



The philosophy of biolinguistics: a perspectival approach

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Abstract

In this paper, I present a philosophical analysis of Biolinguistics, including a number of proposals that point in the direction of its recognition as a full-fledged discipline or, perhaps, an interfield theory. My approach shall be a perspectival one, both in the spirit of Michela Massimi's perspectival realism, but also following William Wimsatt's investigation of complexity, where perspectivism also plays a key role, sometimes overlapping Massimi's view. In doing so, I will focus on what today may be considered to be the two mainstream frameworks within Biolinguistics, namely Chomsky's Minimalist Program and Kirby's culturalist approach, in order to illustrate how an analysis in terms of perspectives seems to better carve the geography of the issues than other proposals. This will allow me to identify a number of additional perspectives that may concern anyone interested in the biolinguistic enterprise, which I put forth here to complement, qualify and, in some cases, replace the traditional questions that have so far driven research in linguistics and biolinguistics.

Keywords Philosophy of science · Biolinguistics · Perspectival realism · Complex systems

[N]ature seems to respond positively to many approaches, not only to one.
Feyerabend (1999, 212)

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1 Introduction

Linguistics is not one of the favorite disciplines of philosophers of science. To be sure, most examinations of the language sciences published both by outsiders and insiders in the last forty years are more interested in bringing into focus a number of historical and sociological aspects of linguistics —especially those concerning the figure of Noam Chomsky and the development of generative grammar (e.g., Botha, 1989; Harris, 2021; Huck & Goldsmith, 1995)— than to carry out rigorous philosophical analyses of linguistics as a science. There certainly exist some exceptions, starting with the (critical) work of Jerrold Katz in the early 1980s (Katz, 1981) and, more recently, those of McGilvray (1999), Devitt (2006), Collins (2008), Ludlow (2011), or Rey (2020), which nonetheless continue to be too much Chomsky-centered (both in laudatory or in derogatory mode) and fail to match the kind of philosophical work we nowadays see in the context of other special sciences like, for example, biology. It must therefore be saluted as a positive development the fact that a number of philosophers of science have lately turned their attention to linguistics. One of the most active ones is Ryan Nefdt, who not long ago has joined the list of authors of the article ‘Philosophy of Linguistics’ in the *Stanford Encyclopedia of Philosophy* (Scholz et al., 2024).¹ Nefdt has mostly focused on issues concerning the place and role of modeling in linguistics (Nefdt, 2016a, b) and on different foundational and ontological aspects of the discipline, proposing an interpretation of those from the perspective of structural realism (Nefdt, 2018, 2019a, 2019c, 2021). He is also the author of three monographs dealing with these and other, related issues, including one of the latest additions to the *Cambridge Elements in the Philosophy of Science* series (Nefdt, 2023b, 2024, 2025). Another of his recent publications is an examination of the more biologically inspired approaches within linguistics (Nefdt, 2023a), so, given my goals in this paper and having myself devoted some time and effort to thinking about these matters, Nefdt’s paper shall be an inevitable reference point throughout this work, which is nonetheless not to be taken as a full-fledged commentary or critique of Nefdt’s proposals.

The paper is organized as follows. After these introductory remarks, in Section 2 I briefly consider the status of Biolinguistics as a discipline on the grounds of what may be considered its research agenda. This opens the way in Section 3 to discuss its structure in terms of perspectives by focusing on two particular instantiations of this research agenda, namely the one outlined by Noam Chomsky and collaborators within the approach known as the Minimalist Program (e.g., Berwick & Chomsky, 2016; Chomsky, 1995; Chomsky et al., 2023, among others) and what may be dubbed the ‘Edinburgh School’ (henceforth ES), known for its emphasis on the computational modeling of the cultural transmission of language through iterated learning processes (e.g., Kirby, 2017; Smith & Kirby, 2008). The paper culminates with Sec-

¹ Most of the work in the field has a marked encyclopedic flavor, very much in the spirit of the SEP article, and is meant to be part of miscellaneous handbooks or introductory surveys (e.g., Égré, 2018; Nefdt, 2019a, b, c, d; Pullum, 2019; Stainton, 2014). Notable recent exceptions to this general trend are Égré (2015), Terzian (2021) and Terzian & Corbalán (2021), for example.

tion 4, where I sketch a complementary research agenda for Biolinguistics expanding on the perspectival account developed in the previous section. Finally, Section 5 closes the paper with some concluding remarks.

2 The *bio*- part of biolinguistics

At the very outset of the introductory section of *Sociolinguistic patterns*, William Labov writes: “I have resisted the term *sociolinguistics* for many years, since it implies that there can be a successful linguistic theory or practice which is not social” (Labov, 1972, xiii; italics in the original). I disagree. Or, rather, I disagree with the presupposition behind Labov’s words that any approach to the phenomenon of language that abstracts away from some factor one believes to be central does not deserve to be recognized as Linguistics. Attitudes like this one are legion in the language sciences and have often been the source of acrimonious debates, which have acquired a particularly virulent tone especially when the prefix *bio*- is involved; for a taste of it, the reader may refer to any of the following: Koster (2009), Postal (2009), Behme (2015), or Levine (2018). It is not my intention to engage in a debate over these issues here, mostly because I would like to follow a different strategy in order to develop the ideas I want to address in the next section. I shall characterize, first, a subject matter or, perhaps, a collection of conceptual and scientific *problems* (or *puzzles*), directly—and, hopefully, uncontroversially—posed by the phenomenon of language and for which it makes sense to appeal to biological matters; only afterwards shall I address the question what this contribution should (or could) be, also, in this case, taking into account Nefdt’s own views, which I shall briefly contrast with mine. To be sure, these problems, as I hope to show, may be approached from different *perspectives*, where my use of the word ‘perspective’ here is somewhat technical, as a deliberate attempt to align myself with recent work on perspectival realism by philosophers of science Ronald Giere or Michela Massimi (Giere, 2006; Massimi, 2022b) and with the analysis of complex systems proffered mainly by William Wimsatt, especially in Wimsatt (1974, 1976, 1994); but see also Griesemer (2006, 2021).² My allusion to these two slightly (but overlapping) takes on perspectivism will become relevant and made explicit as I proceed.

Now, the collection of problems I want to focus on here is the one concerned with the question how it is possible for any individual of the human species to acquire and use one or more languages, including, of course, the evolutionary history of whatever underpins these abilities. This characterization is sufficiently broad to embrace a number of approaches within the general cognitive sciences, from psychology to neuroscience, and also within biology at large, while at the same time it is perhaps too liberal, such that some may contend that this is not linguistics (or even biolinguistics, for that matter), on the grounds, for example, that it transcends the strict limits of

² Most of Wimsatt’s papers referred to here have been reprinted as chapters of Wimsatt (2007) without (much) editing, so that quotations and page references to the originals should be easily traceable in the reprints.

linguistic reality (e.g., Devitt, 2006), whatever might that be. I am not interested in starting an argument about property rights here, hence, if this is not linguistics, so be it, I'd be happy to use a different label were there one available. But, alas!, for lack of a better term, and despite the fact of not being myself particularly keen on it, I shall use the label Biolinguistics (capitalized), with the added warning to the reader that this does not imply my adherence to any particular perspective like, for instance, Chomsky's MP. It does imply, however, a commitment with methodological naturalism as characterized, for example, by Chomsky (2000a, b, Ch. 4) or Giere (2006, 11–13)—see also Godfrey-Smith (2021, Ch. 9)—that allows one to safely establish a straightforward historical connection with problems posed by language that were already recognized as such by the linguists of the late 19th and early twentieth centuries. Such is the case, for example, of Hermann Osthoff and Karl Brugmann who, in the preface of their *Morphologische Untersuchungen*, wrote that “[t]he human speech mechanism has a twofold aspect, a mental and a physical” (Osthoff & Brugmann, 1878, III).³ Or, for that matter, Ferdinand de Saussure when, with a touch of uneasiness, admitted that “if we study language from different perspectives at the same time, the object appears to us as a confuse and motley cluster of things entirely disconnected from each other. It is when we proceed in this way that we open the door to other sciences —psychology, anthropology, normative grammar, philology, etc.,— that we must keep separate from linguistics” (de Saussure, 1916, 24–25; my translation). The only difference between the Swiss scholar and the Junggrammatiker is that the former relinquished naturalism and surrendered most problems to the other sciences and set forth to define a specific object of study for linguistics (‘la langue’) and thus arguably became the first Platonist in the field (Lorenzo, 2005, for a discussion of these issues).

The identification of such venerable historical antecedents in the struggle to deal with these problems justifies, in my opinion, considering Biolinguistics as a scientific *discipline* (or *field* or *domain*) in its own right, as it seems to satisfy most (if not all) the criteria mentioned by Bechtel (1986, 8) for aspiring at such a status. To be sure, it looks like a discipline (or domain) from the point of view of the objects studied in the sense of Shapere (1974) (also Shapere, 1984, Ch. 14); from the point of view of the cognitive activities involved, as analyzed, for example, by Toulmin (1972) or Darden and Maull (1977) it would qualify as a field as defined by the latter; and it would fit the bill, too, for the more sociologically-minded philosophers (Bourdieu, 1975; Foucault, 1969; Knorr-Cetina, 1982; Lenoir, 1997). I won't elaborate a thorough defense of this contention and other related issues such as whether Biolinguistics is an established discipline or one still in the making and building its scientific ideology (Canguilhem, 1977), because this would require a whole paper on its own. I shall nonetheless advance the suggestion that Biolinguistics qualifies as an instance of an interfield theory in the sense of Darden and Maull (1977) to the extent that it shares “an interest in explaining different aspects of the same phenomenon” (Darden & Maull, 1977, 49) with what traditionally has been recognized as Linguistics, where-

³The translation is by W. P. Lehmann as it appeared in Lehmann (1967, Ch. XIV).

with there exists an incontestable historical connection.⁴ Darden and Maull do not explore the question in detail, but towards the end of their paper, in a footnote, they contemplate the possibility that the emergence of interfield theories may be one of the mechanisms for the development of new fields (Darden & Maull, 1977, 59, fn. 16). I would contend that this is the case of Biolinguistics.

According to Darden and Maull, an interfield theory may have a number of different functions, which the authors group into five main items (Darden & Maull, 1977, 59). I believe that, at least in connection with the first three items in Darden and Maull's list, Biolinguistics has something to say about language, but concentrating for the time being only on the second one ("To answer questions which, although they arise within a field, cannot be answered using the concepts and techniques of that field alone") it is revealing to bring into focus Chomsky's research questions for linguistics (Chomsky, 1986, 1988). Chomsky's four questions are these (as set in Chomsky, 1988, 3):

- (Q1) What is the system of knowledge? What is in the mind/brain of the speaker of English or Spanish or Japanese?
- (Q2) How does this system of knowledge arise in the mind/brain?
- (Q3) How is this knowledge put to use in speech (or secondary systems such as writing)?
- (Q4) What are the physical mechanisms that serve as the material basis for this system of knowledge and for the use of this knowledge?

These four questions have remained fairly stable since the mid 1980's and have driven research in generative grammar for the last decades, with special emphasis on (Q1) and (Q2). Indeed, it is relevant to point out that in the early stages of the MP the agenda for linguistics had little to do with what later became the agenda for Biolinguistics, since initially the MP was mostly defined as a program to investigate questions of architectural design concerning the conditions that the system of knowledge must satisfy in order to interact smoothly with the other mechanisms it is connected within its material basis. Biology played only a relatively marginal role beyond that of serving as a background to emphasize the anomaly of the presumed perfection of the system in question, with evolutionary considerations remaining eminently anecdotal until the early 2000s; see (Chomsky, 1995, 1–11) for the first point and Chomsky (2000a, 94) for the original version of the parable of the primate who had his

⁴I therefore disagree with Lorenzo (2015), who argues that Biolinguistics doesn't have an object of study that is different from that of Linguistics and that the distinction is only methodological. My point is that it is precisely because of this methodological contrast that Biolinguistics qualifies as an interfield theory different from, but necessarily interfacing with, Linguistics. Thus, I am ready to accept that there is an area of research, let's call it Linguistics, whose main focus is the identification of 'linguistically real patterns' and whose connections with biology (cognition, psychology, etc.) are either very weak or entirely absent; see Nefdt (2023b, Ch. 5) for a detailed articulation of a proposal along similar lines and Haspelmath (2020, 2021) for a like-minded view, expressed in terms of the distinction between g-linguistics (general linguistics, aka biolinguistics) and p-linguistics (particular linguistics, i.e., comparative-descriptive linguistics). As it will become clear as I proceed, however, this is the only point about which I disagree with Lorenzo's theses.

brain rewired. So, in some sense, one could safely contend that the MP is a relatively recent incorporation to mainstream Biolinguistics as roughly characterized below.

Only later, some tried to identify the strong parallelisms that exist with the program for ethology that Niko Tinbergen first sketched in Tinbergen (1952) and later presented in a more articulated form in Tinbergen (1963).⁵ According to this parallelism, question (Q1) corresponds to Chomsky's Humboldt's Problem which, Boeckx (2015, 130) contends, has no equivalent in Tinbergen's program; question (Q2) is Plato's Problem and matches Tinbergen's ontogenetic question, while (Q3) —Descartes's Problem— roughly corresponds to Tinbergen's functional question; finally, (Q4) is what Boeckx (2015) calls Broca's Problem and agrees with Tinbergen's mechanistic question. Indeed, Tinbergen also included a question about phylogeny, which Boeckx (2009) dubbed Darwin's Problem and which was only recognized as a tractable research question in Chomskyan circles around the early 2000s with the publication of Hauser et al. (2002), but which had been already the focus of attention outside the MP since, at least, the early 1990s.⁶ To these five research questions we should add a sixth, namely that oriented to answering why language/languages are the way they are (and not shaped differently), which we can only spot in Chomsky's program for going 'beyond explanatory adequacy' (Chomsky, 2004, 2005, 2007), but which to my knowledge was first formulated explicitly, albeit in a somewhat different form, by Simon Kirby in the late 1990s (Kirby, 1999), although the seeds had already been sown by Hurford (1987, §1.3).⁷ Boeckx (2021, 2) asserts that Darwin's prob-

⁵This idea, as far as I can tell, it was first made explicit in Balari (2006a, b) and Boeckx & Grohmann (2007). Later, Cedric Boeckx (Boeckx, 2009, 2015) followed suit with a slightly more streamlined proposal by adding some nice new names for those questions that Chomsky had not already labeled before.

⁶For example, Derek Bickerton published *Language & Species* in 1990 (Bickerton, 1990), the same year in which Steven Pinker and Paul Bloom published "Natural language and natural selection" (Pinker & Bloom, 1990); Terrence Deacon published *The Symbolic Species* in 1997 (Deacon, 1997); the first Evolution of Language International Conference was held in Edinburgh in 1996. But, leaving aside Eric Lenneberg's (still illuminating) speculations in chapter 6 of his *Biological Foundations of Language* (Lenneberg, 1967), the pioneer in language evolution research was perhaps Philip Lieberman with *The Biology and Evolution of Language* (Lieberman, 1984) and subsequent work.

⁷No fancy label for this question, although it may well be the case that we won't need one, because it may turn out to be a reformulation of Darwin's problem from a different perspective. Thus, Levins (1996, 107–108; my emphasis) in his assessment of the research process writes:

In approaching a new problem, my Marxism encourages me to ask two basic questions: why are things the way they are instead of a little bit different, and *why are things the way they are instead of very different?* ... This first question is that of the self-regulation of systems, of homeostasis ... *The second question is the question of evolution, history, and development.*

This paper by Levins was reprinted as chapter 16 of Lewontin & Levins (2007), a book which in Part II includes other works where these ideas are discussed in connection with the problem of investigating complexity. That much would strengthen the idea expressed in the text that Biolinguistics as an interfield theory only came out once the members of the scientific community started to take seriously these evolutionary issues. It also hints at the idea that the MP's biolinguistics, claims to the contrary notwithstanding (e.g., Jenkins, 2000), is a relatively newcomer to the Biolinguistic enterprise. Indeed, in a paper coauthored with Howard Lasnik, originally written in 1993 and reprinted as Chapter 1 of Chomsky (1995) we read, in the context of a brief discussion of the research questions (Q1–Q4) plus Darwin's problem, that "[p]roblems (2d–e) [i.e., (Q4) above plus Darwin's problem; the author] appear to be beyond serious inquiry for the time being, along with many similar questions about cognition generally ... We will put these matters aside." (Chomsky, 1995, 18).

lem is probably the most interdisciplinary of all the Tinbergian questions. I concur, and I surmise that this is precisely the question that, along with the sixth, cannot be answered using the concepts and techniques of the field of Linguistics alone and that inevitably triggered the emergence of Biolinguistics as a field.

It is not at all obvious that these six questions exhaust the different kinds of problems a fully articulated biolinguistic program may want to address, an issue to which I shall turn towards the end of this paper. For now it suffices to note that they deploy what I earlier informally characterized as ‘a collection of conceptual and scientific problems’ in the shape of a specific *problem agenda* in the sense of Love (2008) and that they should provide enough context to gauge how things stand and how the different perspectives within the field have tried to address them. My emphasis shall be, chiefly and for the reasons discussed in the previous paragraph, on Darwin’s problem, although it will be seldom possible to completely separate one question from the others. This is what I propose to do in the next section.

3 Biolinguistics has a perspectival structure

I shall take the preceding discussion to offer sufficient grounds to consider Biolinguistics as an interfield theory, partially overlapping with what traditionally is known as Linguistics but not identical to it. The distinction is justified, in my opinion, precisely because Biolinguistics typically incorporates concepts and techniques (usually from biology but also from other disciplines) that are alien to traditional linguistic practice as carried out by descriptive grammarians, typologists, etc., with the aim of answering such questions as how linguistic abilities develop in the individual and how such abilities emerged in the species. For example, a brief inspection of, for example, the papers in Part III of Boeckx and Grohmann (2013) will reveal such plurality of concepts and techniques, going much beyond such fields as neuroscience and genetics, which some very restrictive and reductionist views often pinpoint as the main referents of biolinguistic research. In this respect, my analysis partly agrees with the one proffered by Nefdt (2023a) who, inspired by Martins and Boeckx (2016), considers “a number of ways in which biology and linguistics can be combined or jointly pursued” (Nefdt, 2023a, 2), which he approaches taking Quine’s ‘three grades of modal involvement’ as a model (Quine, 1953). On his account, the three degrees of involvement would be:

- (D1) Unification or, better, reduction, i.e., linguistics *is* biology (under a certain understanding of what biology is supposed to be);
- (D2) Collaboration, i.e., linguistics is not biology but linguistic research should be constrained by biological principles; and
- (D2) Business as usual (Boeckx & Grohmann, 2007, 2), i.e., linguistics is biology but at a ‘certain level of abstraction’.

Nefdt rejects (D1) and (D3) on several grounds and favors (D2), which comes close to the kind of interfield theory I characterized above. However, I find this analysis ill-suited because I believe it does not quite work in the direction of Nefdt’s pur-

ported attempt to “disambiguate the different senses of the ... term [biolinguistics]” (Nefdt, 2023a, 2). Indeed, I maintain that it fails to accurately carve the geography of the various approaches that make up the biolinguistic enterprise, as I don’t think Quine’s formal approach to modality is easily translatable to a situation in which, as one would expect when dealing with perspectives, many pragmatic aspects enter into play that don’t find a direct and quantifiable gloss in terms of degrees of involvement: one must not forget that perspectives, as characterized below, are historically and culturally situated scientific practices. I’ll try to spell out this in the following subsections in order to show that a perspectival view better captures the structure of the field, where Nefdt’s three degrees of involvement are better interpreted in terms of such pragmatic factors as what Massimi (2022b, Ch. 5) calls (*physical*) *conceivability*, pertaining to a specific perspective and the specific vantage point from which it looks into the target system of interest. Before moving to that, however, let me say something about perspectival realism and how it may be helpful in the analysis of a discipline like Biolinguistics.

3.1 Perspectivism, realism, and the reliability of scientific knowledge claims

Perspectival realism has been described as “one of the latest attempts at a middle ground in between scientific realism and antirealism” (Massimi, 2018b, 164). I think Giere (1999) offered ample reasons for believing that the alternative is promising; there he argued that many philosophers (and sociologists) of science’s overreaction to the challenges originally posed by Kuhn and Feyerabend in the late 1960s and early 1970s resulted in the failure to appreciate a number of critical points that perspectivism tries to incorporate along with the most widely accepted one of granting a role to history. I think that these critical points may be safely summarized as the following:

- (i) Science is not —it cannot be— carried out from a ‘view from nowhere’ (or ‘everywhere’), but it is situated, it is always carried out adopting a view from somewhere.
- (ii) Naïve forms of realism must be abandoned, especially those based on standard analyses of truth and reference. Reality is not a construction, however, but we can only aspire at offering a series of partial, idealized, and imprecise images of it.
- (iii) The main, and perhaps the only, way to have access to such mind-independent world is through the construction of models of the target systems of interest.

Taken together, these points suggest that the success of science is the product of the interaction, diachronically and synchronically, of different perspectives that have been able to produce reliable scientific claims. Massimi (2023, 6; her emphasis) offers the following informal definition of a scientific perspective: “I think of a scientific perspective in broad terms as the *historically and culturally situated sci-*

entific practice of a community at a given historical time”.⁸ Thus, ‘perspective’ in this context refers to the scientific practices of specific (historically and culturally situated) epistemic communities, equipped with their own experimental and theoretical resources devised to derive scientific knowledge claims about the phenomena of interest. Additionally, perspectives also incorporate a number of methodological principles designed to justify their knowledge claims. This will be my perspective on perspectives throughout this section, only later (Sect. 4) shall I try to establish the connection between this notion of perspective and the one developed by Wimsatt and others in the their investigation of complexity.

Before this subsection gets too long, let me highlight a couple of points about perspectives that deserve, at least, some brief consideration. These are the following⁹:

- (a) How do the reliable scientific claims advanced by one perspective turn into *actual* scientific knowledge?
- (b) What is the role that perspectival modeling plays in this process of turning scientific claims into actual scientific knowledge?

Perspectivism entails scientific (and model) pluralism—but not the other way round, as Massimi is at pains of demonstrating in Massimi (2022b, Chs. 3–4). Indeed, the variety of perspectivism that is of particular interest here is the synchronic variety, which is complementary to the traditional diachronic version, typically intended to supersede Kuhn’s stage model of scientific progress of new paradigms replacing earlier ones (Massimi, 2018c). The synchronicity of perspectives implies that they are culturally situated *within the same historical period* carrying out their own research programs and modeling practices and delivering situated, perspectival knowledge. Of course, *perspectival* knowledge is not *actual* scientific knowledge: perspectivism is not a form of relativism, but rather an attempt to save realism. How is the transition from perspectival to actual knowledge brought about? The second key factor, in addition to the simultaneous existence of a plurality of perspectives focusing on the same target system, is the kinds of modeling practices and the specific perspectival nature that Massimi attributes to these practices.

⁸ For a more complete and formal definition, see Massimi (2022b, 5–6), but it does not add very much to the one given in the text. Giere never really gave a definition of perspective and he used the analogy with color vision to characterize perspectivism: “a general characteristic of chromatic visual perspectives [is] that different perspectives are always *compatible*. There seems no way to generate a genuine inconsistency between different chromatic systems. They are just different” (Giere, 2006, 33; his emphasis); and, later, he adds that “scientific knowledge is perspectival in ways strongly analogous to the way our knowledge of colors is perspectival” (ibid., 36). Giere also saw perspectives as very similar to what Kuhn (2012, 181–186) called ‘disciplinary matrices’ (Giere, 2006, 82). The study of the Kuhnian and Feysabendian connections of perspectival realism is a fascinating enterprise that clearly transcends the goals of this paper; but see Giere (2013, 2016).

⁹ Since what follows shall only be a very concise sketch, I urge the interested reader to consult the relevant chapters of Massimi (2022b), plus Massimi (2018a, 2022a) for issues related to question (a), and Massimi (2018b, 2019) plus Gelfert (2016) for question (b).

According to Massimi, a distinguishing feature of perspectivism —one that sets it apart from other varieties of scientific and model pluralism— is *perspectival modeling*, a modeling practice that involves the construction of models that act as (more or less partial) representations of the target system from a specific vantage point—they are *perspectival*₁ in Massimi’s terminology: that much is inevitable once one gives up the idea that it is possible to look into a target system from a God’s eye view. Perspectival models are also, and crucially, *exploratory*:

Perspectival models are still representational in that they have a representational content (i.e., they are about X). But their being about X is being about possibilities (as opposed to actual or fictional states of affairs). And this is to be expected since their primary function is exploratory (i.e., the entities at issue are possible, neither known to be actual nor known to be fictional).

(Massimi, 2018b, 349)

As we will see the exploratory character of modeling both within the MP and the ES matches perfectly this characterization of perspectival model, bolstering up my thesis that perspectivism better captures the structure of Biolinguistics.

But the real power of perspectival models lies on their not hinging on the representation-as-mapping relation between model and target system but, rather, on relying on an inferential conception of scientific representation (Suárez, 2024) such that “they allow different epistemic communities to make relevant and appropriate inferences about what is possible concerning the phenomenon” (Massimi, 2022b, 72). This makes them *perspectival*₂ in their being also directed towards specific focal points of the target system.

Now, there are two dimensions to take into account regarding the inferential function of perspectival models: the perspective-internal dimension and the cross-perspectival one. As for the first, the exploratory nature of the models drives an investigation of the possible, constrained by what is physically conceivable within a historically and culturally situated perspective, deriving inferences that, to the extent that they are robust, sanction reliable scientific claims; as for the second, the robustness of inferences across perspectives provides the grounds for identifying modally robust phenomena and generating actual scientific knowledge. This provides the basis for the realist part of perspectival realism.

Let’s not lose track of the idea of robustness because this shall be one of the points that will make the linking with Wimsatt’s perspectives possible, but, hopefully, this succinct account should be sufficient for the analysis I want to develop in what’s left of this section.

3.2 Biolinguistic perspectives

My plan here is to look at the MP and the ES qua historically and culturally situated epistemic communities (i.e., perspectives) in order to present, first, the respective knowledge claims advanced by each in connection to their investigation of Darwin’s

problem.¹⁰ Next, I'll offer a brief assessment of the applicability of Nefdt's idea of degrees of involvement and, finally, I'll try to identify likely areas of intersection between the two perspectives and the potential of turning some of these claims into actual scientific knowledge.

I shall start my analysis by focusing, first, on the perspective defended by Chomsky and collaborators, namely the MP as shaped by the Strong Minimalist Thesis (SMT; see Berwick & Chomsky, 2016, 2017; Chomsky, 2021, 2024; Chomsky et al., 2023, for some recent references)¹¹ and, later, I will introduce —initially as an antagonist, soon to be revealed as a potentially reconcilable opponent— the work carried out within the ES.

3.2.1 The MP and the ES perspectives compared

The MP focuses on two different but intimately connected targets: (i) the system of knowledge that develops in the mind of speakers when they acquire their language (or languages) —what traditionally was known as a generative grammar and today is usually referred to as 'I-language'; and (ii) the mechanisms that enable the development of 'I-language', i.e., the 'faculty of language' (FL).¹² Crucially, both I-language and FL are systems internal to the minds of humans, with the former being an individual property (the result of individual development) while the latter is assumed to be a species property (the result of an evolutionary process). As we shall immediately see, the goal of meeting the explanatory challenges posed by the two targets —the *learnability condition* for (i) and the *evolvability condition*¹³ for (ii)— is perceived as generating the essential tension that drives current research under the SMT (Chomsky, 2021, 2024a, b).

FL is characterized as a computational system (CS) capable of building complex syntactic objects by means of a simple combinatorial operation (Merge) acting on a finite collection of lexical atoms. Merge is recursive in the sense that it can take as

¹⁰ I won't, however, devote much space to describe the historical and cultural contexts of each perspective. Chomsky (2009) is a comprehensive exercise in tracing the historical antecedents of generative grammar, and, in fact, any of Chomsky's shorter pieces almost always devotes the very first pages to put the research program in context. As for the ES, it is partly a fairly recent spin-off of the generative program that also tries to incorporate a number of ideas that were developed in pre-generative times; as noted in SubSection 3.2.1, its roots can easily be traced back to the work of Jim Hurford in the late 1980s.

¹¹ Thus, for space reasons, I focus mostly on the version of the MP promoted by Berwick and Chomsky in the last ten years or so. I thank an anonymous reviewer for alerting me about this —even when it comes to the MP, things are not as monolithic as the exposition that follows might suggest and, also in this case, one may need to recognize the existence of concurring (sub)perspectives. This is a characteristic feature of perspectivism and a desirable one at that, since, as noted, it is only through the simultaneous existence of a plurality of communities entering dialog and comparing results that increasingly reliable knowledge may be generated; see, for examples of this, Massimi (2022b, Chs. 4.a–4.c).

¹² Terminology has varied over the years and other terms have been used to refer to the same object(s) both by Chomsky and his critics such as Universal Grammar (UG) and Language Acquisition Device (LAD). In general, however, FL is used to name the initial state of the system prior to the start of the acquisition process, while nowadays UG is usually kept to refer to the theory of FL; I-language refers to the result of the acquisition process. In this paper, I shall stick to this terminology whenever it is necessary to distinguish between the two.

¹³ Not to be confused with 'evolvability' as a technical term used in theoretical biology; see Watson (2021).

input its own output such that the number and complexity of the objects it may construct is potentially unbounded. This accounts for the widely accepted idea among linguists that ‘the longest sentence in language L’ is a meaningless statement because, given some sentence s_n , the CS has the resources to take s_n and build a longer sentence s_m . As a consequence of that, the number of possible sentences in a language L is potentially infinite.¹⁴ This is the Basic Property (BP), the minimal constraint that is imposed on the workings of the FL. Now, given the nature of the atoms that make up the lexicon it is assumed that the expressions built by Merge are thoughts that may be externalized in the form of some public code through the sensory-motor system as speech (in the vocal-auditory modality of language) or as gestures (in the case of visual and tactile sign languages). This is the linguistic phenotype, “a system for generation of thought” (Chomsky, 2021, 7) implemented in the human brain and interacting with other cognitive systems including those that serve the (secondary) purpose of communication (i.e., externalization). Clearly, Chomsky’s current position is therefore now closer to that of Jerry Fodor (Fodor, 1975, 2008), at least with respect to the basic architectural assumptions, in the sense that Chomsky’s I-language comes closer to Fodor’s LOT (but it’s not identical to it), while Chomsky’s externalization system would be something similar to Fodor’s language module, such that the only expressions with semantic properties are those of LOT, whereas expressions in public language just inherit these properties by being associated with them.¹⁵ Therefore, and coherently with the assumption that thought is prior to externalization/communication, the evolutionary sequence that is assumed is one in which, necessarily, thought emerges before externalization, the latter process being the one accounting for the emergence of *languages* with their morphological, phonological and other grammatical properties typically associated with the constraints imposed by the externalization channel of choice (e.g., word order) and the only locus of linguistic variation: “the problem of ‘externalizing’ language was solved in unique ways, after the appearance of the BP” (Berwick & Chomsky, 2017, 170), a model that is compatible with the polygenetic theory of the origins of languages (Huybregts, 2017). Indeed, this contention is taken as evidence not only of the fact that linguistic variation is merely a superficial effect coupled to the development of externalization, but also of the contention that, strictly speaking, this was not an evolutionary event (e.g., Berwick & Chomsky, 2016, 83). Thus, Chomskyan Biolinguistics appears to be steadily getting close to becoming a ‘science of thought’ in the sense of Hinzen

¹⁴ This is sometimes called ‘the infinitude claim’. This claim is widely accepted but not uncontroversial, see the papers in van der Hulst (2010), especially those in Part II.

¹⁵ See, for example, Chomsky (2012, 71–72). Fodor and Chomsky would nonetheless (strongly) disagree on how the atoms of thought acquire the semantic properties they have, but this is clearly the topic for an entirely different paper. It is important to take into account however that this is precisely the reason why Chomsky believes that the human conceptual system is special, an idea that goes back, at least, to *Cartesian Linguistics* (Chomsky, 2009; originally published in 1966).

and Sheehan (2013), a conclusion Chomsky seems to be quite ready to accept (e.g., Chomsky, 2021, 2024a).¹⁶

Now, what is the *biological* evidence underpinning these hypotheses? Note, before getting to the details, that what we have here are *three* uniqueness claims, namely:

(U1) The contention that at some point in the evolution of our lineage human-like concepts emerged. How this may have happened is considered to be a mystery (Berwick & Chomsky, 2016, 87).

(U2) The contention that FL as a tool for thought is the product of an evolutionary event (i.e., genomic change) resulting in a rewiring of the brain that made the appearance of Merge possible. This event is assumed to have occurred after the evolutionary split between *Homo sapiens* and Neandertals/Denisovans and it involves the emergence of an evolutionary innovation; it might even be the cause of such a speciation event but the most plausible timing is assumed to be that of the arrival of Anatomically Modern Humans (Berwick & Chomsky, 2016, 2017; Chomsky, 2021).

(U3) The contention that the solution to the problem of externalization of thought (i.e., the creation of the ‘amalgam of language and sensory-motor systems’) implies the appearance of an entirely new character, an autapomorphy (Berwick & Chomsky, 2017, 170).

Claims (U1) and (U2), taken together, are essentially a version of the widely accepted idea that language is exceptional, an assumption that is shared by the majority of scholars and in line with the idea that its emergence is the hallmark of the, so far, last major transition in evolution (Maynard Smith & Szathmáry, 1995; Szathmáry, 2015). To be sure, disagreement arises when it comes to pin down the causes and the precise target of such exceptionality. The current minimalist position is essentially a streamlined version of the scenario sketched by Hauser et al. (2002), where the CS equipped with the Merge operation (what in that paper was named the Faculty of Language in the Narrow sense or FLN) is identified as the only missing ingredient that eventually connected other, pre-existing systems (externalization, conceptual system, etc.) in order to get the full-fledged FL of contemporary humans. Note that, given (U1–U3) above, this event is at present conceived as a three-step process starting with the emergence of human-like concepts, followed by the connection giving rise to LOT, followed in turn by the connection with externalization at a later time.¹⁷ Thus, and given the three-factor model of Chomsky (2005), the CS with Merge plus the lexicon is the only human- and language-specific innate cognitive machinery appealed to in the explanation of the design features of language (and of languages), with the other

¹⁶ Indeed, occasionally, Chomsky has voiced fairly radical opinions on this respect; to wit:

[J]ust about everything that’s been studied in linguistics for the last 2500 years is not language. It’s the study of some amalgam of language and sensory-motor systems. But if we want to really think through the internal language, the entity that’s computing away and giving you semantic interpretations, that doesn’t have any of these properties. It doesn’t have operations of reconstruction, doesn’t have linear order; just operates along producing thoughts in your mind which you can then try to externalize somehow.

(Chomsky, 2020, 27).

¹⁷ Although the relative order between (U1) and (U2) is sometimes reversed as in Chomsky (2024b, 20).

two factors (experience and other, domain-general principles) carrying most of the explanatory burden. This is in agreement with the SMT, which, in its role of basic principle to justify knowledge claims, requires that the first factor be minimal (if not zero) in order to satisfy the evolvability condition (Chomsky, 2024a, 16; Chomsky et al., 2023, 11), namely the assumption that the event in (U2) above is a relatively recent one, which imposes the constraint that any language-specific biases necessarily cannot be too rich, in contradistinction to what was traditionally assumed in earlier generative models.

The main pieces of evidence for these uniqueness claims are linguistic, with special emphasis on the fact that the semantic interpretation of a sentence is sensitive to hierarchical structure and not to linear order, what is known as the property of ‘structure dependence’ (Berwick et al., 2011; Everaert et al., 2015), which calls for some kind of procedure to build this structure that is eventually linearized for the sake of externalization.¹⁸ This is the main argument for Merge, whose format quite resembles that of what Currie and Killin (2019) call *minimal-capacity inferences* where, on the basis of the presumed (cognitive) pre-conditions for the production of some object (in this case, a linguistic expression), the presence of such pre-conditions is inferred from the properties of the object in question.¹⁹ To this we need to add that the hypothesis that Merge (understood as binary set-formation) is the basic operation implemented in the CS abides by the SMT, which has the “disciplinary function” of restricting “the mechanisms that are available for description of language” (Chomsky, 2024a, 20) —i.e., in Massimi’s terminology, the SMT acts as the basic principle to justify the reliability of the knowledge claims made within the perspective. The reason for this is that Merge is characterized as the simplest possible computational operation capable of accounting for the challenges posed by learnability (e.g., structure-dependence, which doesn’t appear to be learnable just on the basis of experience) and evolvability, since simplicity squares well with the presumed recent emergence of the CS (Berwick & Chomsky, 2016, 2017) and the Galilean and Leibnizian maxims from which the SMT is derived, according to which “nature is simple and it is the task of the scientist to show it” (Chomsky, 2024b, 17; and references therein).²⁰ And, last but not least, these considerations have resurfaced the idea that language presents a num-

¹⁸ Structure dependence refers to the fact that speakers-hearers, even very young ones, typically do not pay attention to linear order when interpreting an utterance, but rather to structural order. A classic example of this, discussed in detail in the references in the text, concerns the difference between sentences like ‘instinctively birds that fly swim’ vs. ‘birds that fly instinctively swim’, where the second one is ambiguous between a reading where the adverb ‘instinctively’ can modify either the verb ‘fly’ or the verb ‘swim’, while the first is not and the adverb can only modify the (linearly farther but structurally closer), verb ‘swim’; for some experimental evidence in young infants for this and other cases see Shi et al. (2020) and Zhu & Gavarró (2025).

¹⁹ Minimal-capacity inferences are also invoked within the MP in those contexts in which Currie & Killin (2019) originally applied this notion, namely the analysis of archaeological remains. Indeed, this is a quite common practice not just within the MP but also within other perspectives and one where a minimal consensus doesn’t seem to be forthcoming; see Currie & Killin (2019) for discussion and references, and Botha (2016) for a detailed analysis.

²⁰ Although Chomsky is not always generous in his writings when it comes to references and he just drops a few names as if what he means were obvious; so for Galileo, see Allott et al. (2021), for Leibniz’s Optimality Principle, see McDonough (2022), for Einstein, see Einstein (1995, 341–356), and for Weinberg, see Weinberg (1976).

ber of unusual properties for a biological system and that these (like the emergence of the CS and its connection with other cognitive systems) approach, if not directly instantiate, a perfect solution to the required specifications —e.g., “The substantive thesis is that language design may really be optimal in some respects, approaching a ‘perfect solution’ to minimal design specifications. The conclusion would be surprising, hence interesting if true.” (Chomsky, 2000a, 92); for the revival see, especially, Chomsky (2024b).

This is not to say that no biological evidence is adduced in support of these claims, on the contrary. I shall not present a detailed overview of the grounds offered to substantiate them, as they are easily traceable through a quick inspection especially of Berwick and Chomsky (2016, especially Chs. 1 and 4), Berwick and Chomsky (2017) and other works. On this grounds, Berwick and Chomsky conclude that this “whole cluster of empirical evidence aligns to support our overall thesis: a species-specific, relatively rapid appearance of Merge and the faculty of language, with externalization secondary; small evolutionary (genomic) change from other hominins coupled with correspondingly large phenotypic change in modern humans; and little or no genomic change in modern humans since the emergence of language” (Berwick & Chomsky, 2017, 172).

So let’s move now to consider the case of the ES which, unlike the MP, has much less worked out philosophical underpinnings, therefore what follows shall inevitably be briefer than my presentation of the MP.

The ES was originally instituted around the likes of James Hurford and Simon Kirby. This framework has its roots in three seminal papers published by Hurford between 1989 and 1992 (Hurford, 1989, 1990, 1992) which, in a nutshell, set out to meet Saussure’s challenge to study the “confuse and motley cluster of things entirely disconnected from each other” as a unified (and tractable) phenomenon. Hurford’s insight involves the definition of a space —what he calls the Arena of Use— where the individual and populational dimensions of language come into contact and which acts as an interface mediating the different individual processes of cultural transmission and subsequent induction of grammatical knowledge that unfold in historical time, with communication playing a central role. This view is inherently dynamic and systemic and, Hurford claims, has the virtue of making it possible to look at language, qua phenomenon, at three different timescales: the phylogenetic one, the glossogenetic (historical or cultural) one, and the ontogenetic one. The central idea is that ontogeny and transmission are made possible by mechanisms of language acquisition and production that are the result of biological evolution and that the iteration of such acts of transmission and learning through the Arena of Use gives rise to a new process, *linguistic evolution*, that explains the evolution of *languages*, what is generically known as *language change*. Ontologically, then, not only individual I-languages (or FLs or UGs or LADs, i.e., Chomsky’s target) are recognized, but also lineages of E-languages (English, French, Italian), seen as historical kinds of sorts (Millikan, 1999; Wagner & Tomlinson, 2022), connected by their synchronic existence within the Arena of Use. This conceptualization of the phenomenon naturally leads to a view favoring, both methodologically and ontologically, an approach in terms of complex adaptive systems (Gell-Mann, 1992; Holland, 1992; Kirby, 2012, 2013) and most research in the area is characterized by a strong reliance on computa-

tional modeling. This already marks a potential radical difference between the MP's appeal to the Galilean Style and the presumed simplicity of the world and the ES's (and others) views assuming its often messy complexity; another point of potential disagreement concerns one of the research questions that has been the focus of most modeling efforts, namely 'how much linguistic structure can be explained as an effect of culture?'—where 'culture' here is better understood as a (non-genetic) system of inheritance as defined in the dual-inheritance models developed by Robert Boyd and Peter Richerson (Boyd & Richerson, 1985; McElreath & Henrich, 2007)—in a clear attempt to prioritize externalist explanations over internalist ones. These models, named iterated learning models (ILM), implement Hurford's original idea of iterated sequences of language transmission and grammar induction with simulations involving populations of agents that learn their language by observing the linguistic behavior of the agents of the previous generation. The simulations are run to last several generations, typically starting with a holistic system of communication²¹ which, over time and after a number of cycles of transmission, eventually develops certain critical linguistic properties such as compositionality. Some relevant results are summarized below; see Smith and Kirby (2008) and Kirby et al. (2015) and references therein for a detailed exposition of these results, and Kirby (2017) for an overview:

(R1) A holistic communication system is guaranteed to evolve into a system with structured signals, provided that the two following conditions are met:

- (a) The meaning-space about which agents want to communicate is sufficiently rich and, hence, there is a potential for the emergence of a system with high expressivity; and
- (b) There is poverty of stimulus or, as put by the authors, there must exist a 'learning bottleneck' in the sense that learners are never able to observe all possible signal-meaning pairs of the system.

(R2) Assuming that learning biases (i.e., FL, UG or LAD) are innate, it is unlikely that these biases are language-specific, in other words, if there are any strong biases on language learning these are most probably of the domain-general kind.

On the basis of these results, the authors conclude that most of the observed features of languages (e.g., structured and regular signals) are adaptations to enhance learnability and expressivity, i.e., that languages gradually adapt to the cognitive biases of learners in order to become more learnable and as much expressive as possible;

²¹ A system of communication is said to use holistic coding if signals and meaning are paired in no systematic way and signals do not possess any structure reflecting the underlying semantic structure, unlike human language where words and phrases are typically analyzable into meaningful smaller units. Wray (1998) assumes that proto-language was holistic in this sense and this assumption has been directly incorporated into ILM. Note that, by definition, holistic systems cannot be open systems of communication since all signal-meaning pairs must be learned by rote. This is another relevant point of disagreement with the MP, since the ES clearly assumes that there exists continuity between the human system of signals and some ancestral one, while the MP does not foresee any continuity between human externalization and non-human communication systems.

Table 1 The two perspectives compared. References in the table point to some relevant textual evidence for the item in each row

Minimalist program			Edinburgh school	
		References		References
Target system	I-language	Berwick and Chomsky (2016); Chomsky (2021); Chomsky et al. (2023)	E-language	Hurford (1990); Kirby (1999, 2007a)
Main function	Generation of thought	Chomsky (2020, 2021, 2024a)	Communication	Hurford (1989); Kirby (2007a, 2017)
Ontological status	Internal	Berwick and Chomsky (2016); Chomsky (2021); Chomsky et al. (2023)	External	Hurford (1990); Kirby (1999, 2007a)
Main Explanatory goals	Learnability & evolvability	Chomsky (2021, 2024a, 2024b)	Learnability & efficiency (in communication)	Brighton et al. (2005); Kirby et al. (2015); Smith and Kirby (2008)
Principles justifying the reliability of knowledge claims	Galilean style (e.g., simplicity), SMT	Allott et al. (2021); Chomsky (2024a, 2024b)	Prioritization of externalist explanations	Kirby (2002); Kirby et al. (2007)
Main experimental & theoretical resources	Minimal capacity inferences	Berwick and Chomsky (2016); Everaert et al. (2015)	Computational modeling & experiment	Scott-Phillips & Kirby (2010); Smith (2012); Tamariz (2017)

indeed, learnability is often perceived as the main and distinctive property of human language compared to other animal communication systems with its openness (i.e., potential infinity) being, somewhat paradoxically, the key factor (Oliphant, 2002a, b). Additionally, these cognitive biases, which are biological and have evolved by natural selection, cannot be language-specific. Note, by the way, that, besides the pressures for expressivity, other functional pressures to improve the communicative efficiency of languages are also assumed to be relevant but weaker compared to the pressures for learnability.²² Thus, while it is generally accepted that learning, culture and biological evolution interact, biological constraints are assumed to play a minimal role in the shaping of languages because “the ILM demonstrates that we cannot draw a direct parallel between the innate properties of the language user and the structure of language” and, therefore, “given some universal property of language that we wish to explain, we should not necessarily place the whole burden of that explanation on an innate biological property” (Kirby & Hurford, 2002, 144).

In Table 1 I have tried to offer a comparative overview of the main tenets of the two perspectives, with some additional relevant references.

²² Similar conclusions are reached by Nick Chater, Morten Christiansen and colleagues (e.g., Chater & Christiansen, 2010; Christiansen & Chater, 2008, 2016), although these authors tend to put much more emphasis on the role of communicative efficiency and language use in the shaping of languages. Also, they usually express a stronger commitment with a particular theory of grammatical structure than the ES, namely Construction Grammar (Goldberg, 2013, for an overview).

3.2.2 Involvement or conceivability in addressing Darwin's problem?

Now, and given the considerations above, the question is whether it is possible to assess the grade of involvement with biology of each perspective, as claimed by Nefdt (2023a), and, of course, the possibility of ranking perspectives according to this, which is implicit in Nefdt's analysis; or, alternatively, whether the obvious differences between one and the other can be given a different explanation. Here, as I advanced at the beginning of the section, I'll try to justify my rejection of the idea of degree of involvement (and ranking) in favor of an explanation based on a purely perspectival view where differences between perspectives are explained by the fact that they are historically and culturally situated scientific practices influenced by such pragmatic factors as what Massimi (2022b, Ch. 5) calls physical conceivability.

There are a number of indicators we could use in order to estimate the putative degree of involvement of each discipline. One is the number of references to the biological literature focusing both on general matters and on the genetics and the neurobiology of language. In this case, for example, Berwick and Chomsky (henceforth B&C, which I shall also use as keyword to refer to the particular variety of the MP I am considering here) appear to be much more concerned with biology, especially with the most recent developments in genetics than the ES, as can be appreciated by consulting, for example, Berwick and Chomsky (2016) and Berwick and Chomsky (2017), even compared with recent works like Arnon et al. (2025).²³ Compare this with the ES explicit agenda to try to explain as much of language *without* actually appealing to biological constraints. There are a couple of considerations in connection to this that deserve at least to be mentioned, although a deeper analysis would require much more space than I can use here. This first one, concerns the strict acceptance (by both perspectives) of the Culture vs. Biology dichotomy that has often been called into question, for example by Oyama (2003), and to which I shall return briefly in Section 4; the second one—which is much more explicit in the case of the ES but to which the MP is not entirely alien—concerns the very notion of 'evolution': note that B&C do not consider the emergence of externalization and subsequent 'evolution' of languages to be a biological question, but rather something concerning the theory of language change, whereas the ES explicitly identifies glossogeny as an evolutionary process. Again, I shall add some additional considerations in connection to this in Section 4, but note that this has a lot to do with, using the terminology coined by Walsh et al. (2017), the 'substrate neutrality' of the statistical models applied,²⁴ directly importing a characteristic property of the Modern Synthesis (MS) orthodoxy that was originally exploited by Boyd and Richerson (1985) in their models of cultural evolution on which the ILM of the ES are based. I will not pursue a detailed analysis of these issues but I think it is important to highlight that both B&C and the ES seem to be strongly influenced by the biological perspectives from which they have developed their own accounts—because biology has also a perspectival

²³ I thank an anonymous reviewer for calling my attention to this reference.

²⁴ The idea of 'substrate neutrality' has to do with the fact that Darwinian evolutionary models may be applied to different populational phenomena even though the populations of interest are not biological populations (du Crest et al., 2023; Godfrey-Smith, 2009).

structure; see Walsh and Rupik (2023) and Balari (2026) for a slightly different take on the question with a special focus on the case of language.

There are two questions to which I shall nonetheless pay some more attention, especially to one of them, because they conform the two main axes along which Nefdt (2023a) articulates his criticisms of the MP and which he later takes to be determinant when taking sides in favor of the ES. The first one concerns B&C's saltationism and their construal of the biological evidence, which in the opinion of many researchers (Nefdt included) is more plausibly interpreted in terms of a gradual evolutionary scenario instead of a saltational one; the second one has to do with the issue of uniqueness and the purported anomalous status of language in the organic world.

As for the second point, Nefdt (2023a, 14, fn. 17) asserts that "biological uniqueness is a stronger claim that requires stronger evidence to prove". I don't think that it is my role here to take sides, but, yes, uniqueness claims in biology are very hard to maintain and even harder to underpin, and B&C's are no exception; see DiFrisco et al. (2023) for some general considerations, and Balari (2026) for the case of language and its putative status as an evolutionary novelty. As for the second point, the issue of gradualism vs. saltationism is a vexed one and an old debate already in biology since the development of the Modern Synthesis. It is not obvious however that this debate can be resolved as a black or white question and, of course, that it can be taken as the key point to assess the degree of involvement of one or the other perspective. I'll try to say a couple of things about this before presenting my own understanding of the matter.

Recall, first, that B&C conjecture, on the basis of their interpretation of the genetic and paleoanthropological data, that the emergence of language is a relatively recent event circumscribed to the human lineage and *therefore* that its innate basis cannot be too rich—this is the evolvability condition of Chomsky (2021, 2024a) and Chomsky et al. (2023), which squares perfectly with the explanatory standards set by the SMT (Chomsky, 2024a, b). Recall, also, that 'language' for B&C is an internal cognitive system. Compare now this view with that of the ES, where 'language' is a system of signals that has evolved from a primitive, possibly holistic, system of communication and that has acquired its properties through iterated processes of cultural transmission, where communication and innate learning cognitive biases have interacted to deliver its current shape. The ES has little to say about the evolution of these learning cognitive biases beyond that they must have a genetic basis and that this genetic basis might have co-evolved with cultural transmission processes (Hurford, 1990, 1992; Kirby, 2017; Thompson et al., 2016). Again, I believe that this is an issue intimately connected to the different biological perspectives from which each biolinguistic perspective has drawn most of its inspiration. Note that the majority of criticisms to the MP have focused on a reading of the non-gradual scenario consisting of a one-gene-one-mutation event with a direct or linear phenotypic effect giving rise to Merge (e.g., Chater et al., 2009; de Boer et al., 2020). This view is however not taking into account the widely established assumption among many biologists that the genotype–phenotype map is not linear and that the dynamics of development may have a strong influence on the phenotypic variation on which natural selection operates, giving rise to discontinuities in relatively short periods of time (Alberch, 1982, 1991); indeed, that "alterations of the genome are to some extent peripheral to

the problem of morphological change” (Müller, 1990, 99). Admittedly, this interpretation has often been fostered by Chomsky’s occasional references to “some rewiring of the brain” due to “a small mutation” (e.g., Chomsky, 2007, 14), but it is no less true that it has been qualified so as to assume “a more subtle relationship between genotype and phenotype than a reductive ‘one gene-one-mutation’ view” (Berwick & Chomsky, 2017, 168) that is entirely compatible with some contemporary views on the mechanisms of phenotypic change for which the appeal to punctuated processes is as standard and plausible as it is to gradual ones (e.g., West-Eberhard, 2003, Ch. 24), even with the “scenario in which genetic bases of our linguistic ability evolved through a gradual accumulation of smaller biological changes” proposed by de Boer et al. (2020) as an alternative to the suggestion that “the biological foundation of Merge is a single genetic mutation”. In general, I believe that the debate on gradual vs. non-gradual change has often been muddled by the (sometimes implicit) assumption that gradualism applies simultaneously at all levels of analysis, from the genetic to the behavioral one, when this is not necessarily true. Indeed, it seems that today the debate appears to have transitioned to a plurality of views (i.e., perspectives; Pievani, 2016) which clearly have more or less influence on each biolinguistic perspective given their respective preferences in terms of epistemic principles and patterns of explanation. I’m afraid that interpreting this in terms of a higher or lower degree of involvement with biology eventually reduces to the dubious presupposition that ‘this biology is better than that one’.

I think that a more suitable account is possible and the conceptual tools for its development come, again, from Massimi’s perspectivism and her notion of physical conceivability, which she defines thus:

p is physically conceivable for an epistemic subject *S* (or an epistemic community *C*) if *S*’s (or *C*’s) imagining that *p* not only complies with the state of knowledge and conceptual resources of *S* (or *C*) but is also consistent with the laws of nature known by *S* (or *C*).

(Massimi, 2022b, 150–151).

where *p* is a proposition articulated within some perspective. Now my point (and Massimi’s) is that perspectives articulate and present their scientific claims on the grounds of what is physically conceivable by the members of the community, which, in turn, is a function of the state of knowledge and conceptual resources available to each perspective. Of course, the origin of this knowledge and these conceptual resources are not always the same for all perspectives, as we have just seen for the case of the MP and the ES, and it is this what explains the differences that we observe at the level of the kinds of scientific claims advanced by each and the kinds of justifications presented to support them. An interesting aspect of Massimi’s notion of physical conceivability is, not only that it defines a space of potential paths to pursue in further research within the perspective, but also that it can change as the perspective follows one or another path within this space (Massimi, 2022b, 154–156). And changes in what is physically conceivable is precisely what we have observed, especially in Chomskyan biolinguistics in the last sixty years or so since its inaugu-

ration; the ES is perhaps too young for showing clear changes in its space of physical conceivability, although some signs of change can perhaps already be perceived especially concerning the conception of learning/developmental mechanisms (e.g., Singh et al., 2021).

That much is to be expected in any case of perspectival modeling, as it also is that Biolinguistics, qua discipline at the interface of (at least) linguistics and biology, somehow reflects the perspectival structure of biology itself. To be sure, as noted biology arguably also consists of a plurality of different perspectives, understood as historically and culturally situated scientific practices of real scientific communities, like the MS, EvoDevo, etc., with their own bodies of scientific claims, their own experimental, theoretical, and technological resources, and their own methodological-epistemic principles. Clearly, Minimalism and the cultural account do not draw their biological constraints from the same perspectival boxes and the perspectivism of biology percolates up into the biolinguistic approaches. For example, on the one hand, Minimalism has steadily drifted from a relatively close observance of the orthodoxy of the MS characteristic of the genetic determinism of earlier Chomskyan views, towards a more nuanced point of view which clearly incorporates a number of assumptions that are standard within EvoDevo, like the rejection of the gradualist dogma and the recognition of the active role of developmental (and other, non-genetic) processes in the generation of change. On the other hand, the cultural account feels much more comfortable with the basic tenets of the MS in assuming a relatively linear genotype–phenotype mapping and of developmental processes in general, including learning, and the gradualism of evolutionary processes. Of course these choices are not the result of sheer chance but are due to the respective preferences in terms of epistemic principles and patterns of explanation which quite neatly match the contrast between internalist and externalist models as characterized, for example, by Godfrey-Smith (1996), a point often recognized by the authors themselves (Chomsky, 2000b, Chs. 6, 7; Kirby, 2002, 196–197). Somehow inevitably these principles also have an impact on how each perspective identifies the causal processes that are supposed to explain certain phenomena (e.g., the causes of variation) in a way that mirrors certain debates within evolutionary biology about the connection of the aggregate actions of individual organisms and their analysis at the level of population dynamics (Pence, 2021, for an avoerview of the debate in evolutionary biology). Similar observations would apply to other methodological principles like simplicity (Terzian & Corbalán, 2021) and the so-called ‘Galilean Style’ (Allott et al., 2021) vs. the emphasis on computational modeling (Smith, 2012) and experiment (Tamariz, 2017). One could even argue that both accounts have constructed their own perspectival ontology (Massimi, 2022a, 2b) in the likes of such phenomena as Merge (or the computational system) —often referred to as organ-like (Chomsky, 1980)— and ‘language’ understood as an adaptive system —often referred to as organism-like (Christiansen & Chater, 2008), which are the main targets of their own perspectival exploratory models. But, as I shall try to show below, despite all these apparently irreconcilable features, both perspectives are partially overlapping and intersecting, which, in principle, makes it possible to assess their respective knowledge claims from the point of view of the other perspective which, as argued by Massimi (2018a),

is a key requirement to “secure *reliable scientific knowledge* production” (Massimi, 2022a, 223; her emphasis).

I really think that this is a much better explanation of the differences observed and one that, moreover, makes it possible the development of an account of the points of contact and similarities between perspectives, a point to which I turn in the following subsection.

3.2.3 Intersecting perspectives and assessing knowledge claims

Now, the knowledge claims discussed above are the product of the kinds of inferences sanctioned by the respective models constructed within each perspective. Recall that Massimi assumes that perspectival models are, by their very nature, exploratory, that is investigations of the possible.²⁵ Accordingly, true scientific knowledge can only emerge through the identification of inferences capable of grounding modally robust phenomena. Massimi’s idea of robustness is not too far from the one originally developed by Levins (1966) and further elaborated by Wimsatt (1981), which, essentially, points to the coincidence of results or conclusions reached by two or more different models. The question is whether we can identify some of these coincidences in the knowledge claims advanced by the MP and the ES. My contention is that we can, as I expect to show in the following brief consideration of the issues.

Take, first, the claim that most features of languages are not the product of strong innate biases, typically associated with the ES and the reiterated result obtained in their computational models that “at least some of the distinctive features of human language are adaptations to its cultural transmission, and cultural evolution potentially plays a major role in explaining why the human communication system has the particular features that it does” (Smith & Kirby, 2008, 3591). This is quite compatible with the contention that most (if not all) surface properties of natural languages are the result of “a process of problem solving using existing capacities found in other animals” (Berwick & Chomsky, 2016, 83) that has nothing to do with biological evolution. Similarly, on both accounts a rich (or human-like) conceptual system or “a representational scheme ... of sufficient complexity” (Kirby, 2007b, 256) is assumed to be an essential prerequisite such that “fundamental properties of language ... may arise from the structure of the thoughts language is required to express”

²⁵The exploratory nature of the MP has always been explicitly assumed by Chomsky since the very beginning, hence the use of the word ‘program’. The literature on modeling practices in linguistics is scarce, but from the few existing references I believe one may come to agree that the nature of the models is mostly exploratory. For example, Terzian (2021), in her examination of the Principles & Parameters framework that has the MP as its offshoot, concludes that P&P is a toy model that “qualifies as ... a ‘retrospective model’, and that in this capacity it deserves recognition as a fruitful vehicle of understanding” (Terzian, 2021, 1–2). Similar considerations apply to the computational models of Kirby and collaborators which, their mathematical sophistication notwithstanding, are extremely idealized (for obvious reasons) and “in a quite fundamental way ... explanatory incomplete” (Woensdregt et al., 2021, 213).

(Christiansen & Chater, 2008, 501).²⁶ Both perspectives also agree on the importance of the poverty of stimulus or learning bottleneck, although they clearly diverge on its explanatory role for certain properties like, for example, compositionality (Berwick et al., 2011). Indeed, compositionality is *one* of the bones of contention, the other one being gradualism. For B&C compositionality observed at the surface is a direct consequence of the compositionality of LOT which, together with other properties like structure dependence, follows from the computational operation Merge. Recall that Merge is assumed to be the product of a non-gradual evolutionary (i.e., biological) event. For Kirby and collaborators, compositionality is the result of the gradual adaptation of signal systems to the (compositional?) structure of the meaning-space through the action of domain-general learning biases which are the result of biological evolution.²⁷ Neither side has much to say about the origin of human-like conceptual systems and B&C hardly deal with the emergence of the grammatical systems through processes of grammatical change *once LOT is in place*.²⁸ Grammatical systems, however, *are* the target of the ILM paradigm; recall that in these models LOT (or whatever) is a given, a requirement for grammars to emerge out of holistic protolanguage through linguistic evolution or glossogeny, which is assumed to be the *same* process operating in language change. So, the ‘language’ which is the object of claims of gradual or non-gradual emergence is a totally different entity altogether in one case and the other: a piece of computational (or brain) machinery in the case of B&C and the complex consisting of a grammar and its output in behavior in the case of Kirby and others, a point that has also been made explicit by Berwick and Chomsky (2017).

This leaves us with the presumed domain-specificity of Merge and its non-gradual emergence. As for the first point, the possibility of its non-specificity has always been open and it is not ruled out that its operation might be eventually revealed in other species, not necessarily associated to communicative capacities (Hauser et al., 2002); for example, Berwick (2011) conjectured that Merge might be the basic operation underlying the structure of birdsong, although he attributed its presence in two so distant lineages to convergent evolution (see also Berwick et al., 2012). However, Balari and Lorenzo (2013) put up a long argument in support of the hypothesis that what is at stake in the case of birds and humans (and possibly other animals) is not convergence but homology of the same computational system in different species under different forms and functions (see Tomlinson, 2018, for some like-minded musings). So, B&C’s reluctance notwithstanding, the case is still clearly open.

²⁶ Which, by the way, essentially brings us back to square one, since this is the core of Bickerton’s original proposal; to wit: “[L]anguage was first and foremost a system of representation ... Precisely because of this, language was able to increase, by several orders of magnitude, not just the things but the *kinds* of things that creatures could communicate about. No mere communicative mechanism could ever have done this.” (Bickerton, 1990, 75; italics in the original).

²⁷ This interpretation of the results of Kirby’s models on the emergence of compositionality is not just mine. Thus, for example, Skyrms (2010, 146) also comes the conclusion that it is signal systems that eventually adapt to the “structured mental meanings that are meant to be conveyed”.

²⁸ But see Niyogi & Berwick (1995, 2009) according to whom language change may show both gradual and punctuated processes, although this has traditionally been a debated issue among language change scholars; see Lightfoot (1999) for discussion.

There are certainly many blanks to fill in, but the comparison above leaves us with the skeleton of a basic scenario for the evolution of the human linguistic capacities in which we can identify three basic milestones:

- (i) The emergence of a rich conceptual system.
- (ii) The evolution (or perhaps co-evolution) of a cognitive system, possibly non-human- or language-specific, capable of accounting for some deep properties of natural language like compositionality, given its close connection with the conceptual system.²⁹
- (iii) The emergence of surface and language particular properties, like phonology, morphology, word order, etc., through the action of glossogenetic processes capable of modifying the structure of signal systems.³⁰

Brief as it is, I nonetheless believe that this sketch suffices to show that perspectival realism provides a much more solid collection of conceptual tools to analyze the status and structure of Biolinguistics as an emerging interfield theory organized into a collection of partially intersecting perspectives. It also proves helpful in the assessment of the relationships that exist among the different perspectives in terms of overlap and intersection (Massimi, 2022a) and their capacity to create reliable scientific knowledge. Indeed, I think this view offers a much richer picture of the field than others that have interpreted this overlap in terms of convergence or rapprochement (e.g., Pleyer & Hartmann, 2019), or as signs that unification into a single and comprehensive framework is possible (Nefdt, 2023a), a question that I explore in the following section.

4 Not unification but a richer perspectival structure

In order to present the final collection of ideas I wish to put forth in this paper let me take some stock. In Section 2 I have defended the status of Biolinguistics as discipline, perhaps an interfield theory, partially independent from Linguistics but closely related to it through a number of historical, cultural and scientific connections. In Section 3 I have argued that within Biolinguistics a number of scientific perspectives may be identified, understood as the practices of real scientific communities, which I illustrated by comparing the MP and the ES. There, I described the differences but I also tried to emphasize the points of overlap and intersection between the two with the aim of highlighting their potential to create reliable scientific knowledge about

²⁹ What remains open here is the nature of the mechanisms underlying the cognitive system. B&C clearly bet on a computational system implementing a structure building operation (i.e., Merge) that would account for such phenomena as structure-dependence and compositionality; the ES is not very explicit about the nature of the mechanisms beyond the assumption that they are learning mechanisms perhaps implementing some kind of Bayesian inference.

³⁰ The main controversial point here is whether there has been continuity between full-fledged human languages and some ancestral system of signals. The ES models are all constructed with the assumption that there was, while B&C assume that the creation of the amalgam between I-language and externalization is something entirely new, with no connection with ancestral communication systems.

the phenomenon of language. Towards the end, I also hinted at the idea that not only this view better captures the actual state of the field but also that this pluralism is desirable and the only reasonable way to go, because any attempt at designing new, more ecumenical frameworks implies the more or less implicit attempt to revive the view from nowhere but just results in the introduction of some new perspective. My goal in this section is to argue that ecumenism is not only not advisable, but is impossible given the complexity of the world.

To do so, I'll start first looking at a couple of ecumenical projects, the Maximalist Program (MP+) sketched by Nefdt (2023a) in the final section of the paper and the one proposed by Arnon et al. (2025). This will offer me the opportunity to introduce a complementary notion of perspective, this one with a more ontological flavor and due to William Wimsatt (e.g., Wimsatt, 1994), which is partially coincident with the notion of perspective developed by Massimi through its connection with Massimi's characterization of phenomena. The section will culminate with a proposal for a refurbished agenda for Biolinguistics constructed on the basis of Wimsatt's notion of perspective, with the potential to accommodate diverse epistemic communities (i.e., perspectives in Massimi's sense) working on one or more of the kinds of problems posed by the different perspectives (in Wimsatt's sense).

4.1 On the pointlessness of ecumenism

"[T]he knowledge we get comes from one perspective or another, not from no perspective at all. Multiplying perspectives does not eliminate perspectives" (Giere, 2006, 92). These words by Giere should already be an indication of the point I want to make here, but let me elaborate a bit more using Nefdt's and Arnon and collaborator's projects as supporting evidence.

Starting with Nefdt, he sees his MP+ as a broad general framework he advances with the hope that it "allows for the possibility of some compatibilism between the approaches" (Nefdt, 2023a, 19),³¹ where 'the approaches' are, at least, the MP and the cultural account discussed in the previous section. I've already shown that there exists some degree of overlap between the two and, to the extent that there seem to be some shared goals I believe it would be fruitful to examine Nefdt's proposal under the light of the perspectival account I sketched.

The core of Nefdt's proposal is to pitch for a *systems biolinguistics* molded on the same methodological principles as systems biology (see, e.g., the papers in Boogerd et al., 2007). His reasons are that "genuine discovery with relation to language evolution is only possible at the systems level" (20) because "[l]anguage is a complex system in which robust structures emerge from the dynamic interaction of multiple interconnected parts" (23), a definition he arrives at after a brief examination of the phenomenon of language under the lens of standard characterizations of complex systems (e.g., Ladyman & Wiesner, 2020). The MP+ may therefore be seen as an alternative approach to meet Saussure's challenge very much along the lines of the perspective developed by Hurford and Kirby, which Nefdt often pinpoints as very close to his own views (e.g., 25). Nefdt's presentation is inevitably sketchy but a

³¹ From here on, numbers within parentheses refer to pages in Nefdt's paper.

number of central themes may be inferred from his account. Thus, for example, he advocates for a shift towards a more processual ontology (Nicholson & Dupré, 2018) and a less dichotomic approach of the organism-environment relationship, very much along the lines of the pioneering work of Lewontin (1983) that influenced the Developmental Systems Perspective (Griffiths & Gray, 1994; Oyama, 2000) and other systems-oriented views in contemporary biology (e.g., Callebaut et al., 2007). Methodologically, Nefdt puts a strong emphasis on network science (Newman, 2018) as the main modeling tool to capture the intrinsically interactive nature of systems, both at the intrasystemic level and at the intersystemic level, and as the best “way to integrate information from different systems as networks of networks” (32), from the socio-cultural level at which human agents interact to the neuro-cognitive level at which individual processes of language comprehension, production and acquisition occur. The view therefore shares many elements with others, more or less indebted to the seminal ideas put forth by Herbert Simon in the early 1960s (Simon, 1962, 1996) and his hierarchical view of nature as a near-decomposable system of systems (e.g., Deacon, 2003, 2006; Wimsatt, 1994, among others).

A major shortcoming of Nefdt’s proposal concerns, in my opinion, his apparent radical holism, suggested both by his occasional praise of this kind of approach (e.g., 21) and his reiterated negative remarks on the MP’s “isolationist approach” (24). Leaving aside the potential contradiction of defending holism and, at the same time, favor a hierarchical approach to complex systems (Wimsatt, 1974, 1976), Nefdt’s strictly hierarchical approach runs the risk of neglecting certain relevant phenomena that would, for example, be captured by Wimsatt’s notion of perspective. Wimsatt defined perspectives as “intriguingly quasi-subjective (or at least observer, technique, or technology-relative) cuts on the phenomena characteristic of a system, which needn’t be bound to given levels” (Wimsatt, 1994, 256); in later work he observed that perspectives constitute “a descriptive causal and explanatory niche for or cross section on the system” (Wimsatt, 2021, 32). Note that Wimsatt’s view defies the strictly hierarchical and compositional view of Deacon’s three levels of emergence and allows for a richer approach to complexity in recognizing, both ontologically and epistemically, areas of interest that often transcend a neat arrangement into levels of organization. Thus, for example, Wimsatt mentions the difficulties in shoehorning a levels view when considering the “anatomical versus physiological, biochemical versus developmental, or functional versus genetic versus evolutionary mappings of the significance of different subsystems of the organism” (Wimsatt, 1976, 254), which clearly calls for a multiperspectival characterization of complex systems that nevertheless does not imply that “the articulated sum of perspectives ... will exhaust its description or causal interactions” (Wimsatt, 2021, 32).

Nefdt’s proposal should be compared, for example, with that of Arnon et al. (2025), which in principle looks like a unified framework to investigate the origins of language but which defends “that studying language evolution lies well within the scope of scientific enquiry when new sources of data and theoretical perspectives are incorporated” (Arnon et al., 2025, 1). The paper examines with some detail three of these perspectives—the study of vocal production and learning, the study of the emergence of linguistic structure, and the study of the social underpinnings

of language, which would easily match Wimsatt's definition of perspective.³² That much suggests that a true multiperspectival Biolinguistics implies the recognition of perspectives beyond the reach of systems biology and capable of offering complementary views to the broad top-down approach proposed by the ES and the emergentist framework proffered by Deacon that Nefdt appears to favor. So, the MP+ cannot really be taken as a project of unification but just as a new perspective to be added to the already existing ones.

Now, and to close the circle, note that Wimsatt insists on the fact that levels and perspectives have an ontological dimension (in addition to the epistemic one) to the extent that they are robust and "[t]hings are robust if they are accessible (detectable, measurable, derivable, producible, or the like) in a variety of independent ways" (Wimsatt, 1994, 211); see also Wimsatt (1981). Additionally, robustness, is a property that is inferred on the grounds of multiple detectability: "Inferential robustness is the detectability, measurability, or derivability of an object or property by multiple independent and individually fallible means" (Wimsatt, 2021, 25). And robustness is precisely what connects Wimsatt's perspectives with Massimi's, since, I contend, the former are identified through robust inferences within different perspectival models, which is what bestows their ontological status.³³ A detailed analysis of this idea would require a full paper on its own, but I surmise that it is on the right track.

4.2 Enriching the perspectival landscape

To conclude, then, and inspired by a draft proposal by Balari and Lorenzo (2021) for an evo-devo of language and cognition, I would like to reinterpret Balari and Lorenzo's problems as a collection of perspectives (*sensu* Wimsatt) that might be relevant in this endeavor, each requiring perhaps the further identification of new perspectives (in both senses in this case), as they cut across those of Chomsky and Kirby and perhaps others. The proposal is also influenced by Wagner (2016) and his quasi-perspectival approach in terms of 'styles' of thinking in biology, namely 'population thinking', 'tree thinking' and 'homology thinking' (see also Ereshefsky, 2012; O'Hara, 1997). My list is not necessarily exhaustive and I present it in an order that does not reflect the major or minor importance of the view in question. The first item in the list, however, is the developmental perspective, which, I submit, impinges on the rest more or less directly. The list is this:

The developmental perspective Somewhat surprisingly, developmental considerations are notably absent in both the MP and the culturalist view. To be sure, the MP's account of language acquisition still relies on a mysterious process of parameter setting, but little is said about the actual mechanisms participating in this process or, for that matter, what role is Merge supposed to play (Longa & Lorenzo, 2008; Lorenzo & Longa, 2009). Similarly, the Kirby models basically rely on the traditional learned vs.

³² Arnon et al. (2025) use the word 'perspective' without actually suggesting that it is a technical use of the term, but I believe that the match with Wimsatt's notion is quite clear.

³³ Massimi has, for all I know, never established this connection with Wimsatt's perspectives, but she came close to it in Massimi (2022b, 119), where she discusses robustness and cites Wimsatt (and Levins).

innate dichotomy and assume a process in which a ‘language-ready brain’ internalizes pieces of grammatical knowledge through some kind of learning process more or less influenced by innate biases. Thereby both approaches effectively black-box development overlooking the fact that “the phenotypic traits, the patterns of behavior, whether classified as learned or instinctive ... must all be reconstructed from the changing complex of developmental interactants or resources at hand” (Oyama, 2003, 181); indeed both approaches hold out quite comfortably without questioning such ‘developmental dualisms’ mentioned by Oyama as inherited vs. acquired, innate vs. learned, transmission vs. development, and biology vs. culture, when it is not at all obvious that such dichotomies make actual sense in the case of language, or other aspects of cognition for that matter (Balari & Lorenzo, 2015b; Balari et al., 2020). Interestingly, similar criticisms have been raised towards traditional dual-inheritance theories in connection with cultural evolution; see, for example, Wimsatt and Griesemer (2007), Wimsatt (2013), and Charbonneau (2021).

The homological perspective Balari and Lorenzo (2021) included the problem of ontology in their list, but I believe it belongs more properly in here. Their reason for keeping it as a separate category is the pervading confusion that persists when it comes to identify the referent of the word ‘language’ as used by different perspectives; but this inevitably affects any claims identifying it as unique or as an evolutionary novelty, which, I believe, justifies collapsing the ontological question with that of homology vs. novelty. Recall that ‘language’ has variously been characterized as unique, as an autapomorphy or as lacking clear homologs. These claims hardly ever have the same target (if one is capable to identify a clear one), as it can be a behavior, the mechanisms underlying this behavior, some internal mental capacity (e.g., LOT), you name it. Such a confusion has its roots in biolinguists’ inability to address two closely connected problems: (i) the lack of accuracy at the time of defining the trait or character to which such claims are supposed to apply, which partly has to do with (ii) the indeterminacy as to what level of analysis the character in question belongs. The first problem is an effect of the systematic confusion in the use of the terms ‘trait’, ‘character’, and ‘phenotype’, where a ‘trait’ is simply a property of an organism, while ‘characters’ are traits individuated across species and ‘phenotypes’ are traits individuated within species (Figdor, 2022, 2024); but traits, characters and phenotypes may be defined and characterized in slightly different ways depending on the biological perspective and its particular explanatory goals, that is on whether the focus is on adaptation, phylogenetic analysis or the historical nature of homologs. Whence the potential compatibility of such claims that language is an autapomorphy (tree thinking), grounded in computational mechanisms that constitute a Type II novelty that is homologous to similar mechanisms in other species (homology thinking; Balari & Lorenzo, 2015a) and even that E-languages are adaptations to said underlying mechanisms (population thinking; Christiansen & Chater, 2008; Kirby, 2017) or the instantiation of a certain class of developmental hybrid (Balari & Lorenzo, 2018); see Balari (2026) for an in-depth analysis of the question of homology in connection to language.

The adaptationist perspective All considerations above have a direct impact on what (and if) has been selected, a question that inevitably meshes with the question of the units of selection, understood as “the biological forms of organization that have the required properties to be causally affected by natural selection and/or to evolve by natural selection” (Suárez & Lloyd, 2023, 3). Concomitantly, since the problem of selection is in the province of population thinking, we have the problem of adaptation, which may require different answers depending on the forms of organization pinpointed by the different perspectives as the objects of selection. In particular, in this context, I think it is particularly relevant the debate on whether culture evolves or not; see Lewens (2015, 2024) for a detailed examination of the issues and Fracchia and Lewontin (1999) for some arguments against cultural evolution.

The representational perspective Focusing more on the cognitive aspect of human linguistic abilities, any biolinguistic project will eventually be faced with the problem of representation(s) (Cummins, 1989; Ramsey, 2007; Shea, 2018), which, following Cummins (1989), may be succinctly characterized by the question: in a biological system, what can a representation possibly be and how does it represent? Here, in addition to the question of mental representation as traditionally discussed in the context of the philosophy of mind (of which the works cited above are but three reference points within the massive amount of publications on the topic), I wish to include what Jackendoff (2007) calls *mental structures*, which he defines as “what is in the mind” when a subject perceives a linguistic utterance and which are “best not thought of as a representation or a symbol of anything” (Jackendoff, 2007, 5); in other words, mental structures are what linguists represent (on paper, on a blackboard, on a screen) when they draw syntactic trees, attribute-value matrices or autosegmental tiers, i.e., these graph-like objects are models of mental structures. Thus, mental structures, as opposed to mental representations, don’t have content or, if they do, they have it (and acquire it) in a very different sense from which, say, the concept DOORKNOB has it (and acquired it). Of course, mental structures and mental representations may enter into contact in the processes of language production, comprehension and acquisition, but they are not necessarily the same kinds of entities: one way to think about them, using Chomsky’s distinction between thought and externalization, is to see representations as having something to do with the former and structures as having something to do with the latter. As pointed out by Balari and Lorenzo (2021, 1229), “it may seem that the problem of representation(s) has little to do with either evolution or development, but nothing could be further from the truth. Indeed, taking language as a paradigmatic case, it is reasonable to suppose that representations [and structures; the author] are also subject to developmental [or reproduction, and selection; the author] processes” (e.g., Millikan, 1984).

The computational perspective This perspective is closely connected to the previous one and it is based on the assumption that (at least some) cognitive processes are computational (or can be characterized or modeled computationally). While the idea is controversial, starting with the problematic nature of the notion of ‘computation’ (Haugeland, 2002; Smith, 2002), a number of philosophers have strived to develop

more precise definitions thereof (Anderson & Piccinini, 2024; Fresco, 2014; Piccinini, 2015; Shagrir, 2022) that point in the direction that the idea that cognition is (some kind of) computation is here to stay (Miłkowski, 2013; Piccinini, 2020). For example, Piccinini (2015) identifies as the landmark of computation the ability to process a special class of physical entities according to rules that are sensitive to certain formal properties of said entities; he refers to the physical entities that are the objects of computation with the term *vehicles*, which would correspond to the representations and structures introduced above. Piccinini's definition is useful here because it serves to illustrate how investigation into the nature of computation may inform research in the neurobiology of language, as it is the case, for example, of Embick and Poeppel's project of 'using language to learn how the brain works' (Embick & Poeppel, 2015; Poeppel, 2012; Poeppel & Embick, 2005). Embick and Poeppel's proposal is to take seriously the representations/structures and the operations over them postulated by linguists in order to support the study of computation in the brain, instead of just pursuing the standard bottom-up approach in neuroscience that seeks to validate these categories by neurobiological data, a proposal that has inspired some interesting research in the field already, as it is the case of Murphy (2021, 2024), which I take to be a perfect example of an investigation with a focus on a specific perspective, in this case the computational one. This is only an example, but one that shows how Bechtel and Abrahamsen's (2010) dynamic mechanistic explanations may be sought also in the case of language by applying this strategy. Yet, the computational perspective does not reduce to the question whether cognitive processes are computational or not, since the contribution of the theory of computation goes well beyond this. To be sure, as suggested independently by a number of researchers (Balari & Lorenzo, 2013; Cantlon & Piantadosi, 2024; Frixione, 2001; Tsotsos, 1990; van Rooij, 2008), the analysis in terms of computational complexity of the problems faced by specific cognitive tasks is essential even if the answer to the question above is a negative one. This idea finds support in Scott Aaronson's arguments that there are no reasons "to believe that humans *can* solve NP-complete problems in polynomial time, and excellent reasons to believe the opposite" (Aaronson, 2013) —see also Aaronson (2005); but there exist already some proofs that certain cognitive *theories* are NP-complete (Rich et al., 2020; Ristad, 1993; van Rooij et al., 2019), which evokes Jerry Fodor's perplexity when recognizing that "[i]t appears that the processes by which we evaluate nondemonstrative inferences for simplicity, coherence, conservatism, and the like are *both* sensitive to global properties of our cognitive commitments, and *tractable*. What cognitive psychology would like to understand, but doesn't, is how on earth that could be so" (Fodor, 2008, 124; Fodor's *italics*). Now, as argued by van Rooij (2008) these results should not discourage us, but should rather spur further research in revised theories and in the identification of constraints capable of capturing the tractability of these tasks; see also (van Rooij et al., 2012, 2019). There is no reason whatsoever to believe that humans (and animals) go about solving NP-complete problems by sheer magic.

The comparative perspective Roughly corresponding to Wagner's tree thinking, this perspective spans several levels of analysis, depending on the character or phenotype we want to focus on. In general, we will be interested in identifying the mechanisms of change and in determining whether there are constraints on what is possible. I therefore submit that the notion of possible vs. impossible language is not meaningless (Moro, 2016), just as the notion of possible vs. impossible animal is not senseless either (Alberch, 1989, 1993, 1998). Thus, we will be interested in finding what are the mechanisms of linguistic change, but also in establishing what is the relevant level at which change takes place, E-language, I-language, or both (Labov, 1994; Lightfoot, 1999), and whether there are biological and cultural biases and to what an extent it is possible to tell them apart. Finally, and in intimate connection with the homological perspective, we may want to study intraspecific variation at the level of the cognitive phenotype and its impact on the behavioral phenotype, but also, and to the extent that one wants to take seriously Balari and Lorenzo's (2013, 2015a) homology claims, we may want to study interspecific variation and how it is realized in different animals under any variety of form and function (Balari, 2026).

As I advanced above, this list is probably not exhaustive and each perspective may well be broken down into different but related subperspectives. Thoroughness was not my goal here, though, only the wish to sketch some of the many possible perspectives from which the biolinguistic enterprise may be pursued.

5 Concluding remarks

In the wake of Ryan Nefdt's philosophical analysis of Biolinguistics, I have presented a number of proposals—some complementary, some alternative—that point in the direction of its recognition as a full-fledged discipline or, perhaps, an interfield theory. My approach has been a perspectival one, both in the spirit of Michela Mas-simi's perspectival realism, but also following William Wimsatt's investigation of complexity, where perspectivism also plays a key role, sometimes overlapping Mas-simi's view, but crucially introducing an ontological dimension into the notion of perspective. In doing so, I focused on what today may be considered to be the two mainstream frameworks within Biolinguistics, namely Chomsky's MP and Kirby's culturalist approach, in order to illustrate how an analysis in terms of perspectives seems to better carve the geography of the issues than other proposals. This allowed me to identify a number of additional perspectives that may concern anyone interested in the biolinguistic enterprise, which I put forth here to complement, qualify and, in some cases, replace the traditional research questions of linguistics that have driven research in linguistics and generative grammar for the past forty years. It is my hope that some of these proposals may encourage and foster further research in both Biolinguistics and its philosophy, just as Nefdt's work has been an inspiration and a stimulus for me in writing this paper.

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Declarations

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