

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

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From Zero to Fifty: Considerations on Eric Lenneberg's *Biological Foundations of Language* and Updates

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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).



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 pre-frontal 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 (Real & 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 Royauumont 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 Royacoumont debate, clearly stressed that the issue of structure-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, Tuszyński, & 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 equipage 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 established¹¹ 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.

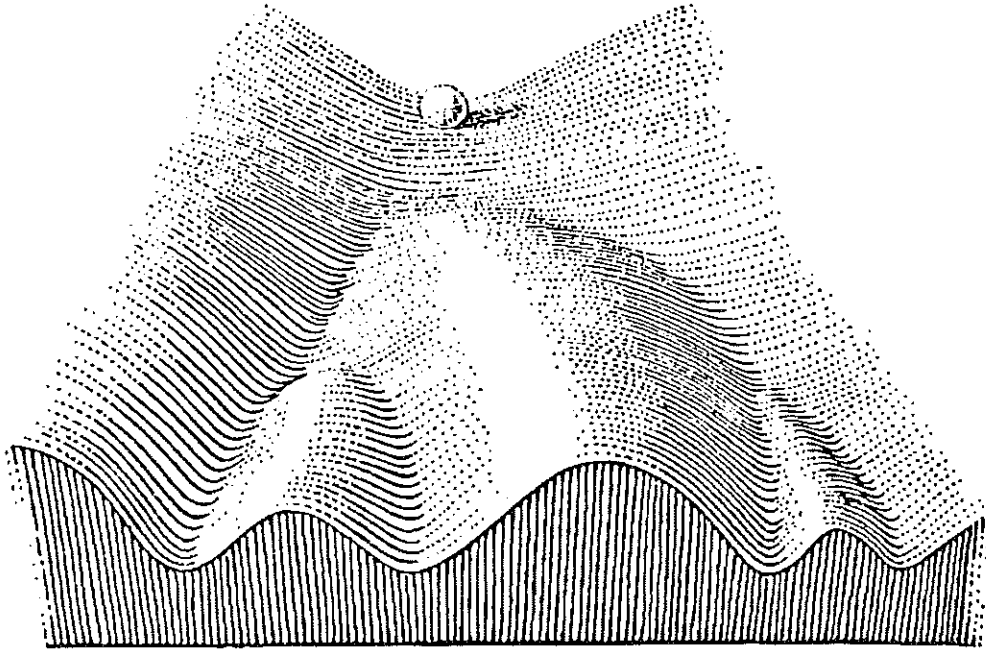


Figure 1: Reproduction of figure 6.10 from Lenneberg (1967: 262). The original caption in the book says: 'An 'epigenetic landscape.' A representation of a developmental system as a surface (sloping towards the observer) on which there are valleys along which the processes of differentiation tend to run. 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 separately¹⁴ 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- α , to principles-and-parameters¹⁷ 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.

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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)

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