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Reply to the commentaries

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The dialogue opened up by the commentaries touches on a wide range of issues. While I am very grateful for the dialogue, I cannot do justice to all the issues here. In this brief response, I first point to some areas of agreement between me and some of the commentators, and then continue the conversation on four more open-ended questions.

1   Areas of agreement

I agree completely with Embick and Haspelmath that lexical categories are not atomic, but rather complexes of syntactic features. Within generative grammar, this idea can be traced back to Chomsky's (1970: 208) statement that one can "regard all symbols of the grammar as sets of features". Embick rightly observes that if one adopts this view, then the claim that lexical categories are universal amounts to the claim that all languages must select the features constituting them from the universal feature inventory. As he points out, it is an important question what the ultimately correct feature decomposition is for N, V, and A (or, in DM, for the category-defining heads n, v, and a). Chomsky (1981) decomposed these categories, plus prepositions, into the features [N] and [V], as follows: nouns are [+N,−V], verbs are [−N,+V], adjectives are [+N,+V], and prepositions are [−N,−V]. This is quite a rudimentary feature system: the names for the features can be viewed as placeholders for more revealing content to be filled in later, and it is not clear how it would scale up to the (ever-growing set of) functional categories. But for the purposes of describing the Chamorro distributional patterns that I discuss in Section 4, this rudimentary system works well. The hard question, addressed mostly recently by Baker (2003), is what the substantive content of these features might be. I concur with Koontz-Garboden that part of the answer may lie in the generalizations that govern the mapping between lexical categories and their model-theoretic denotations. Another part of the answer may lie in conceptual knowledge – broader principles of human cognition, whatever those might be.

Second, like Embick, I believe that there is a limit to what morphosyntactic distribution can reveal about the structure of a language's lexical category system. (Haspelmath's commentary can be read as making a similar point; Croft and van Lier take the opposite view.) As Embick puts it, "the decision of where to stop identifying new categories on distributional and morphosyntactic grounds is relatively arbitrary". This I take to be related to a point made at the beginning of his commentary; namely, the theory behind the investigation frames the questions asked.

To illustrate: In generative grammar, the lexical categories constitute just one type of syntactic category. There are also functional categories, such as C(omplementizer), D(eterminer), T(ense), Top(ic), Voice, and the like, whose universal status or not is currently rather unclear. Since Grimshaw (1991), it has been assumed that every lexical category is characteristically associated with one or more functional categories that it typically combines with. V is characteristically associated with Voice, N is characteristically associated with D and Num(ber), and so on. In a generative grammar that adopts this theory of extended projection, there is no guarantee that a given pattern of morphosyntactic distribution will tell us anything about how *lexical* categories are organized. Such a pattern might instead reflect how the associated functional categories are organized (or it might reflect some relational aspect of syntactic structure, such as selection). In this very particular sense, there is a limit to what generative grammarians can expect morphosyntactic distribution to reveal about a language's lexical category system.

Having identified these areas of agreement, let me now turn to some more controversial questions.

2 Methodological opportunism?

In their joint commentary, Croft and van Lier say, "Chung – and Topping – choose the constructions that lead to the conclusions they are interested in. This is METHODOLOGICAL OPPORTUNISM . . . : choose the constructions that make the theoretical point that you want to make. This is the real problem."

But is this a problem? A more constructive – and accurate – way of framing Croft and van Lier's observation is to say that theories make predictions that can be tested empirically. The empirical testing naturally focuses on facts that bear, either positively or negatively, on the prediction.

To take a concrete example, consider van Lier's singly-authored commentary, which reacts to my demonstration (in Section 5) that some apparent multifunctionality in Chamorro actually involves conversion or zero-derivation. (The spe-

cific types of conversion in Chamorro include denominal verb formation and denominal adjective formation.) I had characterized the phenomenon of multifunctionality as the ability of content words to serve as nouns, verbs, or adjectives depending on the context. Van Lier's commentary begins by defining a different notion, which she calls *real multifunctionality*, and which "involves the productive use of a certain semantic group of lexical items in multiple syntactic functions with fully compositional interpretations". She then argues that Chamorro and the Oceanic language Teop exhibit "real multifunctionality" as well as conversion. (The Chamorro evidence for "real multifunctionality" is that nouns can serve as predicates of clauses.) Her investigation of "real multifunctionality" in these languages focuses on "the use of object words in predicative function and the use of action words in referential function". Now, predicative and referential function are two of the three propositional act functions which, according to Croft and van Lier, explain "why there are three major parts of speech" (their p. 61). In other words, the subtypes of "real multifunctionality" investigated are consistent with the expectations of Croft's theory. One *could* call such an investigation methodologically opportunistic, since it chooses "the constructions that lead to the conclusions [van Lier is] interested in". I prefer to see it as another empirical investigation that attempts to test a prediction. Radical Construction Grammar predicts that "real multifunctionality" should include cases of "object words" used predicatively and "action words" used referentially; van Lier argues that this prediction holds. Whether or not one finds the investigation convincing, it is not *methodologically* superior to Topping's or to my own.

3 How many lexical categories?

Haspelmath raises the very interesting question of whether the Chamorro evidence points to a universal lexical category system with *four*, rather than three, categories. These categories he calls transitival (= transitive verb), intransitiverb (= intransitive verb), adjectival, and nominal. He observes that such a four-category system would straightforwardly account for Topping's evidence for a Chamorro-specific two-category system as well as the distributional evidence I discuss, and that it is consistent with evidence from English and other languages. The underlying question is how one could ever tell whether this proposed category system or the tripartite category system is superior.

Let me take a moment to show that Haspelmath's question can be answered in minimalist syntax, and just how it is answered. Some Chamorro facts to begin with: Chamorro does indeed distinguish transitive from intransitive verbs, where by *transitive verbs* I mean the Chamorro verbs that take a DP complement in the

unmarked morphological case. Topping originally pointed out various properties that differentiate transitive verbs from other lexical categories in Chamorro: transitive verbs passivize, they cannot have weak pronoun subjects, and so on. Significantly, Chamorro also draws a distinction between transitive and intransitive *preposition*s. The language has a small number of prepositions, which differ from morphological case inflection in that they are insensitive to the internal structure of the DP with which they combine. (Morphological case reflects whether the DP is a common noun, pronoun, name, or place name; prepositions do not.). Most Chamorro prepositions are transitive: they take a DP complement in the unmarked morphological case. (The unmarked case is realized as *si* before names, but is unrealized before common nouns and pronouns.)

(1) a. **ki**   si   Ana
       than   UNM   Ana
       'than Ana' (CD, entry for *menus*)

    b. **ginin**   hågu   mismu
       from   you   self
       'from you yourself' (EM 132)

But a few prepositions are intransitive: they take a PP complement, or no complement at all:

(2) a. **fuera**   ki   i   lepblu
       except   than   the   book
       'except the book' (CD, entry for *fuera*)

    b. **åntis**   (di   Betnis)
       before   PREP   Friday
       'before (Friday)'

In this respect, verbs and prepositions pattern alike. The similarity argues that verbs and prepositions (the [−N] categories in Chomsky's [1981] system) should be cross-classified for transitivity – say, by Haspelmath's feature [TR].

     Back now to the question: is [TR] one of the features that define lexical categories universally? In minimalist syntax, the answer is probably no, for the following reason. Minimalist syntax posits a Case-licensing feature that enables verbs to take a DP complement. However, this feature is not one of the features that constitute the lexical category V (or, in DM, the category-defining head v), but instead is one of the features that constitute Voice, a functional category typically found above V in the syntactic structure. The theory likewise posits a Case-licensing feature on P that enables prepositions to take a DP complement. It is

reasonable to assume that the Case-licensing feature on both Voice and P is [TR]. But then [TR] is not a defining feature of *lexical* categories, but a feature of functional categories, some of which (including P) are often assumed not to be universal.

4 The idiosyncracy of conversion

The Chamorro processes of denominal verb formation and denominal adjective formation are not completely unrestricted; instead, there are arbitrary lexical gaps. I argued that these processes do not apply directly to roots, but instead to nouns – in DM, to syntactic structures consisting of a category-defining head n that has already combined with a root. Both Embick and Koontz-Garboden observe that if this is so, an issue arises for DM, a theory in which merger of a category-defining head with a structure headed by another category-defining head should occur completely freely if it is possible at all. The reason is that locality constraints prevent the higher category-defining head from looking beyond the lower category-defining head to see the root. Embick and Koontz-Garboden each suggest promising directions for exploring this issue further. It should be noted that what is at stake here is the status of certain theoretical assumptions of DM, not the accuracy of the claim that Chamorro distinguishes e.g. denominal verbs from verbs that merely share a common root with a noun. As Koontz-Garboden observes (in his note 5), the Chamorro claim could be recast in a theory, such as Kiparsky's (1982, 1997), which does not take a syntactic approach to complex word formation. If this were done, the empirical support for the claim that Chamorro has nouns, verbs, and adjectives would remain just as strong.

5 What understudied languages can contribute

In the conclusion to his commentary, Haspelmath states that "the assumption of universal categories carries the very real danger of ethnocentrism". He also asserts, "on the Chomskyan, aprioristic approach, small languages studied by few linguists cannot have a real impact on general questions of linguistics . . . because aprioristic category hypotheses tend to be set up on the basis of the major languages." I take these remarks, and the surrounding discussion, to raise the following objection to the generative enterprise. First, generative grammar is a theory with deductive structure ("aprioristic"). Second, the universals proposed in generative grammar are often supported by evidence from what Haspelmath

calls "the major languages" or "European languages". Because of this, under-studied languages cannot have a real impact on theoretical questions.

I would argue that the conclusion does not follow. Theories with deductive structure lead to hypotheses that can be evaluated on the basis of empirical evidence, including evidence from understudied languages. Further, it does not seem surprising or unreasonable that proposed universals are often supported by evidence from "European languages". What would be unreasonable would be to hold that no proposed universal that was supported by such evidence could be correct. It would be even more unreasonable to claim that evidence from "small languages" or "non-European languages" could never contribute to the under-standing of a proposed universal that found support in "European languages".

Still, the underlying issue that I take Haspelmath to be addressing, which involves the socio-political dimensions of language, is a real one. I would put it this way. Due to historical and socio-political circumstances, we know far more about a small circle of languages associated with socio-economic power (including certain Indo-European languages, Chinese, and Japanese) than we know about the full diversity of the world's languages. This situation has had a negative impact on many endeavors in linguistic science, including the search for linguistic universals, the understanding of linguistic typology and other aspects of language diversity, the understanding of language production and comprehension, and so on.

How can the situation be remedied? In my view, the goal should be to pursue the in-depth investigation of languages that are currently understudied (such as Chamorro), to the point where our knowledge of them is as intricate and wide-ranging as our knowledge of languages associated with socio-economic power (such as English). The goal should *not* be to deny that languages such as English can contribute to the search for linguistic universals, or to limit the theoretical con-tribution of understudied languages to issues whose resolution does not require the in-depth study of any language. Such approaches run the very real danger of romanticizing the exotic – of *over-exoticization*, to use Matthewson's (2011) term.

As the commentators point out in their different ways and as I have observed as well, the hypothesis that all languages have nouns, verbs, and adjectives is not obviously correct. Nor has it been deeply integrated into any linguistic theory. What I hope to have shown, nonetheless, is that this hypothesis is supported by evidence from Chamorro *and* English. Both languages provide intricate, detailed evidence for a tripartite lexical category system in which the packaging of seman-tic material into syntactic categories is broadly similar in architecture (though by no means the same in detail). The more general point is this. All languages have the potential to contribute equally and significantly to all aspects of linguistic theory. But they can do so only if they are studied rigorously and in enough depth for that potential to be realized.

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