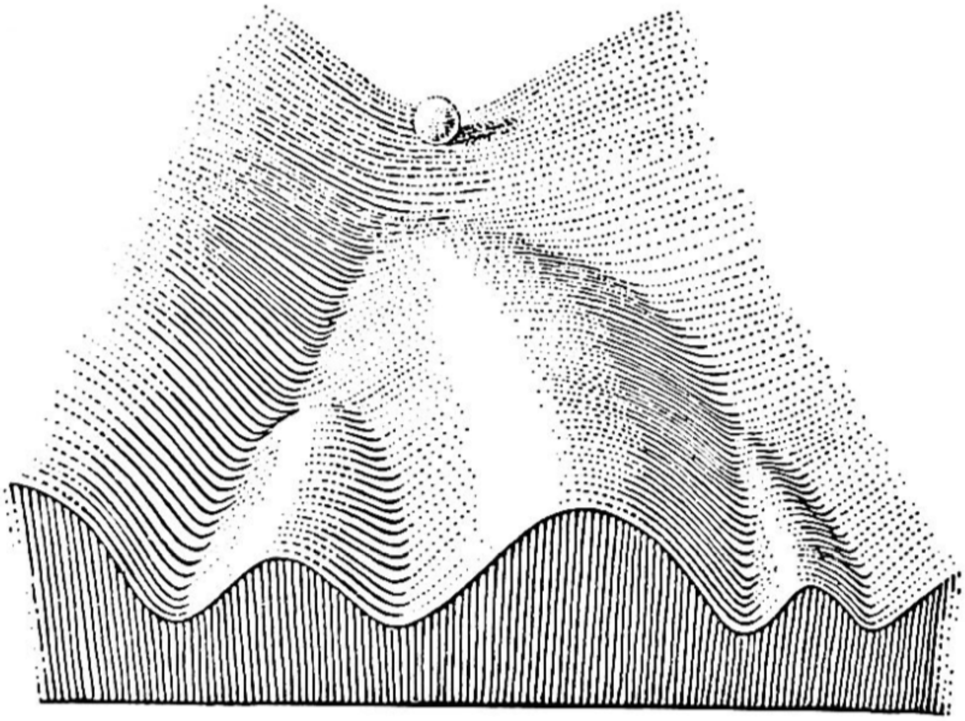


Figure 1: Reproduction of figure 6.10 from Lenneberg (1967: 262). The original caption in the book says: 'An 'epigenetic landscape.' A representation of a developmental system as a surface (sloping towards the observer) on which there are valleys along which the processes of differentiation tend to run. Evolutionary changes would alter the landscape in such a way that the ball now runs down a different valley from its former course."

of his work, see Levit, Hossfeld, & Olsson 2006).[13] Coordination and canalization stress the interdependence of organs and functions in evolution and development. In his essay of 1964, in Russian, entitled "Problems with Darwinism" Schmalhausen says:

> Since the organism is an interconnected whole, it must keep its property of wholeness also in the course of evolution. This would mean the coordinated [evolutionary] transformation of its organs and parts.
>
> (translated and cited in Levit, Hossfeld & Olsson 2006)

In harmony with what, later on, became evo-devo (see above), Schmalhausen drew a picture of evolution as an evolution of whole, highly integrated, organisms. As Lenneberg reminds us, all these scholars had, like himself, problems with Darwinism, in particular, with the atomistic notion of natural selection acting on each trait separately[14] and with the idea that utility shapes form. In several passages of his book, this idea is rightly criticized.

---

[13] I am indebted to Richard Lewontin for pointing me to the work of Schmalhausen and for stressing its importance.

[14] This atomistic conception of natural selection, gene by gene, trait by trait, was labeled, critically and somewhat humorously, by Ernst Mayr "beanbag genetics", an approach that has been energetically defended by the staunch Darwinian J. B. S. Haldane (Haldane 1964). He concludes his 1964 article saying: "I hope to devote my remaining years largely to beanbag

From the very opening, and then in many subsequent passages, in fact, Lenneberg dwells on the monumental pioneering work of D'Arcy Wentworth Thompson and collaborators (Thompson & Bonner 1917/1992), largely ignored, to this day, by militant researchers in biology and genetics.[15] We are reminded that D'Arcy Thompson had discovered basic, simple, topological transformations covering allometric growth in the morphology of close species. He had also shown the pervasiveness of fundamental anatomical structures that instantiate the physical laws of material stress, obeying the principles of optimal levers and minimal muscular effort. The inter-dependence of organs and functions is stressed by Lenneberg, all this being often a far cry from piecemeal natural selection.

On page 265, Lenneberg says:

> The evolutionary process underlying language is analogous to the geometric transformations of form, described by D'Arcy Thompson, or perhaps comparable to the changes in allometric tendencies in different species. (1967: 265)

The crucial importance of fundamental physical and chemical laws was further shown by Alan Mathison Turing, by means of elegant mathematical solutions to the formation of many biological patterns, exclusively based on spontaneous molecular diffusion and the optimization of overlapping morphogenetic gradients (Turing 1952). Significant further confirmations of the correctness and the explanatory power of Turing's approach have been found recently in more biological forms (Economou et al. 2012, Reinitz 2012, Tompkins et al. 2014). The materialization, in biological structures, functions and behaviors, of physical principles of optimization, maximum efficiency and minimal stress are now abundant: found in the structure of the genetic code (Itzkovitz & Alon 2007), the evolution of insect wings (Kingsolver & Koehl 1985), the optimal wing angle for flight and takeoff in birds (Dial, Jackson, & Segre 2008), respiratory patterns in birdsongs (Trevisan, Mindlin, & Goller 2006), brain wiring and brain location (Cherniak 2010, Cherniak, Mokhtarzada, Rodriguez-Esteban, & Changizi 2004), and optimal energy expenditure in migrating birds (Liechti 1995).

These results, and more that I will not report here for reasons of space, confirm that optimal solutions and materializations of physical principles are ubiquitous in biology. This vindicates the work of Schmalhausen, Waddington, D'Arcy Thompson and Turing, corroborates Lenneberg's intuitions and, presently, testifies to the legitimacy of the core thesis of the Minimalist Program. It is not true, as some critics have claimed, that the optimization criteria invoked in Minimalism contradict all we know about biology and evolution, where, allegedly, optimal structures are never found. On the contrary, they are found all over.

---

genetics".

[15] In hindsight, I must report that, in the years when I was doing research in molecular genetics at the Institut Pasteur, under the guidance of the Nobel laureate Jacques Monod (one of the most intelligent and cultivated intellectuals I have ever known), no mention was ever made of this line of inquiry. In fact, Monod claimed that physicists could not understand biology, because "every biological structure is also a fossil" (sic!). An unflinching neo-Darwinian, Monod was persuaded that the vagaries of natural selection were all one needed to understand evolution. He once proudly announced to his whole laboratory that he had discontinued the subscription to the *Journal of Theoretical Biology*. He explained this by asserting that there is no such thing as theoretical biology.

7.2. *Towards a Physics of Language*

Spontaneous instantiations of the Fibonacci patterns (the series, the sequence, the spiral) are to be found everywhere in nature, from galaxies to flowers, from the horns of the ram to patterns formed in inorganic systems (Douady & Couder 1992). These patterns materialize the optimal, self-organizing compromise between opposing factors. Their instantiation in the domain of language and why it matters have been evidenced by David Medeiros (Medeiros, 2008), by Medeiros and me (Medeiros & Piattelli-Palmarini, in press), by William Idsardi and Juan Uriagereka (Idsardi & Uriagereka 2009) and in my work with the Italian physicist Giuseppe Vitiello (Piattelli-Palmarini & Vitiello 2015, 2017, in press). Important, still unpublished, work is going on in Reading UK, under the supervision of Doug Saddy. It has been shown that, in identifying sequences of tones or syllables, in predicting their continuation and in remembering them, humans have a special facility when the sequences are Fibonacci sequences, even with respect to superficially similar sequences. Since the Fibonacci sequences cannot be easily, intuitively guessed by humans by probabilistic expectations, one term after the other, the special facility of identification and memorization attested by Saddy and colleagues rules out a Bayesian explanation, much to the regret of those who are pertinaciously attempting to explain language structures with Bayesian models (Tenenbaum & Griffiths 2001, Xu & Tenenbaum 2007).

Medeiros has shown that the Fibonacci numbers govern the structure of syntactic trees and that, in any sentence, the buildup of a higher node in the tree is only forced when the number of syntactically licensed words in the sentence reaches a Fibonacci number, not otherwise. Other mathematically optimal characteristics are satisfied by the growth of binary syntactic trees, at variance with other kinds of abstractly conceivable trees. Vitiello and I have established a relation between the algebra of the most elementary binary matrices in Quantum Field Theory (QFT) and the generation of X-bar trees and their Fibonacci progression for the number of branches. Grounded on basic properties of QFT,[16] we think we can also show the optimality of the constituency of Logical Form and of an internalist semantics (see above).

The qualifications "towards" and "steps to", ante-posed to the very notion of a physics of language, are, at present, crucial, because this is only the beginning of a thorough exploration of the deep physics of language. It stands to reason, we think, that, since language is part of nature, we can expect to see instantiated in it some of the basic laws of nature, including physical laws. Tentative suggestions along these lines are found in many places in Lenneberg's book. The present emphasis of Minimalism on criteria of minimal search, minimal computation, strict locality and recursive grouping (phases) is very germane to us.

---

[16] Quantum Field Theory is the best choice of the branch of physics to explore in connection to language, because it covers interactions of many bodies at room temperature. Moreover, it posits fields, not particles or forces, as the primary entity, in an analogy that we (rightly or wrongly) think is significant with fundamental posits of contemporary Minimalism (strict locality, probe-goal relations, agreement and phases).

8. Conclusion

It is impossible to summarize fifty years of progress in linguistics, from transformations (then) to Minimalism (now). The core of the theory has become progressively deeper, more abstract and leaner. From transformations to syntactic movement, to move-alpha, to principles-and-parameters[17] to feature-checking, to strict locality and minimal computation. The contemporary tools of neuroscience (e.g, electroencephalography, functional magnetic resonance imaging, magnetoencephalography, and near-infrared spectroscopy) have given some important contributions, but are not yet sensitive enough to allow for a test of the refined alternative hypotheses now offered in linguistics.[18] The opening of a domain that looks inside the neurons, as summarized above, might offer a healthy revolution in neuroscience, possibly not dissimilar from the revolution that quantum physics produced in chemistry and the science of materials. Cognitive science and linguistics would then follow, in ways we cannot anticipate. Looking ahead, maybe as far as the next fifty years, we can hope that the best of the present research in biolinguistics, neurolinguistics and generative grammar will be looked upon, in hindsight, in a similar way as we are now looking back to Lenneberg's work: tentative, incomplete, but suggestive, foreshadowing discoveries and theories only dimly intuited, though in the right general direction.

9. References

Allis, C. D., T. Jenuwein, D. Reinberg, & M.-L. Caparros. 2006. *Epigenetics*. Cold Spring Harbor: Cold Spring Harbor Laboratory Press.

Allis, C. D., T. Jenuwein, D. Reinberg, & M.-L. Caparros (Eds.). 2007. *Epigenetics*. Cold Spring Harbor, New York: Cold Spring Harbor Laboratory Press.

Ashley A. Scott-Van Zeeland, B. S. Abrahams, A. I. Alvarez-Retuerto, ..., S. Y. Bookheimer. 2010. Altered functional connectivity in frontal lobe circuits is associated with variation in the autism risk gene CNTNAP2. *Science Translational Medicine* 2, 56 ra80. doi:10.1126/scitranslmed.3001344.

---

[17] A special issue of *Linguistic Analysis* on parameters, edited by Simin Karimi and me, is in press for 2018, with contributions by Baker, Boeckx, Borer, Chomsky, Cinque, Epstein, Lightfoot, Longobardi, among others.

[18] In my opinion, the three most interesting contributions of brain imaging to linguistics are: (1) The work of Musso et al. (2003), showing different brain activations when a subject, respectively, monitors samples of real syntactic structures, versus sentences instantiating impossible non-structure-dependent rules. (2) work by Stanislas Dehaene and collaborators (Pallier et al. 2011) showing identical brain activations for real sentences and Jabberwocky sentences of the same length and syntactic structure. In contrast, brain activations for lists of words of the same length are totally different. (3) Recent data (April 2017) from a broad set of language-related areas showing that activity increased with each successive word in a sentence, but decreased suddenly whenever words could be merged into a phrase (Nelson et al. 2017). These authors conclude:

> Our results provide initial intracranial evidence for the neurophysiological reality of the merge operation postulated by linguists and suggest that the brain compresses syntactically well-formed sequences of words into a hierarchy of nested phrases. (Nelson et al. 2017)

Barry, J. G., I. Yasin, & D. V. M. Bishop. 2007. Heritable risk factors associated with language impairments. *Genes, Brain & Behavior*, 6(1), 66–76.

Berwick, R. C. & Noam Chomsky. 2016. *Why Only Us: Language and Evolution*. Cambridge, MA: MIT Press.

Berwick, R. C., Noam Chomsky, & Massimo Piattelli-Palmarini. 2013. Poverty of the stimulus stands: Why recent challenges fail. In M. Piattelli-Palmarini & R. C. Berwick (Eds.), *Rich Languages from Poor Inputs*. Oxford, UK: Oxford University Press. 19–42.

Berwick, Robert C., Angela D. Friederici, Noam Chomsky, and Johan J. Bolhuis. 2013. Evolution, brain, and the nature of language. *Trends in Cognitive Sciences* 17 (2):89–98. doi:10.1016/j.tics.2012.12.002.

Bishop, D. V. M., C. V. Adams, & C. F. Norbury. 2006. Distinct genetic influences on grammar and phonological short-term memory deficits: evidence from 6-year-old twins. *Genes, Brain and Behavior*, 5(2), 158–169.

Bishop, D. V. M. & C. F. Norbury. 2002. Exploring the borderlands of autistic disorder and specific language impairment: a study using standardised diagnostic instruments. *Journal of Child Psychology and Psychiatry*, 43(7), 917–929.

Bolhuis, J., I. Tattersall, N. Chomsky, & R. Berwick. 2014. How could language have evolved? *PLoS Biology* 12(8), e1001934. doi:10.1371/journal.pbio.1001934.

Bolhuis, J., I. Tattersall, N. Chomsky, & R. Berwick. 2015. Language: UG or not to be, that is the question. *PLoS Biology* 13(2), e1002063. doi:10.1371/journal.pbio.1002063.

Bolhuis, J. J. & M. Everaert (Eds.). 2013. *Birdsong, Speech, and Language: Exploring the Evolution of Mind and Brain*. Cambridge, MA: The MIT Press.

Boyle, E. A., Y. I. Li, & J. K. Pritchard. 2017. An expanded view of complex traits: From polygenic to omnigenic. *Cell* 169(7), 1177–1186. doi:10.1016/j.cell.2017.05.038.

Carroll, S. B. 2005. *Endless Forms Most Beautiful: The New Science of Evo Devo and the Making of the Animal Kingdom*. New York, NY: Norton.

Cherniak, C. 2010. Brain wiring optimization and non-genomic nativism. In M. Piattelli-Palmarini, P. Salaburu, & J. Uriagereka (Eds.), *Of Minds and Language: a dialogue with Noam Chomsky in the Basque Country*. Oxford, UK: Oxford University Press. 108–119.

Cherniak, C., Z. Mokhtarzada, R. Rodriguez-Esteban, & K. Changizi. 2004. Global optimization of cerebral cortex layout. *Proceedings of the National Academy of Sciences of the United States of America* 101(4), 1081–1086.

Chomsky, Carol. 1986. Analytic study of the Tadoma method: Language abilities of three deaf-blind subjects. *Journal of Speech and Hearing Research* 29(3), 332–374.

Christoforou, A., T. Espeseth, G. Davies, C. P. D. Fernandes, S. Giddaluru, M. Mattheisen, . . . , S. Le Hellard. 2014. GWAS-based pathway analysis differentiates between fluid and crystallized intelligence. *Genes, Brain and Behavior* 13(7), 663–674. doi:10.1016/j.cell.2017. 05.038.

Craddock, T. J. A., J. A. Tuszynski & S. Hameroff. 2012. Cytoskeletal signaling: Is memory encoded in microtubule lattices by CaMKII phosphorylation? *PLoS Computational Biology* 8(3), e1002421. doi:10.1371/journal.pcbi.1002421.

Curtiss, S. 2013. Revisiting modularity: Using language as window to the mind. In M. Piattelli-Palmarini & R. C. Berwick (Eds.), *Rich Languages from Poor Inputs*.

Oxford, UK: Oxford University Press. 68–90.

Davidson, E. H. 2006. *The Regulatory Genome: Gene Regulatory Networks in Development and Evolution*. London, UK and San Diego, CA: Elsevier Academic Press.

Davidson, E. H. 2010. Emerging properties of animal gene regulatory networks. *Nature* 468(7326), 911–920.

Davidson, E. H. & D. H. Erwin. 2006. Gene regulatory networks and the evolution of animal body plans. *Science* 311(5762), 796–800. doi:10.1126/science.1113832.

Davies, G., A. Tenesa, A. Payton, J. Yang, S. E. Harris, D. Liewald, & I. J. Deary. 2011. Genome-wide association studies establish that human intelligence is highly heritable and polygenic. *Molecular Psychiatry*, 16 (10), 996–1005.

Day, E. J. 1932. The development of language in twins: I. A comparison of twins and single children. *Child Development* 3(3), 179–199.

Dial, K. P., B. E. Jackson, & P. Segre. 2008. A fundamental avian wing-stroke provides a new perspective on the evolution of flight. *Nature* 451(7181), 985–989. doi:10.1038/nature06517.

Douady, S. & Y. Couder. 1992. Phyllotaxis as a physical self-organized growth process. *Physical Review Letters* 68(13), 2098–2101.

Economou, A. D., A. Ohazama, T. Porntaveetus, . . . , & J. B. A. Green. 2012. Periodic stripe formation by a Turing mechanism operating at growth zones in the mammalian palate. *Nature Genetics* 44(3), 348–351. doi:10.1038/ng.1090.

Everaert, M. B. H., M. A. C. Huybregts, N. Chomsky, R. C. Berwick, & J. J. Bolhuis. 2015. Structures, not strings: Linguistics as part of the cognitive sciences. *Trends in Cognitive Sciences* 19(12), 729–743. doi:10.1016/j.tics.2015.09.008.

Fodor, Jerry & Massimo Piattelli-Palmarini. 2011. *What Darwin Got Wrong*. New York, NY: Picador Macmillan.

Friederici, Aangela D. 2012. The cortical language circuit: from auditory perception to sentence comprehension. *Trends in Cognitive Sciences* 16(5), 262–268. doi:10.1016/j.tics.2012.04.001.

Friederici, A. D. & W. Singer. 2015. Grounding language processing on basic neurophysiological principles. *Trends in Cognitive Sciences* 19(6), 329–338. doi:10.1016/j.tics.2015.03.012.

Friedmann, N. 2006. Speech production in Broca's agrammatic aphasia: Syntactic Tree Pruning. In Y. Grodzinsky & K. Amunts (Eds.), *Broca's Region*. Oxford UK: Oxford University Press.

Friedmann, N., A. Belletti, & L. Rizzi. 2009. Relativized relatives: Types of intervention in the acquisition of A-bar dependencies. *Lingua*, 119, 67–88.

Friedmann, N. & Y. Grodzinsky. 1997. Tense and agreement in agrammatic production: pruning the syntactc treee. *Brain and Language*, 56, 397–425.

Gallistel, C.R. 1998. Symbolic processes in the brain: the case of insect navigation. In D.Scarborough and S.Sternberg (Eds.), *An Invitation to Cognitive Science: Methods, Models,and Conceptual Issues*. Cambridge, MA: MIT Press. 1–51.

Gallistel, C.R. 1999. The replacement of general-purpose learning models with adaptively specialized learning modules. In M.S.Gazzaniga (Ed.), *The New Cognitive Neurosciences*, 2nd Ed. Cambridge, MA: MITPress. 1179–1191.

Gallistel, C. R. (2002). Language and spatial frames of reference in mind and brain. *Trends in Cognitive Sciences* 6(8), 321–323.

Gallistel, C. R. & A. P. King. 2011. *Memory and the Computational Brain: Why Cognitive Science Will Transform Neuroscience*. Chichester, UK: Wiley-Blackwell.

Ghosh, S., K. Aswani, S. Singh, S. Sahu, D. Fujita, & A. Bandyopadhyay. 2014. Design and construction of a brain-like computer: A new class of frequency-fractal computing using wireless communication in a supramolecular organic, inorganic system. *Information* 5, 28–100. doi:10.3390/info5010028.

Ghosh, S., M. Dutta, S. Sahu, D. Fujita, & A. Bandyopadhyay. 2013. Nano molecular-platform: A protocol to write energy transmission program inside a molecule for bio-inspired supramolecular engineering. *Advanced Functional Materials*. doi:10.1002/adfm.201302111.

Haldane, J. B. S. 1964. A defense of beanbag genetics. *Perspectives in Biology and Medicine* 7(3), 343–360. doi:10.1353/pbm.1964.0042.

Halfmann, R. & S. Lindquist. 2010. Epigenetics in the extreme: Prions and the inheritance of environmentally acquired traits. *Science* 330(6004), 629–632. doi:10.1126/science.1191081.

Hameroff, S. 1998. Quantum computation in brain microtubules? The Penrose-Hameroff 'Orch OR' model of consciousness. *Philosophical Transactions of the Royal Society of London. Series A: Mathematical, Physical and Engineering Sciences* 356(1743), 1869–1896.

Herculano-Houzel, S. 2016. *The Human Advantage: A New Understanding of how Our Brain Became Remarkable*. Cambridge, MA: The MIT Press.

Hsiao, E. Y., S. W. McBride, S. Hsien, G. Sharon, E. R. Hyde, T. McCue, . . . & S. K. Mazmanian. 2013. Microbiota modulate behavioral and physiological abnormalities associated with neurodevelopmental disorders. *Cell* 155(7), 1451–1463. doi:10.1016/j.cell.2013.11.024.

Huybregts, R. In press. Phonemic clicks and the mapping asymmetry: How language emerged and speech developed. Neuroscience and Biobehavioral Reviews. doi:10.1016/j.neubiorev.2017.01.041.

Idsardi, W. J. & J. Uriagereka. 2009. Metrical combinatorics and the real half of the Fibonacci sequence. *Biolinguistics* 3(4), 404–406.

Itzkovitz, S. & U. Alon. 2007. The genetic code is nearly optimal for allowing arbitrary additional information within protein-coding sequences. *Genome Research*. doi:10.1101/gr.5987307.

Kam, X.-N. C. & J. D. Fodor. 2013. Children's acquisition of syntax: Simple models are too simple. In M. Piattelli-Palmarini & R. C. Berwick (Eds.), *Rich Languages from Poor Inputs*. Oxford, UK: Oxford University Press. 43–60.

Kingsolver, J. G. & M. A. R. Koehl. 1985. Aerodynamics, thermoregulation, and the evolution of insect wings: differential scaling and evolutionary change. *Evolution* 39, 488–504.

Klima, E. S. & U. Bellugi. 1979. *The Signs of Language*. Cambridge, MA: Harvard University Press.

Laubichler, M. D. & J. Maienschein (Eds.). 2007. *From Embryology to Evo-Devo: A History of Developmental Evolution*. Cambridge, MA: MIT Press.

Levit, G. S., U. Hossfeld, & L. Olsson. 2006. From the "Modern Synthesis" to cybernetics: Ivan Ivanovich Schmalhausen (1884-1963) and his research program for a synthesis of evolutionary and developmental biology. *Journal of Experimental Zoology (Molecular and Developmental Evolution)* 306B, 89–106.

Li, P., L. Abarbanell, L. Gleitman, & A. Papafragou 2011. Spatial reasoning in Tenejapan Mayans. *Cognition* 120(1), 33–53. doi:10.1016/j.cognition.2011.02.012.

Li, P. W. & L. Gleitman. 2002. Turning the tables: Language and spatial reasoning. *Cognition* 83(3), 265–294.

Liechti, F. 1995. Modelling optimal heading and airspeed of migrating birds in relation to energy expenditure and wind influence. *Journal of Avian Biology* 26 (4), 330–336.

Martinez, F. D. 2014. The human microbiome. Early life determinant of health outcomes. *Annals of the American Thoracic Society* 11. doi:10.1513/AnnalsATS.201306-186MG.

McCarthy, D. 1930. *The Language Development of the Pre-School Child*. Minneapolis, MN: University of Minnesota Press.

McGilvray, J. 1998. Meanings are syntactically individuated and found in the head. *Mind and Language* 13, 225–280.

Medeiros, D. & M. Piattelli-Palmarini. In press. The golden phrase: Steps to the physics of language. In A. Gallego & R. Martin (Eds.), *Language, Syntax, and the Natural Sciences*. Cambridge, UK: Cambridge University Press.

Medeiros, D. P. (2008). Optimal growth in phrase structure. *Biolinguistics* 2(3), 152–195.

Musso, M., A. Moro, V. Glauche, M. Rijntjes, J. Reichenbach, C. Buechel, & C. Weiller. 2003. Broca's area and the language instinct. *Nature Neuroscience* 6(7), 774–781.

Nelson, M. J., I. El Karoui, K. Giber, X. Yang, L. Cohen, H. Koopman, . . . , & S. Dehaene. 2017. Neurophysiological dynamics of phrase-structure building during sentence processing. *Proceedings of the National Academy of Sciences of the United States of America* 114(18), E3669–E3678. doi:10.1073/pnas.1701590114.

Nüsslein-Volhard, C. (1995). The identification of genes controlling development in flies and fishes (Nobel Lecture). In N. Ringertz (Ed.), *Nobel Lectures, Physiology or Medicine 1991–1995*. Singapore: World Scientific. 285–306.

Ober, C., & D. L. Nicolae. 2011. Meta-analysis of genome-wide association studies of asthma in ethnically diverse North American populations. *Nature Genetics* 43(9), 887–892. doi:10.1038/ng.888.

Pallier, C., A.-D. Devauchelle, & S. Dehaene. 2011. Cortical representation of the constituent structure of sentences. *Proceedings of the National Academy of Sciences of the United States of America* 108(6), 2522–2527.

Papafragou, A., K. Cassidy, & L. Gleitman. 2007. When we think about thinking: The acquisition of belief verbs. *Cognition* 105(1), 125–165.

Perani, D., M. C. Saccuman, P. Scifo, A. Anwander, . . . , A. D. Friederici. 2011. Neural language networks at birth. *Proceedings of the National Academy of Sciences of the United States of America* 108(45), 16056–16061.

Perfors, A., J. B. Tenenbaum, & T. Regier. 2011a. The learnability of abstract syntactic principles. *Cognition* 118(3), 306–338. doi:10.1016/j.cognition.2010.11.001.

Perfors, A., J. B. Tenenbaum & T. Regier. 2011b. Poverty of the simulus? A rational approach. *Cognition* 118, 306–338.

Piattelli-Palmarini, Massimo (Ed.). 1980. *Language and Learning: The Debate Between Jean Piaget and Noam Chomsky*. Cambridge, MA: Harvard University Press.

Piattelli-Palmarini, Massimo. 1994. Ever since language and learning: afterthoughts on the Piaget-Chomsky debate. *Cognition* 50, 315–346.

Piattelli-Palmarini, Massimo. In press. Normal language in abnormal brains. *Neurosciences and Biobehavioral Reviews*. doi:10.1016/j.cognition.2010.11.001.

Piattelli-Palmarini, Massimo & Robert C. Berwick (Eds.). 2013. *Rich Languages from Poor Inputs*. Oxford, UK: Oxford University Press.

Piattelli-Palmarini, Massimo & Juan Uriagereka. 2011. A geneticist's dream, a linguist's nightmare: The case of FOXP2. In A. M. Di Sciullo & C. Boeckx (Eds.), *The Biolinguistic Enterprise: New Perspectives on the Evolution and Nature of the Human Language Faculty*. Oxford, UK: Oxford University Press. 100–125.

Piattelli-Palmarini, Massimo & Giuseppe Vitiello. 2015. Linguistics and some aspects of its underlying dynamics. *Biolinguistics* 9, 96–115.

Piattelli-Palmarini, Massimo & Giuseppe Vitiello. 2017. Third factors in language design: Some suggestions from Quantum Field Theory. In J. McGilvray (Ed.), *The Cambridge Companion to Chomsky*, 2nd Edition. Cambridge, UK: Cambridge University Press. 134–152.

Piattelli-Palmarini, Massimo & Giuseppe Vitiello. In press. Quantum Field theory and the linguistic Minimalist Program: a remarkable isomorphism. *Journal of Physics: Conference Series* 880, 012016. doi:10.1088/1742-6596/880/1/012016.

Pickrell, J. K., N. Patterson, C. Barbieri, . . . , B. Pakendorf. 2012. The genetic prehistory of southern Africa. *Nature Communications* 3(1143). doi:10.1038/ncomms 2140.

Pietroski, P. M. 2003. *The Character of Natural Language Semantics Epistemology of Language*. Oxford, UK: Oxford University Press.

Pietroski, P. M. 2005. Meaning before truth. In G. Prayer & G. Peters (Eds.), *Contextualism in Philosophy*. Oxford, UK: Oxford University Press. 253–300.

Raff, R. A. 2000. Evo-devo: the evolution of a new discipline. *Nature Reviews Genetics*, 1(1), 74–79.

Reali, F., & M. H. Christiansen. 2005. Uncovering the richness of the stimulus: structure dependence and indirect statistical evidence. *Cognitive Science* 29, 1007–1028.

Reinitz, J. 2012. Turing centenary: Pattern formation. *Nature* 482(7386), 464–464.

Rice, M., K. Wexler, & P. Cleave. 1995. Specific language impairment as a period of Extended Optional Infinitive. *Journal of Speech and Hearing Research* 38, 850–863.

Senghas, A., S. Kita, & A. Özyürek. 2004. Children creating core properties of language: Evidence from an emerging sign language in Nicaragua. *Science* 305(5691), 1779–1782.

Sherman, M. 2007. Hypothesis: Universal Genome in the origin of Metazoa. Thoughts about evolution. *Cell Cycle* 6, 1873–1877.

Sprecher, S. G. & H. Reichert, H. 2003. The urbilateral brain: developmental insights into the evolutionary origin of the brain in insects and vertebrates. *Arthropod Structure and Development* 32(1), 141–156.

Sterelny, K. 2002. *Dawkins vs. Gould. Survival of the Fittest*. New York, NY: Totem Books.

Striedter, G. F. 2006. Précis of "Principles of Brain Evolution". *Behavioral and Brain Sciences* 29, 1–36.

Tenenbaum, J. B. & T. L. Griffiths. 2001. Generalization, similarity, and Bayesian inference. *Behavioral and Brain Sciences* 24, 629–640.

Thompson, D. A. W. & J. T. Bonner. 1917/1992. *On Growth and Form*. Cambridge, UK: Cambridge University Press.

Tompkins, N., N. Li, C. Girabawe, . . . , & S. Fraden. 2014. Testing Turing's theory of morphogenesis in chemical cells. *Proceedings of the National Academy of Sciences of the United States of America* 111(12), 4397–4402.

Trevisan, M. A., G. B. Mindlin, & F. Goller. 2006. Nonlinear model predicts diverse respiratory patterns of birdsongs. *Physical Review Letters* 96(5). doi:10.1103/PhysRevLett.96.058103.

Turing, A. M. 1952. The chemical basis of morphogenesis. *Philosophical Transactions of the Royal Society B: Biological Sciences*, 237(641), 37–72. doi:10.1098/rstb.1952.0012.

Van der Lely, H. K. J. 2005. Domain-specific cognitive systems: Insight from grammatical SLI. *Trends in Cognitive Science* 9(2), 53–59.

Van der Lely, H. K. J. & L. Stollwerck. 1996. A grammatical specific language impairment in children: An autosomal dominant inheritance? *Brain and Language* 52, 484–504.

Vercelli, D. 2004. Genetics, epigenetics and the environment: Switching, buffering, releasing. *Journal of Allergy and Clinical Immunology* 113, 381–386.

von Mutius, E. & D. Vercelli. 2010. Farm living: Effects on childhood asthma and allergy. *Nature Reviews Immunology* 10(12), 861–868. doi:10.1038/nri2871.

Wexler, K. 1994. Optional infinitives, head movement and the economy of derivations. In D. Lightfoot & N. Hornstein (Eds.), *Verb Movement*. New York, NY: Cambridge University Press. 305–350.

Wexler, K. 2013. Tough-movement developmental delay: Another effect of phasal computation. In M. Piattelli-Palmarini & R. C. Berwick (Eds.), *Rich Languages from Poor Inputs*. Oxford, UK: Oxford University Press. 146–167.

Woolf, N. J. & S. R. Hameroff. 2001. A quantum approach to visual consciousness. *Trends in Cognitive Sciences* 5(11), 472–478. doi:10.1016/s1364-6613(00)01774-5.

Xu, F., & J. Tenenbaum. 2007. Word learning as Bayesian inference. *Psychological Review* 114 (2), 245–272.

Massimo Piattelli-Palmarini
University of Arizona
Department of Linguistics
Communication Building Room 305
1103 E University Boulevard
Tucson, AZ 85721
United States of America

massimo@email.arizona.edu

What Would Lenneberg Think?
Biolinguistics in the Third Millennium

W. Tecumseh Fitch

1. Introduction

Biolinguistics, construed broadly as the study of human language from multiple biological viewpoints, was first placed on a solid modern foundation by Eric Lenneberg's impressive *Biological Foundations of Language* in 1967. Lenneberg conceived of our capacity to acquire language as a species-typical aspect of human cognition—a conception so widespread today that it is difficult to realize how radical it seemed to many at the time. Although Lenneberg argued that our language capacity has some species-typical genetic and neural components, he clearly recognized that it has a huge learned, culture-specific component as well. Lenneberg had thus already leap-frogged the unproductive "nature versus nurture" dichotomy that has bedevilled so many debates about language since that time. He also recognized that human language differs in important ways from animal communication, and raised the question of whether the roots of language are best sought in cognition or communication—another prominent preoccupation in modern debates. In short, although he apparently did not adopt the term "biolinguistics" himself, Eric Lenneberg can rightly be seen as an important founding father of contemporary biolinguistics. This makes a celebration in this journal, fifty years later, of his magnum opus highly appropriate.

In this essay, I will first briefly discuss a few of Lenneberg's many insights that I think bear repeating today. Then, I turn to a discussion of modern empirical developments in biolinguistics that I think Lenneberg would find welcome, and in many cases surprising, were he alive today. I will thus focus less on the aspects of Lenneberg's thought that have stood the test of time well, and are still essentially correct today (which covers many of them) and more on aspects where modern data invite a reconsideration of some of his ideas. These come from three general areas: comparative investigations, modern neuroscience and especially molecular genetics. My goal is to provide a concise overview of those developments that I believe, were Lenneberg to appear for a conversation about biolinguistics today, he

---

This paper builds upon a presentation given at a workshop celebrating Eric Lenneberg in Barcelona on 29 September 2017, initiated by Pedro Martins and Cedric Boeckx. I thank them and the participants of the workshop, particularly Angela Friederici, Erich Jarvis, Constance Scharff, Constantina Theofanopoulou, and Sonja Vernes, for comments on this presentation, as well as Simon Fisher for comments on an earlier version of this paper. The work leading up to this paper was supported by the European Research Council (AdG grant #230604).

ISSN 1450–3417

would find most fascinating and in some cases challenging, and to speculate about how he might respond.

2. Lenneberg's Insights

I will first highlight some of Lenneberg's central insights into the biology of language, especially those which appear to have been overlooked in some modern debates. There are many such insights (as well as those where his conceptions, although radical in their time, have become widely accepted), so my selection is personal, and far from comprehensive. I hope that this brief selection inspires contemporary students of biolinguistics to read (or re-read) Lenneberg.

Most crucially, I see Lenneberg's perspective on the biological nature of language acquisition as cutting through some of the most protracted and unproductive debates that continue to rage about language, in particular debates about the roles of genes, environment and culture in language. Lenneberg recognized all of these factors as playing important roles, but did not shy away from highlighting the fact that the human capacity for language is part of human biology and not simply a product of our environment. Starting with the environment, he saw its role as supportive and permissive, but not crucial. In a nice metaphor, he points out that a frog's and a minnow's eggs, developing in the same pond, will reliably yield quite different developmental outcomes (Lenneberg 1967: 373). Of course, the proper nutrients, water, oxygen etc. are needed, but these factors do not determine the outcome: The genetic code and biological aspects of development contained in the eggs at fertilization do. Given this obvious biological fact, why should we be surprised that a human child and a kitten (or a chimpanzee) raised in identical environments should yield different developmental outcomes? The "language-readiness" of the newborn human brain is part of human biology, even if the particular and idiosyncratic end-state reached by any particular individual also obviously incorporates their personal social and environmental history.

Turning to the social and cultural environment, he considered a proper normal social environment to be crucial, but again not instructive in a literal "teaching" sense. Indeed he already was aware that parent's attempts to teach their children specific rules (or to avoid certain words or phrases) are surprisingly ineffective. Acquiring language is not like learning proper etiquette from Miss Manners. Nonetheless, recognizing the centrality of an appropriate linguistic input, he conceptualized the child as "resonating" with this input, rather than memorizing it by rote (see Hoshi, this issue). Cognitively, these led Lenneberg to a conception of "language as a special form of pattern recognition" typical of the human brain, pattern recognition that "can't be accomplished based on probability statistics alone" (Lenneberg 1967: 393).

He explores this idea about resonance in detail, hypothesizing that childhood provides a limited (and again species-specific) period during which the individual is optimally capable of acquiring one or more languages with little effort. This plasticity of childhood is then stabilized at puberty, and the representations formed then undergo consolidation for the rest of an individual's life. Lenneberg can thus be seen as one of the first to recognize the importance of "critical period phenomena" in language acquisition. Today, this is a central and well-accepted concept in child language acquisition, although the more nuanced modern conception, often termed "sensitive periods," acknowledges the more gradual "closing" of the period than the slamming shut at puberty portrayed by Lenneberg. And, again taking an essentially modern biological viewpoint, he recognizes that there is nothing surprising or special about such critical periods: We find them everywhere in biology, from bird imprinting to cat visual development, and human sensitive periods are just one more example.

Also impressive is Lenneberg's conception of the potential nature of "universal" properties of human languages. He clearly recognizes that these must be abstract properties that depend on the universality of the cognitive mechanisms underlying language, and not either the categories or operations seen more superficially in particular languages. It is thus ironic that several recent critiques of the notion of language universals and "universal grammar" have unabashedly taken on the latter rather than the former (e.g., Everett 2005, Tomasello 2005, Evans & Levinson 2009). These critics seek, and then reject the existence of, superficial commonalities, rather than considering the deeper cognitive commonalities that Lenneberg and Chomsky, at around the same time (Chomsky 1965), had in mind when discussing "universal grammar". Contemporary critics also often neglect the kind of statistical or implicational universals, discussed extensively at around the same time by Greenberg and many others (Greenberg 1963, Greenberg, Osgood & Jenkins 1966). This is an example of the dictum that those who fail to learn from history are doomed to repeat it (cf. Fitch 2011).

Finally, Lenneberg weighs in on two central topics in modern debates concerning language evolution. First, considering arguments about the relative importance of peripheral anatomy vs. central cognition in the capacity for language, he comes down clearly on the central side: Neurophysiological factors are central (Lenneberg 1967: 57). But, Lenneberg argues, these central factors are not simply a product of brain size, given that nanocephalic dwarves with brains within the ape size range can possess normal language. The implication is that we cannot use cranial capacity, or other measures obtainable from the fossil record, as an indication of when language arose, or clues as to what its earlier form(s) might have been (cf. Fitch 2009).

Second, in chapter 8 Lenneberg considers and rejects arguments about linguistic relativity made by Benjamin Whorf and others. Although many superficial differences between languages exist in vocabulary, grammatical categories, and many other factors, and these may effect communication, they do not change the central capacity for conceptualization. He concludes that language depends on cognition much more than cognition depends on language.

As mentioned, this is just a taste of the many insights Lenneberg presents in his book. Some of these insights (e.g. regarding critical periods, or the idea that the language-ready brain is part of human biology) have been widely accepted today, at least among academics in the relevant disciplines. Others (e.g. concerning the nature of language universals, or the centrality of cognitive factors) are still debated and/or remain inadequately appreciated by some, even fifty years later. But in my opinion, in all of these cases, Lenneberg was clearly on the right track.

3. How far Have We Come? Biolinguistics Today

Above I highlighted areas where Lenneberg would be either pleased to see his ideas accepted or alternately would roll his eyes at how issues and dichotomies he had already cogently resolved continue to be debated today. I now turn to three areas where I think Lenneberg would be pleasantly surprised at the progress we have made in researching the biological foundations of language. These are comparative biology and animal cognition in particular, neuroscience (especially brain imaging), and molecular genetics. In all of these areas, I think, Lenneberg would be astounded by how much science has progressed, sometimes in ways consistent with his arguments and predictions and sometimes less so.

3.1. Comparative Cognition Research

Although Lenneberg considered animal cognition and communication research briefly, his main concern was distinguishing between these and their human analogs. He, rightly in my opinion, drew a sharp distinction between animal communication systems known at that time and human language; this was one part of his argument as to the primacy of cognitive factors rather than communicative factors in the biology of language. In a detailed discussion of word meanings, he pointed out (again rightly) that human word meanings are nothing like stimulus-response reactions to particular things. Rather, words reflect concepts and categories, often in a very flexible and context-dependent manner (also see Leivada, this issue). Lenneberg argued that words thus "represent" in a very different way from the calls of animal communication systems. He cited two examples of this (Lenneberg 1967: 329–330): A dog trained to "point" at a tree or door in his masters yard is unable to locate and point to the equivalent referents in a neighbor's yard; and a parrot trained to say "goodbye" when people leave the room sometimes do so when people enter as well. In neither case were any references given, so we must assume these are anecdotal reports.

Modern research has elaborated upon these ideas, using well-controlled experimental investigations, in three ways. The first, and perhaps most widely known, was the discovery that some monkeys do produce alarm calls that have rather general apparent referents. For example, vervet monkeys produce three different alarm call types when seeing different predators: large aerial predators like eagles, dangerous land predators like leopards, and less-dangerous large snakes (Struhsaker 1967). Experimental investigations of these calls using playback experiments demonstrated that listening vervets interpret these calls as if they had spotted the relevant predator type themselves (Seyfarth & Cheney 1980, Seyfarth, Cheney & Marler 1980, Seyfarth & Cheney 1984). We now know that such predator-specific alarm calls have evolved, convergently, in many different bird and mammal species, including chickens, ground squirrels, meerkats, and various other monkey species (Sherman 1985, Macedonia 1991, Evans, Evans & Marler 1993, Zuberbühler 2001, Manser, Seyfarth & Cheney 2002, Zuberbühler 2003).

Are these the equivalent of "animal words"? There are several good reasons to think not. Early authors were careful to label these calls "functionally referential" to indicate that they can be parsed by listeners as evidence about predators, but this does not entail that signalers intend to communicate the predator's presence to ignorant receivers (Cheney & Seyfarth 1990). Indeed abundant subsequent work suggests that the latter is not the case, and that indeed monkeys lack a rich enough "theory of mind" to actively represent the ignorance of their conspecifics that would be required to hold such an intention (Seyfarth & Cheney 2003, Cheney & Seyfarth 2007). Thus, it is perfectly possible that, from a signaler's point of view, a particular call mainly reflects arousal caused by sighting a predator, but for the perceiver this functions as an indication of that predator's presence (cf. Wheeler & Fischer 2012, Fitch & Zuberbühler 2013). From a modern perspective then, this particular glass is half-full: it points to a phylogenetic continuity in perceivers (sophisticated, inferential, context-dependent interpretation) but a remaining discontinuity in terms of signalers (Seyfarth & Cheney 2014, 2016). Lenneberg would be fascinated by all this, but point out that the distinction between linguistic meanings in language and "semantics" of these alarm call remains clear.

Regarding dogs' ability to interpret meanings, this issue has now been investigated experimentally in several dogs with extraordinarily large vocabularies including more than 100 object referents (Kaminski, Call & Fischer 2004, Pilley & Reid 2011, Griebel & Oller 2012). Two facets of these data are relevant. First, the border collie "Rico" showed evidence of fast mapping (one trial learning of a word meaning) and long-term retention of that meaning, although the strength of this experimental demonstration has been questioned based on results from another dog (Kaminski, Call & Fischer 2004, Griebel & Oller 2012). Second, another border collie "Chaser" successfully mastered over 1000 object referents, and unlike Rico (who simply retrieved named unique objects) could also produce specific transitive actions to these objects, differentiating the three sentences "nose ball", "paw ball", and "bring ball". Chaser could also use names to refer to categories like "toy" or "ball" that applied to many different objects (Expt. 3, Pilley & Reid 2011). This both shows that dogs are not limited to learning specific object-word pairs, as Lenneberg had claimed, but also have a productive ability to map from utterances to action-object pairings. Lenneberg argues that proper names are in some sense a degenerate form of word meaning (given their typical pairing with specific individuals), and these dog experiments pair specific words with specific objects, and could thus also be seen as degenerate "proper names". But the understanding of novel object/action pairings documented for Chaser belie Lenneberg's argument that the productive, creative ability to understand novel combinations is unique to humans.

Finally, and for me most convincingly, the results from language-trained African gray parrots experimentally demonstrate both flexible, context-dependent interpretation of meaning (including adjectives like shape, color, material, and number) and appropriate productive usage of these abstract categories (Pepperberg 1981, Pepperberg & Brezinsky 1991, Pepperberg 1999). Although many parrots learn to imitate speech, the meaningful comprehension and use of words requires special training (the model-rival paradigm), and few parrots have successfully undergone this intensive procedure, which more closely resembles child language acquisition than more typical training procedures (Pepperberg 1985). Although in the parrot case both vocal imitation and its meaningful deployment are clearly the result of convergent evolution, they nonetheless belie Lenneberg's arguments about human-typical usage of meaning being unique to our species: Important components of this are found in multiple other species.

What would Lenneberg's reaction to these data be? First, he would correctly point out that in all of these cases, it appears that the "semantic" behaviors exhibited by non-human animals evolved convergently relative to our own. Even regarding nonhuman primates, we have no evidence for predator-specific alarm calls in great apes, so the presence of such calls in some monkeys is not evidence that they were present in our common ancestor with these monkeys. Nonetheless, these data clearly show that Lenneberg's claims of "unique to man" are incorrect. More importantly, although Lenneberg recognized that repeated convergent evolution can provide evidence about adaptive function (Chapters 1 and 9), he does not seem to recognize that such analogies are also relevant to testing mechanistic hypotheses, for example hypotheses regarding the brain mechanisms needed to perform these feats (for dogs see e.g. Andics et al. 2014, Andics et al. 2016). Although only homology can be used to infer phylogenetic trajectories, as Lenneberg realized, this does not make analogy irrelevant (cf. Fitch 2010, 2017).

I think that these modern data steals the wind from the sails of Lenneberg's critique of Otto Koehler, with whom Lenneberg only partially agreed (1967: 232). In fact, Koehler was one of the first to persuasively argue for a multi-component perspective on the biology of language (Koehler 1951, 1954). Today, a multi-component approach to biolinguistics, including animal cognition as a key ingredient, is widely seen as the most promising comparative framework within which to consider the evolution of language (Hauser, Chomsky & Fitch 2002, Fitch, Hauser & Chomsky 2005, Fitch 2017); I suspect that given today's data Lenneberg would concur.

3.2. *Neuroscience and Brain Imaging*

I think that modern progress in neuroscientific methodology would have pleased (and astounded) Lenneberg. In his day, essentially the only neural data relevant to language came from either clinical anomalies (like the genetic dwarfism that he highlighted) or patients suffering from brain lesions. In the latter case, there were no methods like computed tomography (CT) scans or magnetic resonance imaging (MRI) to probe brain anatomy in living individuals, so investigation relied either on clinical signs (e.g. hemiparesis) or post-mortem dissection. Thus, the degree to which Lenneberg could interpret data from his observations of living patients in terms of lesion location was extremely limited. Indeed he is somewhat dismissive of the relevance of localized lesions. Although he worked with Norman Geschwind's patients (Lenneberg 1967: 196), he mentions Geschwind's then new but now classic 1965 paper, defending the importance of the arcuate fasiculus in connecting frontal and temporal regions, only in passing as "an opposite point of view" (Lenneberg 1967: 217), without further discussion. In a book which leaves few stones unturned, this is a surprising dismissal.

Since the 1970s and the introduction of CT scans, and then later MRI, neurolinguists have benefited from a much richer and more accurate means of determining lesion location and of linking it to a patient's symptoms. The result has been a revival, with modifications, of the older models of Broca, Wernicke, and Lichtheim which attempted to localize particular sub-components of language (such as producing vs. understanding speech, or processing syntax) to particular brain regions (Geschwind 1970, Caramazza & Zurif 1976, Damasio & Geschwind

1984, Damasio & Damasio 1992). The later introduction of positron emission tomography (PET) and functional MRI (fMRI), which allowed measurement of brain activity in living patients, further amplified this localizationist trend. Although sometimes veering into "neo-phrenology" (cf. Blumstein 1979, Friston 2002), there can be little doubt that these modern brain imaging methods have provided a massive new source of relevant data, and have strongly influenced contemporary neuro-linguistics. Lenneberg would be amazed.

Lenneberg might have been less impressed with the various theoretical edifices which have been built up around this new font of neurolinguistic data, because some make little contact with findings from linguistics, and even those that do often adopt radically different theoretical approaches (compare, for example, Ullman 2001, Hagoort 2005, Price 2010, Friederici 2011, Rogalsky & Hickok 2011). Given that Lenneberg's book ends with an appendix by Noam Chomsky detailing formal considerations for evaluating computational approaches to language, Lenneberg might be disappointed to see how rarely formal and computational linguistic considerations play a role in current discussions of the neural basis of language.

To briefly consider a neurolinguistic approach that does take formal and computational considerations seriously, consider the recent brain imaging work of Angela Friederici and colleagues (Bahlmann, Schubotz & Friederici 2008, Bahlmann et al. 2009, Friederici 2011, 2012, 2017). Starting with two well-defined artificial grammars (a finite-state and context-free grammar), these researchers first found that different frontal regions were activated in participants trained to process the two grammars (Friederici et al. 2006). Furthermore, these two regions were connected to the temporal cortex by very different white matter tracts: the frontal operculum (activated by the simpler grammar) was mainly connected via a ventral pathway, while the portion of Broca's area activated by the context-free grammar (Brodmann area [BA] 44) was connected by a dorsal pathway roughly equivalent to Geschwind's arcuate fasiculus (Friederici et al. 2006). Later experiments showed the same region activated by a strictly hierarchical, center-embedded grammar, suggesting that this activation is not due to any simpler strategy, like counting, being adopted to process the context-free grammar (Bahlmann, Schubotz & Friederici 2008), and that the same region is increasingly activated by increasingly complex syntactic constructions in German (Makuuchi et al. 2009).

The conclusion that portions of Broca's region play a key role in processing syntax, whether in natural language or in various more artificial tasks, was verified in a meta-analysis (Friederici 2011) as well as numerous experiments in other laboratories (Pallier, Devauchelle & Dehaene 2011, Uddén & Bahlmann 2012, Hagoort 2014, Dehaene et al. 2015). It remains unclear to what degree this rather broad region is specifically involved in linguistic syntax (as opposed to, say, musical syntax), but that at least portions play a key role in processing language appears indubitable (Koelsch et al. 2002, Fazio et al. 2009, Fedorenko, Duncan & Kanwisher 2013, Fitch & Martins 2014). For a dissenting view, see Rogalsky & Hickok (2011).

This brain imaging research also makes a fascinating connection to neuroanatomical work in nonhuman primates. Direct cytoarchitectonically guided comparisons between Broca's region in human and chimpanzee post-mortem brains shows that this area is the most enlarged cortical region known in the human brain, with left BA 45 six times and left BA 44 6.6 times larger in humans than in chimpanzees (Schenker et al. 2010). Although the entire human brain is three times larger than a chimpanzee's, primary sensory regions are not greatly enlarged: primary visual cortex is only 1.8 times larger in humans than chimpanzees (Schenker et al. 2010). Furthermore, these areas are much more heavily connected, via a dorsal pathway, to a much wider swathe of parietal and temporal cortex in humans (Rilling et al. 2008).

This combination of comparative and neuroimaging data have led me to suggest that Broca's region plays the computational role of a "stack", a form of auxiliary memory required for processing of grammars above the finite-state or "regular" level (Fitch 2014). According to this "dendrophilia hypothesis" (Fitch 2014), the enlargement of Broca's region greatly expanded the capacity of this auxiliary memory, and its drastically increased connectivity increased its ability to serve multiple functions for multiple brain regions (rather than being limited to a single modality or cognitive domain). I believe that, whether this hypothesis turns out to be correct in its details or not, it is the kind of hypothesis Lenneberg would favor if he were alive today, given his emphasis on a broad range of biological data consistent with linguistic theory.

Two other neurolinguistic issues raised by Lenneberg deserve emphasis. One is his emphasis, typical at that time, on the importance of cerebral asymmetry in human language, and in particular the left hemisphere bias observed in some aspects of language processing. This degree of left bias was thought by many at the time to be unique to humans. For example:

> The phenomenon of cerebral dominance—that is, the predominant importance of one side of the brain for a class of learned behavior—occurs, as far as we know, in no mammal other than man.
>
> (Geschwind 1970: 944)

Geschwind was careful to say "in no mammal" because it was already known from the work of Fernando Nottebohm that song control in some birds was left-lateralized (Nottebohm 1971). In any case, it is now clear that cerebral asymmetry is found, and indeed appears to be typical, in a wide variety of vertebrates including fish, birds and mammals (Bisazza, Rogers & Vallortigara 1998, Vallortigara 2000, Rogers & Andrew 2002). To the extent that aspects of human language are lateralized, they are not unique or even unusual in this respect, compared to perceptual processes in vertebrates in general, or primates in particular (cf. Fitch & Braccini 2013).

In contrast, an observation emphasized by Lenneberg that seems to have held up today concerns the significance of absolute brain size for linguistic ability. Citing the clinical work of Seckel on nanocephalic dwarfism (Seckel 1960), in which humans have very small brains in the range of chimpanzees but nonetheless have preserved linguistic abilities, Lenneberg suggested that the "absolute increase in cell number and axodendritic density have increased man's psychological storage capacity" (Lenneberg 1967: 69) and other more general cognitive abilities, but not our capacity for language. Although nanocephalic dwarves suffer general cognitive deficits, they nonetheless acquire the rudiments of speaking and understanding, and most master verbal skills to at least the level of five-year-old children. Lenneberg concluded, correctly I think, that "the organization of the brain is more important for language than its mass" (Lenneberg 1967: 70). This conclusion is consistent with most current thinking in neurolinguistics.

3.3. *Molecular Genetics, Deep Homology and Paleo-DNA*

I turn finally to a source of data that would probably have most astounded Lenneberg: the accurate sequencing of DNA recovered from long-extinct hominins like Neanderthals and Denisovans (Green et al. 2010, Meyer et al. 2012). The general progress made in genetics since Lenneberg's time (genetic engineering, optogenetics, personal genome sequencing, etc.) is by itself astonishing, and continues to accelerate. But the notion that we would be able to recover DNA from an extinct species in enough detail to begin reconstructing its phenotype seemed fanciful even in 2000, after the first sequence data of the mitochondrial DNA from a Neanderthal was reported (Krings et al. 1997). The reason is that each cell contains many mitochondria (typically hundreds or thousands per cell), each of which has multiple copies of the whole genome, meaning that there are typically thousands copies of the mitochondrial DNA for each copy of nuclear DNA. The mitochondrial genome is also much shorter than the nuclear genome, and is arranged on a circular chromosome yielding greater stability. It is thus easier to piece together from the very short sequences typically recovered from ancient DNA. For these reasons it initially seemed likely mitochondrial DNA would be all we could ever extract and sequence from fossils: useful but very far from complete.

But ten years afterward, due to improved sample quality, sequencing technology, and computational tools, the unimaginable became true and sequence data covering much of the Neanderthal genome was reported (Green et al. 2010), with the bombshell finding that a small amount of interbreeding had occurred between Neanderthals and modern humans (cf. Pääbo 2014). A few years later, a high quality full-coverage genome was released (Prüfer et al. 2014), which allowed a short list of 87 genes with protein-coding differences between Neanderthals and ourselves to be drawn up (see supplementary online material for Prüfer et al. 2014).

To the extent that we can interpret these genetic differences and similarities, the Neanderthal genome offers an unparalleled opportunity to test hypotheses about the existence of particular components of language in this extinct species, and thus to derive inferences about the "protolanguage" that might have characterized this species (cf. Fitch 2017). Our problem at present is that few genetic variants have been isolated that can be definitively linked to any particular component of language. However, one such example does exist already: the human-specific form of the *FOXP2* gene. This gene was first identified due to its mutation in a British family, some of whose members suffer from developmental dypraxia specific to oro-motor control, which leads to a severe developmental speech disorder despite otherwise relatively spared cognitive and linguistic abilities (Vargha-Khadem et al. 1998, Vargha-Khadem et al. 2005). Since this discovery, multiple other individuals with *FOXP2* mutations and a similar phenotype have been discovered (Pääbo 2014, Graham & Fisher 2015).

The *FOXP2* gene codes for a transcription factor (a protein that can bind to DNA and thus modify the expression of other genes) connected with a rather large network of associated genes that can be up- or down-regulated (Vernes et al. 2007).

Crucially, in all modern humans, *FOXP2* exists in a human-specific allele, coding a protein that differs from that of chimpanzees (Enard et al. 2002). Within-species homogeneity is just what we should expect for a gene causally associated with language evolution (since humans around the world have the same capacity to acquire any language), as is a difference from chimpanzees (which lack this capacity). Thus *FOXP2* has the appropriate characteristics required of a language-evolution-related gene, albeit one that is tied to speech output capabilities rather than more central linguistic characteristics like syntax.

Crucially, once the Neanderthal genome was sequenced, it was possible to evaluate the sequence of *FOXP2* in this species as well, and the Neanderthal gene turned out to code for the same derived protein as in modern humans (Krause et al. 2007). This strongly suggests that the selective event that led this derived variant to be selected in our hominin ancestors preceded our divergence from Neanderthals, a split which occurred roughly 500,000 years ago. Although it is important to note that no single gene alone underlies the derived human capability for speech, this finding in turn suggests that selection for improved oro-motor sequencing also preceded this split, suggesting that increased oro-motor sequencing abilities characterized our common ancestor with Neanderthals (cf. Fitch 2017). This is obviously consistent with the hypothesis that this common ancestor had already acquired the capacity for speech. Finally, because the paleontological record strongly suggests that Neanderthals did not have fully modern cognition (Tattersall 2016), this suggests that this common ancestor, like Neanderthals, lacked some other crucial component of language (e.g., hierarchical syntax or theory of mind, cf. Bolhuis et al. 2014). Although no single gene by itself can explain language, or even key sub-components of language like oro-motor sequencing or dendrophilia, this case of *FOXP2* provides a promissory note for future, similar genetic investigations, rather than a complete story in itself.

Of course, the complexity of the mapping between genes, development, and cognitive abilities is highly indirect and complex (DeSalle & Tattersall 2017, Fisher 2017), and we should not let the excitement about *FOXP2* above obscure the fact that most of the known genes associated with language and language disorders have less clear and striking phenotypic effects. Any single gene acts in a context provided by other genes, and its effects often vary from tissue to tissue, and probably differ in different brain regions. It is also important to note that many genetic changes relevant to evolution result in changes in gene regulation during development rather than protein-coding changes in the gene product. Indeed, outside of the protein-coding portion of the *FOXP2* gene, a modification of a putative regulatory region has been discovered which is not shared with Neanderthals (Maricic et al. 2013), suggesting that there may have been waves of selection on this gene, and the most recent one is unique to *Homo sapiens*. We have a long way to go before even this single gene is understood, much less the multiple changes in many genes, related to multiple different mechanisms, that must have accompanied the evolution of language.

Nonetheless, this new source of paleo-genetic data clearly refutes the all-to-common notion that “we will never know” what happened when during hominin evolution, and opens the prospect of testing competing models of language evolution empirically. Lenneberg would be amazed.

Although many other genes besides *FOXP2* have been proposed to be associated with human language abilities (Graham & Fisher 2015, Mozzi et al. 2016), none of these candidates yet has both the clear phenotypic identification and the pattern of within-human homogeneity and human/chimpanzee differences that would be required to firmly identify it as a language-related gene. Fortunately, however, the process of identifying and isolating gene variants in individuals is now part of normal clinical genetic practice and individualized genomics, so we can confidently await more examples in the coming years. For each of these candidate genes, we can immediately cross-reference with the Neanderthal genome (and paleo-genomes from other extinct hominins) to see if they shared the modern human allele or not, and build a clearer and clearer picture of what the (already known) genetic differences between us, Neanderthals, and chimpanzees actually mean for development and the human phenotype. Thus, in my opinion, the most promising data-driven pathway for examining the linguistic (and other cognitive) abilities of Neanderthals and other extinct hominins is by examining their DNA.

4. Conclusion

In this paper I have attempted to revisit some of the key notions discussed by Lenneberg in his ground-breaking book from a modern, empirical perspective. Although, in many ways, Lenneberg was on the right track, and new sources of data have simply reinforced points he made fifty years ago, in others his conclusions have been updated, challenged or even rejected. In sum, however, I think it is fair to say that the broad, data-driven approach that he pioneered, firmly based in both the biology and linguistics of his time, has aged well. There is still much to be found in his book that remains of value for biolinguists today.

In addition, I tried in the second part of this essay to show how far the field of biolinguistics has come since Lenneberg's time, building on a set of questions he was one of the first to cogently ask, and within a framework he laid out beautifully in the late 1960s. I believe that, were he alive today, Lenneberg would be pleased by this progress, and excited by the prospects for the future.

References

Andics, Attila, Anna Gábor, Márta Gácsi, Tamás Faragó, Dóra Szabó & Ádám Miklósi. 2016. Neural mechanisms for lexical processing in dogs. *Science.* doi:10.1126/science.aaf3777.

Andics, Attila, Márta Gácsi, Tamás Faragó, Anna Kis & Ádám Miklósi. 2014. Voice-sensitive regions in the dog and human brain are revealed by comparative fMRI. *Current Biology* 24(5), 574–8.

Bahlmann, Jörg, Ricarda I. Schubotz & Angela D. Friederici. 2008. Hierarchical artificial grammar processing engages Broca's area. *NeuroImage* 42(2), 525–34.

Bahlmann, Jörg, Ricarda I. Schubotz, Jutta L Mueller, Dirk Koester & Angela D. Friederici. 2009. Neural circuits of hierarchical visuo-spatial sequence processing. *Brain Research* 1298, 161–70.

Bisazza, Angelo , Lesley J. Rogers & Giorgio Vallortigara. 1998. The origins of cerebral asymmetry: A review of evidence of behavioural and brain lateralization in fishes, reptiles and amphibians. *Neuroscience and Biobehavioral Reviews* 22(3), 411–26.

Blumstein, Sheila E. 1979. Phrenology, "boxology" and neurology. *Behavioral & Brain Sciences* 2, 460–461.

Bolhuis, Johan J., Ian Tattersall, Noam Chomsky & Robert C. Berwick. 2014. How could language have evolved? *PLOS Biology*. doi:10.1371/journal.pbio.1001934.

Caramazza, Alfonso & Edgar B. Zurif. 1976. Dissociation of algorithmic and heuristic processes in language comprehension: Evidence from aphasia. *Brain and Language* 3(4), 572–582.

Cheney, Dorothy L. & Robert M. Seyfarth. 1990. *How Monkeys See the World: Inside the Mind of Another Species*. Chicago, IL: Chicago University Press.

Cheney, Dorothy L. & Robert M. Seyfarth. 2007. *Baboon Metaphysics: The Evolution of a Social Mind*. Chicago, IL: University of Chicago Press.

Chomsky, Noam. 1965. *Aspects of the Theory of Syntax*. Cambridge, MA: MIT Press.

Damasio, Antonio R. &H. Damasio. 1992. Brain and language. *Scientific American* 267(3), 88–109.

Damasio, Antonio R. & Norman Geschwind. 1984. The neural basis of language. *Annual Review of Neuroscience* 7, 127–147.

Dehaene, Stanislas, Florent Meyniel, Catherine Wacongne, Liping Wang & Christophe Pallier. 2015. The neural representation of sequences: From transition probabilities to algebraic patterns and linguistic trees. *Neuron* 88, 2–19.

DeSalle, Rob & Ian Tattersall. 2017. What aDNA can (and cannot) tell us about the emergence of language and speech. *Journal of Language Evolution*. doi:10.1093/jole/lzx018.

Enard, Wolfgang, Molly Przeworski, Simon E. Fisher, Cecilia S. L. Lai, Victor Wiebe, Takashi Kitano, Anthony P. Monaco, & Svante Paäbo. 2002. Molecular evolution of FOXP2, a gene involved in speech and language. *Nature* 418, 869–872.

Evans, Christopher S., Linda Evans & Peter Marler. 1993. On the meaning of alarm calls: Functional reference in an avian vocal system. *Animal Behaviour* 46, 23–38.

Evans, Nicholas & Stephen C. Levinson. 2009. The myth of language universals: Language diversity and its importance for cognitive science. *Behavioral & Brain Sciences* 32, 429–448.

Everett, Daniel L. 2005. Cultural constraints on grammar and cognition in Pirahã. *Current Anthropology* 46(4), 621–646.

Fazio, Patrik, Anna Cantagallo, Laila Craighero, Alessandro D'Ausilio, Alice C Roy, Thierry Pozzo, Ferdinando Calzolari, Enrico Granieri, & Luciano Fadiga. 2009. Encoding of human action in Broca's area. *Brain* 132(7), 1980–1988.

Fedorenko, Evelina, John Duncan & Nancy Kanwisher. 2013. Broad domain generality in focal regions of frontal and parietal cortex. *Proceedings of the National Academy of Sciences of the United States of America* 110(41), 1281–1296.

Fisher, Simon E. 2017. Evolution of language: Lessons from the genome. *Psychonomic Bulletin & Review* 24(1), 34–40.

Fitch, W. Tecumseh. 2009. Fossil cues to the evolution of speech. In Rudolf P. Botha & Chris Knight (eds.), *The Cradle of Language*, 112–34. Oxford, England: Oxford University Press.

Fitch, W. Tecumseh. 2010. *The Evolution of Language*. Cambridge: Cambridge University Press.

Fitch, W. Tecumseh. 2011. Unity and diversity in human language. *Philosophical Transactions of the Royal Society B: Biological Sciences* 366, 376–88.

Fitch, W. Tecumseh. 2014. Toward a Computational Framework for Cognitive Biology: Unifying approaches from cognitive neuroscience and comparative cognition. *Physics of Life Reviews* 11(3), 329–364.

Fitch, W. Tecumseh. 2017. Empirical approaches to the study of language evolution. *Psychonomic Bulletin & Review* 24(1), 3–33.

Fitch, W. Tecumseh & Stephanie N. Braccini. 2013. Primate laterality and the biology and evolution of human handedness: A review and synthesis. *Annals of the New York Academy of Sciences* 1288(1), 70–85.

Fitch, W. Tecumseh, Marc D. Hauser & Noam Chomsky. 2005. The evolution of the language faculty: Clarifications and implications. *Cognition* 97(2), 179–210.

Fitch, W. Tecumseh & Mauricio D. Martins. 2014. Hierarchical processing in music, language and action: Lashley revisited. *Annals of the New York Academy of Sciences* 1316, 87–104.

Fitch, W. Tecumseh & Klaus Zuberbühler. 2013. Primate precursors to human language: Beyond discontinuity. In Elke Zimmerman, Sabine Schmidt & Eckart Altenmüller (eds.), *The Evolution of Emotional Communication*, 26–48. Oxford: Oxford University Press.

Friederici, Angela D. 2011. The Brain Basis of Language Processing: From Structure to Function. *Physiological Reviews* 91(4), 1357–1392.

Friederici, Angela D. 2012. Language development and the ontogeny of the dorsal pathway. *Frontiers in Evolutionary Neuroscience* 4(3). doi:10.3389/fnevo.2012.00003.

Friederici, Angela D. 2017. Evolution of the neural language network. *Psychonomic Bulletin & Review* 24(1), 41–7.

Friederici, Angela D., Jörg Bahlmann, Stefan Heim, Ricarda I. Schubotz & Alfred Anwander. 2006. The brain differentiates human and non-human grammars: Functional localization and structural connectivity. *Proceedings of the National Academy of Sciences of the United States of America* 103(7), 2458–2463.

Friston, Karl J. 2002. Beyond phrenology: What can neuroimaging tell us about distributed circuitry? *Annual Review of Neuroscience* 25, 221–250.

Geschwind, Norman. 1965. Disconnexion syndromes in animals and man. *Brain* 88, 585–644.

Geschwind, Norman. 1970. The organization of language and the brain. *Science* 170, 940–944.

Graham, Sarah A. & Simon E. Fisher. 2015. Understanding language from a genomic perspective. *Annual Review of Genetics* 49, 131–160.

Green, Richard E., Johannes Krause, Adrian W. Briggs, Tomislav Maricic, Udo Stenzel, Martin Kircher, . . . , & Svante Pääbo. 2010. A draft sequence of the Neandertal genome. *Science* 328(5979), 710–722. doi:10.1126/science.1188021.

Greenberg, Joseph (ed.) 1963. *Universals of Language*. Cambridge, MA: MIT Press.

Greenberg, Joseph, Charles E Osgood & James J Jenkins. 1966. *Memorandum Concerning Language Universals*. The Hague, The Netherlands: Mouton & Co.

Griebel, Ulrike & D. Kimbrough Oller. 2012. Vocabulary learning in a Yorkshire terrier: Slow mapping of spoken words. *PLoS ONE* 7(2), e30182.

Hagoort, Peter. 2005. On Broca, brain, and binding: A new framework. *Trends in Cognitive Sciences* 9(9), 416–423.

Hagoort, Peter. 2014. Nodes and networks in the neural architecture for language: Broca's region and beyond. *Current Opinion in Neurobiology* 28, 136–41.

Hauser, Marc D., Noam Chomsky & W. Tecumseh Fitch. 2002. The Language Faculty: What is it, who has it, and how did it evolve? *Science* 298, 1569–1579.

Kaminski, Juliane, Josep Call & Julia Fischer. 2004. Word learning in a domestic dog: Evidence for 'fast mapping'. *Science* 304, 1682–1683.

Koehler, Otto. 1951. Der Vogelgesang als Vorstufe von Musik und Sprache. *Journal of Ornithology* 93(1), 3–20.

Koehler, Otto. 1954. Vom Erbgut der Sprache. *Homo* 5, 97–104.

Koelsch, Stefan , Thomas C. Gunter, D. Yves von Cramon, Stefan Zysset, Gabriele Lohmann & Angela D. Friederici. 2002. Bach Speaks: A cortical "language-network" serves the processing of music. *NeuroImage* 17, 956–966.

Krause, Johannes, Carles Lalueza-Fox, Ludovic Orlando, Wolfgang Enard, Richard E. Green, Hernán A. Burbano, . . . , & Svante Pääbo. 2007. The derived FOXP2 variant of modern humans was shared with Neandertals. *Current Biology* 17(21), 1908–1912. doi:10.1016/j.cub.2007.10.008.

Krings, Matthias, Anne Stone, Ralf W. Schmitz, Heike Krainitzki, Mark Stoneking & Svante Pääbo. 1997. Neandertal DNA sequences and the origin of modern humans. *Cell* 90, 19–30.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Macedonia, Joseph M. 1991. What is communicated in the antipredator calls of lemurs: Evidence from playback experiments with ring-tailed and ruffed lemurs. *Ethology* 86, 177–190.

Makuuchi, Michiru, Jörg Bahlmann, Alfred Anwander & Angela D. Friederici. 2009. Segregating the core computational faculty of human language from working memory. *Proceedings of the National Academy of Sciences of the United States of America* 106(20), 8362–8367.

Manser, Marta B., Robert M. Seyfarth & Dorothy L. Cheney. 2002. Suricate alarm calls signal predator class and urgency. *Trends in Cognitive Sciences* 6(2), 55–7.

Maricic, Tomislav, Viola Günther, Oleg Georgiev, Sabine Gehre, Marija Curlin, Christiane Schreiweis, . . . , & Svante Pääbo. 2013. A recent evolutionary change affects a regulatory element in the human FOXP2 gene. *Molecular Biology and Evolution* 30(4), 844–852. doi:10.1093/molbev/mss271.

Meyer, Matthias, Martin Kircher, Marie-Theres Gansauge, Heng Li, Fernando Racimo, Swapan Mallick, . . . , & Svante Pääbo. 2012. A high-coverage genome sequence from an archaic Denisovan individual. *Science* 338(6104), 222–226. doi:10.1126/science.1224344.

Mozzi, Alessandra, Diego Forni, Mario Clerici, Uberto Pozzoli, Sara Mascheretti, Stefania Riva, . . . , & Manuela Sironi. 2016. The evolutionary history of genes involved in spoken and written language: Beyond FOXP2. *Scientific Reports* 6, 22157. doi:10.1038/srep22157.

Nottebohm, Fernando. 1971. Neural lateralization of vocal control in a passerine bird. I. Song. *Journal of Experimental Zoology* 177, 229–262.

Pääbo, Svante. 2014. The human condition—A molecular approach. *Cell* 157, 216–226.

Pallier, Christophe, Anne-Dominique Devauchelle & Stanislas Dehaene. 2011. Cortical representation of the constituent structure of sentences. *Proceedings of the National Academy of Sciences of the United States of America* 108(6), 2522–2527.

Pepperberg, Irene M. 1981. Functional vocalizations by an African grey parrot. *Zeitschrift für Tierpsychologie* 55, 139–160.

Pepperberg, Irene M. 1985. Social Modeling Theory: A possible framework for understanding avian vocal learning. *Auk* 102, 854–864.

Pepperberg, Irene M. & M. V. Brezinsky. 1991. Acquisition of a relative class concept by an African Grey parrot (*Psittacus erithacus*): Discriminations based on relative size. *Journal of Comparative Psychology* 105, 286–294.

Pepperberg, Irene M. 1999. *The Alex Studies: Cognitive and Communicative Abilities of Grey Parrots*. Cambridge, MA: Harvard University Press.

Pilley, John W. & Alliston K Reid. 2011. Border collie comprehends object names as verbal referents. *Behavioural Processes* 86(2), 184–195.

Price, Cathy J. 2010. The anatomy of language: A review of 100 fMRI studies published in 2009. *Annals of the New York Academy of Sciences* 1191(2012), 62–88.

Prüfer, Kay, Fernando Racimo, Nick Patterson, Flora Jay, Sriram Sankararaman, Susanna Sawyer, . . . , & Svante Pääbo. 2014. The complete genome sequence of a Neanderthal from the Altai Mountains. *Nature* 505, 43–9. doi:10.1038/nature 12886.

Rilling, James K., Matthew F. Glasser, Todd M Preuss, Xiangyang Ma, Tiejun Zhao, Xiaoping Hu & Timothy E. J. Behrens. 2008. The evolution of the arcuate fasciculus revealed with comparative DTI. *Nature Neuroscience* 11(4), 426–428.

Rogalsky, Corianne & Gregory Hickok. 2011. The role of Broca's area in sentence comprehension. *Journal of Cognitive Neuroscience* 23(7), 1664–1680.

Rogers, Lesley J. & J. R. Andrew. 2002. *Comparative Vertebrate Lateralization*. Cambridge, England: Cambridge University Press.

Schenker, Natalie M., William D. Hopkins, Muhammed A. Spocter, Amy R. Garrison, Cheryl D. Stimpson, Joseph M. Erwin, . . . Chet C. Sherwood. 2010. Broca's area homologue in chimpanzees (*Pan troglodytes*): Probabilistic mapping, asymmetry and comparison to humans. *Cerebral Cortex* 20(3), 730–742. doi:10.1093/cercor/bhp138.

Seckel, Helmut P. G. 1960. *Bird-Headed Dwarfs: Studies in Developmental Anthropology Including Human Proportions*. Basel: Karger.

Seyfarth, Robert M. & Dorothy L. Cheney. 2014. The evolution of language from social cognition. *Current Opinion in Neurobiology* 28, 5–9.

Seyfarth, Robert M. & Dorothy L. Cheney. 2016. Precursors to language: Social cognition and pragmatic inference in primates. *Psychonomic Bulletin & Review* 24(1), 79–84.

Seyfarth, Robert M., Dorothy L. Cheney & Peter Marler. 1980. Vervet monkey alarm calls: semantic communication in a free-ranging primate. *Animal Behaviour* 28, 1070–1094.

Seyfarth, Robert M. & Dorothy L. Cheney. 1980. The ontogeny of vervet monkey alarm-calling behavior: A preliminary report. *Zeitschrift fur Tierpsychologie* 54, 37–56.

Seyfarth, Robert M. & Dorothy L. Cheney. 1984. Grooming, alliances and reciprocal altruism in vervet monkeys. *Nature* 308, 541–543.

Seyfarth, Robert M. & Dorothy L. Cheney. 2003. Signalers and receivers in animal communication. *Annual Review of Psychology* 54, 145–173.

Sherman, Paul W. 1985. Alarm calls of Belding's ground squirrels to aerial predators: Nepotism or self-preservation? *Behavioral Ecology and Sociobiology* 17, 313–323.

Struhsaker, Thomas T. 1967. Auditory communication among vervet monkeys (*Cercopithecus aethiops*). In Stuart A. Altmann (ed.), *Social Communication Among Primates.*, 281–324. Chicago: Chicago University Press.

Tattersall, Ian. 2016. How can we detect when language emerged? *Psychonomic Bulletin & Review* 24(1), 64–7.

Tomasello, Michael. 2005. Comment on Everrett (2005). *Current Anthropology* 46(4), 640–641.

Uddén, Julia & Jörg Bahlmann. 2012. A rostro-caudal gradient of structured sequence processing in the left inferior frontal gyrus. *Philosophical Transactions of The Royal Society B: Biological Sciences* 367(1598), 2023–2032.

Ullman, Michael T. 2001. A neurocognitive perspective on language: The declarative / procedural model. *Nature Reviews Neuroscience* 2, 717–726.

Vallortigara, Giorgio. 2000. Comparative neuropsychology of the dual brain: A stroll through animals' left and right perceptual worlds. *Brain and Language* 73, 189–219.

Vargha-Khadem, Faraneh , Kate Watkins, C. J. Price, J. Ashburner, Katie Alcock, A. Connelly, . . . , & Richard E. Passingham. 1998. Neural basis of an inherited speech and language disorder. *Proceedings of the National Academy of Sciences of the United States of America* 95, 12695–12700. doi:10.1073/pnas.95.21.12695.

Vargha-Khadem, Faraneh, D. G. Gadian, A. Copp & Mortimer Mishkin. 2005. *FOXP2* and the neuroanatomy of speech and language. *Nature Reviews Neuroscience* 6(2), 131–138.

Vernes, Sonja C., Elizabeth Spiteri, J. Nicod, Matthias Groszer, Jennifer M. Taylor, Kay E. Davies, Daniel H. Geschwind, & Simon E. Fisher. 2007. High-throughput analysis of promoter occupancy reveals direct neural targets of FOXP2, a gene mutated in speech and language disorders. *American Journal of Human Genetics* 81(6), 1232–1250. doi:10.1086/522238.

Wheeler, Brandon C. & Julia Fischer. 2012. Functionally referential signals: A promising paradigm whose time has passed. *Evolutionary Anthropology* 21(5), 195–205.

Zuberbühler, Klaus. 2001. Predator-specific alarm calls in Campbell's monkeys, Cercopithecus campbelli. *Behavioral Ecology and Sociobiology* 50(5), 414–22.

Zuberbühler, Klaus. 2003. Referential signaling in non-human primates: Cognitive precursors and limitations for the evolution of language. *Advances in the Study of Behavior* 33, 265–307.

W. Tecumseh Fitch
University of Vienna
Department of Cognitive Biology
Althanstrasse 14
1090 Vienna
Austria

tecumseh.fitch@univie.ac.at

"Language and Brain: Developmental Aspects:" Eric Lenneberg at the Neurosciences Research Program in 1972

Michael A. Arbib

1. Introduction

Neuroscience seems like such an established field that it may surprise readers to learn that it (as distinct from neurophysiology and neuroanatomy as separate disciplines) was established only in 1962, with the founding of the Neurosciences Research Program (NRP) at the Massachusetts Institute of Technology (MIT) by Francis O. Schmitt and a variety of scientists interested in the neural basis of behavior and mind. (The Society for Neuroscience was founded in 1969 with Ralph W. Gerard as Honorary President.) The NRP hosted four summer schools and multiple Work Sessions over the next 20 years to help establish the new field. Schmitt not only brought together a variety of brain-centered disciplines but also championed the application of molecular biology and genetics to the study of the brain. For a history of the NRP, see the essay by George Adelman (2010) who was for many years librarian—and more—for the NRP.

Eric Lenneberg published *Biological Foundations of Language* in 1967. Five years later, he published "Language and Brain: Developmental Aspects" (Lenneberg 1972). This was a Bulletin based on an NRP Work Session organized by Lenneberg and held November 19–21, 1972. The report contained an edited record of the presentations as updated by the participants, and closed with an epilogue written by Lenneberg. This is an almost-forgotten work. When I last checked Google Scholar (August 2017) there were more than 10,000 hits for the book, yet only 3 for Lenneberg's epilogue and none for the work as a whole. The aim of this article is to bring the Bulletin to the attention of all those who value Lenneberg's work, with the bulletin itself made accessible on-line by presenting it as supplementary material in the Appendix to this piece. The article first presents, and to some extent comments on, the individual presentations at the Work Session, grouped under themes as in the bulletin, and closes with a perspective on Lenneberg's epilogue.

2. Neuroanatomical Approaches to the Study of Language

Georges Schaltenbrand (1972: 512–524)[1] reviewed the "Neuroanatomical Aspects of Speech and the Electrical Stimulation of the Brain." He offered clinical obser-

---

[1] Unless otherwise indicated, all page numbers given in the running text of this paper beyond this point reference the original pages and respective contributions in the NRP Bulletin under discussion (Lenneberg 1972).

ISSN 1450–3417

vations related to speech and other vocalizations, but his emphasis (following on the work of Wilder Penfield) was on what he had learned from electrical stimulation of patients undergoing surgery. Such data still provide a useful complement to current research in neurolinguistics which, despite the subsequent wealth of electroencephalography (EEG), positron emission tomography (PET) and functional magnet resonance imaging (fMRI) data, still lacks a unifying framework long after Lenneberg wrote that

> [...] the exact anatomical substrate for language remains elusive, especially for the cognitive side of language. [...] [T]here do not seem to be sharply delimited or structurally well defined areas that are alone responsible for the appearance of specific clinical language deficits.
>
> (1972: 524)

A few pages later, Lenneberg continues as follows:

> Perhaps the function of [...] large, transcortical, intrahemispheric fiber tracts is more closely related to developmental events and cortical differentiation than to "the sending of messages" between cortical functional "centers." This discussion is particularly relevant to our speculations concerning the function of the arcuate fasciculus. Since there are homologous structures in the chimpanzee brain, it is doubtful that man's capacity for language is in any important way related to the presence of this fiber tract. (1972: 526)

I agree that certain pathways and structures may play their crucial role in development, but disagree that homology disqualifies a tract from playing a distinctive role in language. For example, Rilling et al. (2008) used diffusion tensor imaging (DTI) to chart the very different structure of arcuate fasciculus in macaque, chimpanzee, and human (also see the discussion by Friederici, this issue). My own perspective is that arcuate fasciculus in monkey and chimpanzee serves fine manual motor control; and that its expansion in the human reflects the emergence of fine vocal control.

Returning us to developmental issues, Lenneberg adds that

> [t]hese facts lead to but one conclusion: There is some latitude and individual variation in the way neuroanatomical structures may mediate specific behavior types. These variations are not likely to be random but rather are governed by quite specific principles of the development of brain organization. The principles may have something to do with the concepts of critical age and sensitive period. (1972: 526–527)

3. Some Neuroembryological Principles

Given the lack of data directly linking human brain development to the general time course of language acquisition, it was thought valuable to explore in some detail what general neuroembryology could reveal as to developmental principles of brain organization—even though the behavioral aspects considered were far removed from humans and language. Marcus Jacobson (1972: 528–534) spoke on

"The Problem of Regulation: The Visual System in Amphibians." This has been of interest to me because, from the work of (Willshaw & Malsburg 1979) onward, the formation of retinotopic maps has been a major target for computational modeling as the best worked-out example of "how the brain wires itself up" (see Bednar's chapter in Arbib & Bonaiuto 2016 and Hjorth et al. 2015 for reviews). John Szentágothai (1972: 534–536) discussed "Plasticity in the Central Nervous System: Its Possible Significance in Language Mechanisms." Recent evidence of the role of plasticity in shaping language mechanisms is the finding that the brain of a literate person has distinctive brain regions adapted for reading, the visual word form area, for example. Dehaene & Cohen (2011) support the hypothesis that "reading acquisition partially recycles a cortical territory evolved for object and face recognition, the prior properties of which influenced the form of writing systems." This phenomenon has been the target of modeling by Behrmann & Plaut (2013), but will not be considered further here.

4. Language Properties of Special Interest to Neurobiology

As part of an attempt to understand the relationship between brain and language, the work session sought to assess the relevant properties of language by reviewing attempts (as available in 1972) to provide formal descriptions of grammar.

Terence Langendoen (1972: 537–539) asked "Is the Theory of Generative Grammar Relevant to Neurobiology?" and answered that Chomsky's (1957, 1965) competence theory is not of primary relevance—a conclusion I would agree with even today, despite Chomsky's major recasting of his theories across the decades (Arbib 2012: 47–71): rules of grammar should not be confused with the rules that govern the processes of comprehension and production. Langendoen outlined a long series of developmental stages in speech production that yields a set of highly specific phonological rules, while arguing that one should treat the syllable as the proper unit in phonology, not the phoneme.

Ed Klima and Ursula Bellugi (1972: 539–550) introduced "Language in Another Mode," their research on sign languages of the deaf, especially American Sign Language (ASL) and its acquisition as a first language by deaf children of deaf parents (for an updated overview see Bellugi et al., this issue). Following on Stokoe's argument (1960) that ASL was indeed a language rather than an unstructured collection of gestures, Klima and Bellugi showed that ASL had a grammar that shared certain properties with a spoken language like, for example, English but had certain distinctive properties exploiting the visuo-manual mode of communication, and explored the distinction between ASL and Chinese Sign Language (CSL; Klima & Bellugi 1979 presents the result of this research). (Note that we now accept sign languages as human languages in their own right—the grammar of ASL versus CSL does not reflect the differences between the grammars of spoken American English and Chinese.) Klima and Bellugi note that sign languages differ from spoken languages in the way the hands and face can exploit simultaneity:

> Though sign language may well have its roots in pantomime, it is clear that internal pressures toward systematization have resulted in a reanalysis of what may have been highly iconic into what seems to have become increasingly arbitrary. (1972: 550)

Since 1972, sign languages have offered a strong counter-balance to spoken languages and have suggested to some (but perhaps still a minority of) researchers in language evolution the plausibility of a gestural role in language origins, with the path to speech being indirect (Arbib 2012, Armstrong et al. 1994, Armstrong & Wilcox 2007, Kendon 2002). But crucial data for Lenneberg's concerns come from post-1972 studies on development of gesture in relation to speech and sign (Caselli et al. 2012), sign language aphasia (Poizner et al. 1987) and fMRI studies (Emmorey et al. 2014, Emmorey & Özyürek 2014), to name just a few. Returning to the theme of neural plasticity, important information can be gleaned for how having a signed versus spoken language can differentially remodel the brain (recall the earlier discussion of Dehaene et al. 2010).

5. Language in the Clinic

Here the attempt was to revisit the then century-old study of aphasias from a modern perspective. Simeon Locke (1972: 551–555) gave "A Neurologist's Point of View." He raised two "ambitious, and perhaps unanswerable" questions: (1) What is the evidence that language in the human is "innate" (leaving open the possibility that instead of being preprogrammed, all that is there is the potential, which is then developed)? (2) In what sense is language an extension of preexisting systems, and in what sense does it exploit new principles?

> As the studies of Klima and Bellugi show, language can exist without speech. Speech, as distinguished from the speech act [...], functions at the level of acoustic percepts and can be looked at in the absence of its semantic load. (1972: 552)

Locke reported on comparative analysis of the amount of speech produced in light and dark by schizophrenics, aphasics and dements, concluding that schizophrenics speak far less in the dark, suggesting that for them at least visual perception is a key to language production. In fact, language production has been far less studied than comprehension in neurolinguistics, and I would suggest that we should look at the "action-perception cycle" generally to ground our understanding of how language emerged as a new medium of communication as compared with nonhuman primates.

Roch Lecours (1972: 555-564) took up "Linguistic Analysis of Paraphasias," focusing on the case in which words are transformed by replacement of a single linguistic segment, while considering a range of variations of what might broadly be called Wernicke's aphasia (i.e., with the emphasis on errors of production):

> One might suggest that the semantic incoherence of jargonaphasic material, which sometimes precludes all possibility of linguistic communication with subjects whose productions contain too many compounded and complex verbal transformations, is not necessarily the behavioral manifestation of a conceptual deficit. [...] [It is] the result of an expressive, rather than a conceptual, disorder. To the listener, the sequence is no doubt quite incoherent; but its significance for the speaker's linguistic deficit might indeed be of the same order as that of a mere phonemic paraphasia. (1972: 564)

The general notion here is that by seeing what may go wrong on the path from semantic structure to motor control, one might get a new handle on grammatical structure (cf. Langendoen's observations, informed by study of language acquisition).

6. Development of Motor, Language, and Cognitive Behavior

Building on Langendoen's work, there were five presentations on various forms of development in humans. Tom Twitchell (1972: 565-569) integrated this with brain damage in "Development of Motor Coordination in the Presence of Cerebral Lesions," demonstrating that it is easier to see how labyrinthine and proprioceptive mechanisms related to body posture and simple movements are acting than to envisage the role of cutaneous sensory-motor mechanisms in motor function in health and disease. Primitive ballistic movements of, for example, manual control provide the necessary interactions from which coordinated behavior can emerge. (Oztop et al. 2004 offered a computational model for how, over perhaps 9 months, the grasp reflex can be replaced by visually guided preshaping of the hand during reach.)

> If one looks at the development of motor function following cerebral lesion at birth, the story is one of gradual improvement rather than gradual development of abnormalities. Often the improvement in function is so small over the life of the patient that it is of no use to him. One may see improvement occurring (provided it is not a learned type of utilization of the residual mechanism) in the same manner as in the normal infant. (1972: 569)

Lenneberg notes

> [. . .] that synergisms for sucking, mastication, and deglutition are physiologically separable from synergisms for speech articulation; the latter are later in development than the former. In abnormal development one may find normal anatomy and movements of oropharyngeal structures; the young patient has no difficulty in negotiating liquid or solid food.
>
> (1972: 569)

He then calls for studies akin to Twitchell's on motor control. (Interestingly, Lenneberg had at that time already sent off one of his graduate students to study the development of motor control, cf. Cohen, this issue).

Colwyn Trevarthen (1972: 570–585) offers an extended account of "The Psychobiology of Speech Development" in the only part of the Bulletin to be widely cited (over 100 times). He also discusses his commisurectomy studies with Roger Sperry. A diversity of studies with infants attest to his thesis that

> [t]he mechanism of intelligent action, employing a number of aimable and tunable receptors in unison for detection of structure in the world, is one in which a common space context, defined in innately wired circuits of the brain to represent the body's field of action, is used to obtain selective foci. [. . .] Separate focalizing and information-analyzing organs

> like eyes, hands, and mouth can pursue separate local goals individually, but all are periodically brought together and united in a common orienting field of behavior that shares one time base for its actions. This basic structure could not have been put together by learning or any process of association of initially free and independent parts. (1972: 570)

This then places language development firmly within the context of the child's interaction with the world. He discusses visual space in the neonate, voluntary reaching for objects, and (moving toward communication) prespeech in neonates to show that

> [...] the roots of social communication of experience and intention are well established at birth and that they begin to function before inanimate objects are perceived or used effectively. (1972: 579)

Prespeech is

> [...] a highly specific, quieter follow-up of the smile in which the baby, with gaze somewhat defocused from the mother's eye and often unsmiling, performs elaborate lip and tongue movements, sometimes with compressed breath. (1972: 581)

Trevarthen found

> [...] abundant evidence that speech has evolved out of the need for communication of intention and experience between beings of like psychological organization, with the same manner and cadence of attending to the events in the common environment, yet each with his own unique point of view on the world. (1972: 585)

This concept of intersubjectivity draws our attention to locate language within a context of social interaction. In recent years, Trevarthen has extended his study of intersubjectivity to include musicality (Malloch & Trevarthen 2009); whereas the relation between language and music poses a continuing challenge (Arbib 2013).

Tom Bever (1972: 585–588) addresses "The Relation of Language Development to Cognitive Development," starting by noting there are different types of linguistic knowledge such as grammatical knowledge, understanding of language, and speaking—and they interact. How do children perceive sequences of words? Bever demonstrates this by showing how at age two they have a stereotyped way of interpreting sentences that supports comprehension of parts of some sentences but not others, and observes how this changes by age four. The topic of language acquisition remains highly pertinent. A strict reliance on an innate Universal Grammar (e.g., Lightfoot 2006) seems mistaken (Arbib 2007), but computational modeling in concert with large databases seems to show real progress (Chang 2015, MacWhinney 2014).

Jonas Langer (1972: 588-596) discussed "The Ontogeny of Cognitive Functions." He posited three stages for the development of conceptual intelligence: presentational transformation upon objects, representational transformation upon objects, and operational transformation and assessed advances in symbolic systems of action that accompany conceptual progress. He argued that the evidence, though incomplete, seems to support the

> [. . .] working hypotheses that: (1) Conceptual activity defines the pace or sets the limits for symbolic development. (2) Conceptual development cannot be outstripped by symbolic development. [. . .] [He] summarized cognitive development as a shift from interaction and concern [. . .] with actual phenomena or objects located in the environment to a consideration [. . .] of possible phenomena located in his own thoughts. The character of the neural substrates that are necessary for such a shift is not at all clear. It seems reasonable to suggest, however, that such a cognitive shift is unlikely without a parallel inhibition of neural interaction with concrete external events accompanied by a relative activation of intraneural interaction. (1972: 595–596)

One may note here the concern with "mental time travel" that moves language and thought beyond the here-and-now (Suddendorf & Corballis 1997, Suddendorf & Corballis 2007)—but much remains to be done to secure the neural foundations for this expansion of cognitive perspective.

7. Neurobiological Aspects of Animal Communication

Three of the last four presentations asked what could be learned from the study of songbirds. In "Cerebral Lateralization in Birds" Fernando Nottebohm (1972: 597–602) discussed his seminal work on "Central Neural Mechanisms of Vocalizations in Birds," Juan Delius (1972: 602–607) reviewed "Central Neural Mechanisms of Vocalizations in Birds," while Mark Konishi explored "The Development of Auditory Sensitivity in Relation to Mother-Young Vocal Communication in Birds" (possibly an echo of Trevarthen's intersubjectivity?). The study of birdsong and its development, and speculation as to its relation to language, has been very active since these pioneering contributions and so I simply provide a few references to recent research (Berwick et al. 2011, Bolhuis & Everaert 2013, Fitch & Jarvis 2013, Petkov & Jarvis 2012). Since birdsong lacks semantics, it does not provide a model for the "compositional semantics" that language provides; it may better be regarded as exhibiting something akin to a "phonological syntax" (Yip 2010).

Finally, Detlev Ploog returned discussion to primates with his analysis of "Phylogenetic and Ontogenetic Aspects of Vocal Behavior in Squirrel Monkeys." An updated review is provided by (Jürgens 2002, 2009), whereas Aboitiz has placed monkey vocalization and human language within an evolutionary framework, while noting that voice and manual gesture may have played complementary roles in the evolution of the human language-ready brain (Aboitiz 2012, 2017). Thus, the study of ape gesture may be a valuable bridge in assessing how language evolved (Arbib et al. 2008, Rossano & Liebal 2014, Slocombe et al. 2011).

8. Epilogue

Lenneberg's epilogue to the collected presentations is divided into ten sections. Here are the key points that Lenneberg made in each section.

8.1. Language, Speech, and Speakers

Lenneberg states that

> [l]anguage is a system or a "structure" of relations. Relational features pervade every aspect—semantics, syntax, and phonology. [...] [A]nd the reality of these points was driven home to engineers in recent years when they discovered that no mechanical device can recognize speech sound patterns (or syntactic structures) unless it is capable of making elaborate computations of relations. (1972: 619)

A few years later, HEARSAY provided an example of a speech understanding system employing cooperative computation across the levels (Erman et al. 1980, Lesser et al. 1975); Arbib and Caplan (1979) offered initial ideas on the relevance of this to neurolinguistics.

Lenneberg then points out that

> [i]n order to assign phonemic values to sounds, the object [i.e. biological organism] must "compute" relationships between given acoustic heterogeneities [...]. Syntactic and semantic relationships [...] are "computed" in intimate connection with the organism's cognitive activities (including perception, memory, recognition—in short, knowledge) concerning the world around it. The computation of relationships is involved when relationships in the environment are assessed or verified, or when a speaker issues a statement or makes an assertion about relationships that supposedly hold for certain conditions. (1972: 619)

He continues by saying that

> [t]hese considerations should make it clear that language reception and language production are both dependent upon a common, unified mechanism [...]. (1972: 619)

Perhaps this is misleading. Thus, where Lenneberg asks "Where is language located, whatever it may turn out to be?" (p. 620), I would rather speak of "orchestrating" diverse mechanisms for production and comprehension, while noting that learning may have altered not only these mechanisms but their patterns of interaction with others (recall the earlier discussion of reading).

Next, Lenneberg goes on by discussing the explanatory value of association. He remarks that

> [f]or about a century we have been satisfied with the use of association as an explanatory concept. This has engendered the false impression that language knowledge is simply a collection of associative bonds that may be established within the organism by manipulations from the outside and, therefore, that language knowledge may be introduced into a basically passive recipient by any clever trainer. Linguists, especially Chomsky (1957) and his students, have argued compellingly against this view. Observations on clinical language disorders as well as on the normal ontogeny of language development similarly militate against the idea that language consists of immutable items (such as fixed associations) that are either present in an organism or not. (1972: 621)

The modern challenge, also discussed by Trettenbrein and Chomsky (this issue), then is to assess what notion of grammar may best support a study of language use—with perception and production providing occasions for learning and themselves changing (and changing in their relation to action, perception and cognitive processes) in the process. My own vote is to seek a neurologically constrained variant of construction grammar in which meaning and form are intertwined in each construction. Moreover, when we take a performance view, the elements of grammar may constitute dynamic schemas which differ between those available for comprehension and those for perception.

8.2. Language Development

Lenneberg starts out by saying that

> [t]he general trend [of language development] is always from a global, undifferentiated whole towards greater and greater specificity. One is reminded of Coghill's (1930) concept of individuation in the development of motor coordination, which has been mentioned by Twitchell.
>
> (1972: 621)

This sets the stage for what may be read as Lenneberg's manifesto for the work session's overall theme of "Language and Brain: Developmental Aspects:"

> The phonemic structure develops as a system of contrasts and relations (as Jakobson, 1941, showed [...])—not as a linear augmentation of a speech sound repertoire. In syntax, the first joining of words represents an undifferentiated, primitive predicative relation; one word seems to be a comment on the other, but at first it is often not clear which of the words is the topic and which the comment. As sentences increase in length and syntactic complexity, the syntactic relations become more specialized and differentiated; words begin to function more and more clearly as specific syntactic categories. Likewise, in the realm of semantics, the child's usage of words is at first global and, to the adult observer, sloppy. Gradually the primitive semantic fields contract, and more and more precise semantic relationships emerge. This general trend suggests that the underlying neuronal activities responsible for language go through a developmental history themselves, starting from a maturationally undifferentiated stage and moving towards an ever-increasing degree of specialization and differentiation. (1972: 622)

He goes on to say that

> [w]hat is maturing is a capacity for computing special kinds of relations and relations between these relations. (1972: 623)

8.3. Biological Aspects of Language Development

Here, Lenneberg starts by saying that

> [. . .] one of the most general principles of embryogenesis may also be active in the specific (biological) maturational processes underlying language development. I should like to call this principle morphogeny, the embryological trend of local accumulation of anatomical structure and of physiological order. Developing organisms are morphogenic systems. (1972: 623)

(Compare his note on Waddington on his page 631.)

> But perhaps it is not quite so obvious that the development of behavior is actually an extension of embryogenesis. [. . .] An animal's behavior is but the outward manifestation of physiological and anatomical interactions under the impact of environmental stimulations. (1972: 624)

This integration of function and development sits well with the notion of evo-devo (also discussed by Piattelli-Palmarini in this issue), that evolution yields developmental systems rather than directly specifying adult forms. Extending Trevarthen's intersubjectivity, we may now place increasing emphasis on evo-devo-socio—since human language is indeed a primary shaper and shapee of sociocultural evolution.

Next, Lenneberg takes up the now familiar distinction between internal and external language:

> [. . .] [L]anguage may develop at the normal chronological age in children with peripheral handicaps that block the normal outward expression of language. Studies have been made of several children with motor deficits due to fixed CNS [central nervous system] lesions who have never been able to make speech sounds but who have developed language knowledge normally by listening to and interacting with speaking adults. This illustrates that the "mental" exercise of language may be distinguished from the physical output of language, i.e., from speech [. . .] (1972: 625)

Lastly, he concludes the section by noting that

> The idea that the exercise of language itself contributes to and alters the structural complexity of the language system is, incidentally, argument against regarding the brain as a finite-state computer. Though the argument here is based on different considerations from those used by Chomsky (1957), the conclusion is the same as his. (1972: 625)

But such agreement does not endorse a competence theory of generative grammar as the "solution." A rich learning theory of the child's language use while interacting with the physical and social environment is the goal of Lenneberg's enterprise.

8.4. Maturation of the CNS and Language

Here, Lenneberg's key point is that

> [. . .] normal histological and functional development of the mammalian brain is dependent both on the availability of certain sensory stimulation and on the opportunity or processing such information and engaging in correlated behavior. The meager evidence concerning language

> capability in criminally deprived (or wolf-)children seems to support the view that man, too, must be exposed to language during the last stages of brain development or else suffer irreversible language deficits (Lenneberg, 1967). The experiments reported by Szentágothai stress the importance of the interaction between environmental input and maturationally developing patterns, especially of synaptic arrangements. [...] The most dramatic maturational event of clearcut relevance to language is the hemispheric specialization for language that occur largely during the most active period of language acquisition. (1972: 626)

8.5. *The Localization Issue*

Lenneberg already provided a modern way of thinking about the question of localization of language functions. He says:

> In my opinion [...] the cortical loci in question gradually come to be specialized [...] to contribute in a particular physiological manner to those activity patterns that form the neuronal basis or behavior [...]. Behavior such as language is not the product of one particular spot either in the cortex or in any of the subcortical nuclei. Very many parts of the brain must contribute to the proper function or a behavior that is as inseparable from perception, memory, concept formation, and every other cognitive process as is language. (1972: 627)

And, we may add, the different motor demands of spoken and signed language. Indeed, Lenneberg continues with examples from motor coordination and its impairment.

Lenneberg rejects the

> [...] notion of centers with principal control over any particular kind of circumscribed behavior. The brain is not a loose aggregate of autonomous organs, but a single organ (Locke and Trevarthen, this Bulletin). Its anatomical subdivisions undoubtedly have their own specific physiological functions, contributing to various types of behavior in different ways. But, so far we know of no behavioral entity that is the exclusive product of just one brain region alone. (1972: 628–629)

Again, I would rather speak of distributed or cooperative computation across diverse subsystems rather than speak of "a single organ," since this draws attention from the way the overall system changes with development and can reconfigure itself to meet the needs of different situations. I think Lenneberg would be content with this reformulation.

8.6. *Ontogeny of Behavior*

Here, Lenneberg says the following:

> Denny-Brown's concept of amorphosynthesis (Denny-Brown et al., 1952) foreshadowed a new interpretation of the interrelation between the two hemispheres, which has been most lucidly presented by (Kinsbourne,

> 1974). [. . .] He postulates, as Denny-Brown had done before him, a state of functional balance between the hemispheres that continues throughout life, even though the compensatory capacities are lost at the termination of the maturational phase of the brain. [. . .] [But] differentiation that takes place between birth and adolescence and that results in hemispheric functional specialization does not turn one side of the brain into an independent language organ. It causes physiological functions to be more selectively distributed between the left and the right side; it does not abolish all interdependence. [. . .] Twitchell's and Trevarthen's presentations [. . .] [illustrate] how original primitive reflexes (or, perhaps better, "prototype acts") gradually come to be transformed into effective motor patterns, resulting in new capabilities in motor control. As brain tissues become more and more differentiated, they [. . .] transform the previous types of functions into newly integrated patterns. (1972: 630)

8.7. The Embryological Concept of Regulation Applied to Language Development

Lenneberg begins this section by saying that

> [t]he compensatory powers of the infantile human brain with respect to language-relevant brain regions may be [. . .] [an] example of [. . .] [the] regulatory phase of ernbryogenesis. When this period comes to a close, the second phase, one speaks of determination of tissues. [. . .] [T]he organism can no longer rearrange itself to reconstitute the original manner of function or shape. However, even then there are mechanisms in certain animals that may compensate for loss. Regeneration is one such mechanism, though it is irrelevant to language and its biological substrate. (1972: 639)

Given the discovery of formation of new neurons in adult hippocampus, it may be time to see to what extent this may provide new mechanisms for language plasticity even long after key "critical periods" have long since passed.

8.8. Stability of the Course of Development

Here, Lenneberg says that

> [t]he tendency for a growing organism to follow a fairly narrowly defined trajectory of development and to be able to return to that trajectory even if it is temporarily deviated from its prescribed path has [. . .] been the subject of much discussion in theoretical biology, particularly by Waddington, who refers to the phenomenon as homeorhesis, and by the biologists who have joined him in a series of study groups (Waddington, 1968, 1969, 1970, 1972) [of which I (MAA) was one]. Formative principles of this sort are by no means restricted to biological growth. [. . .] There is no way for a system to reach any one given stage without having first traversed the stages that led to it. [. . .] [But] there are also examples in the experimental animal literature and in human embryology in which fetal or perinatal lesions seem at first to leave the infant

> unharmed but produce symptoms at a later stage of development. [. . .] At first these findings may appear to be paradoxical. However, we may yet learn to fit them into a consistent model if we try to explain them in terms of morphogeny and homeorhesis. In cases where the animal preparation or the patient gradually grows into the symptoms, the developmental preconditions for future pattern formation have been interfered with. (1972: 631–632)

He continues as follows:

> It is in these terms that we should also look at language development. [. . .] One of the most intriguing aspects of comparisons between human language and animal communication is precisely the various courses of development of these species-specific behavior types. The work of Konishi and of Nottebohm is especially a propos here. (1972: 632)

8.9. Brain Stimulation Data

Regarding the means for studying the neural substrates of language processing at the time Lenneberg says that

> [o]ur theories for brain mechanisms of language are based, essentially, on two types of evidence: electrical brain stimulation in unanesthetized man, as discussed by Schaltenbrand, and postmortem examination of brain lesions. (1972: 633)

Here, as noted earlier, the relevant database has expanded dramatically (or even overwhelmingly?) to include a wealth of data gained used EEG, PET, fMRI, and other techniques, and these are complemented by comparative neurobiology with other primates as well as further developments in the study of birdsong. Very much as Lenneberg notes with reference to Ploog's presentation, there are

> fundamental, physiological differences between brain correlates of vocalization in man and in so-called lower primates, as well as important similarities and homologies relevant to the correlates of vocalization.
>
> (1972: 634)

Aboitiz (2017) offers a perspective on the evolution of brain mechanisms for speech rooted in comparative neuroanatomy of monkeys and humans; see also my call for a (computational) comparative neuroprimatology (Arbib 2016).

8.10. Clinico-Pathological Correlations

Lenneberg turns finally, to the "morbid anatomy" giving rise to language disturbances and summarizes

> the consensus of the neurologists present [at the Workshop] as follows. There is no lesion that is capable of totally abolishing language capabilities (more specifically, language knowledge) without at the same time interfering dramatically with many other cognitive functions. Clinical

> speech and language disorders that are acquired in adult life present as both reduction and distortion of normal language patterns. [. . .] Perhaps the most important suggestion for future work in the area of aphasia that issued from the interaction with developmental biologists at this Work Session is the analysis of language disorders in their developmental perspective: the consequence of lesions in childhood and the course and progress in the formation of symptoms or recovery from symptoms.
>
> (1972: 635)

9. Conclusion

Lenneberg stated that at the time of the Work Session, there was

> only one type of scientific theory of language structure available, generative grammar, and this was never intended to serve as a model for biological language mechanisms. [. . .] What is most urgently needed is a theory of sentence production and comprehension that has the formal precision of Chomsky's approach but is explicitly intended to explicate the psychobiological underpinnings of language capabilities.
>
> (1972: 635)

Elsewhere, I have argued that computational construction grammar may provide such a framework, while stressing that the very diversity of formulations for such a grammar requires a synthesis of the lessons to be learnt from diverse approaches such as those of Embodied Construction Grammar (Bergen & Chang 2005, Feldman 2010), Template Construction Grammar (Arbib 2017, Arbib & Lee 2008), Fluid Construction Grammar (Steels 2004, Beuls & Steels 2013), and Dynamic Construction Grammar (Hinaut et al. 2015). However, what is striking about Lenneberg's Work Session is the relative unimportance of linguistic analysis and the emphasis on seeking insights from neuroembryology and from the development—and disorders—of motor and perceptual skills and the diverse breakdowns of language function in aphasia. These insights—and those that have extended them in the last 45 years—remain crucial, even as we seek new computational models of language comprehension and production and link them to new sources of data on the human brain and the comparative study of other species.

Appendix

The Supplementary Material contains scans of the original article Lenneberg (1972), prefaced with a republication note that also addresses the copyright situation. It is available for download here: `https://biolinguistics.eu/index.php/biolinguistics/article/downloadSuppFile/507/84`

References

Aboitiz, F. 2012. Gestures, vocalizations and memory in language origins. *Frontiers in Evolutionary Neuroscience*, 4(2). doi:10.3389/fnevo.2012.00002.

Aboitiz, F. 2017. *A Brain for Speech: A View from Evolutionary Neuroanatomy*. Basingstoke, UK: Palgrave Macmillan.

Adelman, G. 2010. The Neurosciences Research Program at MIT and the Beginning of the Modern Field of Neuroscience. *Journal of the History of the Neurosciences*, 19(1), 15–23.

Arbib, M. A. 2007. *How New Languages Emerge (Review of D. Lightfoot, 2006,* How New Languages Emerge*, Cambridge University Press).* https://linguistlist.org/issues/18/18-432.html (Date accessed).

Arbib, M. A. 2012. *How the Brain Got Language: The Mirror System Hypothesis*. New York & Oxford: Oxford University Press.

Arbib, M. A. 2013. Five terms in search of a synthesis. In M. A. Arbib (ed.), *Language, Music, and the Brain: A Mysterious Relationship*. Strüngmann Forum Reports, vol. 10. Cambridge, MA: MIT Press, 3–44.

Arbib, M. A. 2016. Towards a computational comparative neuroprimatology: Framing the language-ready brain. *Physics of Life Reviews*, 16, 1–54.

Arbib, M. A. 2017. Dorsal and ventral streams in the evolution of the language-ready brain: Linking language to the world. *Journal of Neurolinguistics*, 43, 228–253. doi:10.1016/j.jneuroling.2016.12.003

Arbib, M. A. & J. J. Bonaiuto (eds.). 2016. *From Neuron to Cognition via Computational Neuroscience*. Cambridge, MA: MIT Press.

Arbib, M. A. & D. Caplan. 1979. Neurolinguistics must be computational. *Behavioral and Brain Sciences*, 2, 449–483.

Arbib, M. A. & J. Y. Lee. 2008. Describing visual scenes: Towards a neurolinguistics based on construction grammar. *Brain Research*, 1225, 146–162.

Arbib, M. A., K. Liebal & S. Pika. 2008. Primate vocalization, gesture, and the evolution of human language. *Current Anthropology*, 49(6), 1053–1076.

Armstrong, D. F., W. C. Stokoe & S. E. Wilcox. 1994. Signs of the origin of syntax. *Current Anthropology*, 35(4), 349–368.

Armstrong, D. F. & S. E. Wilcox. 2007. *The Gestural Origin of Language*. Oxford, United Kingdom: Oxford University Press.

Behrmann, M. & D. C. Plaut. 2013. Distributed circuits, not circumscribed centers, mediate visual recognition. *Trends in Cognitive Sciences*, 17(5), 210–219.

Bergen, B. K. & N. Chang. 2005. Embodied Construction Grammar in simulation-based language understanding, Construction Grammars: Cognitive Grounding and Theoretical Extensions.

Berwick, R. C., K. Okanoya, G. J. L. Beckers & J. J. Bolhuis. 2011. Songs to syntax: the linguistics of birdsong. *Trends in Cognitive Sciences*, 15(3), 113–121.

Beuls, K. & L. Steels. 2013. Agent-based models of strategies for the emergence and evolution of grammatical agreement. *PloS One*, 8(3). doi:10.1371/journal.pone.0058960.

Bolhuis, J. J. & M. Everaert. 2013. *Birdsong, Speech, and Language: Exploring the Evolution of Mind and Brain*. Cambridge, MA: MIT Press.

Caselli, M. C., R. Rinaldi, S. Stefanini & V. Volterra. 2012. Early action and gesture "vocabulary" and its relation with word comprehension and production. *Child Development*, 83(2), 526–542.

Chang, F. 2015. The role of learning in theories of English and Japanese sentence processing. In M. Nakyama (ed.), *Handbook of Japanese Psycholinguistics*. Boston, MA: de Gruyter, 353–385.

Chomsky, N. 1957. *Syntactic Structures*. The Hague: Mouton.

Chomsky, N. 1965. *Aspects of the Theory of Syntax*. Cambridge, MA: MIT Press.

Coghill, G. E. 1930. Individuation versus integration in the development of behavior. *The Journal of General Psychology*, 3, 431–435.

Dehaene, S. & L. Cohen. 2011. The unique role of the visual word form area in reading. *Trends in Cognitive Sciences*, 15(6), 254–262.

Dehaene, S., F. Pegado, L. W. Braga, P. Ventura, G. N. Filho, A. Jobert, G. Dehaene-Lambertz, R. Kolinsky, J. Morais & L. Cohen. 2010. How learning to read changes the cortical networks for vision and language. *Science*, 330(6009), 1359–1364.

Denny-Brown, D., J. S. Meyer & S. Horenstein. 1952. The significance of perceptual rivalry resulting from parietal lesion. *Brain & Language*, 75, 433–471.

Emmorey, K., S. McCullough, S. Mehta & T. J. Grabowski. 2014. How sensory-motor systems impact the neural organization for language: direct contrasts between spoken and signed language.

Emmorey, K. & A. Ozyurek. 2014. Language in our hands: Neural underpinnings of sign language and co-speech gesture. In Gazzaniga, M. (ed.), *The cognitive neurosciences*. Cambridge, MA: MIT Press, 657–666.

Friederic, A. D. 2017 [= this issue]. Neurobiology of syntax as the core of human language. *Biolinguistics* 11(2).

Erman, L. D., F. Hayes-Roth, V. R. Lesser & D. R. Reddy. 1980. The HEARSAY-II speech understanding system: Integrating knowledge to resolve uncertainty. *Computing Surveys*, 12, 213–253.

Feldman, J. 2010. Embodied language, best-fit analysis, and formal compositionality. *Physics of Life Reviews*, 7(4), 385–410.

Fitch, W. T. & E. D. Jarvis. 2013. Birdsong and other animal models for human speech, song, and vocal learning. In M. A. Arbib (ed.), *Language, Music and the Brain: A Mysterious Relationship*. Strüngmann Forum Reports, vol. 10. Cambridge, MA: MIT Press, 499–539.

Hinaut, X., F. Lance, C. Droin, M. Petit, G. Pointeau & P. F. Dominey. 2015. Corticostriatal response selection in sentence production: Insights from neural network simulation with reservoir computing. *Brain & Language*, 150, 54–68.

Hjorth, J. J. J., D. C. Sterratt, C. S: Cutts, D. J. Willshaw & S. J. Eglen. 2015. Quantitative assessment of computational models for retinotopic map formation. *Developmental Neurobiology*, 75(6), 641–666.

Jakobson, R. 1941. *Kindersprache, Aphasie und allgemeine Lautgesetze*. Uppsala: Almquist & Wiksell.

Jürgens, U. 2002. Neural pathways underlying vocal control. *Neuroscience and Biobehavioral Reviews*, 26(2), 235–258.

Jürgens, U. 2009. The neural control of vocalization in mammals: a review. *Journal of Voice*, 23, 1–10.

Kendon, A. 2002. Historical observations on the relationship between research on sign languages and language origins theory. In D. Armstrong, M. Karchmer & J. V. V. Cleeve (eds.), *The Study of Sign Languages: Essays in Honor of William C. Stokoe*, Washington, DC: Gallaudet University Press, 35–52.

Kinsbourne, M. 1974. Mechanisms of hemisphere interaction in man. In M. Kinsbourne & L. Smith (eds.), *Hemispheric Disconnection and Cerebral Function*. Springfield, IL: Thomas, 71–96.

Klima, E. & U. Bellugi. 1979. *The Signs of Language*. Cambridge, MA: Harvard University Press.

Lenneberg, E. H. 1962. Understanding language without ability to speak: A case report. *Journal of Abnormal and Clinical Psychology*, 65, 419–425.

Lenneberg, E. H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Lenneberg, E. H. 1972. Language and brain: Developmental aspects. A report based on an NRP Work Session held November 19-21, 1972, and updated by participants. *Neurosciences Research Program Bulletin*, 12(4), 511–656.

Lesser, V. R., R. D. Fennel, L. D. Erman & D. R. Reddy. 1975. Organization of the HEARSAY-II speech understanding system. *IEEE Transactions on Acoustics, Speech, and Signal Processing*, 23(1), 11–24.

Lightfoot, D. W. 2006. *How New Languages Emerge*. Cambridge: Cambridge University Press.

MacWhinney, B. 2014. Item-based patterns in early syntactic development. In T. Herbst, H.-J. Schmid & S. Faulhaber (eds.), *Constructions Collocations Patterns*. Location: de Gruyter, 33–69.

Malloch, S. & C. Trevarthen. 2009. *Communicative Musicality: Exploring the Basis of Human Companionship*. Oxford, United Kingdom: Oxford University Press.

Oztop, E., N. S. Bradley & M. A. Arbib. 2004. Infant grasp learning: a computational model. *Experimental Brain Research*, 158(4), 480–503.

Petkov, C. I. & E. Jarvis. 2012. Birds, primates and spoken language origins: Behavioral phenotypes and neurobiological substrates. *Frontiers in Evolutionary Neuroscience*, 4. doi:10.3389/fnevo.2012.00012.

Poizner, H., E. Klima & U. Bellugi. 1987. *What the Hands Reveal About the Brain*. Cambridge, MA: MIT Press.

Rilling, J. K., M. F. Glasser, T. M. Preuss, X. Ma, T. Zhao, X. Hu & T. E. Behrens. 2008. The evolution of the arcuate fasciculus revealed with comparative DTI. *Nature Neuroscience*, 11(4), 426–428.

Rossano, F. & K. Liebal. 2014. "Requests" and "offers" in orangutans and human infants. In P. Drew & E. Couper-Kuhlen (eds.), *Requesting in Social Interaction*. Amsterdam, The Netherlands: John Benjamins, 335–363.

Searle, J. R. 1969. *Speech Acts: An essay in the Philosophy of Language*. Cambridge, United Kingdom: Cambridge University Press.

Slocombe, K. E., B. M. Waller & K. Liebal. 2011. The language void: the need for multimodality in primate communication research. *Animal Behaviour*, 81(5), 919–924.

Steels, L. 2004. *Fluid Construction Grammars: A brief Tutorial*.

Stokoe, W. C. 1960. *Sign Language Structure: An Outline of the Visual Communication Systems of the American Deaf*. Buffalo, NY: University of Buffalo.

Suddendorf, T. & M. C. Corballis. 1997. Mental time travel and the evolution of the human mind. *Genetic Social and General Psychology Monographs*, 123(2), 133–67.

Suddendorf, T. & M. C. Corballis. 2007. The evolution of foresight: What is mental time travel, and is it unique to humans? *Behavioral and Brain Sciences*, 30, 299–351.

Waddington, C. H. (ed.), (1968-1972) Towards a Theoretical Biology, 4 volumes (An International Union of Biological Sciences Symposium). Edinburgh: Edinburgh University Press.

Willshaw, D. J. & C. von der Malsburg. 1979. A marker induction mechanism for the establishment of ordered neural mappings: its application to the retinotectal problem. *Philosophical Transactions of the Royal Society of London. B, Biological Sciences*, 287(1021), 203–243.

Yip, M. J. 2010. Structure in human phonology and in birdsong: A phonologists's perspective. In J. J. Bolhuis & M. Everaert (eds.), *Birdsong, Speech and Language. Converging Mechanisms*. Cambridge, MA: MIT Press.

Michael A. Arbib
University of Southern California
Department
Street name
City
United States of America

arbib@usc.edu

Eric Lenneberg and Motor Control

Avis H. Cohen

1. Preamble

I began my graduate career in 1970. I was somewhat familiar with Eric Lenneberg, having met him during an event for faculty in Psychology and Neuroscience—the fields in which I was interested at the time. He had just arrived at Cornell, as had I, and he didn't have many other graduate students at that time. I chose him as my graduate faculty advisor. He directed me toward the study of the development of motor control, one of his fields of interest (cf. Lenneberg's classic, *Biological Foundations of Language*, 1967). His other students were urged to study the development of language, in which he was most well known. These students went with Eric to New York to study patients with aphasia, while I stayed behind at Cornell in Ithaca, with my young children. That ended up suiting me well!

When I began graduate school, I was unsure of the direction or level I wished to attain. This was the 1960s and women were not particularly accustomed to graduate school or aiming high, especially if already married with children, which I was. My husband was a faculty member in the Cornell Mathematics Department, and our children were quite young: one was six and one was four. Eric Lenneberg, who had just begun his time as a faculty member at Cornell University, had participated in a forum I organized for theoreticians of science, and was the only faculty member of neuroscience I knew at all well, since he had participated in the forum.

My thesis, when finally completed also included results of a project done after Eric's death with Professors Carl Gans, University of Michigan, and Farish Jenkins, Harvard University, on rat muscle activity during running. Both sets of results were integrated into my dissertation on rat locomotion, unfortunately, with Professor Gans as my advisor and without Eric on my committee.

As a post-doctoral fellow, I remained at Cornell for a few years with funding from a National Institutes of Helath (NIH) grant, which fortunately, I was able to obtain independently. At that point I also became interested in mathematical modeling of the phenomena on which I was working, another area that Eric had urged me toward and about which he was enthusiastic. This resulted in my most cited publication: Cohen, Holmes & Rand (1982). It has been perhaps my most important publication and the fact that it was and still is being widely cited is a testament to its importance in establishing theoretical neuroscience.

After this work, which was completed early with two mathematical colleagues, Philip Holmes and Richard Rand, both professors at Cornell at that time, I continued doing research in my own laboratory, also at Cornell. I chose the detailed study

biolinguistics

ISSN 1450–3417
Biolinguistics xx: 481–485, 2017
http://www.biolinguistics.eu

of the production of spinal waves of neural activity as seen in fish swimming as my topic, employing electrophysiological techniques I had learned from Sten Grillner in Sweden while doing post-doctoral work there immediately after completing my thesis. The experimental model I kept relying on I had chosen as a post-doctoral student in Grillner's laboratory prior to returning to Cornell, namely the lamprey spinal cord (Cohen & Wallén 1980). I continued modeling with Philip Holmes and another former Cornell former graduate student, Tim Kiemel. It is this field in which I worked happily and productively until my retirement in August 2013.

You might wonder what the link was between the development of language and motor control—and fish swimming? In fact, the link is very complex, but clear. Speech production is a motor process, albeit difficult to study in precise detail, but interesting nonetheless. For example, apraxia of speech and some forms of aphasia are conditions which rob you of the ability to communicate, to speak, or even use language. They can affect your ability to speak, write, and understand language, both verbally and/or in its written form. This can occur either following a stroke or head injury, as well as following other injuries that impact the parts of the brain that are involved in speech production and language processing. Aphasia is field of study that has a long history, almost as long as the field of speech production if not longer. Not being an expert, myself, I leave that to others to elaborate. My task here is to elaborate a bit on the field of motor control, how Eric led me to it, to what purpose and to what effect.

2. How did Eric lead me to motor control?

Eric suggested that the area of motor control was often neglected, but was extremely important, as it was foundational of all activities, such as speech production and any other type of movement. He would be unable to assist me very much, but would do all he could. I also found that others in the department could be helpful. So I was off and running on motor control! Unfortunately, it was shortly after this decision, that Eric died, so he did not accompany me on this part of the journey.

Shortly after starting graduate school, and after Eric's death, I went with my husband to University of Michigan for the semester, where I found Dr. Carl Gans. He helped me move ahead on my dissertation work with some experiments on rats (Cohen & Gans 1975), plus he really enabled me to dig deeply into motor control, since that was his area of specialization. Around that time, I also took a short trip to Harvard to allow me to incorporate into my thesis work, X-ray cinematography with Professor Farish Jenkins. For this I put white rats into a specially redesigned running wheel with a plastic side wall to allow the X-ray beams to reach the running rats. The contraption worked well, and we made some great images of running rat skeletons (Cohen 1979).

As a post-doctoral fellow at Cornell I had one other diversion. This one took me toward mathematical description of my work, or theoretical neuroscience, another area that had been important to Eric and to which he would have been happy I embarked upon. Fortunately, I had close ties to the Department of Mathematics, as my husband is a mathematician (Marshall Cohen, a topologist at Cornell in those years). A couple of his colleagues, Philip Holmes and Richard Rand worked with me to perform a theoretical dynamical systems analysis of rhythmic movements such as locomotion or swimming. This resulted in perhaps my most important, and certainly my most cited paper, already mentioned above, co-authored with Philip Holmes and Richard Rand who were in Theoretical and Applied Mechanics at Cornell at that time (Cohen, Holmes, & Rand 1982). The paper also helped one to understand the movement that I was beginning to study: swimming of a very primitive fish-like vertebrate, the lamprey, which is the animal at the base of the vertebrate tree. It lacks bones and cartilage, the latter only appears later in sharks.

3. What is motor control and why is it important?

Motor control has been defined in many ways. My definition: motor control is the means by which organisms coordinate and activate their muscles to perform any movement . This requires the integration of sensory information from the body of the organism as well as with its environment. The organism must determine the appropriate set of muscle forces to generate the desired movement or action. To do this, there must be cooperative interaction between the central nervous system and the musculoskeletal system. Thus, sensory input must be available to provide the current status of the organism in its environment; it must also be able to provide the impact of any action that is performed. The motor system must be able to utilize this information, and respond appropriately.

4. Where it all led me and how it linked back to the theory of motor control

My thesis from Cornell was a variation on a project begun by Roger Sperry in 1942 (Sperry 1945a, b). He cross innervated muscles to determine if they maintain their functional identity if connected to a new functional destination. That is, if the nerve to a flexor is connected to an extensor, will it continue to activate the muscle at the same time as it did originally. Sperry did it after removing all the other neighboring muscles and their nerves, thereby removing the possibility of their being feedback regarding how their activity is stretching and contracting as compared to the normal activity. I did the same thing, but left all other innervation intact, including the sensory nerves to muscle. This had the disadvantage that if I wanted to know when the muscle was activated, I was required to record its activity. This I did with electromyography of the relevant muscles, and I found that, indeed, the nerve continued to be activated as it usually did, even though it now was attached to a new muscle. However, it was impossible to determine what the nerve was functionally achieving. This I was able to do in normal animals by using X-ray cinematography of the rats moving in running wheels with a plastic side replacing the wire partitions normally present (Cohen & Gans 1975). However, in animals with crossed innervation, it was not possible. So, the result described in my thesis remained largely indeterminate. I ended up calling this resulting paper, colloquially as: 'My life as short communication ... " (Cohen 1978).

Eric had sent me on the road to neuroscience, since he had a joint appointment in the Departments of Biology and Psychology, and I was in Biology (which, at that time, was actually still called Zoology). He gave very little guidance, which suited me fine. Helen Neville, another student of Eric's, and I became good friends, and she helped me navigate the field.

As a consequence, I moved on in related but less complicated directions. In particular, I was determined to keep things simple and determinant. My next preparation was the spinal cord of a primitive vertebrate, the lamprey, an animal at the base of the vertebrate tree. The spinal cord is clear, fully formed and easily removed from skin and muscle for monitoring and manipulating in vitro. What more could I ask? This work was begun with Dr. Sten Grillner at the Karolinska Institute on an NIH post-doctoral fellowship, and continued with Dr. Carl Rovainen and his student Jim Buchanan (Buchanan & Cohen 1982) in St. Louis at Washington University.

After this short stint as a post-doctoral fellow with Dr. Rovainen I returned to Cornell, where my husband was teaching. The university provided me with a lab after I obtained an NIH grant to further study the lamprey spinal cord. After some time on my own at Cornell, I obtained a faculty position at the University of Maryland, College Park. Once there, it was relatively smooth sailing!

Using the simple lamprey spinal cord, activated either chemically (the work begun with Carl Rovainen) or neurally from attached brain activation (much later, done on my own), I was able to specify the impact of the various forms of activation on the lamprey spinal cord (Cohen et al. 1996). Thus, I was able to demonstrate the output of the "central pattern generator" for locomotion in basal vertebrates. Importantly, it was very straightforward. The spinal cord when activated chemically produces a beautiful traveling wave of motor bursts that by themselves can produce the pattern of traveling waves of the fish as it moves through the water. Impressively, the traveling wave under even this condition has the proper delays between adjacent segments, and the strict alternation between the two sides (Buchanan & Cohen 1982). This simple pattern, we showed much later with activation of a theoretical model or robotic animals (Leftwich et al. 2012), produced the proper movement seen in lampreys as they swim naturally. Yes, the brain stimulation could alter the pattern in very complex ways (Cohen et al. 1996), as presumably needed, but when swimming steadily, the lamprey could swim perfectly with simple spinal cord activation, and the simple lamprey spinal cord alone produced the basics all by itself.

In 2005, the Japanese invited me to help them develop motor control, what they called "mobiligence" or smart movement by intelligent design. Their goal was the development of smart robots. So, I spent a month in Japan traveling between researchers giving lectures on motor control. After my departure, they did, indeed, do some beautiful work on mobile robots, some inspired by my visit (Kimura, Fukuoka & Cohen 2006) and a great deal from their own invention.

5. Conclusion

In a nutshell, that is my output during my experimental years in the lab. I also contributed to the literature with *Neural Control of Rhythmic Movements in Vertebrates*, a collection of articles by distinguished scholars in the field (Cohen, Rossignol & Grillner 1988). Lastly, I would like to say that Eric's sensitivity and insights helped to guide me through my intellectual journey, and I remain eternally grateful to him.

6. References

Buchanan, James T. & Avis H. Cohen. 1982. Activity of identified interneurons, motoneurons, and muscle fibers during fictive swimming in the lamprey and the effects of reticulospinal and dorsal cell stimulation. *Journal of Neurophysiology* 47, 948–960.

Cohen, Avis H. 1978. Functional recovery following cross-innervation of antagonistic forelimb muscles in rats. *Acta Physiologica Scandinavica* 103(3), 331–333.

Cohen, Avis H. 1979. Relationship between forelimb coordination and movement asymmetries during fast gaits, canter and gallop. *Brain Research* 164, 325–356.

Cohen, Avis H. & Carl Gans. 1975. Muscle activity in rat locomotion: Movement analysis and electromyography of the flexors and extensors of the elbow. *Journal of Morphology* 146, 177–196.

Cohen, Avis H . & Peter Wallén. 1980. The neuronal correlate to locomotion in fish: "Fictive swimming" induced in an in vitro preparation of the lamprey spinal cord. *Experimental Brain Research* 41, 11–18.

Cohen, Avis H., Philip J. Holmes & Richard H. Rand. 1982. The nature of the coupling between segmental oscillators of the lamprey spinal generator for locomotion: A mathematical model. *Journal of Mathematical Biology* 13, 345–369.

Cohen, Avis H., Serge Rossignol & Sten Grillner (Eds.). 1988. *Neural control of rhythmic movements in vertebrates*. New York, NY: Wiley.

Cohen, Avis H. , Lan Guan, J. Harris, Ranu Jung. & Tim Kiemel. 1996. Interaction between the caudal brainstem and the lamprey central pattern generator for locomotion. *Neuroscience* 74(4), 1161–1173.

Kimura, Hiroshi, Yasuhiro Fukuoka & Avis H. Cohen. 2006. Adaptive dynamic walking of a quadruped robot on natural ground based on biological concepts. *The International Journal of Robotics Research* 26, 475–490.

Leftwich, Megan C., Eric D. Tytell, Avis H. Cohen & Alexander J. Smits. 2012. Wake structure behind a swimming robotic lamprey with a passively flexible tail. *Journal of Experimental Biology* 215, 416–425.

Lenneberg, Eric H. 1967. *Biological Foundation of Language*. New York, NY: Wiley.

Mobiligence: Symposium on learning through motor action. Sapporo, Japan, December, 2005

Sperry, Roger W. 1945a. The problem of central nervous reorganization after nerve regeneration and muscle transposition. *The Quarterly Review of Biology* 20, 311–369.

Sperry Roger W. 1945b. Restoration of vision after crossing of optic nerves and after contralateral transplantation of eye. *Journal of Neurophysiology* 8, 15–28.

Avis H. Cohen
University of Maryland
Department of Biology and
Institute for Systems Research
College Park, MD 20742
United States of America

avis@umd.edu

50 Years Later: A Conversation about the Biological Study of Language with Noam Chomsky

Patrick C. Trettenbrein

It is not an overstatement to consider Eric Lenneberg the founder of the field of biology of language and his *Biological Foundations of Language* one the field's founding documents. Similarly, modern linguistics in the tradition of generative grammar was founded by Noam Chomsky in the 1950s with his *Syntactic Structures* as one of the founding documents of this field.

Figure 1: Noam Chomsky portrayed by Jean-Baptiste Labrune (Creative Commons BY-SA 4.0).

At first, the work of Chomsky and Lenneberg as well as their respective seminal books may seem only vaguely related—after all, *Biological Foundations of Language* surveyed the biological literature while *Syntactic Structures* provided a formal analysis of natural language syntax. However, nothing could be further from the truth: Lenneberg and Chomsky co-founded what today is known as biolinguistics during their time as graduate students at Harvard. Even a quick look at *Biological Foundations of Language* gives this away: Chomsky contributed an appendix on "The formal nature of language" to the book. A closer look reveals that Lenneberg himself heavily relied on formal analysis (of language) just like that provided by Chomsky in order to advance his argument (in this context, see Piattelli-Palmarini, this issue, Becker, this issue).

Consequently, talking to Noam Chomsky as a co-founder of the field, contemporary, and friend of Eric Lenneberg was the obvious thing to do. Luckily, Professor Chomsky took the time to answer some questions about the early days of the field, his work and relation with Lenneberg, and a number of other questions and scientific issues that (still) captivate us 50 years later.

In this document, all questions posed by the interviewer (Patrick C. Trettenbrein) are printed in italics, whereas Professor Chomsky's answers are printed in normal type. Please note that this interview also includes some questions originally raised by colleagues and friends, which the interviewer has tried to reproduce here as accurately as possible. References to books and articles mentioned have been added for the readers' convenience.

biolinguistics

ISSN 1450–3417
Biolinguistics 11.SI: 487–499, 2017
http://www.biolinguistics.eu

First of all, let me thank you for taking the time to answer my questions. As you know, the reason for this interview is the 50th *anniversary of the publication of Eric Lenneberg's monumental* Biological Foundations of Language *(1967), now widely considered one of the founding documents of the field besides, for example, your* Syntactic Structures *(Chomsky 1957/2002). I would like to start out by asking you about your relationship with Eric Lenneberg in the early days of biolinguistics. I take it the two of you first met during your time at Harvard?*

I arrived at Harvard in 1951. Eric did at about the same time. We met very soon and quickly became close friends, the families too.

How closely were you working with Eric Lenneberg in those early days (and also later)? When re-reading Biological Foundations of Language *at the end of last year it struck me how surprisingly modern many of his points of view were despite limited data he could draw from. Similarly, I was struck by how closely many arguments he puts forward align with what I would assume to be your points of view. Consider, for example, some of the points discussed by Massimo Piattelli-Palmarini (this issue): Lenneberg's take on language development, his considerations on a possible genetics of language, or on a more conceptual level, the distinction between competence and performance; even his ideas about the evolution of language. How big would you consider his influence on your thinking about these issues (and beyond)?*

We spent a great deal of time together in the early 1950s, along with our mutual friend Morris Halle, discussing common interests, work we were doing, problems we were thinking about, papers and books we were studying, and the state of the fields in the areas of our interest. And lots more. What drew us together in this connection was a shared scepticism about much of what was virtual orthodoxy at the time in Cambridge: radical behaviourism and behavioural science more generally in the forms it was taking, the reigning doctrines of structural linguistics, euphoria about the prospects for new technologies, and a prevailing attitude, which seemed to us misguided, that these alleged breakthroughs would soon revolutionize the study of human thought and behaviour, casting out traditional mentalist mysticism.

Eric was beginning his investigation of biology of language in those years, but the insights you describe here—and there are many—are mostly from work of his years later. By the mid-1950s Eric and I left Harvard. He went to medical school, I went to the Massachusetts Institute of Technology (MIT). We remained in contact but not with the intensity of the early '50s when we were grad students at Harvard, meeting regularly.

Despite the enormous overlap between your points of view, as far as I could find out, the two of you never ended up publishing a joint paper—though you of course wrote an appendix to Biological Foundations of Language. *In many respects, your work and that of Eric Lenneberg seem complimentary insofar as you focused on many conceptual issues and adopted a more computational respectively theoretical approach whereas he delved deeply into biology. How come the two of you never ended up publishing a joint paper?*

During the years when we were in close contact we were graduate students. For some years afterward, publication possibilities altogether were limited for the kind of work in which we were engaged, which was quite isolated. And by the time publishing opportunities opened up we were pursuing somewhat separate if related paths.

More generally, could you elaborate a little bit on the early days? What led you and Lenneberg to pursue these questions, that is, what led you to look at language from a biological point of view?

To the three of us—Eric, Morris, and me—it simply seemed obvious that the human language faculty is part of human biology, much like the visual and other faculties. If so, it only made sense to try to incorporate the study of language within a general biological framework, the path that Eric pursued in his own way with such intensity and success.

With regard to Eric Lenneberg the person, do you remember what fostered his specific interest in language or where it came from? And do you any idea what made him go to medical school after already having earned his PhD in linguistics and psychology?

When we met, in 1951, we each had a special interest in language and psychology. After Eric finished his studies in linguistics–psychology, given his particular concern with biological foundations, it was quite natural for him to move on to med school.

This may be a somewhat odd question, because you of course wrote a chapter on the formal nature of language that was included as an appendix in Biological Foundations of Language*, meaning that you obviously endorsed the book and what Lenneberg had to say about language and biology. Still, I would like to know what your overall impression of the book was when it first came out, respectively when you first got to read the manuscript?*

I thought it was an instant classic, basically founding a new discipline. It is surprising, in fact shocking, that it is not better known. Just to mention one example, there has been an enormous growth of interest in evolution of language from the 90s—many publications, conferences, societies – but it's quite rare to find a reference to Eric's discussion of the topic in his classic book, though it was pathbreaking at the time and remains today one of the most sophisticated and advanced contributions to the topic.

Okay. Now, looking back 50 years later, what would you consider to be the book's biggest achievement? Why do you think it is still relevant, respectively, why should researchers and students today still bother reading it?

The book developed a sound, deeply informed, comprehensive, carefully executed biological basis for the study of the human language faculty, its use and its evolution, not only bringing together what was known about this topic but substantially extending it, including provocative speculations that were far from conventional at the time but have since in many cases been shown to have been on the right track. And while a great deal has been learned since, this study remains an indispensable source for inquiry into the biology of language in all of its aspects.

Yes, it is very impressive how modern Lenneberg's view of language development and the biological theory of language he sketches at the end of the book in a summarising chapter already were, despite the fact that a lot of the important empirical work on acquisition from which we can draw today had not yet been carried out. While some modifications and updates may be required here and there his theory is still very accurate. In earlier work (Lenneberg 1964) he already indicated that his long-term goal was to do exactly this: come up with a biological theory of language.

Still, it is interesting to note that Biological Foundations of Language *in many ways was kind of a late comer to the party: Your review of* Verbal Behavior *(Chomsky 1959) had already had a huge effect on linguistics and psychology and some reviewers of Lenneberg's book at the time noted that, in a way, it seems that he not only sought to sketch a biological theory of language but also wanted to*

> *[. . .] provide a palpable biological plausibility for conclusions to which a number of uncomfortable Empiricists [. . .] [had] committed themselves on the basis of formal argument alone.* *(Bem & Bem 1968: 498–499)*

Do you agree?

When we arrived at Harvard, Skinner's William James lectures of 1948, the essence of his book *Verbal Behavior*, were widely regarded as the definitive approach to the psychology of language, in part because of the adoption of the basic framework by the highly influential Harvard philosopher W.V.O. Quine. Our triumvirate was almost alone in regarding it as seriously misguided.

My review, to which you refer, was submitted for publication in 1957, when Eric was already in medical school, but we had discussed these matters extensively in earlier years. The review drew from our reading and discussion of ethological literature, which was remote from the reigning orthodoxy. The rigidity of the orthodoxy is illustrated by the fact that even Karl Lashley's important 1951 paper on serial order in behaviour, which pretty much undermined Skinnerian behaviourism, was apparently unknown in the Cambridge behavioural science community. I couldn't find a reference in the relevant literature when I brought it up in the review (and I learned about it from an art historian, Meyer Schapiro). But these were the kinds of topics we were discussing. Eric's exploration of biological foundations was already underway at the time, though fully developed and published only a few years later.

So, you started out in Harvard by reading the European ethological literature in the original German—because Lenneberg grew up in Germany. Of course, you pointed out that language is actually best studied within an ethological framework and there are some obvious points about language that can be made which are still not being universally acknowledged. For example, the very straightforward and obvious idea that your language is a property of you and, first and foremost, your brain is still—maybe somewhat implicitly—deemed controversial when there are cognitive scientists who say that languages evolve(d) *in order to fit human brains (e.g., Christiansen & Müller 2015), whatever that is supposed to mean. What is your take on the current state of the biolinguistic research program today?*

True: "whatever that is supposed to mean." And yes, in the early '50s, Morris, Eric and I were reading ethological literature. They were both fluent in German and read originals. I kept mostly to the English language literature. This was foreign territory in the Cambridge intellectual community of which we were part, and to the extent that it was known at all, it was dismissed. The idea that there could be an instinctive element in human language, or cognitive capacities generally, which seemed to us virtual truism, was regarded by the most influential figures as virtual nonsense. So it was a private preoccupation. Almost. George Miller was receptive to what we were thinking about, along with several fellow students. But few others.

In the years since, and currently, biolinguistics has been flourishing, thanks to much deeper understanding of the nature of language, its acquisition and use, but also in part to new imaging technology that has in some measure overcome the ethical constraints on direct experimentation—and unlike the study of vision and other capacities, experiments with other animals tell us very little because of the species-specificity of the basic properties of language. There are no homologous systems to investigate.

Digressing a little bit for second, I'd like to ask a related question that concerns linguistics as a field more generally. Your own and thus, by extension, Eric Lenneberg's ideas about the scientific study of the language faculty are sometimes still portrayed as "controversial" even today—though it is interesting to see that Lenneberg is hardly ever referenced by critics despite having advocated a very similar point of view. Why do you think that is? One would think that it would be more attractive to attack someone who is no longer around to defend their views?

I don't think that's the reason. You have to ask who the critics have been. In the past 60 years, I've discussed the kind of work we (and by now a great many others) have been doing with people and audiences in many different disciplines. It's often been considered highly controversial (if not absurd) by philosophers, linguists, and a variety of social scientists, but not by physicists, mathematicians, biologists, including distinguished figures, among them Nobel laureates in evolutionary biology, with one of whom I co-taught graduate seminars in biology of language in the 1970s.

Among the critics, Eric's work was barely known, if at all, and if it had been known would have been considered "real science," not subject to this kind of critique. In the hard sciences, explanatory theories that are developed at what David Marr (1982/2010) called the computational/representational and algorithmic levels are considered quite natural, and analogues are familiar in the disciplines. These are matters that have been extensively discussed. To mention one example, Jerry Fodor has devoted much of his distinguished career to explaining the validity of such approaches and countering criticism of them.[1]

---

[1] Interviewer's note: This exchange took place before the passing of Jerry Fodor at the end of November 2017.

Continuing with this topic for just a little bit longer, it seems that there are two different ways of doing linguistics, on some level maybe analogous to the way in which biology was done when it was still called natural history as opposed to how biological research is carried out nowadays. Norbert Hornstein speaks of languistics *as opposed to* linguistics*. To what extend do you think the entire discipline is still caught up in the "cultural history" stage, cataloguing what is found in the world, analogous to what the predecessors of biologists did when biology was still natural history?*

The answer becomes clear when one compares what was appearing in the journals in the '50s with what appears in the (many more) journals today. It illustrates dramatically a change from *languistics* to *linguistics* in Hornstein's sense. Furthermore, the typological range of languages investigated has vastly extended along with the depth of the questions examined, most of them unimaginable not many years ago. In this crucial sense then, "cataloguing what is found in the world" has vastly increased both in scope and depth.

Coming back to my previous question about the relation of your work to that of Eric Lenneberg, I would like to dwell a little more on the relationship of linguistics and biology. While the biological approach to studying language took shape in the '50s and '60s of the past century it seemingly took a while for the label "biolinguistics" to catch on and it has been popularised only in the past decades, amongst other things with the establishment of Biolinguistics*, the journal in which this interview will be published. Recently, some people have complained that the label biolinguistics is kind of a rebranding of generative grammar, whereas actual biolinguistics should be understood more exclusively as a label for all biological investigation of the language faculty (e.g., Martins & Boeckx 2016). What is your take on this?*

I don't see much of an issue. Biological investigation of the language faculty is, by definition, an approach to investigation of the language faculty. Generative grammar is the study of core properties of the language faculty. Why should any issue arise?

I agree. As I understood it, the criticism is about theoretical linguists mostly carrying on with their business "as usual" while labelling their work as "biolinguistics," despite not actively seeking integration with biology. Boeckx & Grohmann (2007), in the inaugural article of this journal, labelled this the two senes of biolinguistics: the "weak" and "strong" sense; the former being that linguists still seek to uncover the properties of grammar and the latter referring to work that requires the integration of linguistic insights with those from other disciplines. Would you agree that this distinction should be made?

Individuals can choose their own research interests and projects. Clearly, however, the domain of "strong" biolinguistics, as defined, can be pursued only to the extent that "linguistic insights"—that is "properties of grammar"—have been developed sufficiently to be combined and integrated. Same quite generally, whatever the specific domain of inquiry: vision (as David Marr famously discussed; see Marr 1982/2010), insect communication, any other. Again, I don't see any issues.

When re-reading many of Eric Lenneberg's publications as preparation for this project it seemed to me that he probably would have agreed with the rather critical assessment that a true integration of linguistics and biology is still missing and that generative grammar has kind of "hijacked" the label biolinguistics. Obviously, there was a clear gap between linguistics and biology in Lenneberg's days and there still is today. Was he generally optimistic about an eventual integration of linguistics and biology? And are you yourself still optimistic about that, respectively do you think we are now closer to a "real biolinguistics" than 50 years ago?

As noted, I don't understand the "hijacking" issue. Surely a lot has been learned about the biology of language—biolinguistics—in the past 50 years. I don't know what a "real biolinguistics" is any more than what a real biology of vision is. There is increasing understanding of the topics. What more can we expect?

I admit that "hijacking" may be a bit strong of a term. Still, I actually had a quote by Lenneberg in mind. He wrote that

> *nothing is gained by labeling the propensity for language as biological unless we can use this insight for new research directions—unless more specific correlates can be uncovered.* *(Lenneberg 1964: 76)*

I suppose you would say that generative grammar offers the "new research directions" and that's why the issue doesn't even arise?

When a language is understood as a biological object, as in generative grammar (an I-language in contemporary terminology), then certain questions arise directly:

- How is language acquired?
- What is its neural basis?
- How did it evolve?
- How is it used?

Such questions cannot be formulated in any clear form if language is regarded as some kind of community property—say, a "sort of contract" in a community (Saussure) or "the totality of utterances made in a speech community" (Bloomfield). Accordingly, though not entirely neglected, such questions could be pursued only in limited ways in terms of such conceptions.

Furthermore, as noted earlier, it is virtual truism that such questions can be pursued seriously only to the extent that the properties of these biological objects are understood. It is not controversial that these "new research directions" have been developed in highly productive ways within the general "biolinguistic framework" that generative grammar adopted from its origins.

Against this background, consider the following quote from a report by Eric Lenneberg written in 1972, rescued from oblivion by Michael Arbib (cf. Arbib, this issue), a few years after Biological Foundations of Language *including your appendix had been published:*

> *At present there is only one type of scientific theory of language structure available, generative grammar, and this was never intended to serve as a model for*

> *biological mechanisms. The intent was to create a formalism that would adequately describe the web of relationships that characterizes a natural language such as English. For a time it looked as if it might turn into an algorithm or language analysis, but this is yet an unrealized dream (except for highly restricted discourse). Generative grammar does help us here and there to define the meaning of complexity in the structure of sentences, but because of the way it has been set up , it will also classify some utterances as complex (i.e., products of a long and complex history of transformations) that are intuitively the simplest of all—e.g., "Water!" which would have to be accounted for as a derivative of "Give me water!" which, in turn, is derived from a more basic form, "*You give me water." What is most urgently needed is a theory of sentence production and comprehension that has the formal precision of Chomsky's approach but is explicitly intended to explicate the psychobiological underpinnings of language capabilities.* (Lenneberg 1972: 635–636)

Do you think that developments in generative grammar in the past decades have brought it closer to the theory of sentence production and comprehension that Lenneberg had called for? In many ways work within the Minimalist Program (MP) seems even further removed from a theory of language production and processing, instead focusing on the formal nature of the involved machinery?

I don't entirely understand the quote. A generative grammar G of language L seeks to determine the structure of the infinite class of expressions of L and their interpretation at the conceptual-intentional (semantic-pragmatic) and sensorimotor interface (the role of the former primary, so recent work suggests). It is a theory of competence. Theories of performance for L will of course access the stored competence characterized by G; and general theories of performance will, correspondingly, access general properties of competence grammars. It all falls within biology, and all has "psychobiological underpinnings".

There has been a great deal of progress in the study of language processing, including extensive and productive work on minimalist parsers, much of it appearing in a forthcoming book on minimalist parsers edited by Robert Berwick (Berwick & Stabler, in press). The study of language production has also progressed, though with a huge gap that holds for all voluntary action. As described figuratively by two leading researchers of voluntary action, Emilio Bizzi and Robert Ajemian (Bizzi & Ajemian 2015), we are learning a good deal about the puppet and the strings, but the role of the puppeteer remains a mystery. In the case of language, it was essentially this mystery that inspired the awe and wonder of some of the great founders of modern science, including Galileo, Descartes and the logician-linguists of Port Royal.

Of course I agree that it all falls within biology, theories of performance as well as competence. In my understanding of the quote, Lenneberg was—in part—pointing to the discrepancy between linguistics and biology that we already briefly discussed. It seems that the study of competence has isolated and still isolates linguistics from psychology and neuroscience; maybe even preventing a closer integration of these respective fields, as in parts of psycholinguistics?

Chesi & Moro (2015) have recently argued that competence and performance are actually interdependent. I would argue that there are essentially three possible scenarios in which the relation of grammar (G) and a parser as a performance system (P) could work out: (i) G could be independent of P, (ii) G could be accessed by P online during processing, or (iii) it could turn out that G is only implemented in wetware insofar as the totality of P's mechanisms gives rise to a system behaving in a way that is captured by the description of G. What are your thoughts about this? And how would you describe the relation of linguistics to psychology and neuroscience?

I don't understand any of this. The study of competence can't be isolated from psychology because it is part of psychology—unless we (perversely) define "psychology" to exclude internally-stored knowledge of language, arithmetic, etc. Psycholinguistics, for the past 50 years, has been closely integrated with the study of linguistic competence. How could it be otherwise? Same with neurolinguistics. Linguistic competence is represented in the brain (not the foot, not in outer space) and the same is true of performances that access this stored knowledge of language.

Speaking personally, I've always regarded linguistics, at least the aspects that interest me, as part of psychology, hence ultimately biology. The relation of linguistics to psychology is similar to the relation of the theory of vision to psychology: part to whole. And insofar as we are concerned with what is happening in the brain, it's integrated with neuroscience. In brief, I don't see how any of these questions even arise except under delimitation of fields that seem quite arbitrary and have never made sense to me.

Of course I agree that linguistic competence is not represented in the foot. What I was trying to get at is that, for example, it could be the case that this internally stored knowledge is only accessed during processing in the sense that it is built into the way in which the performance system works. Does that make any sense or am I completely off the mark here?

The I-language—linguistic competence—is accessed in every use of language: in processing linguistic input, but also production (including internal construction of thought). If it is "built into the way in which the performance system works," then it must be duplicated in each performance system, which does not seem a reasonable proposal. I don't see any way of reformulating this idea that does not reduce to the assumption that I-language is a central system accessed by performance systems, much as knowledge of arithmetic is accessed in calculating.

Okay. You have recently co-authored a paper on the neural basis of language processing with Angela Friederici (Friederici et al. 2017), so you must be somewhat optimistic that real progress can be made in this area. What is your advice for researchers trying to bring linguistic theory and psychology or neuroscience closer together?

Not quite. I contributed some introductory remarks to her very important book (Friederici 2017a). On the rest, I don't think the formulation of the questions is helpful, any more than the question of how to advise researchers trying to bring the theory of visual perception and psychology and neuroscience closer together.

It doesn't seem to me the right way to formulate the issues. There are different approaches to the study of language (vision, etc.), and intriguing problems where they intersect. The advice is to pursue them.

On a related note, one of the biggest successes of the MP has been to reduce the complexity of the postulated cognitive machinery, for example, by relying only on a single operation which we think is at the core of the language faculty, that is Merge. Now, you yourself have speculated that it is not at all clear whether Merge is specific to language, at least in phylogenetic terms. With regard to how an operation like Merge may be implemented in wetware we are still in a situation where we can at best make educated guesses (e.g., Friederici, this issue), as we lack a linking theory between the computational/algorithmic and the implementational level. It seems to me that a scenario where circuitry capable of recursive computation may have evolved for another purpose (e.g., motor planning or music) and later was exapted for language seems quite likely. What is your current take on these questions?

It is often claimed that recursive computation is involved in motor planning, but that seems to me a misunderstanding of both recursive computation and motor planning. Recursive computation holds for systems of digital infinity. Motor planning is not a system of digital infinity (though one can impose an arbitrary digital grid on continuous systems, leaving the issues where they were). And as one looks beyond this initial (and crucial) distinction, divergences proliferate.

As for music and language, it seems to me far more reasonable to suppose that music (to the very limited extent that it involves recursive computation) was exapted from language than the converse. Or, perhaps, as suggested by Jeffrey Watumull and Marc Hauser in recent work (Hauser & Watumull 2016), that recursive computation emerged and was applied in cognitive systems of digital infinity, language and arithmetic, maybe music.

What about labels, respectively Label? You recently argued that Simplest Merge in contrast to the original definition of Merge cannot yield labelled objects by conceptual necessity, and continued by speculating that labels may not exist as syntactic objects. Could you elaborate on this? And what about endocentricity, respectively headedness as a key property of human language, after all, at the heart of X-bar theory?

This takes us off into different and more technical directions, not appropriate in this context, I think.

Well, some colleagues have asked me to pose some questions to you. One of them wanted me to ask you why you are so convinced that sensorimotor systems only play an ancillary role in language, that is, especially when taking a processing perspective? On the computational level this view is, of course, very plausible; but when we look at implementation she is not so sure. Think embodied cognition. Also, she thinks that in the context of the MP these ancillary systems are thought to do a lot more work at the interfaces than previously assumed?

I've explained the reasons elsewhere, both conceptual and empirical (for review of some of them, see *What Kind of Creatures are We?*; Chomsky 2016). They seem to me sound and compelling. In processing, sensorimotor systems play a central role, by definition. I don't understand the rest. "More work" than what? "Than previously assumed" when? Implementation of what? I don't see what else is at stake.

Okay. Next, another colleague wanted me to ask you about your take on the origin of lexical elements which, incidentally, do a lot of the work in current syntactic theory. Lenneberg already put forward an approach to lexical semantics that was non-referential and completely intensional, the importance of which you have repeatedly emphasised. In Why Only Us? *(Berwick & Chomsky 2016), lexical elements were almost completely put aside. Interestingly, Lenneberg seems to have thought that the way in which humans categorise is not qualitatively different from other animals. Also, he considered "words" not as labels for concepts stored in memory but as labels for categorisation processes. What is your current outlook on these questions? Will the evolutionary origin of "words" remain mysterious or eventually turn out to be susceptible to study?*

I think there is very strong evidence, which I've discussed elsewhere, that human concepts/lexical semantics are radically distinct from anything known elsewhere in the animal world. Their evolutionary origin is a mystery, which is why they were "put aside" in our discussion of the evolution of the faculty of language. For the moment, there are, to my knowledge, no useful ideas as to how to investigate this mystery.

Lastly, if I am not mistaken, you started out at MIT as part of a machine translation project that you ended up never really working on because you thought it quite pointless—is that correct? A colleague of mine is interested in translation and, while not at all related to the topic of this special issue, he wanted me to also ask you about how you think one might approach the study of translation within a framework such as that of generative grammar? Is there something like a "translation faculty?" Personally, I might add that this question probably touches upon how we conceive of and understand the mapping to the semantic-pragmatic interface and the degree of variation that this mapping permits as opposed to the mapping to sensorimotor systems?

I happened to be appointed in a machine translation research project, but never worked on the topic. My feeling from the start was that for practical purposes, brute force approaches would be the most feasible. While some day understanding of language might contribute materially to this project, that time was still remote. And research on machine translation did not seem to me the way to advance the project of understanding the nature of language. These expectations have been borne out, as far as I know. I don't know of any reason to suppose that there is a "translation faculty". The question of variation at the semantic-pragmatic/conceptual-intentional interface is an interesting one, at the border of research (my own guess is: not much). But understanding is far from contributing much to improving automatic translation.

Final question. While this interview was intended as a discussion of your scientific work, I would nevertheless like to also include this somewhat more personal question, if that is *appropriate?—Obviously, you have not slowed down a bit despite officially retiring from MIT years ago. And rumour has it that you will soon be moving to the University of Arizona? So, after all these years in science and an arguably heretofore unprecedented academic career, please tell us, what keeps you going?*

Yes, moving to the University of Arizona. What keeps me going is the excitement of learning and discovery. The fields that have always interested me are, I think, opening up new vistas that offer much hope of deeper understanding of the nature of language and mind.

Professor Chomsky, thank you very much for answering my questions.

References

Arbib, Michael A. 2017 [= this issue]. "Language and brain: Developmental aspects:" Eric Lenneberg at the Neurosciences Research Program in 1972. *Biolinguistics*.

Becker, Misha K. 2017 [= this issue]. Innate mechanisms for acquiring syntactic displacement. *Biolinguistics*.

Bem, Daryl J. & Sandra L. Bem. 1968. Nativism revisited: A review of Eric H. Lenneberg's *Biological Foundations of Language*. *Journal of the Experimental Analysis of Behavior* 11(4), 497–501. doi:10.1901/jeab.1968.11-497.

Berwick, Robert C. & Noam Chomsky. 2016. *Why only Us? Language and Evolution*. Cambridge, MA: MIT Press.

Berwick, Robert C. & Edward Stabler. In press. *Parsing with Minimalist Grammars*. Oxford, England: Oxford University Press.

Bizzi, Emilio & Robert Ajemian. 2015. A hard scientific quest: Understanding voluntary movements. *Daedalus* 144(1), 83–95. doi:10.1162/DAED_a_00324.

Boeckx, Cedric & Kleanthes K. Grohmann. 2007. The *Biolinguistics* manifesto. *Biolinguistics*, 1, 1–8.

Chesi, Christiano & Andrea Moro. 2015. The subtle dependency between competence and performance. In Ángel J. Gallego & Dennis Ott (Eds.), *50 Years Later: Reflections on Chomsky's Aspects*. Cambridge, MA: MITWPL. 33–45.

Chomsky, Noam 1957/2002. *Syntactic Structures* (2nd ed.). Berlin, Germany: de Gruyter.

Chomsky, Noam 1959. A review of B. F. Skinner's *Verbal Behavior*. *Language*, 35(1), 26–58.

Chomsky, Noam. 2016. *What Kind of Creatures are We?* New York, NY: Columbia UP.

Christiansen, Morten H., & Ralph-Axel Müller. 2015. Cultural recycling of neural substrates during language evolution and development. In Michael S. Gazzaniga & George R. Mangun (Eds.), *The Cognitive Neurosciences V*. Cambridge, MA: MIT Press. 675–682.

Friederici, Angela D. 2017a. *Language in our Brain: The Origins of a Uniquely Human Capacity*. Cambridge, MA: MIT Press.

Friederici, Angela D. 2017b [= this issue]. Neurobiology of syntax as the core of human language. *Biolinguistics*.

Friederici, Angela D., Noam Chomsky, Robert C. Berwick, Andrea Moro, & Johan J. Bolhuis. 2017. Language, mind and brain. *Nature Human Behaviour*. doi:10.1038/s41562-017-0184-4.

Hauser, Marc D. & Jeffrey Watumull. 2016. The Universal Generative Faculty: The source of our expressive power in language, mathematics, morality, and music. *Journal of Neurolinguistics*. doi:10.1016/j.jneuroling.2016.10.005.

Lashley, Karl S. 1951. The problem of serial order in behavior. In Lloyd A. Jeffress (Ed.), *Cerebral Mechanisms in Behavior*. New York, NY: Wiley. 112–146.

Lenneberg, Eric H. 1964. A biological perspective of language. In Eric H. Lenneberg (Ed.), *New Directions in the Study of Language*. Cambridge, MA: MIT Press. 65–88.

Lenneberg, Eric H. 1967. *Biological Foundations of Language*. New York, NY: Wiley.

Lenneberg, Eric H. 1972. Language and brain: Developmental aspects. A report based on an NRP Work Session held November 19–21, 1972, and updated by participants. *Neuroscience Research Programme Bulletin* 12(4), 511–656.

Marr, David 1982/2010. *Vision: A Computational Investigation into the Human Representation and Processing of Visual Information*. Cambridge, MA: MIT Press.

Martins, Pedro T. & Cedric Boeckx. 2016. What we talk about when we talk about biolinguistics. *Linguistics Vanguard* 2(1). doi:10.1515/lingvan-2016-0007.

Piattelli-Palmarini, Massimo. 2017 [= this issue]. From zero to fifty: Considerations on Eric Lenneberg's *Biological Foundations of Language* and updates. *Biolinguistics*.

Noam Chomsky
University of Arizona and Massachusetts Institute of Technology
Departments of Linguistics
Tucson, AZ 85721
United States of America

chomsky@mit.edu

Patrick C. Trettenbrein
University of Graz
Department of Linguistics
Merangasse 70/III
8010 Graz-St. Leonhard
Austria

patrick.trettenbrein@gmail.com