Not everything is a theory. A reply to Martin Haspelmath

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In a recent article entitled "General linguistics must be based on universals (or nonconventional aspects of language)" Martin Haspelmath (MH henceforth) raises a number of methodological issues that have been at the center of his research agenda and of linguistic debate for several years now. These issues are all legitimate, and MH deserves praise for his persistence in tackling them.

Here, I will focus on just a few, on which I will make some general remarks. The first one, the core of the paper, is the idea that general claims require general data. You can't infer anything about the next language based on what you know about one language. The second one regards the need to establish a shared terminology for our work in linguistics, and keep calling what we do with a term that everybody understands in the same way. This terminological unification has been on MH's agenda for years now (Haspelmath 2006, 2010, 2011, 2012, 2018, 2020, 2021), so it does not come as a surprise that he would insist on this particular issue in a general methodological article. While making his point, MH highlights some of the methodological shortcomings of other approaches, in particular the generative one, his biggest target (nothing new on this front either).

I must say, I always enjoy reading MH's papers, first because they are extremely clear and well argued, and then because every single paper he writes gives me food for thought and provokes some reaction in me – because of the style, because of the topic, because of the way he presents things. The reaction I had while reading this paper was admiration mixed with annoyance. Admiration because I find it very sympathetic that one of the most prominent linguists in the world would still struggle to have everyone agree on using one term and defining what can be called theoretical and what not. People at his level usually just pick one word and the others follow. Haspelmath is not like that: he wants to explain to the reader why he does what he does and why everyone should too, and this deserves praise. In general, defining our ontology should be a primary task for every linguist.

The annoyance derives from the fact that some of the things he writes about the methodology that I and the generativists more generally use (at least in his eyes) are not quite accurate; this makes me wonder: is he simplifying the line of argumentation for the sake of rhetoric, does he explicitly decide not to reproduce the arguments accurately, or are we not making ourselves understood? Whichever the answer is, I can't avoid the feeling of here we go again. As I said, it is impossible to read Haspelmath neutrally.

Let's start with the issues: the first one is that "general linguistics must be based on the empirical study of language universals". No linguist would object to this, at least as a starting point of any linguistic enterprise; MH goes on to argue that in order to establish whether something is universal it must be checked in all languages, with a stress on all; in other words, no inference is allowed from what we know about what we don't know, because many language-particular aspects are idiosyncratic, they are "historically accidental to a large extent and we cannot draw conclusions about Human Language from features that are peculiar to English or to Quechua, such as that the word for 'house' is *wasi* in Quechua and *house* in English".

While the first claim is rather uncontroversial, the second one is controversial to the extreme. First, because it is not true that we cannot draw conclusions about Human Language from *wasi* and *house*: we can. We can draw conclusions about the structure of syllables or at least phonological units that

are possible in HL, we can draw conclusions about the possible sounds used by humans. In general, what we can conclude by examining these words is that what we observe in languages is a possible instantiation of HL. If *wasi* is used as a word in a language, it is a possible word for humans. If *wh-*movement (to which we will return later) happens in some language, that's because it can. What we are asking is not "what happens", which is (at some level at least) rather trivial, but "what cannot happen". This is one of the core points of the generative enterprise and of contemporary linguistic research, a point that MH disregards completely, even if he briefly discusses predictions in §5.2. Whether something is found in all languages of the world, like topic/comment (see Krifka 2008, mentioned in §1), or it is a tendency makes absolutely no difference. In this respect, it totally escapes to me why the general take on linguistics nowadays, especially in some circles, is that you need many tokens of something for it to be worthy of consideration. Imagine that a phenomenon is produced by one speaker of one language (say, Spanish). The speaker has strong intuitions about the phenomenon (meaning, here, that s/he is very coherent when producing it), but it is only one speaker. Can we still call them a Spanish speaker? Of course, an explanation is required as to why the speaker is the only person producing that phenomenon, but this does not mean that this phenomenon should be disregarded altogether. One would need to exclude slips of the tongue for instance (though they are also interesting, but perhaps not as examples of what can or cannot be found in language), but if one collects a small sample of sentences showing that one phenomenon exists, based on expectations from other languages, why would this sample have to contain all possible sentences in all possible languages of the world to be worthwhile considering? The point is: we are not asking what is there, but what can be. A theory makes predictions not only as to what will happen, but also, and especially, as to what will not. That is a key point in my kind of research which makes it radically different from many other token-based, corpus-based approaches.

In order to check whether something can be found in a language, one needs to have a theory of what is possible. Also in this respect, MH offers a (rather funny, I must admit) description of the generative methodology that reads "It is quite common for research articles to consist of two parts: One part lays out the phenomena in a way that is generally comprehensible to any linguist, and another part (typically called "analysis") describes the phenomena a second time, using the highly technical metalanguage of current mainstream generative grammar (or more rarely, of some other generative approach, such as Distributed Morphology or Lexical Functional Grammar)" (§ 5.2). Though a nice caricature of what I do in my daily job, this description lacks a fundamental bit: a theory makes predictions, it doesn't limit itself to descriptions. Take for example my recent papers on Italo-Romance (D'Alessandro 2016, 2017, 2020, D'Alessandro & Ledgeway 2010, D'Alessandro & Van Oostendorp in press). What I argue there, based on a number of empirical observations, is that there is an extra bundle of features in Italo-Romance, richer agreement if you will, which you find over and over again: as the root of person-oriented auxiliaries, as a marker in person-driven DOM, as subject clitics, etc. The observation is that agreement is richer in Italo-Romance varieties than in other Romance varieties including Italian. The generalization is that all Italo-Romance varieties have something in common, namely some extra features. The prediction is that you will find many of these "extra agreement" phenomena in Italo-Romance, and that you will not find them in other Romance languages that do not possess this extra bundle, like Italian or French. The prediction is also that if these features are found in the verbal domain, they will cause all sorts of agreement disruptions, which you will NOT find in other Romance languages that do not have these elements. Crucially, you will be also able to find these agreement mismatches in non-Italo-Romance languages that, like Italo-Romance, present these extra bundles, i.e. these feature-oriented phenomena. This prediction is borne out: we find agreement mismatches in Georgian and

Basque, for instance (Béjar & Rezac 2003, 2009, Nevins 2011, D'Alessandro 2012, Arregi & Nevins 2012).

If we limited ourselves to observing subject clitics as a superficial category, and if we did not look into their structure and the features they encode, and we did not make any generalization about what they represent, and we did not proceed by reductionism, we would not be able to make predictions about what can and cannot be found across languages. Of course, MH can say that this all happens by coincidence, but it seems to me that when you have a way to predict this coincidence and of falsifying it you can no longer call it a coincidence, I would say.

In his attempt to classify the different methodologies and to establish common terminology, MH declares that every approach to the study of languages is theoretical, because "theory-free linguistics is not possible". I beg to disagree on this point: many descriptions are indeed based on a number of abstractions, and use technical metalanguage (though, if we take MH seriously, we should not be using these terms but every language should have its own terminology, at least at the beginning of its description, because what is true for one language cannot be assumed to be true for any other). Abstractions and descriptions in structural terms are, however, not a formal theory yet. A formal theory is built through the convergence of a number of proven hypotheses. These hypotheses apply at different levels of the analysis: we observe an empirical phenomenon and we describe it in abstract- structural terms. We hypothesize that this phenomenon is related to another phenomenon. In other words, we reduce 2 phenomena to a more general phenomenon, and then we proceed by reducing these issues as much as we can, until we hit the general "law". In this sense, we can make predictions all along: we know that if one phenomenon exists, the corresponding one will also exist, we abstract away from the single element and try to find the general law governing these phenomena. This general law is not yet HL, intended as the faculty, which is only human, to master complex system such as languages, nor is it to be attributed to UG right away. The general law could be due to cognitive requirements, it could have functional explanations. Only if we do not find any such functional explanation or general cognitive explanation can we attribute this "law" to HL.

In general, the way HL is conceived is as a set of operations (many reducible to Merge, but also including Agree) and interface conditions, which are what "filters" grammatical sentences that are fine in a language from those that aren't. Also, there is a set of basic "blocks", i.e. features, on which these operations apply. Again: we might not have a complete list of features, but we do know that languages do not operate on "color" features, for instance (there is person agreement, but not color agreement). These generalizations are always based on data and checked on data, but they also bring about implications and predictions. This is what a theory is: not just using technical metalanguage, but the search of the general laws governing language based on converging proven hypotheses.

Have we succeeded in finding the general theory of HL? Not yet. Fields that are much older than ours have not succeeded yet in finding all the laws governing the universe either. Have we found something? Yes, we have. One example: the *ABA constraint on adjectival suppletion discovered by Bobalijk (see Bobalijk 2012). That is a general law, that every language obeys, as far as we know, a law that has been uncovered by looking at many languages but also through linguistic theorizing.

In this sense, the quote taken from my article saying that we can use constraints found in a language to describe what we find in other languages means exactly that: that you can check for correlations, you can check whether something that is found in one language has a correlate in another language. It certainly does not mean that what is found in one language will be found in exactly the same form

in another. MH rephrases a statement I made by saying that 'movement in English is thought to be informative for Chinese, because a notion such as "question pronoun", as well as a notion such as "movement", is thought to be innately given' (§4.3). I'm afraid this is not quite what I wrote. What I wrote is "This, I think, is the key difference between generative grammar and other linguistic enterprises, such as typology: while typologists assume that, say, the existence of *wh*-movement in English cannot tell us anything about Chinese, generativists assume that this isn't true" (D'Alessandro 2019:10). This has nothing to do with the innateness of *wh*-movement, which is an absurd idea to start with. First, *wh*-movement is a shorthand for the observation that some elements receive a "double interpretation" which pertains different domains: for instance a *wh*- is both a question and an object in a sentence like "Which book did Mary read?". The *wh*- phrase appears 'displaced' with respect to the position in which objects usually appear in English, namely after the verb. This is described through the metaphor of movement, but nobody in their right mind would think that *wh*-elements actually physically start walking around in a sentence. Likewise, nobody (for sure not me) ever claimed that *wh*-movement is innate. What I claimed is that *wh*-movement (intended as just explained) exists in languages, and therefore it is a possible grammatical phenomenon in language. Therefore, since it is possible, it can be found in languages other than English because we are all human, and we all make use of the same restricted set of grammatical tools. Variation is not unlimited. This means that we can use what we see in English to search for the same phenomenon (and the correlations it has, see the reductionism above) in other languages. In fact, thinking about movement in this sense turns out to be very valuable when we comparing English (which evidently has it) and Chinese (which seems not to) because closer consideration of the predictions made for overtly *wh*-moving languages with what we see in Chinese reveals that this language also, surprisingly, exhibits a subset of the English movement-restrictions, like the fact that adverbial *wh*-phrases in Chinese may not take scope across islands (see Huang 1982, Xu 1990, Lin 1992, Aoun and Li 1993, Cheng 1997, Cole and Hermon 1998 and many others).

Let us now do the reverse: let us take MH's suggestion seriously. We will not look for *wh*-movement in Chinese because English has it; we will not find it. We will also not look for it in Italian. In fact, we will not look for person pronouns in Italian either because the fact that English has them does not imply that Italian has them too. This is a caricature, you will say. Well, imagine now you have to describe an undocumented language the family of which is unknown. What do you do? Do you start looking for pronouns or not? Do you only consider absolute universals? When is a universal an absolute universal, by the way? When are we allowed to say ok, this exists in 2000 languages, it should also exist in the remaining 5000+ ones? (I am aware of the discussion about it, the point being that there is disagreement there too). In fact, if you can't infer for one part you can't infer for any part of the grammar. This means that one should start from scratch for every new language description. This also means that all our knowledge is going to be completely useless when describing a new language, because we are not allowed to postulate the existence of a category just because it exists in another language, or in 200 others. Is this economical? I don't think so. Isn't it easier to start from postulating that Italian has no number and no *wh*-movement based on Chinese, and then look for evidence to the contrary, then check that also all phenomena that are predicted to be related to number and *wh*-movement are present, and conclude that there is *wh*-movement in Italian? This way the description is as thorough, but we don't need to go back to Aristotle (as H once suggested to me) to start describing a new language. And in fact, one might even spare some time to

concentrate on understanding what HL is about rather than wasting time on tiny details that sometimes take the attention away from the big picture.

Debates are always good. What we should not do is dismiss entire branches of the field based on prejudices. In this sense, MH really deserves praise for engaging over and over again with generative literature, taking it very seriously, and arguing with it. I hope many follow his example.

References

Aoun, Joseph & Yen-hui Audrey Li. 1993. Wh-elements in situ: Syntax or LF? *Linguistic Inquiry* 24:199-238

Arregi, Karlos & Andrew Nevins. 2012. *Morphotactics: Basque Auxiliaries and the Structure of Spellout*. Dordrecht: Springer.

Béjar, Susana & Milan Rezac. 2003. Person licensing and the derivation of PCC effects. In *Romance linguistics: Theory and acquisition*, Y. Roberge & A. T. Pérez-Leroux (eds), 49–62. Elmsford: John Benjamins.

Béjar, Susana & Milan Rezac. 2009. Cyclic agree. *Linguistic Inquiry* 40: 35–73.

Bobaljik, Jonathan David. 2012. *Universals in comparative morphology: Suppletion, superlatives, and the structure of words*. Cambridge, MA: MIT Press.

Cheng, Lisa Lai-Shen. 1997. Partial" wh-movement. In *UCI working papers in linguistics 3*, L. Chen-Sheng Liu & K. Takeda (eds), 27-50. Irvine: University of California, Depart- ment of Linguistics

Cole, Peter & Gabriella Hermon. 1998. The typology of wh-movement: Wh questions in Malay. *Syntax* 1:221-258

D'Alessandro, Roberta. 2012. Merging Probes. A typology of person splits and person-driven differential object marking. Unpublished ms, Leiden University. https://ling.auf.net/lingbuzz/001771

D'Alessandro, Roberta. 2016. Floating Features in Syntax: Person, Deixis and Agreement in Italo-Romance. LSRL 46 (Linguistic Symposium on Romance Languages), University of New York at Stony Brook, March 31-April 3.

D'Alessandro, Roberta. 2017. When you have too many features: auxiliaries, agreement, and clitics in Italian varieties. Glossa: A Journal of General Linguistics, 2(1), 50.

DOI: http://doi.org/10.5334/gjgl.102.

D'Alessandro, Roberta. 2019. The achievements of Generative Syntax: a time chart and some reflections. Catalan Journal of Linguistics 2019. 7–26.

DOI: https://doi.org/10.5565/rev/catjl.232.

D'Alessandro, Roberta. 2020. Agreement across the board. Topic agreement in Ripano. In K. Hartmann, J.Mursell & P. W. Smith (eds), *Agree to Agree: Agreement in the Minimalist Programme*. Open Generative Syntax 4. Language Science Press.

D'Alessandro, Roberta and Adam Ledgeway. 2010. The Abruzzese T-v system: feature spreading and the double auxiliary construction. Roberta D'Alessandro, Adam Ledgeway, and Ian Roberts, (eds), *Syntactic Variation. The dialects of Italy*. Cambridge, UK: Cambridge University Press, 201-210.

D'Alessandro, Roberta & Marc van Oostendorp. in press. Language variation and functional heads. Magnetic Grammar. *Linguistic Analysis*.

Haspelmath, Martin. 2006. Against markedness (and what to replace it with). *Journal of Linguistics* 42(1). 25–70.

Haspelmath, Martin. 2010. Comparative concepts and descriptive categories in crosslinguistic studies. *Language* 86(3). 663–687.

Haspelmath, Martin. 2011. On S, A, P, T, and R as comparative concepts for alignment typology. *Linguistic Typology* 15(3). 535–567.

Haspelmath, Martin. 2012. How to compare major word-classes across the world's languages. In Thomas Graf, Denis Paperno, Anna Szabolcsi & Jos Tellings (eds.), *Theories of everything: in honor of Edward Keenan*, 109–130. (UCLA Working Papers in Linguistics 17). Los Angeles: UCLA.

Haspelmath, Martin. 2018. How comparative concepts and descriptive linguistic categories are different. In Daniël Van Olmen, Tanja Mortelmans & Frank Brisard (eds.), *Aspects of linguistic variation: Studies in honor of Johan van der Auwera*, 83–113. Berlin: De Gruyter Mouton. https://zenodo.org/record/3519206.

Haspelmath, Martin. 2020. Human linguisticality and the building blocks of languages. *Frontiers in Psychology* 10(3056). 1–10. https://doi.org/10.3389/fpsyg.2019.03056.

Haspelmath, Martin. 2021. The structural uniqueness of languages and the value of comparison for description. *Asian Languages and Linguistics* (to appear).

Huang, C.-T. James. 1982. *Logical relations in Chinese and the theory of grammar*. Doctoral dissertation, MIT, Cambridge, Mass.

Krifka, Manfred. 2008. Functional similarities between bimanual coordination and topic/comment structure. In Regine Eckardt, Gerhard Jäger & Tonjes Veenstra (eds.), *Variation, selection, development*, 307–336. Berlin: Mouton de Gruyter

Lin, Jo Wang. 1992. The syntax of zenmeyang 'how' and weishenme 'why' in Mandarin Chinese. *Journal of East Asian Linguistics* 1:293-331.

Nevins, A. Multiple agree with clitics: person complementarity vs. omnivorous number. *Nat Lang Linguist Theory* 29, 939–971 (2011).

DOI:https://doi.org/10.1007/s11049-011-9150-4

Xu, Liejiong. 1990. Remarks on LF movement in Chinese questions. *Linguistics* 28:355-382