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On categorization: Stick to the facts of the languages

DOI 10.1515/lingty-2016-0011

Received March 14, 2016; revised June 6, 2016

Abstract: This paper argues that in doing both description and comparison we should work inductively, staying true to the facts of the languages as manifested in natural data, and not resort to abstractions that lead to classifying languages or constructions in a way that ignores the actual facts of the languages. A non-Structuralist alternative view of communication and typological description is also presented.

Keywords: categories, comparison, methodology

1 Particular and comparative categories

Within the Structuralist paradigm and the Boasian tradition in language description, language particular categories are established on the basis of an inductive analysis of the morphosyntactic distribution of forms in texts in a particular language. The functions ascribed to the forms are inferences based on their distribution. The labels used for the categories that emerge from the analysis may be those used in the description of other languages, as long as there is enough of a “family resemblance” between the two categories. That is not the same as equating the two, or imposing a category that has no justification in the language (“apriorism”), or saying that there are crosslinguistic categories, but is simply saying that the category which needs to be described to make sense of the use of the form in that language is in some way similar to a category posited for another language and so can take the same label, like a greyhound and a beagle are both given the label “dog” even though there are many differences between them. For example, if the speakers of a language use a sound that is made with both lips initially closed and then released suddenly with near zero voice onset time (VOT) for distinguishing meaning (contrasting with sounds made similarly but with different VOTs, for example), we can establish that

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sound as a phonemic category (already a generalization/abstraction over the actual realizations of the sound), and it has been usual practice to give such a category the label /p/, as it is in some ways similar to what is represented in Latin by <p>. In terms of morphosyntax, if we find a construction that is used for constraining the inference of spatial relations such that a referent referred to in the construction is to be understood as moving away from another referent or reference point, then we might give it the name “ablative”, if there is some resemblance to the function and structure of the Latin ablative construction. If that construction is also used when the relevant referent has an instrumental function, we might also label it “instrumental” in those cases, or we might keep the same label (“ablative”) for both uses. Even at the descriptive level, there is quite a bit of abstraction and subjectivity in the labeling,¹ and so it is necessary that while using labels employed for other languages, the specifics of the construction that the label is given to in the language under discussion have to be made clear. One should not just say there is a /p/ and there is an ablative marker, and assume the description of these elements is done. This sort of prototype and resemblance labeling is of course not unique to linguistics; it has been shown (e.g., Rosch 1973, 1977a, b, 1978) to be the usual way humans categorize and label their experiences.

In the case of the labeling of descriptive categories, the resemblance should include resemblance in function, but cannot be limited to that: if a construction has a similar function but no similarity in form, then the label would not be used, e.g., marking source of information lexically and not morphologically would not be labeled “evidential” marking. The “family resemblance” should also be one that seems logical and clear, e.g., one would not use Latin <k> to represent a bilabial sound, or use “vocative” for a marker that has a relational sense of location. Of course, if the construction found is not similar morphologically or functionally to constructions manifested in other languages, then a new label should be given to the construction. For example, in a recent inductive study of Tagalog phrase structure (LaPolla 2014), I did not find anything that could be labeled “noun phrase” or “verb phrase”, and so had to label the constructions found with other more language-specific labels. Often when this is done, linguists working on other languages find that in fact they have found a construction similar to that one in the language they are working on, and so a label gets established in the literature, such as happened in recent times with the use of “ergative” and “evidential”. One of the Tagalog structures I identified

¹ See Chao (1934) on the non-uniqueness of the analysis itself. Of course there is also the influence of cultural and scholarly traditions. As Harris (1981) has argued, we have been locked into a particular way of looking at language since the time of Aristotle, purely a cultural artifact.

has some functions that seem similar in some ways to the “Eskimo Relative Case” construction (Woodbury 1985), but as that has not been generalized as a type of construction, I could not use that as a label.

Comparative categories are dependent on descriptive categories, as they are idealizations or prototypes formed on the basis of the family resemblances found in the descriptions. The International Phonetic Alphabet is an example of this, creating idealizations or prototypes of sounds found to be used for distinguishing meaning in language use, or, in the case of the Cardinal Vowels, sounds that can be defined in a relatively objective way so that they can be used as reference points for language-particular descriptions. As with the IPA, the grammatical comparative categories often are given the same labels as used in the descriptions, as Latin or English (or Chinese in the context of China) categories are taken as key reference points. As William Croft argued in a contribution to the Lingtyp discussion (18 January 2016, see Supplementary Online Materials), “we must be careful to define comparative concepts in consistent and crosslinguistically valid ways, that is, using formal properties that are crosslinguistically valid and consistently applied across languages”. The goal of the early Research Centre for Linguistic Typology International Workshops (e.g., Dixon & Aikhenvald (eds.) 2000) was to come to agreement on how to define the commonly used comparative categories, such as “passive”, “causative”, and “middle voice”, in consistent and crosslinguistically valid ways. The idea was that in descriptive work we could use those concepts in a consistent way in deciding on what labels to give the categories we established inductively in the languages we worked on, and in typological comparison we could determine the crosslinguistic distribution of languages manifesting a form that could be labeled with that category label, and also determine the crosslinguistic variation within that comparative category. Function was also brought into the discussion, and a useful distinction was made between forms that could be properly labeled with the comparative category label on formal grounds and forms that seemed to have a similar function as the forms of the relevant category, but did not have a form that could be said to fit the comparative category. The latter were called “strategies”, that is, strategies for performing the function of the category. This approach assumes that there are more regularities across languages in terms of function than in terms of form. These regularities are not universals, but functions that are found to be frequent in language use, such as predication and reference, and the functions are seen as explanations for the development of the formal categories, such as the frequent conventionalization of categories that have been labeled “noun” and “verb” out of the frequent use of certain items for reference and predication, respectively (e.g., Hopper & Thompson 1984).

Much of the discussion of this topic on the Lingtyp list (see Supplementary Online Materials) assumed the Structuralist paradigm, with fixed sign-signifier pairings, a distinction between diachrony and synchrony, and a distinction between grammar/langue/competency and use/parole/performance, though the problem of how to identify and isolate the particular grammar one is interested in from the many that could be talked about did come up in the discussion.

My own view (LaPolla 1997, 2003, 2015, 2016), developed from my experiences with languages and communication over many years, departs radically from the Structuralist paradigm:² I argue that there is no coding-decoding in communication, and no shared code among speakers of the same language; communication is achieved simply by ostension and abductive inference, regardless of whether linguistic forms are involved or not. The communicator does something (the ostensive act) with the purpose of the addressee inferring the intention to communicate and the reason for the particular ostensive act. By doing this, the addressee creates a meaning in their mind, which the communicator hopes will be similar to the meaning the communicator intended the addressee to create. That is, there is no meaning in the ostensive act (be it linguistic or not); the addressee creates a meaning based on the communicator's use of the particular ostensive act in the particular context by creating a context of interpretation, out of the overall context of assumptions available to him or her at that moment, in which the ostensive act "makes sense". As it is based on abductive inference, though, the outcome is non-deterministic.

Language use is one type of ostensive act. Conventions for the use of particular forms for constraining the interpretation in particular ways develop as speakers repeatedly use similar vocal forms for the ostensive acts, alone or with non-linguistic ostensive acts such as hand movements and facial movements. Although we talk about conventions, each person understands the use of the forms based on their own personal experiences, and so there is no shared code. The conventions are no different in type from the conventions of the "right" ways to cook, eat, dress, and do many of the other things we have conventionalized behavioral patterns for in each culture. Language is seen as a form of behavior, not as a thing, as it is treated in the Structuralist tradition (now even given ISO codes!). In this view language is not a fixed system, but patterns of behavior which emerge out of communicative interaction that are

² See Harris (1981) on the fallacies that underlie the Structuralist paradigm, particularly the ideas that communication is a coding-decoding process involving forms with fixed meanings. See Croft (2001) on the problem of "methodological opportunism" in the use of the distributional method, and arguments for the use of a constructionalist perspective instead.

remembered and used over and over again in different ways. There are no global categories, only roles in individual constructions which are defined functionally, so questions such as how to define word classes in a particular language do not come up, as we just need to look at how positions in particular constructions are used for predication or reference or modification (see, e.g., LaPolla 2013). For this reason (among others) I find it problematic to classify languages as if a single category can define the language, e.g., “ergative” or “SVO”. One needs to see what is going on with the individual constructions (see LaPolla et al. (2011) and the other papers in that special edition of *Studies in Language* for discussion relative to transitivity).

In this view, as language is seen as arising from the desire to constrain the inferential process in particular ways, then both individual language description and typology can be done by looking at how the languages of interest constrain the inference of a particular aspect of meaning, for example, the inference of the time of an action relative to the time of speaking, e.g., only lexically or with conventionalized morphological constructions, and, if they have conventionalized constructions for this, to what extent they “cut up” the possibilities, e.g., past vs. non-past, or three degrees of past vs. non-past, or past vs. present vs. future; and what morphosyntactic form the construction takes. What I meant above by “aspect of meaning” is similar to what Croft (2001) talks about as “conceptual space”, and the actual comparison could take the form of a semantic map or multivariate analysis, among other possibilities.

2 Other opinions

Matthew Dryer (e.g., 1997) and Martin Haspelmath (e.g., 2007, 2010b) both agree that language description should be inductive and based on the facts of the language, and both argue that there are no crosslinguistic categories. Both argue, though, that typological comparison of languages does not depend on the individual descriptions of the languages, and is a separate enterprise altogether. Typologists are free to create categories and apply them at will to languages, using the same labels as used in many descriptions, even if the language does not manifest the particular feature identified in the label. In a posting as part of the Lingtyp discussion (19 January 2016), I said that it is important to distinguish formal from semantic categories (agreeing with Dan Everett’s posting on this; 19 January 2016), and used the example of Chinese, which is often problematically characterized as SVO, when in fact there are no categories that can be labeled Subject or Object (and Verb is also problematic)

and that labeling it as such implies that these categories either determine word order or are determined by it (see LaPolla & Poa (2006) for discussion). Dryer replied (19 January 2016),

[...] classifying languages typologically does not entail that the terms employed in the typological classification correspond to categories in the language. Nor does it mean that these categories determine or are determined by word order. I have certainly made that clear in my work that classifying a language as SVO makes no claim about the categories in the language, nor that these categories determine word order even if the language has such categories.

I find this problematic because most people who see a description of Chinese as SVO will in fact assume that the label was given to the language because those categories are significant for determining word order in the language, otherwise why would someone apply such a characterization to the language? There is no way to tell when a person is saying it is SVO because it has these categories and when they are using it in some abstract way like Dryer's that does not imply that the label has any connection to the facts of the language.

In a later post on the same day Dryer says (19 January 2016; emphasis in original),

[...] languages in which word order does not code grammatical relations and in which the word order is not based on grammatical relations but in which VO word order is more common tend to have word order properties associated with VO word order, like prepositions, while analogous languages in which OV word order is more common tend to have word order properties associated with OV word order, like postpositions. What this means is that the GRAMMARS of what I classify as VO languages have nothing in common. It is only the languages that have something in common at the level of usage.

I don't subscribe to the competence vs. performance view that Dryer seems to work with, but also this statement is circular and logically problematic. Using the hypothesis to be tested (correlation between constituent order and type of adposition) to justify a classification of problematic data of course leads to results that support the initial hypothesis. To discuss the logic problem we can use the analogy that came up in the discussion, "wings" as a comparative category. From at least some perspectives bats and birds could both be said to have wings. That is not problematic, because there is enough of a family resemblance between the structure and function of the wings of birds and the membranes of bats, but what Dryer is talking about (e.g., classifying Chinese as SVO even though the word order is controlled by information structure and not controlled by factors such as Subject and Object or A and P – which is how Dryer defines "Subject" and "Object" in Dryer (2013) – because in texts counts A's

occur more frequently in initial position) is equivalent to saying that not only can birds and bats be classified as winged entities, but rockets can also be classified as such, because they have flight properties associated with winged entities. That is, ignoring the fact that the A's occur more often in initial position in Chinese because they are more often topical (and not because they are S or A) and classifying the language as SVO is the same as saying that since a bird flies because of its wings, and a rocket flies, we can classify the rocket as a winged entity. This classification would be based solely on functional grounds. I don't find this type of classification helpful. As argued in LaPolla & Poa (2006), it is better to look at the factors that actually determine the word order patterns in the languages, and then we can compare languages in terms of those factors. As Dan Everett said in a post to the discussion (19 January 2016):

Idealizations do not mean that there is no empirical connection between a specific language and the typological category. It is possible that we might have something that fails to correspond to any syntactic notion of subject in a particular language. But if the grammar-writer refers to it as a "subject" on semantic grounds this could be the equivalent of calling a "t" a "p" because it is the frontmost voiceless occlusive in a given language. So the grammar-writer would have introduced an error which could potentially be propagated throughout the typological literature. By the same token, calling that language SVO might not only obscure the actual facts of the language, but it would also be a disservice to typology [...] Subtleties missed only confuse the field.

While I am sympathetic to Haspelmath's goal (2010a and in the discussion) of arguing against crosslinguistic categories the way they are used by the generativists,³ he muddies the water of the discussion, first by lumping together those who follow the methodology I outlined in the first part of Section 1 above with those who assume there are universal categories that just need to be identified in the languages, and then by assuming that positing any connection between the facts of the languages and the comparative categories means one is equating the descriptive categories in different languages. Because I mentioned in my posting that in doing grammatical description it is important to distinguish topics, agents, and grammaticalized pivots, Haspelmath stated (19 January 2016):

In the 1980s, typologists discovered the important differences between agents, topics, and syntactic pivots (as noted by Randy), but such more fine-grained categories are still not sufficient for describing any language. Agents can be different across languages, topics can be different, and syntactic pivots can be different. Thus, even "agent", "topic" and "pivot" can only be used as comparative concepts, not as universally applicable descriptive categories that would somehow have the same meaning in different languages.

³ See Newmeyer (2010) for a generativist's reaction.

No one has said that agents, topics, and in particular grammaticalized pivots are the same across languages, or that the labels are meant to be comprehensive descriptions unlike the comparative categories I talked about above. They are not just comparative concepts; they are the same as /p/ and “ablative” in my discussion in Section 1, defined for an individual language and generalized to others only to the extent that that is warranted by the facts of the languages.

In reality, Haspelmath’s comparative concepts are not different from the concepts we have been using except that he argues that there does not need to be a connection between the facts of the language and the way it is categorized typologically, agreeing with Dryer. This is something I have been arguing against for many years (see, e.g., LaPolla (2002), presented at a conference in 1994) because I think it is confusing to non-specialists, and because it does not produce useful results, as it makes language patterns that aren’t similar look similar. For example, in word order typology, Dryer claims that the grammatical nature of a particular element makes no difference to how it patterns grammatically, but that is not true of the languages that I am familiar with. For example, it is because clausal noun modifiers in Chinese are nominal that they appear in the same construction as nominal noun modifiers and also pattern the way they do, being able to appear either before or after the noun head depending on pragmatic factors, and having no syntactic constraints on the type of semantic relation possible between the head and the modifying clause (see LaPolla (to appear)). In Rawang (Sino-Tibetan; Myanmar) there are both nominalized and non-nominalized clausal noun modifiers, and they pattern differently precisely because of their nominal vs. non-nominal status (LaPolla 2008).

Haspelmath (2010a: 672–673) defines a relative clause as “a clause that is used to narrow the reference of a referential phrase and in which the referent of the phrase plays a semantic role”, and then discusses the clausal noun-modifying construction in Japanese, which, much like in Chinese, can be used for a very wide range of relations between the head and the clausal modifier, and he says “Japanese has no category that closely corresponds to the descriptive category of Relative Clause in English, but for crosslinguistic studies of relative clauses, the construction in 12a⁴ can be taken as a relative clause”. What I find

4 That is, the example among those he cited that looks most like an English relative clause, with a clear relationship between the assumed patient of the modifying clause and the head noun (Haspelmath 2010: 671):

- (12) a. *gakusei ga katta hon*
 [student NOM bought] book
 ‘the book that the student bought’

problematic is that here we see the selection of one use of a construction with many uses singled out for comparison, ignoring the other uses. Again, I don't see how comparisons based on this sort of methodology can produce useful results.

3 Implications for language description

Although I believe the Structuralist paradigm is problematic from a scientific point of view, and would prefer linguists to use the view I outlined at the end of Section 1, I recognize that Structuralism (“basic linguistic theory”) is still the dominant framework for language documentation and description, as well as typology, and so the method outlined at the beginning of Section 1 can be used for language documentation, but the method must be used in a strictly inductive way, that is, no categories should be imposed on the language, and the labels used for the categories should follow the principles mentioned above, with each one explicitly defined.

Glosses are just abbreviations for the categories established inductively and possibly designated by conventionalized labels using family resemblance. Those abbreviations given in the Leipzig glossing conventions (they aren't “rules”) can be used if the label is appropriate on those grounds, but should never be imposed on a language for which they aren't justified.

4 What is typology good for?

I am not a fan of large scale language comparison, as the limitations of the linguist's knowledge become all too obvious. That is, combing hundreds of grammars (of varying quality) and extracting forms that one thinks might fit one's comparative categories (regardless of what the author of the grammar might have said) is very problematic. It is much better to concentrate on languages one has a good knowledge of and contribute to typology by expanding our understanding of what is found and how we might understand it, including its historical origins. Another problem with the Structuralist approach is the denial of the relevance of history, and so the question that started this debate was about “structural congruence as a dimension of language complexity”, and the idea was to look for mystical harmony principles for the facts of word order, when in fact simple reference to the history of the language often

explains the facts, e.g., if you have a possessive construction where reference to the possessor precedes reference to the possessed, and that is reanalyzed as an adposition construction, as often happens, then you end up with “structural congruence” between the adposition construction and the possessive construction. See LaPolla (2002) for discussion of this and other problems with the methods and explanations used in word order typology.

Acknowledgements: I would like to thank Rik De Busser and Alec Coupe for very helpful comments on a draft of this paper.

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