

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 "linguisticity," 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 linguisticity. 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 linguisticity. 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 “hard-wired” 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 us⁹ 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 theoretic-

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*.

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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 Linguisticity 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.

Published on September 28, 2020

<https://inference-review.com/letter/a-collective-action-problem>