Misused Terms in Linguistics

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with replies by
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Misused Terms in Linguistics

Evelina Leivada

The evolutionary biologist Eörs Szathmáry observed that linguists "would rather share each other's toothbrush than each other's terminology."[1] This is far from an isolated view. Peter Hagoort, an eminent cognitive neuroscientist, voiced similar concerns.

> When even at major linguistics conferences the program contains presentation[s] such as "A short tour through the minefield of linguistic terminology," one should realize that this state of affairs is a serious threat to the influence that linguists exert on the research agenda of cognitive science broadly.[2]

Tanja Kupisch and Jason Rothman, psycholinguists working primarily on bilingual development, recently noted that "[o]nce offered to the public domain, terminology can have far-reaching and long-lasting effects, even—perhaps especially—when these are unintended by their original promoters."[3]

An effort to improve the terminological clarity and coherence of theoretical and experimental linguistics is long overdue. In this respect, linguists might consider following the lead of psychologists in identifying and discussing lists of inaccurate, ambiguous, misused, and polysemous terms.[4] The focus throughout should be on key notions of the field. Terms such as feature, parameter, (grammaticality/acceptability) judgment, (language) universal, and Universal Grammar are omnipresent in linguistics.[5] These notions are fundamental to the discipline and their misuse has important implications, not only for the coherence of the field, but also for its standing in the broader context of cognitive science. The following terms do not all satisfy the same criteria of inaccuracy, ambiguity, and misuse. The degrees to which they exhibit these characteristics vary, and this is part of the problem. This review will not focus solely on the conceptual clarity of these ten terms, but also on their inconsistent usage.

1. Universal Grammar and Language Universals

In Noam Chomsky's work, Universal Grammar (UG) is the source of our innate ability to acquire and use a natural language.[6] The faculty of language consists of certain principles that are innate, common to our species, and available prior to any systematic exposure to a given language.[7] UG is sometimes identified with linguistic universals; but this is a mistake. When Chomsky talks about language universals, he does not refer to properties that are universally attested to in all languages, but to computational properties of the mind that are universal because they arise from a species-universal innate ability.[8] A property P in a given language can reflect a universal computational principle even if P is not attested to in another language.[9]

There is no reason to assume that linguistic universals, understood as properties that are shared across languages, are necessarily derived from UG. Although most languages settle on a consistent word order,[10] this preference does not reflect the imperatives of UG. Infants process and reshape input in a way that promotes the regularization of harmonic patterns.[11] In fact, as Chomsky has argued, language development is guided by principles of general cognition, which work together with linguistic experience and UG.[12] Given that a complete list of all the UG principles has not been compiled, the possibility that these principles are, for the most part, not language-specific, but have cognitive, third factor roots, cannot be ruled out.

2. Parameter

In its early days, UG was thought to consist of principles, which are invariant across languages, and a finite set of open parameters that are fixed by experience. The head-directionality parameter, for example, determines the position of the defining element of a phrase in relation to its complements. Some languages such as English are head-initial; others, such as Japanese, head-final. Setting one parameter enables predictions about the others. The original aim was to capture the panorama of cross-linguistic variation while positing as few parameters as possible.[13] How many are a few? Twenty? Eighty? 1,534? 1,465,462? Nobody has an answer to this question, and I suspect nobody ever will. Over the last two decades, the term "parameter" has been applied to every possible point

of variation across languages, eventually giving rise to parameters of various sizes and domains of prediction—macroparameters, mesoparameters, and nanoparameters. This terminological fluidity is not innocent. Apart from having stripped the notion of a parameter of its original substance, it endowed the term with various custom-made definitions and objects of inquiry. Anybody can postulate a new nanoparameter, even on the basis of one lexical item. In such cases, the term "parameter" is still used despite it no longer carrying the same crucial implications as in its original context. Microparameters, for instance, make no robust, whole-grammar predictions of setability. Yet we cannot afford to dispose of these predictions. The notion that UG consists of parameters, which provide a guide into the variation space, relied on these predictions. One cannot simply use the term "parameter," or variants of the term, with an arbitrary meaning.

Terminological fluidity is not without consequences. In the case of the term "parameter," its misuse over the years raised serious concerns about the biological plausibility of an innate endowment for language that consists of millions of minimal points of variation. "If the number of parameters," Frederick Newmeyer observed, "needed to handle the different grammars of the world's languages, dialects, and (possibly) idiolects is in the thousands (or, worse, millions), then ascribing them to an innate UG to my mind loses all semblance of plausibility."[14]

3. Feature

If parameters come with various definitions and are often custom made, so are features. "Custom made" refers to proposals of the following form: feature A is proposed for language B on the basis of an item/structure C. In such proposals, the postulated primitive is purely data driven and designed specifically to fit a portion of the data.[15] As partial descriptions of linguistic objects,[16] features can be anything from roundedness in phonology to the extended projection principle (EPP) in syntax.[17] These are both very different. Calling almost anything a feature creates both disciplinary and interdisciplinary concerns.

Where do features come from? For some scholars, the answer is UG itself: "The study of the feature inventory of UG requires a massive database compiled on the basis of detailed studies of particular grammars."[18] Ascribing an inordinate number of primitives to UG is not an innocent move. It should not be a desirable move either, because this would mean that the basic atoms of linguistics are not derived, but postulated as innate. Linguists propose new features at will, but there is no theory of UG that can explain the existence of the requisite number of primitives. How many exactly? Again, there is no answer, because anything can be a partial description of a linguistic object. As Norbert Hornstein has pointed out, the real problem is that we have no hint of a theory of features.

Postulating the existence of a feature A in order to explain a structure B does not mean that we have shed light on our biological ability for language.[19] Such inventions come at a high cost.[20] Both field-internal terminological coherence and field-external visibility are diminished as a result. Using the same term to talk about very different things inevitably reduces internal coherence and consistency. Meanwhile, a biologist would have a hard time understanding what *kind* of primitives are classified as features in linguistics. This is problematic under the view that features are part of UG—our biological endowment for language. One could, of course, say that "features are properties of syntactic atoms," whereby "a feature [plural] for example is used analogously to chemists' use of H for the real-world thing hydrogen."[21] This analogy is unpersuasive. The definition of a chemical element is specific and unambiguous, made exclusively on the basis of the number of protons within its nucleus. But linguists use the EPP in different ways across research groups.[22] Additionally, we know exactly how many chemical elements have been discovered or created: 118. We can and do keep a count. There is a consensus that this is the right number; it is not a matter of one's definition or viewpoint. The same claim cannot be made for features.

4. The Linguistic Genotype

Linguists often talk about the genetic endowment *for* language; and this suggests that a part of the human genome is dedicated *to* language. There is no dispute that our species is biologically language-ready. This does not justify the claim that a portion of our genome is dedicated to language. According to Ana Villar and Antoni Gomila:

> While it is obvious that any human mental capability requires a genetic make-up as long as it is a biological phenomenon, it cannot be simply assumed that there is going to be some part of the genome specialized for every mental faculty, or for language in particular. Not only because genes can be pleiotropic (i.e., involved in very different processes), but also because they don't work in isolation.[23]

A direct link between genetic makeup and linguistic phenotypic traits is untenable. First, genes do not code for grammatical properties. Second, a set of genes can affect multiple, potentially unrelated, phenotypes so that no direct, one-to-one relation exists between genes and phenotypic traits. Third, genes are not the only players in ontogenetic processes. Epigenetic factors also regulate behavioral functions.[24]

Chomsky uses the term "genetic endowment." But he is also one of the few linguists to acknowledge that working out the process of genetically fixing the values

of linguistic primitives is a far from trivial task. Spelling out the right version of innateness that underlies our ability to do language is an enormous challenge. Hornstein rightly suggested that linguists interested in meeting it would effectively be biologists, regardless of their success.[25]

5. Faculty of Language in the Narrow Sense

In 2002, Chomsky, Marc Hauser, and W. Tecumseh Fitch introduced a distinction between the language faculty in a narrow and broad sense (FLN and FLB, respectively).[26] FLN was described as the "only uniquely human component" of the language faculty, while FLB included "a sensory-motor system, a conceptual-intentional system, and the computational mechanisms for recursion, providing the capacity to generate an infinite range of expressions from a finite set of elements."[27] Although FLB is clearly defined, and some of its components are unambiguously identified, FLN is harder to pin down.

Chomsky, Hauser, and Fitch defined FLN as a component unique to human language, possibly consisting only of recursion. In 2005, they suggested that

> [t]he contents of FLN are to be empirically determined, and could possibly be empty, if empirical findings showed that none of the mechanisms involved are uniquely human or unique to language, and that only the way they are integrated is specific to human language.[28]

In 2009, Fitch then argued that "FLN was intended to have a considerably narrower scope, perhaps even denoting an empty set, but has been read simply as 'language' by some and 'Universal Grammar' by others."[29] This last view points to the terminological confusion that arose in the various readings of the term "FLN." This confusion is unfortunate because the original distinction was "intended as a terminological aid to interdisciplinary discussion and rapprochement."[30]

The identification of FLN and UG is wrong. If UG equals FLN, and if FLN is, indeed, an empty set—a possibility that Chomsky has once again left open in his latest book with Robert Berwick[31]—scholars outside generative linguistics would inevitably question the need to assume a UG-shaped form of innateness. Furthermore, if UG and FLN are indeed the same, why are two terms needed to denote one object?

But, of course, FLN is not the same thing as UG. FLN is unique to humans and unique to language. UG does not have this character.[32] UG is relevant to both FLN's language-specific properties, and FLB's non-language-specific properties. Clearly, FLN is not tantamount to UG. Any suggestions to the contrary represent a misuse of two important terms in linguistics. It is a mistake that many linguists have made.[33]

6. Hardwired

Aside from inborn reflexes, remarkably few abilities are hardwired in humans.[34]

Linguists have talked about hardwired UG,[35] hardwired principles and parameters,[36] hardwired syntactic categories,[37] hardwired Merge,[38] hardwired semantic relations,[39] and hardwired systems of lexicalizable concepts.[40] According to common assumptions, something hardwired is both pre-programmed and behaviorally inflexible. Is the development of our innate ability for language behaviorally inflexible? Do children develop language in a way that is not affected by the environment? Feral children do not receive linguistic input in infancy. What impedes their development of innate syntactic categories? How can we meaningfully talk about a critical period for first language acquisition if a great deal of linguistic machinery is hardwired and will be inexorably manifested?

Few linguistic concepts are hardwired. It is not entirely clear that the brain itself is hardwired. This is evident from the lack of consensus on a number of critical issues, such as the lifetime of neuronal spines.[41]

7. The Metaphors of Language Development

In *Aspects of the Theory of Syntax*, Chomsky argued that

> [l]inguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance.[42]

In his later work, Chomsky introduced the notion of instantaneous acquisition.[43] This is a metaphor for an idealized version of acquisition, one that abstracts away from actual developmental stages on the assumption that, barring extreme cases, these stages are uniform and have no impact on the acquired grammar.[44] Chomsky took care to highlight that these are *idealizations*.[45]

Eventually, Chomsky's completely homogeneous speech community and ideal speaker became embodied in the idea that, absent a severe pathology, adult performance is essentially homogeneous.[46] This is demonstrably false, since cognitive phenotypes are not always binary. Individuals with a pathogenic variant of a gene can be impaired in different ways. This may lead to different cognitive phenotypes, at times not even reaching a cutoff point where the diagnosis of a specific pathology can be made.[47] Variation can be found in the absence of any pathology even among speakers of the same language, and even within a speaker past the acquisition period.

Over the last few decades, some linguists converted these idealizations into facts. Consider the claim of linguist Guglielmo Cinque:

> The problem of parameter setting is usually posed in the context of the simplifying assumption that acquisition is instantaneous (cf. Chomsky 1975, pp. 119–22). My point is that such an assumption may not be just a convenient (and innocuous) idealization. It may well prove to be the only pertinent way to approach the question. Considerations of the actual stages of acquisition … run the risk of distorting matters.[48]

Cinque suggests that we can approach the question of language acquisition by focusing on primitives he takes to be innately available, while ignoring both the way acquisition unfolds and the role of the environment. The problem is that the metaphor of instantaneous acquisition assumes that innateness and the environment are fully separable. But innateness does not work alone because genes do not work alone. Development is rarely hardwired,[49] and language acquisition cannot be an instantaneous, almost reflex-like realization of innate structures and primitives. The metaphors of language development were once useful. It is through their subsequent use that they became unhelpful. Catchy metaphors, of course, make for popular quotes that conveniently express half-truths. And it is for this reason that they should never be taken too seriously or treated as dogma. According to Norbert Wiener and Arturo Rosenblueth, "the price of metaphor is eternal vigilance."[50]

8. Grammaticality Judgment

Despite being frequently used, "grammaticality judgment" is a problematic term. Some linguists have argued that acceptability and grammaticality are distinct;[51] others have argued the reverse. Do the terms "acceptability" and "grammaticality" refer to the same thing? If they do, why don't we just settle on one term? If they do not, do speakers provide different kinds of answers when asked about the grammaticality or the acceptability of a sentence in their native language?

A grammatical sentence conforms to the rules of a given language. An acceptable sentence refers to a speaker's perception of the stimulus in terms of her own linguistic repertoire.[52] There are sentences that are not acceptable or in use—but for reasons that have nothing to do with violating linguistic rules. The sentence, "That that that Bill left Mary amused Sam is interesting is sad," is a grammatical sentence of English.[53] Native speakers would not produce it often and they would certainly not rate it as acceptable as the very similar, "It is sad that it is interesting that it amused Sam that Bill left Mary."[54]

If grammaticality also reflects cognitive biases, asking a speaker to provide grammaticality judgments means asking for judgments about the interactions of *all* linguistic and cognitive factors that determine the limits of grammar. No speaker can provide this. Nor can any linguist. The term "grammaticality judgments" has been correctly labelled a misnomer. Speakers have intuitions only about their perception of linguistic stimuli.[55]

9. Bilingual Advantage

The effort of mentally juggling two linguistic systems can be linked to better performance in certain cognitive tasks.[56] Thus the bilingual advantage. The term itself is neither ambiguous, problematic, nor misused. But its use calls for caution. As various scholars tested an ever-growing number of populations in order to discover the magnitude of the bilingual advantage, it became clear that one can also talk about a bilingual *dis*advantage.[57] A more useful term that emerged in the same literature is "trade-off."[58] In this sense, an advantage in one measure may bring along other effects, hence the term "bilingual advantage" might only provide half of the picture with respect to bilingual effects on cognition. Talking about the overall impact or effect of bilingualism on cognition may come closer to acknowledging the surrounding implications.

10. Optimal Design and Perfect System

Hauser, Chomsky, and Fitch suggest that FLN can be thought of as "a kind of 'optimal solution' to the problem of linking the sensory-motor and conceptual-intentional systems,"[59] and of satisfying the interface conditions between FLN and FLB. This view is also present in Chomsky's earlier work under the name "strong minimalist thesis" (SMT).[60] In other work, the language system has been described as perfect.[61] These notions are elusive. Most scholars understand them as denoting principles of parsimony, economy, and elegance,[62] but the way these properties are attributed to language is unclear. Do parsimony, economy, and elegance characterize language, or theories about language?[63]

Language develops some of its characteristics by adaptation to environmental triggers. There is evidence that over time, speakers develop effort–accuracy trade-offs.[64] Languages are also reshaped to become more learnable.[65] One might say that the language system is becoming more optimal in response to adaptation pressures. This trend toward efficiency is probably not due to the language system itself. The human mind has been described as making optimal use of its cognitive resources across domains.[66] It is unclear whether the optimality of language is due to the recruitment of general principles of rationality and adaptation, or whether language, possibly by being unique at some level, stands out from other cognitive modules.

It is harder to argue that all linguistic theories are optimal, especially in relation to the preceding suggestions

that innateness consists of an unknown, ever-growing number of features, parameters, and other primitives.

Taking Stock

The objective of this essay is to attain a higher level of terminological clarity and coherence within the field of linguistics. If successful, this process may help improve its visibility in neighboring fields, such as other parts of psychology, biology, and neuroscience. This visibility is, in fact, extremely important. As scientists of language, linguists should offer robust and cohesive theories and this is particularly true for claims about innateness, which must make sense from a biological point of view. Some months ago, I attended a conference that dealt with certain linguistic primitives. A talk was given by a senior linguist who argued for the removal of a specific primitive from UG. UG was mentioned at the beginning of the talk and never referred to again. Nor was it explained why the feature was allocated to UG in the first place. UG, it should be stressed, is defined as our *biological* endowment for language, and claims made about UG in linguistics must be plausible in biology. I asked the speaker how the feature in question came to be part of UG. I was told that this had been a common assumption among theoretical linguists since the 1970s. I then asked how theories of UG might support such a claim. I found it hard to see how a biologist would be convinced that the biological endowment for language encodes *all* of these grammatical features. Defending our theories from interdisciplinary criticisms, I suggested, might be extremely difficult. To my surprise, the speaker responded that he had no interest in interdisciplinary work. These considerations, I realized, simply did not enter the picture. For the speaker, UG is simply a repository of linguistic primitives that can be disconnected from human biology.

This is why terminological clarity matters.

1. Remi van Trijp, "Use Your Own Toothbrush," *Essays in Linguistics* (blog).
2. Peter Hagoort, "Linguistics quo vadis? An Outsider Perspective," talk presented at SLE 2014: 47th Annual Meeting of the Societas Linguistica Europaea, Adam Mickiewicz University, Poznań, Poland, 2014.
3. Tanja Kupisch and Jason Rothman, "Terminology Matters! Why Difference Is Not Incompleteness and How Early Child Bilinguals Are Heritage Speakers," *International Journal of Bilingualism* 22, no. 5 (2016): 574, doi:10.1177/1367006916654355.
4. Scott Lilienfeld et al., "Fifty Psychological and Psychiatric Terms to Avoid: A List of Inaccurate, Misleading, Misused, Ambiguous, and Logically Confused Words and Phrases," *Frontiers in Psychology* 6 (2015): 1,100, doi:10.3389/fpsyg.2015.01100; Scott Lilienfeld et al., "50 Differences That Make a Difference: A Compendium of Frequently Confused Term Pairs in Psychology," *Frontiers in Education* 2 (2017): 37, doi:10.3389/feduc.2017.00037.
5. Take any issue of a journal dedicated to theoretical or experimental generative linguistics and check how many articles in that issue do not feature any of the aforementioned terms. The answer is likely very few, or none. Each article will probably mention more than one of these terms on multiple occasions. The terms discussed here are not some barely encountered notions, the status of which is of minor relevance to most of the field of linguistics.
6. Noam Chomsky, *Lectures on Government and Binding* (Dordrecht: Foris, 1981).
7. Noam Chomsky, "On the Representation of Form and Function," *The Linguistic Review* 1, no. 1 (1981): 3–40, doi:10.1515/tlir.1981.1.1.3.
8. José-Luis Mendívil-Giró, "Is Universal Grammar Ready for Retirement? A Short Review of a Longstanding Misinterpretation," *Journal of Linguistics* 54, no. 4 (2018): 861, doi:10.1017/s0022226718000166.
9. I thank Norbert Hornstein for this point. For useful feedback, I thank Antonio Benítez-Burraco, Antonio Fábregas, Kleanthes Grohmann, Norbert Hornstein, Myrtani Pieri, Jason Rothman, and Patrick Trettenbrein.
10. Almost 80% of the world's languages surveyed in the World Atlas of Language Structures agree with this pattern. See Matthew Dryer, "Order of Adjective and Noun," in *The World Atlas of Language Structures Online*, eds. Matthew Dryer and Martin Haspelmath (Leipzig: Max Planck Institute for Evolutionary Anthropology, 2008).
11. Jennifer Culbertson, Paul Smolensky, and Géraldine Legendre, "Learning Biases Predict a Word Order Universal," *Cognition* 122, no. 3 (2012): 306–29, doi:10.1016/j.cognition.2011.10.017.
12. Noam Chomsky, "Three Factors in Language Design," *Linguistic Inquiry* 36, no. 1 (2005): 1–22, doi:10.1162/0024389052993655.
13. Janet Dean Fodor, "Setting Syntactic Parameters," in *The Handbook of Contemporary Syntactic Theory*, eds. Mark Baltin and Chris Collins (Malden & Oxford: Blackwell, 2001), 734, doi:10.1002/9780470756416.ch23.
14. Frederick Newmeyer, *Possible and Probable Languages: A Generative Perspective on Linguistic Typology* (Oxford: Oxford University Press, 2005), 83.
15. Some linguists, especially within the cartographic framework, have supported the following equation: one (morphosyntactic) property—one feature—one designated slot in the innate functional hierarchy. See, for example, Guglielmo Cinque and Luigi Rizzi, "The Cartography of Syntactic Structures," in *Studies in Linguistics: CISCL Working Papers on Language and Cognition*, ed. Vincenzo Moscati (Siena: University of Siena, 2008), 5:44. As a result, incredibly rich and ever-grow-

ing arrays of syntactic heads have been cast as innate on the basis of variant morphosyntactic realizations.

16. Greville Corbett, "Features: Essential Notions," in *Features: Perspectives on a Key Notion in Linguistics*, eds. Anna Kibort & Greville Corbett (Oxford: Oxford University Press, 2010), 18.
17. Extended projection principle: A linguistic constraint that suggests that all clauses must have a subject. See Chomsky, *Lectures on Government and Binding*.
18. Ur Shlonsky, "The Cartographic Enterprise in Syntax," *Language and Linguistics Compass* 4, no. 6 (2010): 424, doi:10.1111/j.1749-818x.2010.00202.x.
19. The logic of the argument must be clear: How can we explain structure B in language C? An innate primitive must be behind it—call it feature/parameter A. And what reason do we have to believe that A exists? Structure B. In a similar context, Bridget Samuels has noted that

    > upon observing that a certain language has no consonant clusters, one posits that these facts are not random, but are due to a constraint like *CC. But then, when one asks why the language has no consonant clusters, the answer is because *CC bans them. While perhaps on some level all logic is ultimately circular, this is a very small circle.

    See Bridget Samuels, "Biolinguistics in Phonology: A Prospectus," *Phonological Studies* 18, (2015): 164.
20. Denis Bouchard, "Solving the UG Problem," *Biolinguistics* 6, no. 1 (2012): 12.
21. David Adger and Peter Svenonius, "Features in Minimalist Syntax," in *The Oxford Handbook of Linguistic Minimalism*, ed. Cedric Boeckx (Oxford: Oxford University Press, 2011), 28.
22. It is important that this terminological fluidity is noted in order to avoid talking about the same thing with very different underlying assumptions in mind. Linguists have recognized field-fragmentation as a potential danger for coherence and taken steps in this direction. A good example is the conference "Generative Syntax in the Twenty-First Century: The Road Ahead" that took place in Athens in 2015. Among the challenges that were addressed in this event was the coherence of the field. As suggested during the call,

    > [g]iven the large number of different analytic approaches, it has resulted in small groups working on x, y, or z. From a scientific point of view, this is not problematic, but it raises difficulties when it comes to interaction, funding, recruitment and external visibility.

    The present work has a similar purpose in registering a different danger: if different groups work on x but use x to refer to different things, this state of affairs raises even more difficulties in relation to coherence, interaction, and external visibility.
23. Ana Villar and Antoni Gomila, "A Minor Role for Genetics in Language Evolution," *Journal of Anthropological Sciences* 91 (2013): 31.
24. Cedric Boeckx and Evelina Leivada, "On the Particulars of Universal Grammar: Implications for Acquisition," *Language Sciences* 46 (2014): 189–98, doi:10.1016/j.langsci.2014.03.004; Antonio Benítez-Burraco, "Problematic Aspects of the Genetic Analysis of the Specific Disorders of the Language: FOXP2 as Paradigm," *Neurología* 27, no. 4 (2012): 225–33, doi:10.1016/j.nrleng.2012.05.005.
25. Norbert Hornstein, "How Biological Is Biolinguistics?" *Faculty of Language*, April 27, 2017.
26. Marc Hauser, Noam Chomsky, and W. Tecumseh Fitch, "The Faculty of Language: What Is It, Who Has It, and How Did It Evolve?" *Science* 298, no. 5,598 (2002): 1,569–79, doi:10.1126/science.298.5598.1569.
27. Hauser, Chomsky, and Fitch, "The Faculty of Language," 1,569.
28. W. Tecumseh Fitch, Marc Hauser, and Noam Chomsky, "The Evolution of the Language Faculty: Clarifications and Implications," *Cognition* 97, no. 2 (2005): 181, doi:10.1016/j.cognition.2005.02.005.
29. W. Tecumseh Fitch, "Prolegomena to a Future Science of Biolinguistics," *Biolinguistics* 3, no. 4 (2009): 288.
30. Fitch, Hauser, and Chomsky, "Evolution of the Language Faculty," 181.
31. More specifically, Berwick and Chomsky suggest that humans, but no other animal, have the ability to construct unbounded arrays of hierarchically structured expressions through Merge. Yet they also highlight that this ability may well be implemented by pre-existing, non-language-specific, repurposed wetware. The use of already existing machinery implies that at some level the uniqueness of this ability does not hold, by means of being constrained by non-language-specific biophysical factors. See Robert Berwick and Noam Chomsky, *Why Only Us: Language and Evolution* (Cambridge: MIT Press, 2016), 132.
32. Patrick Trettenbrein, "The 'Grammar' in Universal Grammar: A Biolinguistic Clarification," *Questions and Answers in Linguistics* 2, no. 1 (2015): 4, doi:10.1515/qal-2015-0005.
33. See, for example, Halldór Ármann Sigurðsson, "On UG and Materialization," *Linguistic Analysis* 37 (2011): 371; Frederick Newmeyer, "Goals and Methods of Generative Syntax," in *The Cambridge Handbook of Generative Syntax*, ed. Marcel den Dikken (Cambridge: Cambridge University Press, 2013), 72.
34. Lilienfeld et al., "Fifty Psychological and Psychiatric Terms."
35. Trettenbrein, "The 'Grammar' in Universal Grammar," 3.
36. Richard Kayne, "Antisymmetry and the Lexicon," in *The Biolinguistic Enterprise: New Perspectives on the Evolution and Nature of the Human Language Faculty*, eds. Anna Maria Di Sciullo and Cedric Boeckx (Oxford: Oxford University Press, 2011), 334.
37. Frank Wijnen, "Acquisition of Linguistic Categories: Cross-Domain Convergences," in *Birdsong, Speech, and Language: Exploring the Evolution of Mind and Brain*, eds. Johan Bolhuis & Martin Everaert (Cambridge: MIT Press, 2013), 157.
38. Peter Svenonius, "Linguistic Universals and the Category P," September 16, 2005, 1.

39. Rasha Abdel Rahman and Alissa Melinger, "Dismissing Lexical Competition Does Not Make Speaking Any Easier: A Rejoinder to Mahon and Caramazza (2009)," *Language and Cognitive Processes* 24, no. 5 (2009): 753, doi:10.1080/01690960802648491.
40. Wendy Francis, "Bilingual Semantic and Conceptual Representation," in *Handbook of Bilingualism: Psycholinguistic Approaches*, eds. Judith Kroll and Annette de Groot (Oxford: Oxford University Press, 2005), 251.
41. Ole Ottersen and P. Johannes Helm, "How Hard-wired Is the Brain?" *Nature* 420, no. 6,917 (2002): 752, doi:10.1038/420751a.
42. Noam Chomsky, *Aspects of the Theory of Syntax* (Cambridge: MIT Press, 1965), 3.
43. Noam Chomsky, *Reflections on Language* (New York: Pantheon, 1975), 119.
44. Dalila Ayoun, *Parameter Setting in Language Acquisition* (London: Continuum, 2003), 7.
45. Noam Chomsky, *Aspects*, 4.
46. Stephen Anderson and David Lightfoot, "The Human Language Faculty as an Organ," *Annual Review of Physiology* 62, no. 1 (2000): 698, doi:10.1146/annurev.physiol.62.1.697.
47. For further discussion on this point, see Kleanthes Grohmann and Evelina Leivada, "Reconciling Linguistic Theories on Comparative Variation with an Evolutionarily Plausible Language Faculty," in *A Festschrift for Ian Roberts*, eds. András Bárány et al. (Berlin: Language Science Press, in press).
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62. Fahad Rashed Al-Mutairi, *The Minimalist Program: The Nature and Plausibility of Chomsky's Biolinguistics* (Cambridge: Cambridge University Press, 2014), 34, and references therein.
63. In relation to the second reading, the following claim by Cedric Boeckx is quite illuminating:

    > The minimalist program for linguistic theory *adopts as its working hypothesis* the idea that Universal Grammar is "perfectly" designed, that is, *it contains nothing more than what follows from our best guesses regarding conceptual, biological, physical necessity* [emphases added]. This hypothesis is probably too strong (Chomsky calls it the "strong minimalist thesis"), but in practice scientists often adopt the strongest possible thesis as their working hypothesis. The strongest hypothesis then acts as a limiting case, enabling us to see more precisely where and when the hypothesis fails and how much of it may be true. The strong minimalist thesis is indeed a wild idea.

    Cedric Boeckx, *Linguistic Minimalism: Origins, Concepts, Methods, and Aims* (Oxford: Oxford University Press, 2006), 4.
64. George Zipf, *Human Behavior and the Principle of Least Effort* (Cambridge, MA: Addison-Wesley, 1949).

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On Core Concepts and Terminology

Anna Maria Di Sciullo, reply by Evelina Leivada

In response to "Misused Terms in Linguistics" (Vol. 5, No. 2).

To the editors:

In her essay, Evelina Leivada reports on a range of terminology issues in linguistics. These are drawn from a variety of sources, including citations from articles, blogs, talks, and interactions at conferences. Linguists are urged to follow the lead of psychologists and discuss "lists of inaccurate, ambiguous, misused, and polysemous terms." In closing, Leivada states that clarity matters in linguistics and has consequences for the visibility of linguistics in neighboring fields.

While it is difficult not to agree with this general conclusion, it is also difficult not to view terminology issues, namely problems with labels for concepts, as an epiphenomenon of the rapid evolution of linguistics and the different approaches to language pursued within the generative enterprise, in cognitive sciences and beyond. Although terminology issues are raised at the forefront, the crux of the matter with this essay is not terminology per se, but different views of core concepts in linguistics.

The generative enterprise explores a specific approach to language. It is concerned with what has been termed the basic property of language, namely, the property of the mind to construct an infinite array of structured expressions, each one with a semantic interpretation that expresses a thought that can be externalized in one of the sensorimotor systems. In this approach, the language faculty is a generative system that feeds semantic interpretation directly and sensorimotor interpretation only indirectly.[1]

Within the generative enterprise, the biolinguistic program is concerned with language internal to the individual, the I-language, which is distinct from the external language, the E-language. It aims to provide an explanation for I-language by understanding through its biological basis.[2] Abstracting away from Darwin and the modern synthesis, the biolinguistic program brings to the fore arguments in favor of language as a human-specific trait and the rapid evolution, or emergence, of language.[3] The ultimate goal of the generative enterprise and the biolinguistic program is to provide a genuine explanation of language that will meet the criteria of learnability. The system needs to be acquired by the individual, as well as the criteria of evolvability, as the innate system of the faculty of language needs to evolve. Such explanation cannot be reached with descriptive or behaviorist approaches to language.[4]

Explanation and simplicity are intrinsically related in this framework. This can be seen in the development of generative grammar.[5] Current research aims to explain I-language in terms of the first and the third factor in language design. The first factor is the genetic endowment and the third factor is the principles of efficient computation external to the language faculty.[6]

Other approaches based on different or partially different perspectives on language, with partially different arrays of concepts, are available. Terminological issues may arise within linguistics as well as in neighboring fields.

Terminology Issues

To address the issues in relation to terminology discussed by Leivada, it is useful to differentiate linguistic terms from their use at different points in the development of the generative enterprise. It is also helpful to consider the use of linguistic terms in neighboring sciences. Neuroscience is an interesting case as it faces the map and mapping problem, as pointed out by David Poeppel.[7] Nonetheless, it has been established that Broca's area, corresponding to BA44/45 in the frontal lobe of the brain's dominant hemisphere, supports the processing of syntax in general.[8] A subdivision of syntactic computations within Broca's area for complex syntactic structures has been demonstrated with BA44 activated for center-embedding and for sentences involving displacement of syntactic constituents, and BA45 selectively adapted to displacement.[9] Psycholinguistics is also interesting as it faces the problem of attesting the psychological reality of linguistic concepts. It is difficult to probe the abstract properties of I-language with behavioral experiments alone, and brain imaging studies are often used in addition to behavioral experiments. The best possible outcome is when results

from independent studies converge. Biolinguistics is perhaps the most interesting case as it relies on interactions between different sciences including linguistics, biology, mathematics, and physics.

There is no doubt that clarity and coherence are imperative in any field of inquiry, whether theoretical or applied, disciplinary or interdisciplinary. To this end, it is instructive to reflect on how terms for core notions in a given field emerge, evolve, disappear, and also sometimes reappear as relics from past eras to measure achievements and identify new problems. The misuse of terms in linguistics might be better understood by reflecting on the life of core concepts in the field and on the terms used to investigate them. It may also be helpful to consider terms used to study core concepts in linguistics as names for objects yet to be better understood. The terms used for given concepts will change over time; some will be redefined or eliminated in light of advances made in their understanding and new problems that arise in the investigation.

Terminological Confusion

Advances have been made through the development of the generative enterprise. During the late 1970s and early 1980s, the principles and parameters model of grammar replaced the so-called standard theory.[10] In this model, a grammar of a language is a theory of that language, and Universal Grammar is a theory of all languages. Leivada observes that the terms Universal Grammar (UG) and language universals are misused in linguistics.

> UG is sometimes identified with linguistic universals; but this is a mistake. When Chomsky talks about language universals, he does not refer to properties that are universally attested to in all languages, but to computational properties of the mind that are universal because they arise from a species-universal innate ability.

Why would these terms be misused? One possible reason is that they were proposed within different theories of language. As defined by Joseph Greenberg, language universals are not part of Universal Grammar and mainly state universals, absolute and relative, based on the surface distribution of major syntactic constituents: subject, verb, and object.[11] Not all possible orders are attested in the languages of the world, which indicates that more abstract properties of languages are at play for characterizing Universal Grammar. Greenberg's work is important and enabled the development of further research capable of deriving specific language universals from independent syntactic properties.[12] This is a step forward in simplifying linguistic theory. Leivada writes:

> There is no reason to assume that linguistic universals, understood as properties that are shared across languages, are necessarily derived from UG. Although most languages settle on a consistent word order, this preference does not reflect the imperatives of UG. … Given that a complete list of all the UG principles has not been compiled, the possibility that these principles are, for the most part, not language-specific, but have cognitive, third factor roots, cannot be ruled out.

Third factor principles are at play in the derivation of syntactic constituents. It has been observed through the diachronic development of the Indo-European languages that languages tend to regularize adpositions to either prepositions or postpositions. Both preposition and postpositions are observed in Latin nominal constituents including a preposition and a personal pronoun, e.g., *cum me*, *me cum*. In modern Italian, only the prepositional variant remains, e.g., *con me*, notwithstanding the fact that more complex structures emerged in old Italian, e.g., *con me meco*, *come*, *comeco*, *con esso meco*. This historical development has not been attributed to principles of UG, but to third factor principles reducing complexity akin to natural laws.[13]

In current minimalist research, the externalization of linguistic constituents and their absence in some cases has also been attributed to third factor principles of efficient computation external to the basic property of language. This is the case for principles of pronunciation. According to pronounce the minimum, the copy left by a displaced constituent is generally not pronounced, e.g., *what did you say?* vs. **what did you say what?*[14] Other examples in which principles of pronunciation play a role in the derivation of linguistic expressions occur when certain categories, which are not copies of displaced constituents, are not externalized (< >). This is the case for prepositions such as "at" in locative expressions—e.g., *I prefer to stay <AT> home*—discussed by Christopher Collins and elsewhere, as well as unpronounced coordinators in multiple conjunctions—e.g., *I saw Paul <AND> John <AND> Mary*—and several other categories.[15] That third factor principles are at play in language is not only a possibility as mentioned by Leivada. It is a working hypothesis that has already been proposed and investigated in the generative enterprise for quite some time.

If confusion with the content of linguistic terms arises, whether in linguistics or neighboring fields, it is important to keep in mind that these terms emerge from different approaches. Language universals gave rise to empirical generalizations, some of which have been derived from independent principles in later stages of the generative enterprise. It is important to clarify why language universals cannot be equated with Universal Grammar, why it matters for the understanding of I-language, and why surface phenomena such as word order are subject to principles external to I-language. Such a perspective is absent from Leivada's essay.

Terminological Fluidity

Leivada asserts that a degree of terminological fluidity is associated with certain linguistic terms, such as parameters and features. In particular, she draws attention to metaphors of language development used in Chomsky's earlier works, including the notion of instantaneous acquisition.[16]

> The problem is that the metaphor of instantaneous acquisition assumes that innateness and the environment are fully separable. ... The metaphors of language development were once useful. It is through their subsequent use that they became unhelpful.

On this point, I disagree with Leivada. The notion of parameters, initially defined as options left open in the principles of UG, gave rise to lively research.[17] The term has been defined in different ways through the development of the generative enterprise, given extended work on linguistic variation, both within single languages and cross-linguistically. Several parameters have been proposed, as well as different formalizations of this notion. Mark Baker's polysynthetic parameter, for example, identifies hierarchical dependencies between parameters.[18] In the minimalist program, it has been proposed that parameters could be reduced to minimal differences in the features of functional categories. Several parameters have been eliminated and their effects attributed to independent principles of language. These include the head-directionality parameter, which has been proposed to derive from the directionality of parsing.[19] In his recent book, Ian Roberts redefines parameters in terms of structured hierarchies of features related to third factor principles.[20] This enables elegant analyses of different historical language changes, including negation. The reduction of parameters to principles external to the language faculty contributes to an explanatory theory of language.

The purpose of the generative enterprise is to provide a genuine explanation for I-language. It comes as no surprise that current minimalist research eliminates the notion of parameters as options left open by UG, as defined in the principles and parameters model, where the different modules of UG were each associated with a set of primitives, axioms, and parameters.[21]

It is unfortunate that Leivada's discussion of the term does not report its current status, as subsumed under third factor principles, and the progress achieved since it was proposed in the principles and parameters model. Instead, her essay focuses on criticisms of the usage of this term during previous stages of the generative enterprise.

> Terminological fluidity is not without consequences. In the case of the term "parameter," its misuse over the years raised serious concerns about the biological plausibility of an innate endowment for language that consists of millions of minimal points of variation. "If the number of parameters," Frederick Newmeyer observed, "needed to handle the different grammars of the world's languages, dialects, and (possibly) idiolects is in the thousands (or, worse, millions), then ascribing them to an innate UG to my mind loses all semblance of plausibility."

When confusion arises about the putative terminological fluidity of parameters, it is useful to clarify the notion of parameter and identify its trajectory within the generative enterprise. It is also useful to explain why the notion of parameters is dispensed with in recent works in the minimalist program, along with the different principles of UG they depend upon in the principles and parameters model.

The elimination of the notion of parameter provides an explanation for the absence of acquisition. If language is not learned through experience, that is, by contact with the environment, then the logical problem of language acquisition can be solved. Namely, notwithstanding the fact that the empirical data available to the infant is partial, language acquisition unfolds in a very limited time.[22] The optimal hypothesis is that no learning takes place.

Leivada's discussion of the term feature also reports criticisms instead of reporting the progress made from the introduction of syntactic features in the standard theory to current understanding in the minimalist program. The hypothesis that syntactic features are part of the syntactic component of the grammar was proposed by Chomsky as part of the standard theory.[23] He extended this idea in "Remarks on Nominalizations," where nouns, verbs, adjectives, and prepositions are defined in terms of two binary features [± N] and [± V].[24] The introduction of syntactic features enabled cross-categorial generalizations and provided an account for natural classes of categories.[25] In the minimalist framework, syntactic features associated with syntactic constituents enter into local agreement relations, which may lead to displacement.[26] Features have been proposed to account for the fact that syntactic constituents are displaced to higher positions in syntactic structures. Certain syntactic features, such as the extended projection principle feature mentioned by Leivada, have been eliminated. Their effects are derived by independent properties, including labeling and simplest,[27] according to which Merge, the core combinatorial operation of the language faculty, applies freely. Derivations are canceled if, for example, constituents cannot be labeled.

While Norbert Hornstein is right in suggesting that "the real problem is that we have no hint of a theory of features," as quoted by Leivada, support for a limited set of syntactic features in linguistic theory comes from the fact that they feed the semantic interface. The features [± N] and [± V] can be thought of as being legible as [± argumental] and [± predicative] at the semantic interface, but not at the sensorimotor interface. This satisfies the strong

minimalist thesis, according to which language is the best solution to interface legibility conditions.

In the generative enterprise's current state of development, Leivada's discussion on misused terms such as parameters and features is obsolete. If confusion arises with respect to certain terms in linguistics, it is useful to understand why such terms have been proposed, what they accounted for, how their content and role have been modified to provide a genuine explanation for language, and what problems and questions they left open for further research. It goes without saying that criticism is useful to the extent that it leads to alternatives with greater explanatory capacities.

Further Terminological Confusion

Questions arise among linguists whether the architecture of the language faculty, as defined by Marc Hauser, Chomsky, and W. Tecumseh Fitch, is the optimal solution to the problem of linking the sensorimotor to the conceptual-intentional systems.[28] "It is harder to argue that all linguistic theories are optimal," Leivada writes, "especially in relation to the preceding suggestions that innateness consists of an unknown, ever-growing number of features, parameters, and other primitives."

This remark is obsolete in relation to current research in the generative enterprise, where features, parameters, and other primitives are reduced to their minimum or subsumed in third factor principles as discussed above. She reports confusion that arose with the term language faculty in the narrow sense (FLN), which Hauser, Chomsky, and Fitch distinguished from the language faculty in the broad sense (FLB):

> But, of course, FLN is not the same thing as UG. FLN is unique to humans and unique to language. UG does not have this character. UG is relevant to both FLN's language-specific properties, and FLB's non-language-specific properties. Clearly, FLN is not tantamount to UG. Any suggestions to the contrary represent a misuse of two important terms in linguistics. It is a mistake that many linguists have made.

Linguistics cannot be reduced to terminology. Over time, terms used for given concepts will change; some of them will be redefined or eliminated, given the advances made in understanding and the new problems that arise in the investigation of language.

Chomsky reiterates the particular approach to language pursued in the generative enterprise, which aims to explain the basic property of language.[29] In this particular approach, a genuine explanation meets two conditions: learnability and evolvability. Given that I-language is internal to the individual, it is not acquired. The simplest explanation for the basic property of language is that it can be explained on the basis of two of the three factors of language design: genetic endowment and the principles of efficient computation. In this perspective, FLN reduces to Merge, the simplest form of which would be internal Merge. An argument to this effect comes from the fact that internal Merge is simpler than external Merge. The latter requires an additional operation of search in the lexicon. Another argument is that internal Merge also derives the successor function in arithmetic. This leads to further problems and questions, including whether there are other variants of Merge, and why external Merge would be necessary.

Leivada's essay is not applicable to current research in the generative enterprise. It is less informative about the misused terms themselves than it is about different approaches to language. The terminological issues discussed in the essay should be understood as differences in the understanding of core concepts in linguistics.

The visibility of linguistics in neighboring fields is important. On this point, I agree with Leivada. But it is surprising that the investigation of core concepts in linguistics would be reduced to normative terminological issues in her essay. The consequences of doing so are indeed detrimental to both linguistics and fruitful interdisciplinary research.

While controversies arise in the investigation of different approaches to language within the generative enterprise, it is difficult to reduce them to terminological issues. Linguistics is, of course, much more than a repertoire of terms and their proper use. As part of the generative enterprise, biolinguistics is a research program aiming to provide a genuine explanation for I-language. In order to engage in fruitful interdisciplinary work, it might be useful to bring to the fore the latest achievements in explaining I-language and why they are important, as well as the new problems and questions that they prompt.

Anna Maria Di Sciullo

Evelina Leivada *replies:*

Anna Maria Di Sciullo raises various interesting points. The main message of her response is that the use of the terms I identified and discussed as inaccurate is nowadays obsolete. This is not a matter on which we can agree to disagree, because it is not a matter of subjective opinion, but rather of checking the facts. As I suggested in my essay, the terms I discussed are not some barely encountered notions in present-day linguistics. Demonstrating this amounts to an inexpensive experiment that anyone can repeat at home. I should highlight that in doing this exercise in the present reply, I completely agree with Di Sciullo, who argues that we need to differentiate the various uses of the terms at different points in the development of the genera-

tive enterprise. However, I insist that the issues discussed in my essay largely reflect the *current* lay of the land in mainly generative linguistics.

Consider the notion of a parameter, its various conceptions and its various sizes. Is this an obsolete discussion nowadays? If parameters have been reduced to a minimum, as Di Sciullo suggests in her letter, how many are they exactly, according to current linguistic theorizing? How many features? How many functional heads? This inability to provide even approximate numbers is very surprising, given Di Sciullo's claim that we have now successfully settled on the minimum numbers for each inventory. Even if, for the sake of discussion, we agree that we have reduced these inventories to the absolute minimum, have we reached consensus about whether this minimum number of parameters should be ascribed to the functional lexicon, to Universal Grammar, to externalization systems, or to third factor principles? Juxtapose Di Sciullo's view that parameters have been reduced to a minimum or subsumed in third factor principles with Luigi Rizzi's view that

> the size of the set of parameters may well be large: cartographic studies suggest that the functional lexicon is very rich, hence if the parametrization is associated to this component the system will specify many parameters.[30]

Crucially, this claim was made in 2017, not 40 years ago. This serves to show that the ambiguity I identified regarding the use of certain terms refers to their current use, and is not obsolete as Di Sciullo suggests.

On May 7, 2020, three days after the publication of my essay in *Inference*—and 52 years and five days after the student revolution began in France—the first live debate from a series of video conferences called "Linguistics Flash-Mobs. Epic Battles in History" took place.[31] In each conference, two seminal scholars were invited to discuss longstanding theoretical issues in linguistics. The first debate, moderated by Maria Rita Manzini and organized by Cecilia Poletto, hosted Giuseppe Longobardi and Ian Roberts, two distinguished professors of linguistics from York and Cambridge, respectively. The three questions that the two invitees addressed had to do with different aspects of one notion: parameters. In that debate, the discussion of parametric hierarchies made amply clear that for some linguists, parameters come in different sizes: macro, meso, and micro. This is part of the terminological fluidity that I analyzed in my essay and that Di Sciullo argues does not characterize the current state of affairs in the generative enterprise. This conference is not the exception. Parameters are very much part of the current lay of the land in generative linguistics and so is the quest to examine more structures from different languages and language families in order to uncover more of them, contra to Di Sciullo's claim that the field has now settled on the minimum number.[32]

Consider the usage of another term: linguistic genotype or genetic endowment for language. It has been suggested that "children have triggering experiences that stimulate their *genetic properties* to develop into their phenotypic properties [emphasis added],"[33] that "linguistic knowledge is part of the child's *genetic makeup* [emphasis added],"[34] and that parameters are principles to which "you *genetically* fix the value [emphasis added]."[35] These views, and many similar ones that are abundant in linguistics, imply the position that a part of the human genome is dedicated to language. Di Sciullo may have hoped that these claims belong to a different era and are no longer entertained. This is not the case. They are still popular, and mainstream enough to be diffused to the general public as established theses. A good example of how this is so is offered by Martin Haspelmath on his blog.[36] He presents part of the interview that linguist Jessica Coon gave on the occasion of the release of the 2016 movie *Arrival*, for which she did consulting work. The question was, "So if universal language theory only applies to humans, there's a real danger that if an alien race started communicating we'd have no hope of deciphering it?" Coon's reply was,

> Yeah, definitely. When people talk about universal grammar it's just the genetic endowment that allows humans to acquire language. There are grammatical properties we could imagine that we just don't ever find in any human language, so we know what's specific to humans and our endowment for language.[37]

Perhaps an important question is whether there are more terms on which we should keep a close eye. In her letter, Di Sciullo correctly argues that it is helpful to consider the use of linguistic terms in neighboring sciences. She refers to a thesis she claims has been established: "Nonetheless, it has been established that Broca's area, corresponding to BA44/45 in the frontal lobe of the brain's dominant hemisphere, supports the processing of syntax in general." This looks like an oversimplified depiction of the neuroanatomical underpinnings of syntax. The way it is used in passing implies localization—a slippery terrain. Language operations do not reside in single brain regions; they are subserved by networks of brain regions.[38] Certain operations used in syntax processing may typically have a certain distribution of labor, but the latter relies on networks spanning over many areas. In this sense, there is no one-to-one correspondence between a domain of linguistic analysis and an area in the brain.

To offer a second example, Di Sciullo uses the term "module," as I do too in my original essay, but this is yet another term that has been used in a terminologically fluid way. More specifically, Di Sciullo talks about modules of Universal Grammar, but she does not explain the sense in which the term is used. Again, this terminological imprecision is not without consequences for the field. It is not

at all clear that Universal Grammar consists of modules or even that there *is* a language module per se. The term must be qualified, describing the sense in which it is employed; otherwise, its use may cause terminological confusion and erroneously exaggerate inexistent physical discreteness.[39]

There are several other points on which I respectfully disagree with Di Sciullo. For reasons of space I will discuss only two. First, she situates a biolinguistic approach to language within the generative enterprise: "Within the generative enterprise, the biolinguistic program is concerned with language internal to the individual, the I-language, which is distinct from the external language, the E-language." This is wrong, because biolinguistics is not specific to the generative approach. Of course, one can do excellent work on biolinguistics within a generative framework, but one can also do excellent work addressing questions about the evolution and biology of language, without using tools of generative linguistics.[40] Second, Di Sciullo argues that even if terminological discrepancies exist, the crux of the matter in my essay is not terminology per se, but differing views of core concepts in linguistics. The answer to this is that I personally find it more useful to acknowledge the existence of a problem and then work on solving it, than to engage in chicken-or-egg dilemmas by discussing whether the focus should be on the cause versus the outcome, and on what counts as what. For me, the issue has to do with *both* the fluid way in which some terms are used to mean different things across studies (e.g., "parameters"), *and* with the use of terms that are simply wrong (e.g., "linguistic genotype"). It could be argued that the ambiguity that surrounds the use of some terms derives from different views of core concepts, but it could also be argued that some linguists hold alternative views of core concepts precisely because the terms have long been defined in ambiguous, unclear, or untenable ways. The question of what came first is not the most important one for me. The focus should not be on where ambiguity comes from, especially since we have reasonable knowledge about possible sources, and Di Sciullo also correctly identifies some, but on caring to acknowledge its existence and then work on it in a way that maximizes field-internal coherence and field-external visibility.

To conclude, I thank Anna Maria Di Sciullo for her response to my essay. I read it with great interest and I agree with the part of her epilogue that claims that "the terminological issues discussed in [Leivada's] essay should be understood as differences in the understanding of core concepts in linguistics." This is true; the contents of Di Sciullo's response made me realize once more that the use of many terms in linguistics is a matter of perspective, and a good degree of the terminological unclarity I talked about derives precisely from the holding of different perspectives. Terms like "the syntax area of the brain," "module," and "I- versus E-language" are not included in my original list of misused, ambiguous, and polysemous terms, so perhaps a follow-up study is due. With respect to the main point of Di Sciullo's critique, I want to reaffirm that the terms I discussed, together with their portrayed use, are not absent from present-day linguistics. I encourage readers to go through Di Sciullo's arguments carefully and then also read Juan Uriagereka's response to my original essay. I think Uriagereka is right about everything he writes in his letter. He is especially right about the title he chose to put on his reply. For some or perhaps even many linguists, these truly are *terms of endearment*. For this reason, attempts to discuss problems in their use may be brushed aside as not necessary, not timely, or not appropriate, especially if coming from junior scholars. But as the students in the French revolution of May 1968 said, if not us, who? If not now, when?

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25. For example, according to case theory, postulated in the principles and parameters model, all nominal constituents are case marked, whether the case markers are overt, e.g., in Latin, old Germanic, or not, e.g., in Italian, English. Only [-N] categories, namely verbs and prepositions, assign case to their nominal complement. An empirical consequence of case theory is that in languages such as Italian and English, a preposition intervened between the first and the second nominal constituent, e.g., *the demonstration of the theorem* vs. **the demonstration the theorem*. In the minimalist program, case theory has been eliminated; along with the other principles of UG postulated previously.
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36. Martin Haspelmath, "Chomsky Now Rejects Universal Grammar (and Comments on Alien Languages)," *Diversity Linguistics Comment* (blog), September 13, 2018.
37. The interview was titled "The Real-Life Linguist behind *Arrival* Weighs the Chilling Challenges of Alien Contact," and it was given to *Playboy* in 2016. To the best of my knowledge, it is no longer available online. The part I provide here was taken from Martin Haspelmath's blogpost referenced above.
38. Peter Hagoort, "MUC (Memory, Unification, Control) and Beyond," *Frontiers in Psychology* 4 (2013): 416, doi:10.3389/fpsyg.2013.00416.
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Terms of Endearment

Juan Uriagereka, reply by Evelina Leivada

In response to "Misused Terms in Linguistics" (Vol. 5, No. 2).

To the editors:

I would like to start by thanking Evelina Leivada for an essay that is useful in both scope and tone. Civil discourse over matters that matter—and terms certainly matter—is something we can all benefit from. Apologies on behalf of the field, also, about the amazing anecdote that closes the piece. If linguistics is not an interdisciplinary science, no science is.

A pedantic nitpick: what Leivada calls Universal Grammar (UG) is really a *theory* of the mental organ it is supposed to describe. Noam Chomsky coined the term I-language in 1986, long before anyone, so far as I know, started to put "i" in front of just about anything. I am not sure whether that term is any less confusing. I have no troubles with UG, so long as we understand that there is a biological entity of some sort, and then a theory about that entity. This theory has, of course, changed over the years. Indeed, the theory Leivada refers to as involving different kinds of factors, including genetic endowment, history, and economy principles—usually called the minimalist program—has only been around for the last quarter century.

Since I find myself in the same minority as Leivada, I will make the rest of my comments from that admittedly narrow perspective, triggered in large part by the need to safely place linguistics among the disciplines with which it interacts, as well as the permanent questions the field has faced about learnability and evolvability.

Many of Leivada's questions and clarifications stem from our theories of variation, which I entirely agree need attention. In fact, the situation may be even worse than she alluded to, whether we are speaking of parameters, features, or, for that matter, rules, as linguists did in the not-too-distant past. The numerical questions raised during the 1970s continue to be as pressing today as they were then. That is, so long as we assume that children acquiring a first language cannot rely on instructions, or what is commonly called negative data.

The problem emerges from the functions related to language variation. Call the number of objects of variation, whether parameters, features, rules, etc., X. Take n to be the number of variations that number X allows, which is assumed to be at least 2, often 3 for privative features, or occasionally a scale for degrees of variation. That will grow as n^X. It matters much if said objects of variation are optional or obligatory, which yields 2^X possibilities, and whether the elements that add up to X are ordered among themselves, which yields X! permutations. At that point, the explosion is served. This is why we know that either the entire approach is wrong or X has to be very small.

It is always possible to be wrong, although the issue then is to find an alternative theory that still does not rely on negative data or some other miracle. As for limiting X, one can stomp one's feet about this, in various ways. My parameters are of course better than yours, just as my dialect sounds cooler than yours. The issue is how to make progress in a manner that is nonparochial and testable by current techniques.

Back in the 1980s, some of us already thought features could be interesting, so long as they avoid Norbert Hornstein's pitfall, which Leivada mentions. Incidentally, Hornstein contributed to this pitfall, in my view, as did several others among my best friends, by taking θ-roles as features. Since that is just a dispute among siblings, I won't spill any blood here. But a serious issue remains: features should be what features are when observed across the world's languages. Period.

We probably will not debate whether "tense" or "person" or "number" and so on are features that simply show up across dialects, a question that drove philosophers in the Vienna Circle crazy. These are the imperfections of language. Linguists like me salivate over them. Not because we are library rats, checking dusty grammars for this kind of thing, but because it is the equivalent of astronomers finding a weird object out there in space. We presume there is some I-language, which UG is supposed to theorize about, and when we find its offspring, we get teary eyed and open the champagne. Our theories are such stuff as those imperfections are made on.

Incidentally, this approach has created a number of headaches for those of us, happy savages, who come from

the worlds of exotic languages. The deal was normally this: if your feature from a godforsaken dialect proved some pet theory of the moment—the *moment* being Chomsky's Fall Class at MIT, which some of us religiously attended whenever we possibly could—you were the toast of the town for the next fifteen minutes. Or the next fifteen years, if you milked it properly. Alas, should the feature make the opposite case, it depended on how well you behaved. You could bring it up in the hope that someone in an ivory tower would find a reasonable way to incorporate it, sooner or later, at which point you might get some credit *if* the feature came from your own native language. But if you decided to make an amendment to the theory yourself on the basis of your feature... may all your papers be in proper order.

I kid because I care, and because frankly things have improved, in my view, largely due to the herculean effort of Ken Hale. He crisscrossed the world over in search of said features, in the process making friends with the locals whose languages he acquired. Note, I haven't said "learned"; he somehow *acquired* them! You simply cannot describe as *appropriating* anything done by a friendly man who comes to live in your hut and fully acquire the local language, and at the same time introduces the principles of linguistics. I can name several groups of linguists, some very close to my heart, who came of age through Ken's vision.

Anyway: *that's* a feature. Nothing more, nothing less. If you postulate an abstract feature to make your trains run, cool: you do what you have to do. But be ready to work with others who may help your theory, to find the damn thing in some form or another in the world's languages. If it ain't there, it ain't a feature. Not yet anyway. Call that, if you will, Uriagereka's razor: *Do not claim a feature you need, unless you find it*.

In thinking about the matter for half a minute and without trying to be exhaustive, I came up with the following inventory of features that can be found in language after language.

> *Features generally presupposed in generative systems*: perspective, wh-/focus, tense, negation/emphasis, mood, aspect, voice, person, number, gender, case, definiteness.

I'm sure we can all think of more, but that is plenty to make my point, simply by making X equal, say, 12, for the 12 features just listed.

I understand that classical learnability, based on something like the subset principle—that learners guess the smallest possible formal language compatible with the evidence received—is irrelevant in I-language terms, as it is unclear what the term smallest possible means when not dealing with set-theoretic objects, as in an E-language. Still, there has to be a time *t*, positive of course, that it takes a learner to figure out a parametric option, whether a rule, a parameter, a feature, or whatever varies. Say you are at an intersection looking at Google Maps and hesitate whether to follow its directions or take that other left the nice neighbor offered as an alternative. How large is *t*? Since we are dealing with a cognitive process, possibly small. Slower than a mere reflex, but perhaps faster than an immune response.[1]

If we are going to claim physiological clout for the discipline, we may as well take it moderately seriously. Say you determine, for example, one minute for *t* on average, just to make the calculations easy. Of course, you must also assume that children pass the time doing things other than acquiring language; they sleep, eat, poop, and spend an enormous amount of time messing around, attempting to steal their parents' gadgets, etc. I have no idea what would be a realistic average time that a normal developing child devotes to language acquisition. Again, just for simplicity, let's assume, unrealistically, that a child devotes eight hours a day to language acquisition, or 480 minutes. In a good day, the child can make 480 linguistic decisions.

Returning to the above list of features generally presupposed in generative systems, those features, if binary, would take 2^{12} = 4,096 minutes to set, under the assumptions just run. Not bad, right? That very efficient child, working eight hours a day on the task, would be done in less than ten days. But is the case feature really binary? How about person? Perhaps it is a bit more than that. Just for clarity, $2^{10} \times 3^2$ = 9,216, even though it is not clear that case features, at least in principle, are merely ternary; you can be inherent (of several types), structural (of usually four types: nominaccusative, ergabsolutive, dative, and genitive, two of which divide further), without going into lexical cases. Similar issues can be raised about person values. These often vary across the world's languages in terms of including or excluding the addressee, and in Thai—just to drive the point home—whether you are addressing His Royal Majesty. Depending on how you count, that's really anywhere between four and ten values that UG provides! And, again for clarity: $2^{10} \times 4^2$ = 16,384, $2^{10} \times 10^2$ = 102,400. Well, 213 (eight-hour) days is probably still not too bad—after all, kids also work weekends.

Except that we haven't even started. Remember the optionality of features? Jacqueline Lecarme was one of those out in the bush who publicized the remarkable fact that Somali codes tense *within noun phrases*, contrary to what our most revered forefather Marcus Terentius Varro preached. In his *De Lingua Latina*, Varro first taught us how to divide speech into four parts, one in which the words have cases, a second in which they have indications of time, a third in which they have neither, and a fourth in which they have both.[2] Varro was wrong: UG allows languages to indicate time even if they *also* indicate case, and Somali is there to prove the point. Ah, but that means features may or may not be in categories that a learner is trying to acquire. This puts our ongoing calculation at

something more like 102,400 × 2^{12} = 419,430,400 minutes. 2,394 years to learn a language is a lot of time, even for children working very hard at it. If it took a second, instead of a minute, to set those options, it would all amount to forty years. Didn't it take Someone thirty to come of age?

I am afraid we may not be done yet: the features have yet to be ordered. This is being pursued seriously by some theorists, so as to determine how agreement relations work. If the ordering follows from something else, such as syntactic configuration, which is itself dependent on more elementary matters, perhaps there is nothing to worry about. But if it is truly the case that features can *also* be linearly ordered—in addition to possibly having multiple values, *and* allowing for obligatory (tense in V) versus optional (tense in N) manifestations—then the crazy number arrived at in the previous paragraph must be multiplied by the factorial function that arises from permuting the features. And, boy, does that baby grow. If only one or two features need to be ordered, it is not a big deal. If it is half of the 12 features listed above, it is already 720 possibilities. With a dozen features, the number of combinations is 479,001,600. That result alone is a nightmare; even if assigning one second to time *t* for evaluating each of these many options, the task would take over 45 years. When one combines that with the considerations above, we lack a theory.

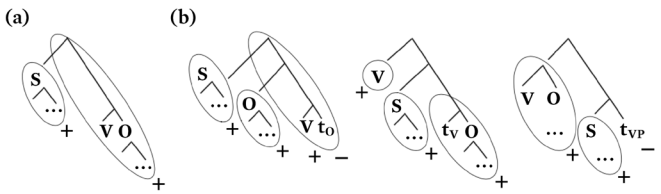
None of these issues are new. A textbook summary can be found in the second chapter of *Syntactic Structures Revisited* by Howard Lasnik et al., published in 2000.[3] These were the simple mathematical considerations that helped us move from rules to principles in the 1970s. To be honest, I am not sure how to address this crisis. One can reduce the time *t* it would take to evaluate these options to the order of nanoseconds, whatever that means in neurophysiological and cognitive terms, or else we need to restrict our theories. This was known half a century ago, and it is no less true today.

If I were to make a positive suggestion to sharpen Uriagereka's razor, it would be with the following statement: you need parameters that correspond to actual development.

This touches on Leivada's comments about development, which are entirely apropos regarding metaphors, although they also can be qualified via what we know about development. For instance, if a neural language network is dictating matters, as Angela Friederici has claimed,[4] that ought to determine a definite phase one starting in utero. Adam Wilkins, Richard Wrangham, and W. Tecumseh Fitch, as well as Cedric Boeckx et al., have provocatively suggested that neural crest development could be playing a role much earlier, related to what may have been aspects of domestication, in the technical sense, in language evolution.[5] This is all highly speculative, but, from the point of view of the working linguist, highly plausible too, in possibly reducing the class of variations from the get-go.

Suppose that some version of Richard Kayne's linear correspondence axiom, or just about any other variant that maps hierarchical structures to linear orders in phonology, can be assumed.[6] Key to these general ideas is that they massively change phrasal topology. Kayne's version,[7] whose ultimate veracity is entirely immaterial to the argument I am trying to make, would yield something like the following.

Figure 1.



a) Linear correspondence axiom default, b) marked possibilities arising via movement.

The point is this: Kayne, or anyone else attempting what he tried, is plausibly predicting rather different (broad) prosodic envelopes for the objects in Figure 1, each object corresponding to one of the ovals. These objects are meant to separate default clausal orderings in a language such as, say, Chinese, as represented in Figure 1a, from those in, for instance, Japanese, Irish, or Malagasy, in that order for the examples in Figure 1b. I personally find it moving that Kathleen Wermke et al. have demonstrated babies cry slightly differently in the context of different languages, presumably as a consequence of setting these very early, entirely core, parameters.[8] I like to think of these as dark parameters.

Suppose the picture—dare I say theory?—of parameters is something along the lines of the model presented in Table 1, which I obscurely proposed back in 2007:[9]

Table 1.

...Time...	a. Development	b. Acquisition	c. Learning
Neurobiological	Cranio-facial dev.?	Neural language network	Prefrontal cortex
Psychological	Sleep?	Sub-case parameters	Idioms, fads
Sociological	Mother	Speaker populations	Cliques

A model of language development, acquisition, and learning.

Again, it does not matter whether this model is precisely correct. The only point it makes, in relation to the numerical explosions above, is that it reduces parametric possibilities to

1. those in genuine development, as in dark parameters,
2. those in genuine acquisition via standard positive data, as in sub-case parameters, and
3. those that can be tweaked up until adolescence, as the pre-frontal cortex continues to mature and group identities are formed, as in microvariation.

In that order. Note that order here would not be strictly *acquired*, as it is dictated by development as one transitions from being a baby, to a child, to a teenager.

Homework is needed, though. We still want to see what sorts of features make it where and why. How, for instance, case features may have a bearing on the orderings that stem from Kayne's rationalization, in which case (pun intended), those would be effectively dark-featured. As opposed, perhaps, to adjustments on default values that continue pretty much throughout life. I, for one, have had as much trouble adjusting to using default gender values more appropriate to today's sensitivities as I did to using Zoom the last few weeks. I think of that as bona fide learning of the sort driven by purely statistical considerations, while earlier decisions in the model in Table 1 would be more akin to growing.

Last but not least, the theory presumed in Table 1 also makes predictions about acceptability, another important point Leivada discusses. A violation of grammar corresponding to superficial matters, such as those in column (c), need not be even remotely in the same league as violations corresponding to the dark parameters of column (a). In physiology in general, it seems kind of goofy to speak in terms of absolute grammaticality, as if a fever of over 100°F is ungrammatical but one under is only dispreferred. It all is what it is, and degrees of acceptability hopefully correspond to the stabs we are taking at modeling it all, whether as in Table 1 or any other approach one may reasonably attempt. I have tried to convince my experimentalist friends, so far without success, that this is a good idea to test.

I don't mean to plug my own work or that of my associates. But these are the perspectives that I have learned from the scholars cited in this commentary, plus a few others, especially Željko Bošković, Stephen Crain, Bill Idsardi, Tony Kroch, David Lightfoot, Massimo Piattelli-Palmarini, Eduardo Raposo, Ian Roberts, Doug Saddy, and William Snyder. To me, it all suggests that we are not as far from one another as it may at first seem, even if it is not always easy to agree on the terms we use, whether to endear ourselves with one another's proposals or to challenge them—all of which is, of course, useful.

Juan Uriagereka

Evelina Leivada *replies:*

I want to start by thanking Juan Uriagereka for the very thoughtful and interesting letter that usefully expands on all the important points of my essay. This will be a very short reply, not because I do not have more things to say on the topic of misused, ambiguous, and polysemous terms in linguistics,[10] but because I want to limit myself to the contents of his reply, and, unsurprisingly, it seems that I agree with everything he wrote. I want to highlight three points of agreement that I find crucial.

First, it is indeed true that the situation may be even worse than what I described, especially if we bring under scrutiny the primitives of inventories that feed one another: parameters that are localized to functional heads and heads that multiply to accommodate new features, according to the one feature, one head approach. I should perhaps explain that I had good reasons to tone down this bit of the discussion. The first reason has to do with personal preferences over tone and discourse. The second reason has to do with the reaction I received when presenting this work in conferences. One of the comments I got was that a junior linguist is in no position to tell other linguists how to do linguistics. Although I hope that my essay has made clear that this is far from my intention, I find the logic of this argument to be part of the problem. Uriagereka is right; the situation *is* worse, because certain terms of endearment are defended to the degree that attempts to track progress or inconsistencies in their use over the years is criticized by some as unnecessary nitpicking. I find the logic of this argument interesting too, especially when it comes from theoretical linguists. Following the exact same reasoning, who was Noam Chomsky in 1959 when he published his seminal review of B. F. Skinner's *Verbal Behavior*, if not a recently graduated young academic who reviewed the work of a prominent and much senior figure in the field? Scientific claims do not gain veracity based on how long they, or their proponents, have been around.

The second point on which I agree with Uriagereka is the claim that the problem arises mainly from the study of language variation. Enter "understudied varieties" here and amazing things happen. Uriagereka's description of the feature-discovery process is exquisite and it summarizes the situation much better than I did. Uriagereka's Razor, a term that I hope will find its way to linguistic textbooks—*Do not claim a feature you need, unless you find it*—is spot-on too. If I could add something to this formulation, it would read: *Do not claim a feature you need, unless you find it. And do not claim you found it unless its existence has been independently and repeatedly verified*—meaning the feature is seen by someone other than you, your three students, and your two lab associates. If that is not the case, it is fine, and nothing prevents you from still working with it. Call it a working notation based on your reading of the data, call it a hypothesis to be explored, perhaps describe its anticipated cognitive function—why does the system you describe need it?—but don't just say that this is a new feature A that describes a structure B, nefariously building on the unmentioned and unproven assumption that this

feature is innate and you have just discovered a previously unknown primitive of human cognition or biology.

The last point of agreement I want to highlight has to do with the notion of endearment. Linguistics is a small field, or at least it seems so if one looks at the number of grants that are given to linguists from big funding schemes in Europe.[11] Do we want it to remain small? Having a strong preference for terms that have been around for decades is perfectly understandable. However, the need to resolve issues that pertain to terminological coherence in order to boost the influence that linguistics exerts on neighboring fields should be stronger than one's affection for certain terms, precisely because the field is small and "we are not as far from one another as it may at first seem," as Uriagereka correctly observed.

1. A tradition dating back to the work of B. Elan Dresher and Jonathan Kaye in the 1990s argues for a cue-based approach to these issues, whereby the learner is supposed to tackle not the primary linguistic data, but some, more sophisticated, higher order data (the cue), to settle on given options to the exclusion of others. B. Elan Dresher, and Jonathan Kaye, "A Computational Learning Model for Metrical Phonology," *Cognition* 34 (1990): 137–95, doi:10.1016/0010-0277(90)90042-I. In such a model, the time for each decision is not multiplied, but merely summed, to the others. The jury is still out on how such an approach would work in the sorts of syntactic instances we care about in syntax, particularly for unbiased learners whose data is of the sort human children arguably encounter. See, for example, Lisa Pearl, Timothy Ho, and Zephyr Detrano, "An Argument From Acquisition: Comparing English Metrical Stress Representations by How Learnable They Are From Child-Directed Speech," *Language Acquisition: A Journal of Developmental Linguistics* 24, no. 4 (2017): 307–42, doi:10.1080/10489223.2016.1194422 for discussion.
2. Jacqueline Lecarme, "Tense in the Nominal System: the Somali DP," in *Studies in Afroasiatic Grammar*, ed. Lecarme, Jean Lowenstamm, and Ur Shlonsky (The Hague: Holland Academic Graphic, 1996).
3. Howard Lasnik, Marcela Depiante, and Arthur Stepanov, *Syntactic Structures Revisited* (Cambridge: MIT Press, 2000).
4. Angela Friederici, *Language in our Brain* (Cambridge: MIT Press, 2017).
5. Adam Wilkins, Richard Wrangham, and W. Tecumseh Fitch, "The 'Domestication Syndrome' in Mammals: A Unified Explanation Based on Neural Crest Cell Behavior and Genetics," *Genetics* 197, no. 3 (2014), doi:10.1534/genetics.114.165423; Cedric Boeckx et al. "Dosage Analysis of the 7q11.23 Williams Region Identifies BAZ1B as a Major Human Gene Patterning the Modern Human Face and Underlying Self-Domestication," *Science Advances* 5, no. 12 (2019), doi:10.1126/sciadv.aaw7908.
6. Richard Kayne, *The Antisymmetry of Syntax* (Cambridge: MIT Press, 1994).
7. The ultimate veracity of Kayne's version is entirely immaterial to the argument I am trying to make.
8. Kathleen Wermke et al., "Fundamental Frequency Variation in Crying of Mandarin and German Neonates," *Voice* 31, no. 2 (2016), doi:10.1016/j.jvoice.2016.06.009.
9. Juan Uriagereka, "Clarifying the Notion 'Parameter,'" *Biolinguistics* 1 (2007): 99–113.
10. In fact, as I also say in my response to Anna Maria Di Sciullo's letter, many more terms can be critically discussed for content and use.
11. From the 6,907 projects funded by the European Research Council in 2017, 2,445 belonged to the panel Life Sciences, 3,113 to the panel Physical Sciences and Engineering, and 1,349 (a mere 19.5%) to the panel Social Sciences and Humanities, of which linguistics is a small part. Eva Kondorosi, "The ERC: Funding Investigator Driven Frontier Research," presentation at the European Research Council's Scientific Council Working Group on Widening European Participation, Tbilisi, Georgia, April 20–21, 2017.

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https://inference-review.com/letter/terms-of-endearment

A Collective Action Problem

Martin Haspelmath, reply by Evelina Leivada

In response to "Misused Terms in Linguistics" (Vol. 5, No. 2).

To the editors:

All linguists are aware that our terminology is often unclear, and sometimes downright confusing. This is true of not only high-level explanatory terms such as "parameter" or "universal grammar," but also everyday terms such as "sentence," "word," "pronoun," "gender," and "morph," or "morpheme." If anything, the problem is worse in the case of everyday terms, because many linguists who are not specialists in morphosyntax are not even aware that these terms do not have clear meanings that are widely shared. Anyone who talks about universal grammar knows about the minefield they are entering, but most people who work on syntax and morphology seem to be quite unaware that there is no clear general definition of "word," and as a result no good reason to separate morphosyntax into two distinct domains.

What is wrong with linguists? Do we simply pay insufficient attention to careful methodology? Would better education help? There is currently an effort underway to share data more widely and to make our research more reproducible.[1] There are also ongoing discussions about the best primary-data methodologies, about language sampling,[2] and so on. In short, linguists do not appear unconcerned about methodologies.

But if it is not a case of scientific neglect, what is the problem?

I have been thinking about terminological issues in my field for a long time, and I have arrived at two preliminary conclusions. First, most linguists think that our terminology can only be as good as our theories, so we need to work on our theories. The better the theories become, the closer we will get to solving our terminological problems. Second, uniform terminology is a collective action problem, and in the absence of an authoritative organization, there is no way in which uniformity can be achieved.

Within biology, there is a subfield known as theoretical biology, and most ordinary working biologists are not too worried about the discussions taking place in that field—probably because contributions to understanding a single species or ecosystems are highly valued. But in linguistics, most of us are greatly interested in general theories. We also often think that we cannot work on a single language without a solid basis in some general theory, and that we must contribute to general theory. This is in some sense tragic, because there is so little consensus on general theory. As a result, many works on languages are dependent on some specific jargon or notation, and are hard to understand for readers who are not familiar with a particular theoretical orientation. This leads to a fragmentation of the field that is often deplored, but rarely understood as resulting from the widespread focus on general theory.

With a range of fragmented general theories, what are the prospects for improving terminology by improving the theories?

It seems that as a first step, linguists need to decouple their terminology from their theories. There are of course many things on which there is no serious disagreement, and linguists could discuss basic terms for such phenomena. There is no disagreement that language is a species-specific trait of humans. We could call this trait "linguisticality," using the analogy of "musicality."[3] This trait was called the *faculté du langage* (capacity for language) by Ferdinand de Saussure a century ago. This name was clear enough at the time, but since the 1960s, the term "language faculty" has come to be associated with a contentious view of what is important in language, so it is not widely accepted as a term for linguisticality. Some linguists even suggest that there is no language faculty, by which they surely do not mean that they reject the idea of language as a species-specific trait of humans. Another example is the term "morph," which can be used for a minimal linguistic form,[4] regardless of one's general theoretical predilections. The term "morpheme," associated with a particular view of how complex "words" should be described, is widely rejected by general theorists, even though it is used for morphs all the time. Next, linguists could define a complex grammatical concept such as "serial verb construction" in a rigorous way that is independent of particular theories,[5] and then try to theorize about the phenomena that are described by this term. If

there were a common nomenclature of a few dozen terms, this would make the lives of linguists much easier. They could focus on describing particular languages and would not have to worry constantly about the general theoretical proposals that are currently in vogue.

Why are we not doing this already?

Some of my colleagues would likely raise the objection that biologists and chemists do not proceed in this way either—they first identify objects of nature and give them labels afterward. Carl Linnaeus knew how to identify species before he gave them names, and chemists converged on a unified nomenclature only toward the end of the nineteenth century, beginning with an 1892 Geneva conference, when the most important issues had been settled. Is linguistics dealing with natural objects in the same way in which biologists and chemists are dealing with natural objects? Well, not really. Most of the time, linguists study culturally specific phenomena. And if general linguistics is a branch of psychology, then it is a branch of cross-cultural psychology.[6] Linguists may eventually be able to reduce phenomena such as serial verbs, relative clauses, or ergative constructions to primitive features of the human mind, maybe an innate grammar blueprint, a domain-specific aspect of human linguisticality. The same may be true for other concepts that are used by psychologists—empathy, introversion, cognitive bias, etc.—but psychologists do not suggest that these terms need not have the same meaning for everyone. The objects that linguists identify in practice are culture-specific phenomena, not objects of nature like chemical elements. A good strategy for linguists might be to aim for a range of commonly understood terms, and to try to use introductory textbooks that do not rely on highly specific theoretical claims. In subfields such as syntax and morphology, this is not currently done.

Most linguists assumedly are convinced that it is useful to decouple theories from core terminology—but how would a more rigorous terminological practice come about?

This is a collective action problem of the sort that theoretical linguists rarely, if ever, contemplate. We happily come together at conferences, but we never act together. Governments never ask us for our opinions, so there is no need to formulate a minimal consensus. We are content if some colleagues volunteer to organize a conference and serve as journal editors, and we enjoy the wide range of different points of view found in our discussions. But we do not dream of delegating decisions on terminology to some kind of terminology committee. At least not so far. Maybe the future will bring changes.

Other fields have had terminology committees for many decades. The work undertaken by these committees is perhaps not all that exciting, but it is generally regarded as indispensable, even if the decisions are sometimes annoying. Was it really necessary for the International Astronomical Union to redefine the term "planet" in 2006 in such a way that Pluto no longer qualified and was degraded to a dwarf planet? The specialists must have had good reasons. Experts should, of course, be careful with terms that are widely used by the general public—nobody wants to see linguists make authoritative pronouncements about a definition of "word" that defies most spelling conventions. We should try to define nontechnical terms such as "sentence," "question," "synonym," "language," and "linguistics" in an intuitive way. But technical terms such as "morph" or "serial verb construction" may well be defined in ways that not every linguist finds immediately intuitive, since their meanings are purely conventional for a group of professionals. Individual intuitions will not automatically converge, but many linguists may be willing to converge in their usage once a terminology committee has made a proposal.

In Evelina Leivada's essay, she rightly emphasizes that terminological clarity matters, but what is missing is a path toward such clarity. Could a committee help with the ten problematic terms that she discusses? Maybe such a committee would recommend that the terms "hardwired" and "grammaticality judgment" should be avoided, because we do not need them. Indeed, the latter is widely thought to be internally contradictory. The term "feature" would likely be judged unproblematic, because a feature is simply a property of a class of linguistic forms or other units. But most of the other seven terms are intimately bound up with particular theoretical proposals, especially proposals coming from the generative grammar tradition.

Although terms such as "parameter," "universal grammar," "optimal design," and "faculty of language" in the broad or narrow sense, are terms that have been influential among Chomsky's students and their students, these ideas have never spread to linguistics as a discipline. Since the group of generative grammarians is large and highly visible in linguistics, it is easy to mistake generative linguistics as linguistics itself, but the core idea of this approach—that a substantial amount of knowledge of a language is contributed by an innate blueprint for grammar—has been more presupposed than supported by robust evidence. One could perhaps imagine a committee just for this particular approach to linguistics, but even among the Chomskyans, there are many divergent views, and probably not enough common ground to agree on clear definitions of terms like "universal grammar" or "optimal design." It also seems that these expressions do not really have the status of technical terms. They instead refer to speculative ideas, which are hoped to bring greater insight eventually, but which are not necessarily part of the discipline's textbook knowledge.

Although I applaud Leivada's goal of improving terminological clarity, I do not see reasons for being optimistic when it comes to highly contentious, speculative concepts and associated terms. Maybe linguists and psycholinguists should simply accept that we will not make serious progress on these larger issues anytime soon and instead

focus their attention on more tractable problems. I also do not think that terms such as "misuse" and "inaccuracy" are helpful in bringing everyone on board. It is not inconceivable that some scholarly association, such as the Permanent International Committee of Linguists, might organize a terminology committee at some point in the future when enough linguists recognize that our basic terminology can be decoupled from theory.

Martin Haspelmath

Evelina Leivada *replies:*

Martin Haspelmath begins his letter to the editors with a claim that needs little defense: "All linguists are aware that our terminology is often unclear, and sometimes downright confusing." Although most linguists will agree with this view, it is less clear that most linguists will agree on precisely which terms are ambiguous, unclear, or downright confusing in present-day linguistics.

Haspelmath offers a couple of very useful insights about the problem of terminological unclarity in linguistics. The first links terms with theories. More specifically, Haspelmath suggests that as the theories become better, terminological problems are closer to being solved. Although this must be true in some cases, it is also possible that precisely because theories have various levels, while one level becomes better and more complete—tested against more languages, verified through different experimental techniques, and expanded to cover diverse populations—another level is weakened. Here is a concrete example. The notion of parameter was well-defined and unambiguous in its early days. It is through the subsequent research on language variation that linguists found that a handful of macro-parameters could not explain the full range of the attested variation. Cross-linguistic research progressively led to an unknown number of variably sized parameters and accordingly tailored definitions of the term. From a descriptive point of view, the theory became more complete because linguists developed a better idea of how parameters behave cross-linguistically. But a high degree of adequacy at one level brought along a decreased accuracy at another. As Theresa Biberauer et al. argue in their discussion of comparative syntax and the way parametric models capture variation, recent linguistic descriptions have achieved a high level of descriptive adequacy, but this was done at the expense of explanatory adequacy.[7] As the theory more accurately described parametric variation across different languages, the core notion was redescribed in various ways, but the nature of the relevant observations was not properly explained and a riotous polysemy ensued. One could argue that a theory does not really become better until all levels of adequacy are developed, but I suspect that linguists from different subfields would not agree on the criteria that can be used to evaluate whether a theory has actually reached a satisfying level of development across different levels of adequacy.

This brings me to Haspelmath's second important point: Reaching agreement. He views uniform terminology as a collective action problem, further arguing that uniformity cannot be achieved in the absence of an authoritative organization. The first thing to consider about this proposal is the source of such authority, its limits, and how it would be manifested in practice. Of course, an organization may offer definitions of certain key terms and compile lists of landmark references, but adhering to these definitions would be up to individual discretion. Essentially, initiatives like *Glottopedia*[8] (Haspelmath is on its Scientific Advisory Council) are already doing an excellent job in providing such a service, yet uniformity has not been reached. The second aspect to be considered is the composition of the organization itself. Recent developments in the field of linguistics about who gets to speak for us[9] have made it clear that some (not only junior) linguists feel that the values promoted by some prominent figures—who are likely to participate in an organization that exerts authority—do not represent them. Although this matter is at present orthogonal to the use of linguistic terms, deciding who sits on the committee can be a turbulent issue. As recent experience has shown, breaking through the narrative of the privileged voices is hard for some marginalized groups.[10]

I agree with many of Haspelmath's other points, especially his claim that what is missing from my essay is a path to achieving terminological clarity. Although I believe that acknowledging a problem is always the first step, such a path is indeed absent.

I disagree with Haspelmath on two points, the first one more important than the second, due to its relevance to the topic of terminological clarity and uniformity. Haspelmath argues that "the core idea of this [generative] approach—that a substantial amount of knowledge of a language is contributed by an innate blueprint for grammar—has been more presupposed than supported by robust evidence." This formulation is not an accurate representation of Chomsky's use of the term Universal Grammar. More specifically, the blueprint is not *for grammar*, as Haspelmath suggests; it is rather about how the physical signal is determined by universal, innate, language-independent principles that relate semantic and phonetic information, mediated by syntax.[11] The thesis that there is such an innate predisposition for developing language in our species *is* supported by robust evidence. To name just one classical book, Eric Lenneberg's *Biological Foundations of Language* is dedicated to the biology of this *language-readiness*, that is to uncovering those biological principles that explain the development of language, as a unique behavior displayed by a single species.[12]

The second point on which I disagree has to do with the disconnect that Haspelmath finds between theoreti-

cal biology and working biologists. He writes that "within biology, there is a subfield known as theoretical biology, and most ordinary working biologists are not too worried about the discussions taking place in that field." The reality is that although there are biology journals devoted to theoretical hypotheses, there is no disconnect between theory and experimental practice, because the hypotheses advanced in theoretical journals are testable and formulated to be confirmed experimentally. The best explanations for the obtained results are *then* synthesized into theories that guide practice, forming a direct connection between the theory and the actual practices of working biologists.[13]

Haspelmath is right when he writes that linguists of different persuasions happily come together at conferences, but never (or, in my opinion, rarely) act together. Perhaps the first step to remedy this problem is for linguists not to familiarize themselves with the terms used in other linguistic frameworks, but to use the definitions of these terms as they were put forth by their original proponents.[14] Using a term is not the same thing as knowing its correct meaning and scope. The next step is to decouple terms from theories, as Haspelmath correctly proposes, in order to establish common ground that will enable linguists to act in a collective way. This will be to the benefit of our field, and we are in it *together*.

1. E.g., Andrea Berez-Kroeker et al., "Reproducible Research in Linguistics: A Position Statement on Data Citation and Attribution in Our Field," *Linguistics* 56, no. 1 (2018): 1–18, doi:10.1515/ling-2017-0032.
2. E.g., Matti Miestamo, Dik Bakker, and Antti Arppe, "Sampling for Variety," *Linguistic Typology* 20 no. 2 (2016): 233–96, doi:10.1515/lingty-2016-0006.
3. Martin Haspelmath, "Human Linguisticality and the Building Blocks of Languages," *Frontiers in Psychology* 10, no. 3,056 (2020): 1–10, doi:10.3389/fpsyg.2019.03056.
4. Martin Haspelmath, "The Morph as a Minimal Linguistic Form," *Morphology* 30, no. 2 (2020): 117–34, doi:10.1007/s11525-020-09355-5.
5. Martin Haspelmath, "The Serial Verb Construction: Comparative Concept and Cross-Linguistic Generalizations," *Language and Linguistics* 17, no. 3 (2016): 291–319, doi:10.1177/2397002215626895.
6. John Berry et al., *Cross-Cultural Psychology: Research and Applications* (Cambridge: Cambridge University Press, 2002).
7. Theresa Biberauer et al., "Complexity in Comparative Syntax: The View from Modern Parametric Theory," in *Measuring Grammatical Complexity*, ed. Frederick J. Newmeyer and Laurel Preston (Oxford: Oxford University Press, 2014), 104.
8. *Glottopedia*.org, ed. Sven Naumann and Jan Wohlgemuth.
9. Itamar Kastner et al., "Who Speaks for Us? Lessons from the Pinker Letter," *lingbuzz*/005381 (2020).
10. For an excellent analysis of this problem in the context of Steven Pinker's letter, see Gillian Ramchand, "Pinker, Free Speech and Academic Integrity," *Gillian Ramchand Homepage* (blog), July 9, 2020.
11. For an early presentation of the term Universal Grammar that goes far beyond grammar, see Noam Chomsky, "The Formal Nature of Language," in Eric Lenneberg, *Biological Foundations of Language* (New York: John Wiley & Sons, 1967), 397–442, doi:10.1017/CBO9780511791222.008.
12. Lenneberg, *Biological Foundations of Language*.
13. I thank Myrtani Pieri for feedback on this point.
14. For example, the formulation "innate blueprint for grammar," which Haspelmath attributes to the generative tradition, is used mostly or perhaps exclusively by linguists working outside of it. See also endnote 11.

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On Attitudes Toward Terminology

Fahad Rashed Al-Mutairi, reply by Evelina Leivada

In response to "Misused Terms in Linguistics" (Vol. 5, No. 2).

To the editors:

The objective of Evelina Leivada's essay, as she makes clear, "is to attain a higher level of terminological clarity and coherence within the field of linguistics." There is no doubt that linguistics is in need of such an initiative, and Leivada's attempt at fulfilling it is in itself worthy of praise. But out of the ten key notions she focuses on, three seem highly problematic: Universal Grammar, faculty of language in the narrow sense, and grammaticality judgment. Leivada's discussion of these terms appear to be wanting in one respect or another.

Linguistics is a vast field of research, and generative grammar is one of its subfields. Chomskyan linguistics belongs to the latter and it is this branch of linguistics that the ten terms Leivada chose to focus on belong to. The title of her essay should really have been "Misused Terms in Chomskyan Linguistics." If not for the sake of accuracy, it would have at least been fair to many linguists who, sadly enough, may well be unfamiliar with, or simply uninterested in, Chomsky's work.

As to the objective of Leivada's essay, terminological clarity in linguistics—and for that matter, in any field of research—is certainly a must. Quarreling over the meaning of a term, aside from being tedious, is also a sign of a communication failure among practitioners in any field of knowledge, linguistics included. Leivada begins her essay by quoting Eörs Szathmáry, who complains—rightly so, in my view—that linguists "would rather share each other's toothbrush than each other's terminology." "This," as Leivada asserts, "is far from an isolated view." Now, if this is true, and I believe it is, it suggests that the problem goes deeper than simply fixing the semantic content of linguistic terms. It is not merely a problem of terminology; it is principally one of attitude.

Leivada's essay would have been much more satisfactory and useful to linguists had she taken the trouble to dig deeper than terminological clarity and consider linguists' attitudes toward both their own terminology and that of others. With respect to the former, it would be more instructive, for instance, to ask why theoretical terms come into fashion and fall out of it, and whether this has to do with empirical considerations or is simply a matter of caprice. I have in mind the so-called economy principles such as "procrastinate" and "greed," which had been given considerable attention in early stages of the minimalist program before they gradually fell out of use. As to the attitude which many linguists have toward each other's terminologies, it would be interesting to compare linguistics with physics in this respect. The latter is a field that takes terminology very seriously—so seriously, in fact, that a lack of consensus on a term's definition can suffice as a reason to jettison the term entirely. In a symposium on the physical concept of weight, one physicist complained that

> the physics teaching community should at least agree on a definition of the word "weight." That we do not is very strong evidence to me that the word should simply never appear in the literature or textbooks.[1]

One could only wonder what consequences such a high standard would have for linguistics.

Leivada is right in saying that achieving terminological clarity in linguistics "may help improve its visibility in neighboring fields, such as other parts of psychology, biology, and neuroscience." But on several occasions Leivada appears to take for granted that if Chomsky means X and not Y, then this suffices for the clarification of a term. It may well be the case that the coiner of a term is the originator of the confusion associated with it. More importantly, rather than limiting herself to terminological clarification, Leivada would have offered an even better service to linguistics had she posed and attempted to answer the following question pertaining to the attitude of linguists toward their own terminology: Is it worth fixing the meaning of X without specifying the possible insights X would lead to?

In relation to the three problematic notion mentioned at the outset, consider first Universal Grammar (UG). Leivada asserts that, for Chomsky, "language universals" refer not "to properties that are universally attested to in

all languages, but to computational properties of the mind that are universal because they arise from a species-universal innate ability." "A property P in a given language," she continues, "can reflect a universal computational principle even if P is not attested to in another language." On what grounds, then, can it be said that P arises from a species-universal innate ability? The pre-minimalist strategy, as is well known, has always been that if P cannot possibly be derived from linguistic experience, then P must be *innate*, as opposed to learned. The answer to the above question is provided by the poverty-of-stimulus argument, according to which linguistic knowledge goes far beyond what the linguistic environment actually justifies. Now, since "innate" does not necessarily mean "genetically innate," the poverty-of-stimulus argument survived the transition to minimalism whose emphasis is on non-genetic nativism. In light of this, it is surprising to read Leivada saying that,

> given that a complete list of all the UG principles has not been compiled, the possibility that these principles are, for the most part, not language-specific, but have cognitive, third factor roots, cannot be ruled out.

I fail to see the link between "a complete list of all the UG principles"—whatever that means—and their language-specificity. Given the minimalist bottom-up approach to UG, it would be appropriate to say that an almost empty list of UG principles or properties is an empirical target based on the assumption that third factor roots have something to do with language.

The second notion is the faculty of language in the narrow sense (FLN). Leivada asserts categorically that "FLN is not the same thing as UG. FLN is unique to humans and unique to language. UG does not have this character." The matter is not so straightforward. Although Leivada is certainly right in stating that confusing these two terms "is a mistake that many linguists have made," the confusion itself is something to be expected given the strong similarity between the meanings of the terms. More importantly, Chomsky himself seems to have contributed to this confusion. He defines UG as the "theory of the genetic endowment of" the language faculty—a definition that suggests an asymmetry between UG and FLN.[2] The former concerns properties of language that are *genetically determined*, whereas the latter refers to properties of language that are *genetically unique* to it.[3] In that sense, FLN is a special case of UG. But at another point in the same source, Chomsky defines UG as "the theory of the distinguishing features of human language," and again elsewhere he states that "UG consists of the mechanisms specific to [the language faculty]."[4] From this perspective, there is no longer an asymmetry between UG and FLN, the two terms seem to be definitionally identical.

The third and final problematic notion is "grammaticality judgment." The distinction between "grammaticality" and "acceptability" is notorious for being a source of confusion, and Chomsky himself cannot be blamed for it. Indeed, as a careful reading of his early work confirms, he was both clear and consistent in his distinction between these two notions. Unfortunately, Leivada's attempt at clarifying what should have been clear all along has the opposite effect. Although she correctly defines grammaticality as conformity to the rules of a grammar, she nevertheless approves of labeling "grammaticality judgments" as a misnomer. "Speakers," she adds, "have intuitions only about their perception of linguistic stimuli." Speakers cannot judge a sentence's grammaticality but only its acceptability. But, as I have argued at length elsewhere, it is simply a fact that "native speakers can, in certain cases, judge a sentence's grammaticality," and it is precisely this fact that "makes linguistic intuition both (1) a convenient tool for the investigation of linguistic structure, and (2) an explanandum for which a theory is needed."[5] This was indeed the strategy that Chomsky adopted in the mid-1950s.

Fahad Rashed Al-Mutairi

Evelina Leivada *replies:*

Finding common ground is a useful technique to aid dialogue. Unfortunately, when it comes to our views on the topic discussed in my essay and his reply, Fahad Rashed Al-Mutairi and I seem to disagree on almost everything. Having such disagreements is fine, especially when it comes to views and not facts. As Charles Darwin wrote in the epilogue of one of his most influential books,

> False facts are highly injurious to the progress of science, for they often endure long; *but false views, if supported by some evidence, do little harm, for everyone takes a salutary pleasure in proving their falseness* [emphasis added]: and when this is done, one path towards error is closed and the road to truth is often at the same time opened.[6]

The first point on which I disagree with Al-Mutairi concerns the title I gave to my essay. Instead of "Misused Terms in Linguistics," he suggests I should have used "Misused Terms in Chomskyan Linguistics." I view "Chomskyan linguistics" as a term that deserves to be listed in a second list of misused, ambiguous, and confusing terms. If one uses a notion introduced by a specific scholar, this does not entail that she endorses all of this person's theses or that she wishes for her work to be embedded into an area of study that bears another person's name. This is not to deny the relevance of Noam Chomsky's work in the terms I discuss. He is indeed the proponent of many of them. This makes my essay a work that *in part* discusses terms used within the generative tradition, but this does not make it an essay in Chomskyan linguistics. More importantly, it is incorrect to attribute the misuse of some of the terms I discussed

exclusively to the generative tradition, when there is ample evidence for such misuse in other linguistic frameworks.[7]

I am also not convinced by the motivation that Al-Mutairi provides for the title he suggests. He writes,

> The title of her essay should really have been "Misused Terms in Chomskyan Linguistics." If not for the sake of accuracy, it would have at least been fair to many linguists who, sadly enough, may well be unfamiliar with, or simply uninterested in, Chomsky's work.

My essay provided more than ten references to Chomsky's work for those who are unfamiliar with it. As for the uninterested ones, they are free to not read Chomsky, but my essay is not signed with Chomsky's name and should not be considered Chomsky's work, so this latter concern is unrelated to my discussion of misused terms in linguistics.

I agree with Al-Mutairi's suggestion that considering linguists' attitudes toward their terminology would provide a service to the field. This, however, requires a proper experimental study, while the aim of my essay was a theoretical review of the actual use of certain terms. I hope such an initiative will be undertaken in the near future.

In relation to terminological concerns, Al-Mutairi argues that in my essay I appear to take for granted that if Chomsky means X and not Y when he introduces a term, this suffices to clarify this term. I am not sure how he reached this conclusion, but this is not a claim I pursued. What I proposed instead is that when a scholar introduces a term X further defining it in a way Y and then another person uses the same term X and defines it in a very different way W, the term becomes polysemous, and communication problems may arise. Chomsky's endorsement of one definition instead of another obviously does not suffice for the clarification of a term; if it did, we would have no reason to talk about misused, inaccurate, ambiguous, or polysemous terms in (generative) linguistics.

Perhaps the greatest distance between Al-Mutairi's views and mine is found in the context of our understanding of Universal Grammar (UG). He argues "I fail to see the link between 'a complete list of all the UG principles'—whatever that means—and their language-specificity." The link is robust and perhaps spelling it out in different words may be useful to other readers too. What are the UG principles? Imagine we compile all the candidates ever proposed in the literature. Then we can ask how many of these candidates have analogues in other cognitive domains or other species. Al-Mutairi is right that the list may be empty in light of the third factor approach, but this has not been demonstrated yet. If he thinks it has, it is surprising that he provides no references to this demonstration.

I would like to close this reply by drawing the readers' attention to Al-Mutairi's discussion of grammaticality and acceptability, because it a unique opportunity to show in practice how terminological confusion may arise. Al-Mutairi thinks I am wrong to have labeled the term grammaticality judgments a misnomer, because in previous work he has argued that native speakers can judge a sentence's grammaticality. This is an excellent example of how linguists use terms in different ways. Obviously, native naive participants can be asked if a sentence is grammatical. However, in linguistic tasks, participants are often instructed to disregard what the official books list as correct and provide an answer as to whether a sentence appears fine to them. In other words, the relevant tasks aim to tap into their idiolects; the tasks are not formal exams about how well participants remember the official grammar they learned at school. Therefore, native naive participants provide judgments of acceptability, and calling *this response* a grammaticality judgment is a misnomer. Al-Mutairi does not provide any arguments to refute this point; he simply argues that it is a fact that speakers have judgments about grammaticality, citing previous work where he makes the same claim, again without backing it up with arguments. To repeat, it is trivially true that if a person is asked whether a sentence is fine according to the official grammar books, they may be able to provide an answer, if they are literate and familiar with books of grammar. One may call this grammar exam a grammaticality judgment task, if one wishes to use this term in a way that is not typically used. However, even in this case, processing factors come into play and may lead participants to make an incorrect judgment, precisely because acceptability and grammaticality are separate notions.[8]

1. Ronald Brown, "Weight–Don't Use the Word at All," The *Physics Teacher* 37, no. 4 (1999): 241, doi:10.1119/1.880241.
2. Noam Chomsky, "On Phases," in *Foundational Issues in Linguistic Theory: Essays in Honor of Jean-Roger Vergnaud*, ed. Robert Freidin, Carlos Otero, and Maria Luisa Zubizarreta (Cambridge, MA: MIT Press, 2008), 134.
3. As an example, consider Chomsky's description of unbounded Merge as "not only a genetically determined property of language, but also unique to it." Chomsky, "Approaching UG from Below," in *Interfaces + Recursion = Language?: Chomsky's Minimalism and the View from Syntax-Semantics*, ed. Uli Sauerland and Hans-Martin Gärtner (Cambridge, MA: MIT Press, 2007), 5.
4. Chomsky, "Approaching UG from Below," 3.
5. Fahad Rashed Al-Mutairi, *The Minimalist Program: The Nature and Plausibility of Chomsky's Biolinguistics* (Cambridge: Cambridge University Press, 2014), 13.
6. Charles Darwin, *The Descent of Man* (Princeton: Princeton University Press, 1871).

7. For example, the term "genetic endowment" or "blueprint" has been used by critics of the generative enterprise, even in works that recognize that Chomsky's original aim was to look to the *overall biology* of the organism as the source for grammar. See, for instance, Daniel L. Everett, "An Evaluation of Universal Grammar and the Phonological Mind," *Frontiers in Psychology* 7, no. 15 (2016): 1, doi:10.3389/fpsyg.2016.00015.
   It is worth highlighting that Everett argues that Chomsky "proposes that all languages are simply local manifestations of a biologically transmitted Universal Grammar" (p. 1), but to the best of my knowledge, Chomsky never claimed that Universal Grammar is transmitted. The process of genetic transmission refers to the passing of genetic material from parents to offspring. Language is neither a gene nor a chromosome, hence the use of the term *transmission* is wrong in that context. Given that Everett does not provide a reference to Chomsky's work when he attributes this point to him, I conclude that terms introduced within the generative tradition may be used incorrectly both inside and outside of it. This makes Al-Mutairi's suggestion for the title (i.e., "Misused Terms in Chomskyan Linguistics") a poor fit.
8. Evelina Leivada and Marit Westergaard, "Acceptable Ungrammatical Sentences, Unacceptable Grammatical Sentences, and the Role of the Cognitive Parser," *Frontiers in Psychology* 11, no. 363 (2020), doi:10.3389/fpsyg.2020.00364.

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Terminology and Toothbrushes

Kleanthes Grohmann, reply by Evelina Leivada

In response to "Misused Terms in Linguistics" (Vol. 5, No. 2).

To the editors:

Although Evelina Leivada raises a number of important issues concerning terminological (mis)use and confusion in linguistic theorizing, her introductory quote should not be taken too literally. It may indeed be the case that "linguists 'would rather share each other's toothbrush than each other's terminology'"[1]—but so what? There are many situations that might prompt the sharing of toothbrushes, such as an unexpected stayover, a group of people stuck in the middle of nowhere with only a single toothbrush, and so on. In all such situations, there would arguably be more pressing concerns than dental hygiene. And sharing toothbrushes is not a problem if you disinfect the toothbrush first. It doesn't take much time or effort to prepare one's toothbrush for someone else's use. And this is where terminological conundrums in linguistic theorizing enter.

Terminology matters. And, indeed, it is everywhere, ranging from apparently banal such as, say, "bilingualism,"[2] to the specific terms that are "misused," as Leivada writes, and even to linguistics as a whole—take, for example, recent reflections by Martin Haspelmath.[3] But let me start from the end of her essay. Leivada suggests that "terminological clarity matters" because a "senior linguist" assumes that "UG is simply a repository of linguistic primitives that can be disconnected from human biology." Leivada strongly disagrees with this statement and employs it as an example of the misuse of linguistic terminology.

This scenario is a great example of a much more serious matter, which Leivada only mentions in passing. While she makes several statements about "the biological plausibility of an innate endowment for language," what is completely missing from Leivada's essay is any discussion of *bio*linguistics as the relevant field of study. By way of a partial disclaimer, I should add that I recently contributed to a survey of biolinguistics and its relevance for cognitive linguistics at large.[4] Leivada is heavily involved in this research agenda herself, as one of the initial team members and as associate editor of the free, open-access journal *Biolinguistics*, which I cofounded.[5]

The relevance of biolinguistics is that not every linguist subscribes to it as the designated research program. The senior linguist mentioned by Leivada is presumably one such researcher. Many linguists do not assume a biological point of view, or at least not the linguist's need to contribute to it. What makes matters worse, and I say this as a generative biolinguist, is that not every linguist is a generative linguist. And what Leivada really worries about is terminology used and misused by generative linguists themselves—mostly, though not exclusively, within the Chomskyan conception of linguistic minimalism.[6]

Here one can detect a category mistake. Some of the terms predate generative linguistics and were adopted or extended, such as "features." Others were co-opted and adapted, such as "universal grammar." Yet others were introduced by it, as in "faculty of language in the narrow sense." Still other terms enjoy heavy engagement outside linguistics, such as "bilingual advantage." Haspelmath is a general linguist who tries to put some order in terminology and definitions for a wider readership,[7] though frankly his attempts often seem to include all linguists *except* generativists.

Biolinguistics and generative grammar, or even linguistic minimalism, are not synonyms. Biolinguistics does not require subscribing to the minimalist program, nor can it be equated with generative grammar as a whole.[8] There are minimalist syntacticians who care little for essentially biolinguistic issues. There are also generative linguists both within and outside minimalism who do not, explicitly so, even if they assume a language faculty. Many linguists concerned with essentially biolinguistic issues even use the term "biolinguistics" in their work, but are anything but generative in their theoretical persuasion, from functional to other cognitive approaches.[9]

It is not immediately clear whether sharing terminology across disciplines, "such as other parts of psychology, biology, and neuroscience," as Leivada suggests, is really more important than sharing terminology across the subdisciplines of linguistics. If one's response is, "Yes, it is," then we might perpetuate the multi-forked approach to

language science. One route is nativist and generative, perhaps involving minimalist biolinguists pursuing their current research agenda, either alone—biolinguistics in the weak sense—or teaming up across disciplines—possibly biolinguistics in the strong sense. In that case, none of the terminological conundrums identified by Leivada really matter. The relevant players will work with their—or Leivada's?—understanding and pass it on to colleagues and collaborators from different fields. Other linguists will remain at a loss, as will anyone else who does not subscribe to these understandings.

Alternatively, linguists might define, redefine, or establish a common definition of all of these terms in a way that a larger group of linguists can agree with. But this means preparing terminology within the large, multifaceted field of language science. There is a rich tradition of adopting, adapting, co-opting, and (mis)using terminology in recent history, ranging from structuralism to generativism to the so-called linguistic wars and beyond.[10] To experience just some highlights,[11] read up on the nonsensical nature-versus-nurture debate,[12] apparently false dichotomies,[13] and discussion concerning the granularity mismatch problem.[14]

There is definitely room for improvement in the current state of linguistics, how languages can be studied, and why linguists should care about diversity in approaches and outlooks. But we should also try to discern who our readers are and cater to them. If we only talk to generative linguists and biolinguists, we should say so from the outset and avoid referring to linguistics as a whole, or at least not be surprised if others express disapproval. If we want to include all kinds of linguistics, we may want to start by putting the issues, notions, and terms in a wider perspective.

We should also clarify why we would want to bring this discussion out into the open. Is the discussed being conducted for the benefit of that senior linguist and colleagues of a similar mind? At this point, there are senior linguists who talk publicly about biolinguists on the one hand and philologists on the other. Or, put differently, they talk about a distinction between "*linguistics* (or biolinguistics, with focus on [the faculty of language]) and *languistics* (with focus on … 'language data')—a state of affairs reminiscent of the distinction between biolinguistics in the strong and biolinguistics in the weak sense."[15] Is a terminological clearing intended for linguistics thus conceived? Or is it in order to talk to neighboring fields, such as psychology, biology, and neuroscience? Does every linguist want to do that, to begin with? Let's just say that the target audience of Leivada's piece would be, roughly, nativist linguists who appreciate and perhaps to some extent follow Noam Chomsky's generative approach from the past seven decades.

Chomsky's usage was, from the very beginning, different from that other "philosophical grammar (or general grammar, or universal grammar)."[16] From his earliest writings, it was quite clear that, for generative linguists, "Universal Grammar (UG) is the source of our innate ability to acquire and use a natural language," as Leivada put it. UG is neither a language nor a set of grammatical properties. It is, in Chomsky's words, "the theory of the genetically based language faculty."[17] He introduced this conception of UG in the 1960s.[18] He later wrote that the "principles of universal grammar are exceptionless, because they constitute the language faculty itself, a framework for any particular human language, the basis for the acquisition of language." In the context of the principles and parameters (P&P) theory, "the principles of universal grammar have certain *parameters*, which can be fixed by experience," so that

> we may think of the language faculty as a complex and intricate network of some sort associated with a switch box consisting of an array of switches that can be in one of two positions.

Thus, "the fixed network is the system of principles of universal grammar; the switches are the parameters to be fixed by experience."[19]

This short passage mentions several of Leivada's points of contention, including that "principles of universal grammar are part of the fixed structure of the mind/brain"[20] and that "the language faculty, a physical mechanism … has certain definite properties … that the theory of universal grammar seeks to formulate and describe."[21] The technical aspects of the P&P approach may have changed, as Leivada notes, but is there really arbitrary meaning and fluidity in its present-day usage and adaptations? Not every linguist, generative or otherwise, joined Chomsky and his followers on the explicit biolinguistic journey, or on the reduction of the language faculty, which eventually led to the FLB/FLN distinction.[22] This may also hold be true of the senior linguist for whom Leivada created her list.

The Chomsky quotes above are taken from the same book. Elsewhere he writes:

> We should be concerned to abstract from successful grammars and successful theories those more general properties that account for their success, and to develop [universal grammar] as a theory of these abstract properties, which might be realized in a variety of different ways.[23]

And,

> The theory of UG must meet two obvious conditions. On the one hand, it must be compatible with the diversity of existing (indeed, possible) grammars. At the same time, UG must be sufficiently constrained and restrictive in the options it permits so as to account for the fact that each of these grammars develops in the mind on the basis of quite limited evidence.[24]

I would think that all *generative* linguists subscribe to these descriptions, even today, in one form or another.

Things might get murkier when talking about "the principles of phrase structure, binding theory, and other subsystems of universal grammar,"[25] where "one component of universal grammar is case theory [emphasis omitted], a system that stands alongside of binding theory and other subsystems of the language faculty."[26] This refers back to the classically modular conception of the language faculty in a complex P&P architecture. Once more, Leivada's senior linguist might have followed Chomsky all the way through decades of generative theorizing—but then decided to remain in a general P&P approach. If this was indeed the case, the senior linguist is far from alone.

In a larger sense, I do agree that there is a terminological jungle. But this is not restricted to the biolinguistics research agenda, which Leivada did not mention by name, or even generative linguistics in general, which she did, though perhaps she did not make it sufficiently clear. Looking at the selection Leivada provided, it is not immediately clear that avoiding misuses will really help linguistics as a whole improve its visibility in neighboring fields. Misconceptions and mistrust already exist and the so-called linguistic wars did their part without the need to worry about terminology. Before linguists can talk about improved visibility, we should be clear about our field, or rather *fields*, of study in the first place. That means getting Leivada's senior linguist onto the same page as, say, Haspelmath, macro- and micro-cognitive linguists,[27] and many others.

In the meantime, I will be eagerly awaiting Leivada's next installment of abused, confused, and misused terminological notions in scientific language research. If the next selection is as sharp as this one, it will be indispensable for many us.

Kleanthes Grohmann

Evelina Leivada *replies:*

In his letter to the editors, Kleanthes Grohmann offers a balanced discussion of what my essay did and did not do. His suggestions about the latter provide a great opportunity to expand on some important issues, so I will focus on these and address the questions he raises.

According to Grohmann, an important omission in my essay is any reference to biolinguistics as the relevant field of study for some of the terms I discussed. He is right in observing so, and there is a reason behind my decision. First, let me highlight that my essay lists half a dozen references that mention biolinguistics and discuss core aspects of the biolinguistic enterprise. If the term itself is absent from the discussion, this is because I purposely tried to maximize relatability to the main message of my essay. One point of criticism I repeatedly received when presenting this work in a way that featured the biolinguistic approach was *why does this matter for us? This biolinguistic approach does not have a focus on the main theoretical interests we generative linguists have*. It seems to me that the problem is not that "many linguists do not assume a biological point of view, or at least not the linguist's need to contribute to it," as Grohmann writes. Instead it is that many (generative) linguists who *do* work in biolinguistics—for example, discussing the lexicon, working with universal principles, evoking primitives they allocate to Universal Grammar, using brain imaging techniques to approach issues about language, etc.—explicitly distance themselves from it. I fully agree with Grohmann that biolinguistics and generative grammar are not synonymous. But I think this claim is not directly related to my essay, because I did not write an essay *for* generative linguists. The terms I discussed are not used inconsistently or incorrectly only by generative linguists or biolinguists or cognitive linguists; they are used inconsistently or incorrectly across frameworks.[28]

Grohmann also identifies a potential source of confusion that may arise from my discussion. He observes that it is not clear whether sharing terminology across disciplines (e.g., linguistics, other branches of psychology, biology, neuroscience) is more important than sharing terminology across subdisciplines of linguistics. I think this question shows how easy it is to mix two critical problems, both of which occur within and across linguistic frameworks: (i) the inconsistent use of certain terms and (ii) the fact that, in some cases, terms are ascribed definitions that defy well-known theses of other disciplines. To offer an example of the latter, if a linguist argues that Universal Grammar is genetically transmitted from parents to children in the form of a language gene, a biologist would probably reply that there is no specific gene dedicated to language, but many: some doing X in developmental stage A and Y in developmental stage B. These two problems go hand in hand. Asking which is more important is not the right way to go about them, because their solutions are intertwined: Getting rid of definitions that defy what a biologist would consider common sense—i.e., problem (ii)—also addresses problem (i) by means of reducing the alternative definitions that are associated with a polysemous term.

The third question Grohmann raises asks why we would want to bring this discussion about terminological clarity out into the open and whether doing so is to the benefit of senior linguists like the one I mention in the anecdote that closes my essay. For me, the answer does not have anything to do with specific people. It has to do with the status of the field. Discussing matters of terminology may dispel ambiguities and bring to light incorrect uses of certain terms. It may show that some definitions are more sustainable and some terms more ambiguous than others. These are important pieces of knowledge, or so psychologists think.[29] Precisely because the focus is on the field

and not specific scholars, I am not sure why Grohmann does not exclusively emphasize that terminological clarity matters for the coherence of the field itself; instead he focuses also on how I suggest "that 'terminological clarity matters' because a 'senior linguist' assumes that 'UG is simply a repository of linguistic primitives that can be disconnected from human biology.'" The anecdote at the end of my essay is merely an example of the kind of disruption that communication may face, even among linguists who work with the same notions and admire each other's work (admittedly, I can only be sure about my admiration of the other person's work). It is just one example among the many, and terminological clarity matters in ways that go far beyond this incident. I briefly talk about these ways in my reply to Juan Uriagereka's letter, but Grohmann's letter gives me a useful opportunity to mention them again. Linguistics is a small field. Diminished internal coherence translates to poor external visibility, to few or no grants in major research calls (with all the consequences this may have for graduate fellowships, at least in Europe), and to a loss of valuable opportunities for communication and collaboration with neighboring fields. In sum, terminological incoherence matters in light of the many challenges the discipline faces as a consequence.

These challenges are real in present-day linguistics, which relates to Grohmann's inquiry about whether there really is inconsistency in the present-day usage of some of the terms of I discussed. The answer is yes. I offer concrete examples in my reply to Anna Maria Di Sciullo's letter.

Overall, I agree with all the other insights that Grohmann offers in his letter, except just one thing. He recommends to discern who our readers are and cater to them. I disagree for reasons I only hinted at in my essay but will try to properly justify in the rest of this reply. I think all colleagues will agree that linguists have the responsibility to articulate robust and cohesive theories about their objects of study. In my opinion, this responsibility does not change depending on who sits in the audience, because theories should not be presented in *substantially* different ways depending on who is listening. Different sources of evidence in favor of a theory can of course be adduced, but the burden of terminological and conceptual clarity should remain unaltered regardless of the audience—regardless of whether the audience includes a biologist, who can easily pick holes in an unsustainable definition of Universal Grammar, or not. This brings me to Grohmann's next suggestion. If one wishes to embed their "issues, notions, and terms in a wider perspective," as Grohmann puts it, this is fine. If they do not, this is fine too. Concerns arise only when a theory is ridden with inconsistencies that weaken its defensibility. Adopting a wider perspective to cater to different audiences should be a matter of choice. Adopting a perspective that refrains from attaching unsustainable definitions to basic terms should be a minimum requisite in any scientific endeavor. Moreover, since we have no control over who will be influenced by our theories and when, we may shift our attention to more pressing issues, such as the need to register ambiguities that surround the use of other terms.

Grohmann is right to view a second installment of misused, polysemous, and inconsistently used terms as indispensable for the field. For this reason, I refer the interested reader to the excellent commentary by José-Luis Mendívil-Giró on language evolution, and I hope more colleagues will join forces in this initiative.

1. In Leivada's essay, the quote is credited to Eörs Szathmáry, an evolutionary biologist, conveyed in Remi van Trijp, "Use Your Own Toothbrush," *Essays in Linguistics* (blog). Incidentally, Szathmáry's field has its own terminological and explanatory issues that also touch on relevance for language.
2. Tanja Kupisch and Jason Rothman, "Terminology Matters! Why Difference Is Not Incompleteness and How Early Child Bilinguals Are Heritage Speakers," *International Journal of Bilingualism* 22, no. 5 (2018): 564–82, doi:10.1177/1367006916654355.
3. See, for example, Martin Haspelmath, *Diversity Linguistics Comment* (blog), and various papers posted and discussed there.
4. Kleanthes Grohmann and Maria Kambanaros, "Foundational Issues in Biolinguistics," in *The Routledge Handbook of Cognitive Linguistics*, eds. John Taylor and Xu Wen (London: Routledge, forthcoming).
5. *Biolinguistics* journal.
6. This refers to Noam Chomsky, *The Minimalist Program* (Cambridge, MA: MIT Press, 1995), and so much work since. For a comprehensive textbook introduction, see Norbert Hornstein, Jairo Nunes, and Kleanthes Grohmann, *Understanding Minimalism* (Cambridge: Cambridge University Press, 2005). See also the volume in preparation by Kleanthes Grohmann and Evelina Leivada (eds.), *The Cambridge Handbook of Minimalism* (Cambridge: Cambridge University Press, forthcoming).
7. Haspelmath, *Diversity Linguistics Comment* (blog).
8. See e.g., Cedric Boeckx, "Biolinguistics: Fact, Fiction, and Forecast," *Biolinguistics* 7 (2013): 316–28, responding to Ray Jackendoff, "What Is the Human Language Faculty? Two Views," *Language* 87 (2011): 586–624, doi:10.1353/lan.2011.0063.
9. For more discussion and references, see Grohmann and Kambanaros, "Foundational Issues in Biolinguistics."
10. A quarter of a century ago, this was tackled by Frederick Newmeyer, *Generative Linguistics: A Historical Perspective* (New York: Routledge, 1996). There are some more recent reflections out there as well.

11. There is more to be found in Grohmann and Kambanaros, "Foundational Issues in Biolinguistics."
12. A classic text would be *Language and Learning: The Debate between Jean Piaget and Noam Chomsky*, ed. Massimo Piattelli-Palmarini (Cambridge MA: Harvard University Press, 1980). For a recent perspective, see Stephen Crain and Paul Pietroski, "Nature, Nurture, and Universal Grammar," *Linguistics and Philosophy* 24 (2001): 139–86, doi:10.1023/a:1005694100138.
13. See Ronald Langacker, *Foundations of Cognitive Grammar*, vol. 1 (Stanford, CA: Stanford University Press, 1987).
14. For starters, see David Poeppel and David Embick, "Defining the Relation between Linguistics and Neuroscience," in *Twenty-First Century Psycholinguistics: Four Cornerstones*, ed. Anne Cutler (Mahwah, NJ: Lawrence Erlbaum Associates, 2005), 103–18.
15. The quote is from Evelina Leivada, "The Nature and Limits of Variation across Languages and Pathologies" (PhD dissertation, University of Barcelona, 2015), 94. She refers to Norbert Hornstein, "LSA Summer Camp," *Faculty of Language* (blog), July 24, 2003, on languistics/linguistics, and Cedric Boeckx and Kleanthes Grohmann, "The *Biolinguistics* Manifesto," *Biolinguistics* 1 (2007): 1–8, on the strong/weak senses of biolinguistics.
16. Noam Chomsky, *Language and Problems of Knowledge: The Managua Lectures* (Cambridge, MA: MIT Press, 1988), 2–3.
17. Noam Chomsky, "The Galilean Challenge: Architecture and Evolution of Language," *Journal of Physics: Conference Series* 880 (2017): 3, doi:10.1088/1742-6596/880/1/012015.
18. E.g., Noam Chomsky, *Language and Mind* (New York: Harcourt, Brace & World, 1968; Cambridge: Cambridge University Press, 2006).
19. Chomsky, *Language and Problems of Knowledge*, 62–63.
20. Chomsky, *Language and Problems of Knowledge*, 73.
21. Chomsky, *Language and Problems of Knowledge*, 149.
22. For recent critical reflections, see also Sławomir Wacewicz et al., "*Language* in Language Evolution Research: In Defense of a Pluralistic View," *Biolinguistics* 14 (forthcoming).
23. Noam Chomsky, *Essays on Form and Interpretation* (Amsterdam: North-Holland, 1977), 207.
24. Noam Chomsky, *Lectures on Government and Binding* (Dordrecht: Foris, 1981), 3.
25. Chomsky, *Language and Problems of Knowledge*, 91.
26. Chomsky, *Language and Problems of Knowledge*, 101.
27. Grohmann and Kambanaros, "Foundational Issues in Biolinguistics."
28. I expand on this point in my reply to the letter to the editors authored by Fahad Rashed Al-Mutairi.
29. Scott Lilienfeld et al., "Fifty Psychological and Psychiatric Terms to Avoid: A List of Inaccurate, Misleading, Misused, Ambiguous, and Logically Confused Words and Phrases," *Frontiers in Psychology* 6 (2015): 1,100, doi:10.3389/fpsyg.2015.01100.

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On Language Evolution

José-Luis Mendívil-Giró

In response to "Misused Terms in Linguistics" (Vol. 5, No. 2).

To the editors:

I share Evelina Leivada's conclusion that terminological clarity matters. Leivada has done a great job compiling in a few pages the overwhelming issue of harmful vagueness in linguistics terminology. Her selection is relevant and ambitious. In this letter, I would like to add another expression that in my opinion is frequently misused in current linguistics: language evolution.

In English, the term "language evolution" presents an "unfortunate ambiguity,"[1] to use James Hurford's term, due to the fact that, unlike what happens in French and other languages, the same word is used to designate the languages spoken by people (French *langue*) and the capacity of language as such (French *langage*). Language evolution refers, in one of the senses, to the evolution of language as a characteristic faculty of the human species and, in the other sense, to the fact that languages change over time. Typically, the context is sufficient to distinguish between both uses. In current linguistics, things get complicated because a strong tradition has resurfaced according to which both processes are closely connected. Indeed, for many authors,[2] language evolution is a consequence of language change, so the two phenomena are mixed and even identified as the same.

Sticking with the traditional use of the terms, I argue here that the term "language evolution" should be used to refer to the plausibly biological processes that formed the modern human faculty of language, and that the term "language change" should be reserved to refer to processes that alter the structure of languages over historical time, as in, for example, the shift from Latin to Spanish. With this proposal, I take sides with those authors for whom there is no connection between the fact that languages change over time and the fact that the human faculty of language is the result of natural evolution.[3] But even if I fail to convince the reader that language evolution cannot be explained as a consequence of language change, I think it is necessary to maintain this terminological distinction.[4]

Language Evolution and Language Change

Language change is a cultural phenomenon that occurs on the scale of historical time, over hundreds or thousands of years. Its main consequence is the creation of new languages through the modification of others, just as Spanish emerged from Latin some 1,500 years ago, or just as Latin emerged from Proto-Indo-European around 5,000 years ago.

In contrast, language evolution is not a cultural process, but is part of natural evolution, which occurs on a geological timescale of hundreds of thousands and millions of years. Its main consequence is the appearance of the human capacity for language sometime between 6 million and 100,000 years ago.

There is, of course, no reason not to use the word "evolution" to refer to cultural changes, and, therefore, to historical change in languages. In fact, there are some signs of evolution in language change.[5] But my argument is that interchanging both types of evolution—language change and natural evolution—is misleading and empirically inadequate. Reserving the term "evolution" for biology is an arbitrary convention, but a necessary one to avoid confusing two phenomena of very different natures. As Noam Chomsky has suggested,

> Confusion about these matters could be mitigated by replacing the metaphorical notions "evolution of language" and "language change" by their more exact counterparts: evolution of the organisms that use language, and change in the ways they do so. In these terms, emergence of the language faculty involved evolution, while historical change (which goes on constantly) does not.[6]

The main reason to combat vagueness in the use of the expression "language evolution" is that it is not possible to explain the origin of human language as a process of cultural change from the languages of the ancestors of *Homo sapiens*. Cultural evolution involving phonetic, syntactic, and semantic changes in the ways of speaking is insufficient to explain the change in the language capacity of human beings with respect to their closest living evolu-

tionary relatives, the chimpanzees, which separated from our common evolutionary trunk some 6 million years ago . It also took a very intense biological evolution in the anatomy and physiology of the brain, at least.

Our language is not only a cultural invention, but also a specifically human cognitive system, including all those cognitive developments that we may have shared with our most immediate extinct ancestors. The languages spoken by human beings are not languages that have changed, or evolved, from chimpanzee languages or from the primitive languages of other species. They are instead based on human cognitive attributes that are the result of the biological evolution of our species. Thus, the differences between language change and language evolution have to do with the different nature of the objects that change in each case: human languages and the faculty of language.

What Language Is and What Languages Are

Human language is a system of knowledge, a cognitive organ, common to all members of the species: all people learn a language, except those with severe pathologies. It is also specific to the species: no other organism, natural or artificial, can learn a language. Although there are many different languages, they all belong to the same cognitive class—human language. To put it briefly, languages are different forms, historically modified, of a single cognitive capacity, language.

As Samuel Epstein et al. remark, people are not like tape recorders, capable of recording and reproducing the sounds of the environment, but they are capable of creating and understanding expressions that they have never heard or produced before.[7] There is something in their nature that makes them capable of developing an unlimited knowledge system from a limited exposure to stimuli from the environment. In addition, the stimuli that children perceive, against intuition, do not contain phonemes, morphemes, words, or sentences, but are simply noises, disturbances of air molecules that press the eardrums, perceived in certain circumstances.[8]

In this context, a crucial question arises: How is it possible for a finite organism to develop the knowledge of entities that are not present in the stimuli of the environment, including digital infinity? The well-known answer Chomsky suggested is that we must assume that a central part of human cognition is a recursive computational system. Actually, his answer is inspired by the same problem that arises in biology to explain the development of organisms: How does a frog develop from a tadpole?

As in the biological sphere, a cognitive organ such as language develops from a genetic program in interaction with environmental stimuli of a certain type. Tadpoles do not become frogs because of the type of food they receive—there is no *frogness* in their food. Inadequate food intake can ensure that the frog does not develop well, but not that the tadpole turns into something else, like a salamander. It is the organism itself, its internal program, written by natural evolution, that determines which stimuli are relevant to development. In essence, the same thing happens with ontogenetic language development. Certain external stimuli are required for language to develop according to an internal program; these stimuli do not contain syntax or semantics, but are, in the usual case, certain types of acoustic disturbances.

Nonetheless, there is an important difference: tadpoles always produce frogs, but not all people end up speaking the same language. Note that, although frogs are very similar to each other, they are not identical. Of course, the differences between languages are very notable, as evidenced by the fact that understanding one does not guarantee understanding the others. But this does not mean that they cannot be conceived as relatively superficial variants of the same knowledge system, built by natural evolution.

To better understand the difference between what has biologically evolved—the capacity of language—and what has historically changed—languages—it is useful to differentiate between two different states of a person's faculty of language (FL).

On the one hand, there is the *initial state* of the FL. The initial state of the FL includes all the biological properties that make it possible for any human being to be able to learn and use any language. Chimpanzees or parrots lack these properties. Raised in an environment similar to that of children, they do not learn Spanish, English, or Japanese. The argument that this deficiency is not due to a lack of a faculty of language but of other general cognitive abilities is strange, if not incoherent. Whatever its origin, there is an innate capacity in human nature that is not present in other organisms. This capacity is the result of biological evolution and constitutes the initial state of the FL.

Children do learn languages, spontaneously and effectively. When children reach puberty, they already have a full knowledge of the grammar of their mother tongue(s). Let's say they have moved from the initial state to the *steady state* of the FL. Of course, some aspects of their language will continue to change throughout life: new words, new meanings, mastery of special registers, etc. But it will no longer happen that their language becomes another. Borrowing Chomsky's terminology, we can call each person's steady state of FL its *internal language* (I-language). Each person has at least one I-language in the brain, which is the result of the development of the initial state of FL. An I-language is a person's language organ.

The initial state is, by definition, common to all members of the species and, therefore, universal—invariable in space and time. It is the result of natural evolution and does not change in historical time. The I-language, however, is

not common and universal, but, varies in each individual, as each frog is different from the others. Of course, a person's I-language does not vary as much as to be very different from the I-languages of the other members of the same community, so we do not say that each person speaks a different language. Instead, we group people's I-languages by their degree of similarity. These groups of similar I-languages are what we informally call languages, such as Spanish, English, or Japanese. What we normally call "a language" is just a group of similar I-languages—or an E-language, with "E" standing for external and extensional. There are billions of I-languages, at least as many as there are people. But they can be grouped into some five or six thousand different E-languages, the number of different languages that are spoken in the world today.

Why There Are Different Languages

Although human beings are endowed with the same language capacity, not all human beings speak the same language. Why is it that all people start from the same initial state, but not everyone reaches the same steady state? The explanation has to do with the role of environmental stimuli in the development process that leads from the initial to the steady state. Unlike what happens with frogs, or with lungs, or with the visual system, the language organ, in addition to its own development program, has a cultural component that is learned from the environment. This component is susceptible to historical changes in its transmission from generation to generation.

Chomsky's claim that an alien scientist would conclude that all humans speak the same language is as famous as it is controversial. But, as human researchers, we observe that each natural species, whether blue whales, chimpanzees, or bees, has a single communication system, and not several. Each species is equipped with its own communication system that is biologically determined. This implies that individuals do not have to learn it, but it is part of their nature, as are fins, hair, or the instinct to search for pollen. What Chomsky wants to imply, therefore, is that human beings, despite appearances, do not have to be different.

And we are not. There are about six thousand languages in the world—that is, about six thousand different ways of speaking that do not allow mutual understanding. To explain this fact, it is necessary to consider in a little more detail the language of other animals, such as whales or certain songbirds. Ethologists have observed that there are subtle differences in the songs of different groups of whales of the same species and in the songs of different groups of birds of the same species. Some birds do not precisely imitate the singing of adults, but instead produce certain innovations that make the songs they use no longer identical to what they heard when they were nestlings. This causes successive generations of birds to hear slightly different songs than the previous generation, resulting in birds of the same species from different regions singing different songs.

This is not all that different from what happens with human languages: in each of those six thousand different languages there are different dialects. The difference between dialects and languages is not a class difference, but a reflection of the similarity between two different ways of speaking. Two different ways of speaking English are considered two dialects of the same language because these forms are similar enough—their users usually understand each other. The much fewer similarities between a French speaker and an English speaker means that their ways of speaking cannot be considered dialects of the same language. Instead, they are considered different languages, because these forms are much less similar, and their users do not understand each other. Whales or songbirds supposedly only have dialects because the margin of variation in their systems is small, while users of human language not only have dialects, they also have languages because the margin of variation in human language allowed by biology is greater than in non-human language.

The question to answer, then, is why human language allows more variation than the rest. Perhaps the answer has to do with what kind of animals we humans are. We have a language specific to our species, common to all people and different from those of other species. But, on the other hand, we are special animals, in the sense that we are much more capable to learn from the environment and develop and transmit culture. Nature has endowed us with an incredible ability to learn, including the ability to enrich our FL by developing it in interaction with other minds, which has the consequence that we can enjoy languages of a complexity that could not be genetically encoded. Any human language has many more words than there are genes in the human genome, which is about 20,000.

The consequence of having to learn some aspects of language is that it is not only normal that different dialects arise, but also that they differ so much from each other that they end up giving rise to what we call different languages. There is a correlation between the degree of variation of a system and the degree of learning involved in its development.

What Changes When Languages Change

Despite what it may seem when we compare Latin and Spanish, linguistic changes do not have the capacity to alter the common internal structure of languages. Changes can only alter their *surface*. This is precisely why we cannot accept that the evolution of language can be explained as

a succession of linguistic changes that would convert supposed primitive languages or prehuman languages into the human languages that we speak now.

But what is the surface of a language? To answer this question, we must consider which components make up an I-language. According to the influential model proposed by Chomsky, Marc Hauser, and W. Tecumseh Fitch, the human FL includes, minimally, three main components: a conceptual system related to semantic interpretation, a sensorimotor system related to the production and perception of sounds and visual signs, and a computational system, which is the syntax in the narrow sense responsible for the compositional and productive structure that underlies linguistic expressions.[9]

The relevant question now is in which of these three components—semantics, phonetics, or syntax—do the changes that result in the diversity of languages occur. The answer, although it seems surprising, is *none of them*. The three components are part of the FL that characterizes us as a species; all three are the product of natural evolution and, therefore, do not vary in historical time. None of them has changed substantially in the last 1,500 years, although in that time we have gone from Latin to Spanish, two very different languages.

To understand this answer, we need to dig a little deeper into how these three essential ingredients relate to each other. Some authors have proposed that the relationship between the computational system and the conceptual and sensorimotor systems is asymmetric, in the sense that the computational system, or syntax, would have evolved adapting to the conceptual system, forming an internal language of thought (IloT) aimed at the creation of thought.[10]

That IloT, essentially common to the species, and probably the hallmark of human cognition, would later have been connected to the sensorimotor system for externalization and, therefore, for communication, probably reusing an old communication system. According to this vision, externalization would be secondary, that is, a process exposed to fluctuation in the environment and therefore susceptible to change and diversification.

This scenario implies that any I-language must also include a component derived from the environment—that is, internalized—whose mission would be to systematically connect the structures generated by the IloT with the sensorimotor system. The crucial idea now is that this component is the only one that results from learning and therefore is also the only one that is exposed to change and variation. Let us call this component the *internalized-lexicon*, or I-lexicon.

Note that it is then implied that the essential function of language—understood as semantics + syntax—would be thought, not communication. Although very important for our species, communication is a secondary use of language.[11]

The connection of the ILoT with the sensorimotor system then allows the externalization of thoughts as physical signals, typically sound waves, that can go from one individual to another. But for this to be possible, it is necessary to establish a shared link between, on the one hand, abstract syntactic-semantic representations and, on the other, the system capable of producing signals through muscle movements. That is the function of the I-lexicon, a domain of long-term memory that provides a stable and I-language-specific connection between the internal syntactic-semantic structures and the sensorimotor systems that process and produce the signals that human beings perceive and produce when they use language for communication.

The I-lexicon can be thought of as a repertoire of exponents, or morphemes, that systematically match abstract syntactic structures and sounds, that is, that translate the former into the latter.[12] As these exponents can be different in each language, we can say that the I-lexicon—the code to link thoughts and sounds—is responsible for the diversity of languages. The reason that the exponents we use to externalize language may be different in different speech communities is that those exponents are cultural entities passed down from generation to generation.

According to this model, learning the language of the environment actually involves the task of learning to externalize the ILoT in the same way that other members of the linguistic community do. In this process, mismatches between form and structure, which are traditionally known in historical linguistics as reanalyses, can occur. A reanalysis is an alteration of the relationship between an underlying structure and a linguistic expression. These relations are the only thing that linguistic changes can alter.

If this is so, the claim that language change would be responsible for language evolution is meaningless, since language change cannot in any way alter the components of the FL that have not been learned.

How and Why Language Change Takes Place

Although I have argued that language change and language evolution are different processes, it is still true, as Darwin himself suggested, that the mechanisms of linguistic and biological change are similar. Thus, I suggest that the reanalysis mechanism in language change is the equivalent to the genetic mutation in natural evolution.

Consider a simplified example, such as the passage from the analytical late Latin future—*amare habeo*, lit. "(I) to love have," "I will love"—to the synthetic Romance future—e.g., Spanish *amaré*, "I will love." The essential idea is that for speaker S_1 expression E */amaré/* has a specific structure—roughly *amar* + *é* = V + Aux.Tense—while for speaker S_2 *the same* expression E has a different structure—*amaré* = V.Tense. Speaker S_2 reanalyses expression

E and assigns it a different underlying structure than speaker S_1. The I-language, or the I-lexicon, of speaker S_2 then has a mutation, in the sense that the relation between the expression E and the elements of its underlying structure is different from that of the I-language of speaker S_1.

The task of listeners is to use their I-language, including their own I-lexicon, to discover meaning by analysing the sound wave. In the ideal case, the syntactic-semantic structure that a listener obtains is identical to the one that the speaker had in mind. When this is not the case, we can say that a reanalysis has occurred. A reanalysis is basically a decoding or acquisition error. When that error or mutation stabilizes in the listener's I-language and spreads to other speakers, we say that a linguistic change has occurred.

Conclusion

There is no evidence that the oldest known or reconstructed languages were more primitive, less efficient, or less sophisticated than current languages. Languages are not cultural inventions, but a complex mix of biology and culture. The parts of languages subject to cultural transmission and change are concentrated in the I-lexicon. There is no reason to think that this type of cultural change has been the causal factor in the emergence of the human faculty of language. For this reason, the expressions *language evolution* and *language change* should be kept separate.

1. James Hurford, "An Approach to the Phylogeny of the Language Faculty," in *The Evolution of Human Languages*, ed. John Hawkins and Murray Gell-Mann (Reading, MA: Addison-Wesley, 1992): 273.
2. For example: John Batali, "Computational Simulations of the Emergence of Grammar," in *Approaches to the Evolution of Language: Social and Cognitive Bases*, ed. James Hurford, Michael Studdert-Kennedy, and Chris Knight (Cambridge: Cambridge University Press, 1998), 405–26; Simon Kirby, "Fitness and the Selective Adaptation of Language," in *Approaches to the Evolution of Language*, 359–83; Simon Kirby, "Syntax without Natural Selection: How Compositionality Emerges from Vocabulary in a Population of Learners," in *The Evolutionary Emergence of Language: Social Function and the Origins of Linguistic Form*, ed. Chris Knight, Michael Studdert-Kennedy, and James Hurford (Cambridge: Cambridge University Press, 2000), 303–23, doi:10.1017/CBO9780511606441.019; Simon Kirby, "Learning, Bottlenecks and the Evolution of Recursive Syntax," in *Linguistic Evolution through Language Acquisition: Formal and Computational Models*, ed. Ted Briscoe (Cambridge: Cambridge University Press, 2002), 173–203, doi:10.1017/CBO9780511486524.006; Luc Steels, "Synthesizing the Origins of Language and Meaning Using Coevolution, Self-Organization and Level Formation," in *Approaches to the Evolution of Language*, 384–404; Bernd Heine and Tania Kuteva, *The Genesis of Grammar* (Oxford: Oxford University Press, 2007); Thomas Givon, *The Genesis of Syntactic Complexity* (Amsterdam and Philadelphia: John Benjamins, 2009), doi:10.1075/z.146; Daniel Dor and Eva Jablonka, "Plasticity and Canalization in the Evolution of Linguistic Communication: An Evolutionary Developmental Approach," in *The Evolution of Human Language: Biolinguistic Perspectives*, ed. Richard Larson, Viviane Déprez, and Hiroko Yamakido (Cambridge: Cambridge University Press, 2010), 135–47, doi:10.1017/CBO9780511817755.010.
3. For example: Derek Bickerton, *Language and Species* (Chicago: Chicago University Press, 1990); Steven Pinker and Paul Bloom, "Natural Language and Natural Selection," *The Behavioral and Brain Sciences* 13 (1990): 707–84, doi:10.1017/S0140525X00081061; Noam Chomsky, "Some Simple Evo Devo Theses: How True Might They Be for Language?" in *The Evolution of Human Language*, 45–62, doi:10.1017/CBO9780511817755.003; April McMahon and Robert McMahon, *Evolutionary Linguistics* (Cambridge: Cambridge University Press, 2013); Robert Berwick and Noam Chomsky, *Why Only Us: Language and Evolution* (Cambridge: MIT Press, 2016).
4. I have made this argument in more detail in José-Luis Mendívil-Giró, "Did Language Evolve through Language Change? On Language Change, Language Evolution and Grammaticalization Theory," *Glossa: A Journal of General Linguistics* 4, no. 1 (2019): 124, doi:10.5334/gjgl.895.
5. Östen Dahl, The Growth and Maintenance of Linguistic Complexity (Amsterdam and Philadelphia: John Benjamins, 2004), doi:10.1075/slcs.71.
6. Chomsky, "Some Simple Evo Devo Theses."
7. Samuel Epstein, Hisatsugu Kitahara, and T. Daniel Seely, "Derivation(s)," in *Explorations in Maximizing Syntactic Minimization*, ed. Epstein, Kitahara, and Seely (New York: Routledge, 2015), 1.
8. Sign languages are based on visual and non-auditory stimuli, but the underlying logic is the same, as it is the same language faculty that deaf people use to learn and use these languages.
9. Marc Hauser, Noam Chomsky, and W. Tecumseh Fitch, "The Faculty of Language: What Is It, Who Has It, and How Did It Evolve?" *Science* 298, no. 5,598 (2002): 1,569–79, doi:10.1126/science.298.5598.1569.
10. Berwick and Chomsky, *Why Only Us*.
11. Another serious terminological problem, worthy of being on Leivada's list, is that Chomsky uses the term "language" for the syntax + semantics system, while other authors use it only for the externalization component. See Mendívil-Giró, "Did Language Evolve through Language Change?" for discussion.

12. According to the nanosyntax model, syntactic heads are typically submorphemic, so each lexical entry of the I-lexicon matches a fragment of a syntactic tree with a phonological representation. See Michal Starke, "Nanosyntax: A Short Primer to a New Approach to Language," *Nordlyd* 36, no. 1 (2010): 1–6, doi:10.7557/12.213.

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