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Defining vs. diagnosing linguistic categories: A case study of clitic phenomena*

Abstract: Linguistic generalizations, e.g., about phenomena labeled “clitics,” presuppose that we identify classes of phenomena in a consistent way. But many grammatical terms (including the term “clitic”) are used for quite different phenomena in different languages. This is sometimes obvious, and sometimes less so. In this paper, I contrast two views about categories and their cross-linguistic applicability: The restrictivist approach assumes that there is a universal set of features and categories from which languages may choose, while the non-aprioristic approach makes no such assumption and proposes to compare languages on the basis of a special set of comparative concepts that are not closely related to language-specific descriptive categories. On the restrictivist view, categories exist independently of individual languages (as they are innate and thus given in advance). Thus, they can be identified by diagnostics, much like diseases are identified through their symptoms, and different languages could exhibit different diagnostics. However, for clitics this approach fails demonstrably, as there is no agreement on how to distinguish clitics from affixes or free words. This approach allows no way to go beyond subjective judgements. The non-aprioristic view is that linguistic classes should be defined and thus identified rigorously and objectively. This applies both to language-specific descriptive categories and to comparative concepts. To illustrate this, I propose two new comparative concepts, plenimorph and minimorph, for making a few readily falsifiable claims about the “clitic” domain.

1. Taxonomies and their roles in two theoretical orientations

In this paper, I argue that the best approach to understanding grammatical phenomena in the world’s languages is to devise clearly defined comparative concepts, find

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cross-linguistic generalizations that are formulated in terms of these concepts, and look for explanations of these generalizations. This is what I have called the non-aprioristic approach to comparative morphosyntax in Haspelmath (2014). I contrast this theoretical orientation with the restrictivist orientation (mostly identified with linguistics in the Chomskyan tradition), where there is no need to have clearly defined comparative concepts, but where the emphasis is on diagnosing cross-linguistic categories that are assumed to be part of Universal Grammar. I show that the fundamental difference between the two approaches leads to very different kinds of general statements (see Błaszczak and Klimek-Jankowska (this volume) for a related discussion concerning nouns and verbs).

The contrast between non-aprioristic and restrictivist approaches can be seen in many different areas of grammar (e.g., in discussions of reflexive constructions; cf. Haspelmath (2008a), or in discussions of word-class distinctions; cf. Haspelmath (2012)), but in this paper I focus on “clitic” phenomena. Examples (1)–(4) give some illustrations of phenomena that have been called clitics from Polish and German.¹

- (1) a. *Widziałem go wczoraj.* (Polish)
 saw.1SG him yesterday
 b. *Wczoraj go widziałem.*
 yesterday him saw.1SG
 c. **Go widziałem wczoraj.*
 him saw.1SG yesterday
 ‘I saw him yesterday.’
- (2) a. *Ich hab se gestern gesehen.* (Colloquial German)
 I have her yesterday seen
 [standard: *Ich habe sie gestern gesehen.*]
 b. **Se hab ich gestern gesehen.*
 her have I yesterday seen
 ‘I saw her yesterday.’
- (3) a. *Co tam widziata-ś?* (Polish)
 what there saw-2SG
 b. *Co-ś tam widziata?*
 what-2SG there saw
 ‘What did you see there?’

¹ Abbreviations used in the glosses of examples include the following: 1/2/3, first/second/third person; APPL, applicative; ACC, accusative; CAUS, causative; CONJ, conjunction; DAT, dative; ERG, ergative; FUT, future; M/N, masculine/neuter; MV, monovalent; NEG, negation; NOM, nominative; NONVOL, nonvolitional; PASS, passive; PFV, perfective; PL, plural; POSS, possessive; PST, past; PROG, progressive.

- c. *W którym miejscu go widziała-ś?*
in which place him saw-2SG
- d. **W którym miejscu-ś go widziała?*
in which place-2SG him saw
'In which place did you see him?'
- (4) a. *Ich gehe zu dem Arzt.* (German)
I go to the doctor
- b. *Ich gehe zu-m Arzt.*
I go to-the doctor
'I go to the doctor.'

To understand clitic phenomena and their relation to adjacent phenomena (in particular, affixation and phrasal combination), linguists have used a number of different taxonomies, some of which have become widely known, e.g., those in (5).

- (5) some well-known taxonomies
- a. root vs. affix (Renaissance linguistics)
 - b. morphology vs. syntax (Schleicher 1859)
 - c. free vs. bound (Bloomfield 1933: 160)
 - d. XP vs. X° (Chomsky 1957; Jackendoff 1977)
 - e. phonological word vs. grammatical word (e.g., Dixon 1977)
 - f. special clitic vs. simple clitic vs. bound word (Zwicky 1977)
 - g. phonological word vs. clitic group vs. phonological phrase (Nespor and Vogel 1986)
 - h. clitic pronoun vs. weak pronoun vs. strong pronoun (Cardinaletti and Starke 1999)
 - i. word vs. clitic vs. affix (standard textbooks, e.g., Spencer and Luís (2012))

I argue here that the role of such conceptual distinctions is quite different in restrictivist approaches and non-aprioristic approaches. In restrictivist theories, the innate cognitive endowment for language (Universal Grammar, UG) restricts the child's options in acquiring or internalizing a language. Thus, UG explains both language acquisition and cross-linguistic regularities. In non-aprioristic theories, by contrast, it is claimed that the child constructs a grammar from the bottom up (Tomasello 2003), and that cross-linguistic regularities are primarily explained by general cognitive and social conditions for language use (e.g., Croft 2003; Hawkins 2004; Moravcsik 2011).

For restrictivist theories, there is no real distinction between the conceptual taxonomy and the theory, as the universal categories and the universal architectural design constitute the theory: cross-linguistic generalizations are explained on the basis of the hypothesized innateness of categories and architectures. For example, one could say that morphology and syntax are two distinct components of the grammar and that clitics are generated in a post-morphological com-

ponent (cf. Anderson 1992, 2005). This makes certain predictions about their behaviour cross-linguistically – for example, they should always occur in a position peripheral to affixes and their shape should not depend on the shape of the roots or affixes with which they combine.

For non-aprioristic theories, a well-known taxonomy of the type in (5) may be a starting point for talking about the phenomena, but pre-defined categories or taxonomies are not assumed to constrain children in acquisition or linguists in description (Haspelmath 2007a). Grammatical concepts have an important methodological role, but no theoretical role. The crucial theoretical question is how cognitive and social conditions for language use can explain the observed cross-linguistic regularities. Thus, if a comparative concept of clitic can be formulated in such a way that it can be rigorously applied to all languages, it could be used as a basis for cross-linguistic generalizations about clitics. However, we will see below that no rigorously defined cross-linguistically applicable concept of clitic seems to exist. And I know of no universals (or universal tendencies) of the type “If X is a clitic, then... ,” or “If a language has clitics, then... .” Thus, at present the concept of clitic has no important role in the non-aprioristic approach.

In section 7 below I will suggest a few alternative concepts that can be defined rigorously and that could form the basis for stable cross-linguistic generalizations.

It should be noted that by contrasting a restrictivist with a non-aprioristic approach, I am setting up ideal types, with the goal of elucidating the methodological divisions that become apparent to every advanced student of contemporary cross-linguistically oriented grammatical research. In my discussion of clitic phenomena below, I am also setting up something of a strawman (as a reviewer noted correctly), as few contemporary generative linguists would advocate a “clitic” concept as a concept that restricts the kinds of analyses that linguists should adopt and the kinds of languages that children could acquire. However, my point is that the “clitic” concept is exemplary for the kind of problem that arises if one takes a phenomenon from a single language and hypothesizes that it reflects a universally available category.² So I expect that even if one accepted none of the concepts in (5a) – (5i) as part of UG but still adopted the re-

² A reviewer recommends the procedure, adopted by Cardinaletti and Starke (1999), of reducing disparate phenomena to a completely new set of concepts. However, their tripartition of person forms into *clitics*, *weak pronouns* and *strong pronouns* is really based on a few interesting converging observations in German and Italian, and cannot be extended to many other languages without encountering the familiar problems (see Siewierska (2004: §2.1.2.3) for some discussion from a typological perspective).

strictivist approach using some other distinctions with the same style of argumentation (based on diagnostics rather than criteria), the same problems would arise sooner or later.

2. Some clitic properties and problems with them

The literature on clitic phenomena has discussed a fair number of clitic properties (clitic diagnostics or clitic criteria – see section 3 below). But there is no single set of properties that always uniquely identifies clitics and distinguishes them from affixes. I have discussed this problem in some detail in earlier work (Haspelmath 2011a, 2011b), so I will just summarize a few of the relevant points here.³

2.1. Phonological dependence and syntactic (in)dependence

It is often said that clitics behave syntactically like words but are phonologically dependent. Thus, the German clitic pronoun *se* (in (2a)) has a schwa vowel, which can never be the only vowel in isolated utterances, and which never bears stress. And the Polish clitic *-ś* (in (3a) and (3b)) has no vowel and thus must be attached to a preceding host with a vowel. But this is not a necessary criterion for cliticness. For example, the Polish clitic *go* ‘him’ (see (1)) is phonologically complete (cf. *co* ‘what’, which can occur as an isolated utterance), but it still behaves in a clitic-like way.

Conversely, it is sometimes said that clitics behave like affixes but are phonologically less dependent on their hosts in that they are not in their prosodic domain, e.g., stress or vowel harmony. Thus, the Polish clitic *-śmy* ‘1PL’ is ignored for stress assignment (at least in the conservative, standard variety), which is on the penultimate syllable: *widziéli-śmy* ‘we saw’ has stress on the *e*, not on the *i* (**widzieliśmy*). However, there are many cases where stress-neutral elements are called affixes, not clitics. For example, English has a well-known contrast between *-ity*, which counts for stress assignment and thus leads to a stress shift

³ I will not discuss the delimitation between bound forms (“affixes” and “clitics”) on the one hand and free forms (many independent “words” and most phrases) on the other hand. These can be distinguished by the single criterion of independent occurrence in an isolated utterance (Bloomfield 1933: 160; cf. Haspelmath 2013: 200–201).

(*grammátical* → *grammaticálicity*), and *-ness*, which does not count for stress assignment (*grammáticalness*). But one would not say that *-ness* is a clitic.

Moreover, different phonological criteria may lead to different conclusions about phonological dependence. Thus, one might say that the Turkish element *-ki* is a clitic as it is both outside the vowel harmony domain and outside the stress domain:

- (6) a. *év* 'house'
 b. *ev-dé* 'in the house'
 c. [*ev-dé*]-*ki* '(the one) in the house'
 d. *baş* 'head'
 e. *baş-tá* 'in the head'
 f. [*baş-tá*]-*ki* 'the one in the head' (no harmony: **baş-tá-ki*)

However, many elements are inside the vowel harmony domain but outside the stress domain, e.g., *-me/-ma* 'NEG' (e.g., [*gél*]-*me-di* 'he didn't come') or the question particle *mi/mu/mu/mü* (e.g., [*gel-dí*] *mi* 'did he come?'). Conversely, some elements are inside the stress domain but not inside the vowel harmony domain, e.g., *-yor* 'PROG' ([*geli*]-*yorum* 'I am coming'); see Kabak and Vogel (2001). Neither the stress domain nor the vowel harmony domain coincides with the orthographic word.

In general, different phonological criteria often give different results for the determination of phonological-word domains (Schiering, Bickel, and Hildebrandt 2010).

2.2. Idiosyncrasy and phrasal position

According to a previously widely held view, positioning of a grammatical marker at the edge of a phrase implies that the marker is a clitic also in the sense that its form is predictable and is not subject to lexical or morphophonological idiosyncrasies (e.g., Anderson 2005). However, it has now been recognized that there are also idiosyncratic elements which occur at the edge of a phrase (Anderson et al. 2006). An example of such an element is the Tagalog linker, which occurs between a noun and an adjective, and which has the form *na* after a consonant and *ng* after a vowel. As the examples in (7) show, adjective-noun order in Tagalog is variable, but the linker must occur between the two.

- (7) a. *libro-ng bago* ‘new book’
 b. *damit na bago* ‘new dress’
 c. *bago-ng libro* ‘new book’
 d. *mahal na libro* ‘expensive book’

There is no phonological reason why *libro-na* (or at least *libro-n*) should not be possible, so the alternation is idiosyncratic. The linker *na* is written separately, while the linker *-ng* is written jointly with the preceding word. Thus, the spelling itself shows the unclarity about the clitic or affix status of this element.

2.3. Disparateness and non-convergence of properties

The problems boil down to the cross-linguistic **disparateness** and the within-language **non-convergence** of the identifying properties of clitics.

The properties are disparate across languages in the sense that different properties are used for different languages. This would not be a problem if each property had the same status. We could then say that while in one language properties A, B and C identify clitics, there are other languages where only properties A and B are relevant, and still others where only C and D are used to identify clitics. There would be a finite set of properties that all single out clitics, and it is the linguist’s task to find a subset of relevant properties in each language. The fact that the language-particular sets of properties are disparate would then not be a problem. Basically, each property would be a sufficient criterion for clitic status.

However, the criteria do not necessarily converge within a language. So if more than one criterion is applicable, they need not point in the same direction, as we saw in the preceding subsection. Because of the widespread non-convergence, the disparateness is a fatal problem. If the failure of a presumed clitic to exhibit an expected behaviour is not a clear indication that it is not a clitic, on the grounds that it has some other clitic properties, this means that linguists are not constrained in their choice of criteria. They can basically choose the criteria that suit their preconceived ideas because there is no limit to what could count as a clitic property.

3. Defining criteria vs. diagnostic tests

The frequent non-convergence of identifying properties has often been observed empirically (e.g., Dryer 1997a; Croft 2001; Bickel 2010), but it has not stopped linguists from applying the “test-battery method” to persuade their colleagues

that their categorization is the correct one (see Haspelmath (2011a: 59–60) for a range of examples from the literature).

The reason for this is that on the restrictivist approach, there is no direct relationship between categorization and empirical evidence. This was highlighted by Zwicky (1985: 284–86), who makes a clear distinction between **diagnostic tests** and **defining criteria**. He says that on the restrictivist approach (the only one he considers), the familiar tests for category membership are not “necessary and sufficient conditions for the applicability of a theoretical term,” i. e., they are not criteria for its definition:

“what is normally intended, when such tests are appealed to, is more analogous to medical diagnosis than to operations using an axiomatic system. The tests point to characteristic SYMPTOMS of a linguistic state of affairs, not to invariant concomitants of it.” (Zwicky 1985: 285)

Diagnostic tests in theoretical (restrictivist) morphosyntax, according to Zwicky, are much like diagnostic tests in medical diagnosis: “interfering factors can prevent even clear cases from exhibiting a certain symptom, and a particular symptom might result from some condition other than the one at issue” (Zwicky 1985: 285).

Given the nativist assumption that there is a relatively small set of pre-established, universal (or universally available) cross-linguistic categories, this approach makes very good sense. On this assumption, we trust that there are relatively few underlying categories and that by judiciously applying diagnostic tests and our experience and intuitions, we can identify the correct categories. This is again completely analogous to the classical activity of medical doctors: they trust that there are relatively few underlying diseases and that by judiciously applying diagnostic tests and their experience and intuitions, they can identify the disease that is the underlying cause of the symptoms. In medicine, there is no need to define diseases such as measles or influenza through their symptoms, because we know that these diseases are caused by particular pathogens which exist independently of the symptoms. There is thus abundant evidence that each patient should NOT be described “in her own terms,” but rather in terms of what is known about the pathogen’s typical effects on the organism and how the pathogen can be eliminated. Thus, an aprioristic approach to diseases is very sensible, because it is based on more than the hope that our universal categories will turn out to have a basis in reality.⁴

⁴ This is a fairly idealized picture of medical diagnosis, of course. Many patients have symptoms that cannot be linked clearly to a pathogen or to another single causal factor (this

In linguistics, it is much less clear whether our categories exist independently of the distributions of linguistic units that linguists observe in order to describe a language and that children observe when acquiring a linguistic system. While this assumption has often been made by restrictivists, the evidence for the independent existence of categories is quite ambiguous. It is perfectly possible that just as Boasian fieldworkers describe each language “in its own terms,” i. e., with its own categories rather than with the categories of Standard Average European, children acquire their native language in its own terms, rather than with a set of pre-established innate categories. It is true that the categories of different languages are often strikingly similar (e. g., “nouns” and “verbs” are very similar across languages, much more so than “affixes” and “clitics”), but the perceived similarities are sometimes artifacts of our descriptive traditions. (For this reason, it is often instructive to examine the history of the tradition of the clitic concept, as I will do in section 5 below.)

It should also be noted that the experience-based and intuition-based nature of the categorization or taxonomization process in the restrictivist approach seems to be the source of much frustration in linguistics. Linguists often just cannot agree on the right way of cutting up the phenomena,⁵ and as the categories are seen as the basis of further work (e. g., application to psycholinguistics, or typological comparison of languages), this is a serious problem. Moreover, I think that it is quite likely that the intuition-based nature of the identification of categories leads to the peculiar sociological phenomenon that morphosyntactic theorists tend to rally around charismatic leaders (theoretical frameworks such as RRG (Role and Reference Grammar), HPSG (Head-driven Phrase Structure Grammar), LFG (Lexical Functional Grammar), FDG (Functional Dependency Grammar), and GB (Government and Binding Theory) have arisen around a single researcher with a strong personality). If there is no non-subjective way of distinguishing between competing proposals, the stronger personality tends to attract other researchers who want to be part of a larger group that makes similar

seems to be the case with many psychiatric disorders, for example). In such cases, there are many disagreements among medical researchers about the right categorization of diseases, and researchers with strong personalities (or big budgets) are likely to have a strong influence on how diseases are generally categorized by the discipline.

5 This concerns especially languages that have a larger number of researchers working on them. Where medical patients are diagnosed by a larger number of doctors, we probably see a similar divergence of opinions (at least when no laboratory methods for identifying the pathogen can be applied). With smaller languages which are “treated” by only a single or very few linguists, there is of course less disagreement.

assumptions. There is of course little reason to think that stronger personalities are more likely to hit upon the correct taxonomies and categories.⁶

In the non-aprioristic approach, by contrast, researchers do not work with intuition-based diagnostic tests, but with strict criteria that define a category in the classical way, as jointly necessary and sufficient for category membership. This applies both to **descriptive categories** that are used just for the description of an individual language and to **comparative concepts** that are used for the typological comparison of diverse languages (Haspelmath 2010).

In creating descriptive categories and comparative concepts, linguists are no doubt guided by their intuitions and their experience (cf. Lazard 2005). And there may well be disagreements among different researchers about the best descriptive categories for an individual language, but as these are not foundational elements, these disagreements are less problematic. If a non-aprioristic researcher has no cognitive commitment and confines her goals to a complete account of the rules of the language (phenomenological description; see Haspelmath (2004)), then the differences with other researchers are merely notational or esthetic. If she has a cognitive commitment, then the disagreements are about the primary goal of the research, and hence an inspiration for further research rather than a source of frustration. In either case, there are no repercussions of taxonomic or categorial choices on researchers working on other languages.

There may also be disagreements about the best comparative concepts, but in contrast to descriptive categories, there is no incompatibility between competing comparative concepts. For example, in comparing the behaviour of two-place verbs across languages, one could use both a Comrie-style definition of A(gent) and P(atient) and a Bickel-style definition of A and P (see Haspelmath (2011c) for the different definitions of these terms), and then compare the results of the two comparisons.

6 A reviewer observes that this implies that things were better “if linguistics were dominated by leaderless collectives,” and objects that “it’s unclear that there are any good examples of scientific breakthroughs being made collectively in this fashion,” without influence from “a well-known source” such as Kant, Newton, Aristotle. But the difference between Newton and the various proposals of universal categories of language is that his categories have been confirmed in countless observations and have stood the test of time. It is doubtful whether linguistics has ever seen a real scientific breakthrough, though of course philosophical ideas of leaders like Humboldt, Paul, Frege, Saussure, Jakobson, Chomsky, and Langacker have been important in directing people’s thinking (somewhat like Aristotle and Kant, whose work has not led to breakthroughs either). So in the absence of a prospect for breakthroughs, I do think that leaderless collectives are more likely to advance our knowledge, slowly but steadily (much like in contemporary medicine, where we are not seeing major breakthroughs, but still noticeable progress on many fronts).

Thus, a key difference between the diagnosing and the defining approaches is that on the defining approach, once the criteria have been chosen, their application is automatic and non-subjective. Of course, in practice different humans may arrive at different decisions, but at least in principle, they should have all the resources to arrive at the same classification. Thus, the defining approach is in principle suitable for research projects in which different researchers classify different languages by the same criteria. Table 1 sums up the distinction between diagnostics and defining criteria.

Table 1: Diagnostic symptoms/tests vs. defining criteria

diagnostics (= symptoms)	criteria (= definientia)
<ul style="list-style-type: none"> – point us to an underlying reality that may not be observable directly – may be absent or occasionally contradict each other – ultimately, taxonomy (= theory-building) is done on the basis of the researcher’s experience and intuitions 	<ul style="list-style-type: none"> – give us a set of phenomena that may be useful in some way – jointly necessary and sufficient – category assignment is intended to be objective and verifiable

4. More distinctions than clitic and affix: Makassarese

When confronted with problems in assigning elements to one of the two pre-established classes of affix or clitic, or clitic or word, one possible solution is to set up a new category, intermediate between the established categories. This is basically what happened in the case of the category “clitic” itself, because until the 1950s, it was not widely used in linguistics (see section 5 below).

Let us briefly look at a concrete example of an intermediate case, from a detailed description of the Makassarese language by Basri, Broselow, and Finer (1999) and Jukes (2006). According to these authors, there are three kinds of bound elements in Makassarese:⁷

- Affixes (count for stress, attach to words)
- Clitics (do not count for stress)
- Afflicitics (count for stress, attach to phrases)

⁷ As in Croft (2001), I capitalize language-particular categories to highlight their non-general nature.

Affixes count for stress and are attached to bases which are either roots or consist of roots and other affixes. They do not attach to root + clitic combinations. Examples of affixes are given in (8) (Jukes 2006: 138).

- (8) a. *pi-kánre*
 PASS-eat
 'be eaten'
 b. *pi-kanré-ang*
 PASS-eat-APPL
 'let something be eaten'
 c. *pap-pi-kanré-ang*
 CAUS-PASS-eat-APPL
 'let something be eaten (by)'

Stress is on the penultimate syllable in Roots and Root-Affix combinations, so postposed Affixes (suffixes) are clearly distinct from postposed Clitics. Clitics are written with a “=” boundary marker (Jukes 2006: 143); cf. (9).

- (9) *la=ku=ápa=ma=ko*
 FUT=1SG.ERG=do.what=PFV=2 F
 'Now what will I do with you?'

Moreover, Clitics have a variable position and may also occur in second position. The Clitic =*mo* may occur after negative *tena*, for example (Jukes 2006: 148):

- (10) *téna=mo n=ak-külle ac-cíni'*
 NEG=PFV 3.ERG=MV-can MV-see
 'He can't see any more.'

Affclitics are intermediate between Affixes and Clitics. On the one hand, they count for stress (as seen in (11a) and (11b)), but on the other hand, they attach to phrases rather than words (as seen in (12a) and (12b)) (Jukes 2006: 154–155). Affclitics are written with the special boundary marker ≡ by Jukes (2006).⁸ They are mostly postposed possessive person indexes.

⁸ Jukes actually calls Affclitics “affixal clitics,” but this term is confusing as it suggests that they are a subtype of clitics, which they are not on Basri, Broselow, and Finer’s and Jukes’s analysis. (An alternative term he suggests in a footnote is “phrasal affix,” Jukes (2006: 151), but this term has the same problem.) Thus, I have made up the term Affclitic as an ad hoc term for this intermediate category, for clarity of presentation in this paper.

- (11) a. *bálla'*
house
'house'
- b. *ballá'≡na*
house≡3.POSS
'his/her/their house'
- (12) a. *miong le'léng≡ku*
cat black≡1.POSS
'my black cat'
- b. *kalimbu'* *ta = tas-sungké≡ku*
[mosquito.net NEG = NONVOL-open≡1.POSS]
'my mosquito net which is unopened'

From the descriptive, language-particular point of view, the additional category of Affclitic is a good solution. The alternatives would have been either to set up two subclasses of Affixes, or two subclasses of Clitics, but none of these impose themselves.

A linguist following the restrictivist approach might be tempted to adopt this tripartition as a general solution to classificatory problems. One could hope that just as an additional category intermediate between clitics and affixes in Makassarese solves a problem for Makassarese, adopting the same category for other languages would provide solutions there as well. Affclitics would be assumed to be part of Universal Grammar, along with affixes and clitics. Thus, one might want to say that while *se* in German (2a) is a clitic, *-m* (in (4b)) is an affclitic, or that while *go* in Polish is a clitic, *-ś* is an affclitic.

However, there is no reason to expect that the classificatory problems would diminish. The issue of disparateness and non-convergence of properties would raise its ugly head in exactly the same way as for the distinction between affixes and clitics (see section 2). In fact, not even Makassarese is described sufficiently with these three categories.

Jukes (2006: 89) notes that there are some elements which cannot be readily classified as clitics or word: *ri* 'preposition', *na* 'and' and *ka* 'because':

"I posit these three elements *ri*, *na* and *ka* as particles somewhere between clitics and words – grammatical elements which cannot occur in isolation and do not take stress, but which have a degree of phonological independence."

Moreover, the definite marker $\equiv a$ does not behave uniformly. Jukes writes it with the boundary marker $\equiv a$ and calls it an Affclitic, and after vowels it does behave like the possessive person indexes in that it counts for stress:

- (13) a. *bátu*
 ‘stone’
 b. *batú=a*
 ‘the stone’

However, following a consonant-final base, it behaves like a clitic in that it does not count for stress (Jukes 2006: 151):

- (14) a. *kóngkong*
 ‘dog’
 b. *kóngkong=a*
 ‘the dog’

Thus, not even Makassarese, the language for which the tripartite system was proposed, can be satisfactorily described with it. For complete coverage, one would have to add at least two additional categories, though these would not have many members.

This situation seems to be quite typical of language systems: while there are some broad categories with many members and productive use, there are also many idiosyncratic expressions which defy easy categorization. This was emphasized, for example, by Culicover (1999) with respect to atypical English words such as *enough*, *ago* and *both*.⁹ These behave in special ways that cannot be described exhaustively by subsuming them under some larger category.

For the restrictivist approach, such cases present a puzzle, because it is expected that the limited number of possibilities of Universal Grammar restrict languages: if there is at least one language that has a certain category, then this should be a possibility for any language, and it should be more widely attested. For the non-aprioristic approach, the challenge is the opposite: to explain why most languages have large productive classes of elements that behave alike and why there is not even more diversity, with hundreds of small classes of items. But in the non-aprioristic approach, the issue of explanation is separate from categorization and description (cf. Haspelmath 2004; Dryer 2006; Haspelmath 2014).

One reviewer objects to this and observes that “the surface phenomena of any language may be highly idiosyncratic, even unique [...] There is absolutely no incompatibility between the assumption of UG and the idiosyncratic surface phenomena of individual languages.” But if this view is adopted, then UG can no

⁹ Another example is the English element *let’s*, which was recently highlighted by De Smet (2014+). De Smet notes that “distributionally, *let’s* does not clearly pattern with any other grammatical elements,” so it defies classification.

longer be seen as restrictive, i. e., as limiting the options for the child in language acquisition and for the linguist in analyzing a language. There is certainly a tendency in contemporary Chomskyan circles to downplay the restrictive role of the features and categories that are assumed (e. g., Kayne (2013: 136, n. 20) says that he agrees with much of Haspelmath (2007a)), but to the extent that this is true, this work is no longer in the spirit of the long dominant restrictivist approach, and may indeed come closer to the non-aprioristic approach.

5. On the historical sources of our taxonomic categories

It is not often recognized that the taxonomic categories that linguists typically work with, that they use in their papers and teach their students, are usually not the result of any kind of systematic research, but for the most part derive from the vagaries of history. Zwicky (1985) tries to find diagnostics for the two distinct categories of *particle* and *clitic*, but why should we hypothesize in the first place that there is such a universal categorial distinction?

Apparently, in all cases where categorial universalists hypothesize that a category is part of Universal Grammar and try to identify it in some language by a range of diagnostics, the historical origin was a fairly modest one: some linguist observed a distinction in some language and coined a term for it that some other linguist found useful. Almost all linguists who study a new language have also studied some other language on the basis of works by other linguists, and linguists have always had a habit of carrying over terms from one language to another one, starting with the Roman grammarians who adopted Greek grammatical concepts wholesale. So some Greek linguist (perhaps Dionysius Thrax) coined the concept of “preposition,” which has been with us ever since, because linguists kept finding it useful for many languages. If the history of linguistics had been different, for instance if the first influential grammarian had described a language with two rather diverse classes of “prepositional” words (say, grammatical prepositions and semantically richer prepositions), we might well have inherited two different terms for these two classes, and our habits of dividing up the phenomena in the adpositional space would be rather different.

A particularly striking case in the area of clitics is Zwicky (1977), with his distinction between simple clitics, special clitics and bound words:

- (15) a. Simple clitic a short, reduced item that has a longer counterpart, e.g., English 'll in *Kim'll do it* (cf. *Kim will do it*)
 b. Special clitic a short item that behaves differently from other elements in its semantic class, e.g., Romance clitic pronouns
 c. Bound word an element that is a word in the syntax but has special syntactic and morphological properties, e.g., English genitive 's

Zwicky's paper was found insightful by many linguists, and it was widely applied to other languages (e.g., Nübling (2005) on German). Even though Zwicky (1985: 284) considers his (1977) work "pre-theoretical," it has been widely assumed that these categories are cross-linguistic categories that could be found in any language. The same is true of the term "clitic" itself, though it is much older.

The roots of the term lie in the term **enclitic**, which comes from the grammar of Ancient Greek. Greek grammarians were quite concerned with matters of stress, and to this day Greek is the only language of Europe where word stress is always marked in the orthography. In Ancient Greek, some words behave in a peculiar way prosodically in that they do not have a stress of their own and sometimes cause a second stress on the preceding word, depending on its prosodic properties. These items were called *en-clitic* ("on-leaning") words, a term that was apparently first used by Trypho in Alexandria (first century BCE). Examples are:

- (16) a. *agathós estin*
 good.M.N.SG is
 'he is good'
 b. *érkhetáí tis*
 comes who
 'somebody is coming'

The enclitics of Ancient Greek are pronouns, high-frequency verbs and a few particles. The term *enclitic* was later also used by Latin grammarians to refer to a few Latin items such as *que* 'and', as in (17).¹⁰

- (17) *senatus populus que romanus (SPQR)*
 senate people and Roman
 'the Roman senate and people'

¹⁰ In modern practice, *que* is usually spelled as a suffix (*populusque*), perhaps because the element *-que* is part of the stress domain (*pópulus* vs. *populúsque*). But whoever created the well-known abbreviation *SPQR* apparently thought of *que* as a separate word.

In the 19th century, unstressed short elements such as the French person forms *je, tu, il*, etc., which generally precede the verb, were sometimes called *proclitic*.

On the basis of the terms *proclitic* and *enclitic*, which referred to short word-like elements that had no stress of their own, Eugene Nida coined the general term *clitic* in his influential (1946) textbook. *Clitic* is thus originally a phonological term (referring to “simple clitics” or “bound words”), but by association it was carried over to elements with special syntactic behavior (“special clitics”). The contemporary sense of the term is thus somewhat removed from the original sense. Spencer and Luís (2012: §3.2.2) note that “the principal interest of clitics lies in the so-called special clitics and from now on when we use the term clitic it will usually mean special clitic.”

While Nida (1946) first used the term *clitic* (without prefix *en-* or *pro-*), it became widely known only after Perlmutter (1970) and Kayne (1975) talked about Romance clitic pronouns, and especially after Zwicky (1977) wrote his widely cited paper “On clitics.” However, by 1994, Zwicky no longer believed that there was a cross-linguistic category of clitic in the sense of a “theoretical construct:”

“*clitic* [...] is an umbrella term, not a genuine category in grammatical theory. Umbrella terms are names for “problems,” for phenomena that present “mixed” properties of some kind, not names for theoretical constructs.” (Zwicky 1994: xiii)

In the same vein, Spencer and Luís write at the end of the concluding chapter of their book on clitics:

“while the CATEGORY of clitic may not exist, some sort of CONCEPT of clitic remains ubiquitous [...] as an umbrella term. [...] the term usefully points to elements which cannot easily be classified as normal affixes or normal function words.” (Spencer and Luís 2012: 327)

But other contemporary authors such as Vogel (2009) are still clinging to a clitic notion in the sense of a cross-linguistic category, and of course the very fact that Spencer and Luís have written an entire book (an overview book suitable as an introduction) will help the term *clitic* survive. Many linguists will probably continue to assume that it is a pre-established category of universal grammar.

My suspicion is that the content linguists have tended to give to the term “clitic” is whatever properties they associate with typical textbook examples, and these have varied over time. They may well reflect accidents of the history of linguistics (and linguistics textbooks), rather than something real about languages.

6. Defining the canonical clitic

In my own work (e.g., Haspelmath 2010, 2012) I have advanced an approach in which comparative concepts for cross-linguistic comparison are defined in the classical way using a set of jointly necessary and sufficient criteria. But one could alternatively prefer a more flexible approach. Many linguists have argued that linguistic categorization works by prototypes rather than with clear-cut categories (see Taylor 1989), and one might wish to extend something like this to typological categories as well. In this section, I will briefly discuss one such approach.

An idea that a number of linguists have been pursuing recently, following Greville Corbett's proposals, is that grammatical concepts should be defined in a "canonical" way (e.g., Corbett 2005; Brown, Chumakina, and Corbett 2013). In this approach, a linguist defines a canonical ideal, e.g., canonical agreement, canonical suppletion or canonical reflexivization. But this need not be the most common or prototypical instance. The canonical definition instead comprises the clearest and least controversial cases of a phenomenon. Thus, the clearest inflectional paradigm has no syncretism, allomorphy, suppletion, defectiveness or periphrasis, but such ideal cases may not be common.

Following this approach to defining grammatical concepts for typology, Spencer and Luís (2013) discuss the possibility of formulating a canonical definition of clitics, and they end up with (18).

(18) The canonical clitic

"has the canonical form properties of an affix [monomoraic CV syllable, prosodically dependent] and the canonical distributional properties of a function word [phrasal placement, wide scope over coordination]." (Spencer and Luís 2013: 149)

This means that elements like the English definite article (cf. [*the [girls and boys]*]) and the complementizer *that* (cf. [*that [I know it and you don't]*]) are canonical clitics, whereas the great majority of clitic phenomena that have been discussed in the relevant literature (e.g., Spencer and Luís 2012) are not canonical clitics. This is perhaps not a problem for the canonical approach, which wants to define an ideal.

But problems do arise for the cross-linguistic application of the definition in (18), because the criterion of wide scope over coordination makes crucial use of the concept of coordination, which in many languages is not a prominent feature and has been found absent in quite a few languages (cf. Haspelmath (2007b) for

some discussion).¹¹ And according to (18), some elements which nobody would call a clitic would end up as canonical clitics. For example, the Japanese past-tense suffix *-ta* has the canonical form properties of an affix (it is monomoraic and prosodically dependent on the preceding verb), it is placed at the end of the verb phrase (because the verb is always phrase-final), and it has wide scope over coordination, as seen in (19) (Fukushima 1999: 297).

- (19) *Taroo-ga uta-i (sosite) Hanako-ga odot-ta.*
 Taro-NOM sing-CONJ and Hanako-NOM dance-PST
 ‘Taro sang and Hanako danced.’

So it is not clear that (18) even singles out the least controversial cases of clitics.

Be that as it may, the canonical approach is rather different from what I had in mind when I proposed that comparative concepts for typology should be kept apart from descriptive categories (Haspelmath 2010). While Brown and Chumakina (2013) argue that this is an important distinction,¹² and thus take a view that is clearly distinct from the restrictivist approach that equates descriptive categories and comparative concepts, the definitions of canonical typology are unlike the comparative concepts that I had in mind, in two ways.

First, the canonical definitions do not allow us to make general statements or to carry out large-scale cross-linguistic studies, as exemplified by the *World Atlas of Language Structures* (Haspelmath et al. 2005). For example, we cannot make a world map contrasting languages with clitic subject indexes and languages with affixal subject indexes (cf. Dryer 2005). The great majority of elements will be noncanonical both as affixes and as clitics. This is because canonical definitions are not designed to put languages into types. Perhaps we can make general statements such as “all canonical clitics have property X,” but since there are so few canonical clitics, this would not be a very interesting statement. But this is not a failure of the approach, because it was not designed for the purpose of typological classification or generalization. Rather it was designed to “build theoretical spaces of possibilities,” to be able to “investigate how this space is built with real instances” (Corbett 2013: 48).

¹¹ Since there are many unclear cases of coordination, one wonders whether coordination should be understood in a canonical sense here, i.e., whether the canonical definition is recursively canonical.

¹² “Canonical Typology addresses the issue of how cross-linguistic concepts can be accurately related to specific categories in a given language (the relationship between comparative concepts and descriptive concepts as they are called in Haspelmath (2010) and references there).” (Brown and Chumakina 2013: 3)

The second way in which the canonical approach seems to differ from my non-aprioristic approach, at least in practice so far, is that it seems to take traditional concepts such as suppletion, inflection, agreement, and clitic for granted. Spencer and Luís (2013) merely ask the question how one could define entities in the canonical way, but they do not ask whether any general definitions of the notions of word, clitic and affix are useful. This could be due to a tacit assumption that what was handed down to us by our forebears (from Trypho to Nida and Zwicky) must be something that is real. But this need not be the case. We could have inherited a three-way distinction as in Jukes's analysis of Makassarese, or some other set of concepts that became entrenched through tradition. So while I emphasize in Haspelmath (2010) that comparative concepts must be useful for formulating cross-linguistic generalizations (or at least for stating salient typological differences), it is not quite clear what constrains canonical concepts other than the tradition of inherited grammatical terms.

7. Generalizations concerning small elements of grammar: Towards a non-aprioristic approach

After all the critical comments of the previous sections, I will now sketch an approach to clitic phenomena ("small elements" of grammars) that is very different from the restrictivist, categorial universalist view exemplified by Zwicky (1985) in that it follows the non-aprioristic orientation and looks for general properties of human language via cross-linguistic generalizations of broad scope. The general expectation of this orientation is that constraints on language structure mostly come from general social and cognitive conditions on language use, not from innate categories or architectures. Thus, the latter are not presupposed by the approach. A final goal is to explain the observed cross-linguistic generalizations, and thus to contribute to answering the question of why languages are the way they are (cf. Dryer 2006).

Thus, we would need to find generalizations via cross-linguistic surveys of clitic phenomena (in the Greenbergian fashion; cf. Greenberg (1963) and subsequent work in this tradition). This has hardly been done, and it will not be done in this paper, whose purpose is metatheoretical and programmatic. But we will see some toy examples of possible generalizations, based on my impressionistic assessment of what occurs and does not occur in the world's languages.

The first prerequisite for a cross-linguistic survey of small elements of grammars is a strictly defined comparative concept that allows us to identify them in any language, and in a next step, we will look for properties that they all share

(“All clitics are...,” or “If X is a clitic, it has...”). The term *clitic* might well be used as a basis for such a comparative concept. Many comparative concepts that typologists are using successfully (e.g., *ergative*, *relative clause*, *perfective*) started out as descriptive categories used for particular languages. They were then extended to other languages, but most or all elements called “ergative,” “perfective,” etc. shared certain core properties, so that it is possible to formulate a comparative concept of ergative (etc.) (Haspelmath 2010). Other terms such as “infinitive” or “weak declension” either were hardly extended to other languages, or the properties of the items called “infinitive” (etc.) across languages are so diverse that one cannot define a comparative concept. Unfortunately, we have seen that “clitic” is in the category of “infinitive,” not in the category of “ergative.” As I documented in Haspelmath (2011a), attempts to delimit words from affixes as comparative concepts were not successful. We saw above that Zwicky and Pullum (1983), the best-known paper on the affix-clitic distinction, was not even intended as a list of defining criteria for comparative concepts of affixes or clitics, but just as a set of diagnostic tests that might be relevant for assigning elements to the cross-linguistic categories of affix and clitics.

Thus, we have to start from scratch. I suggest that it will be useful to have the two concepts in (20a) and (20b) as comparative concepts for cross-linguistic comparison.

- (20) a. **plenimorph** = a morph that denotes a thing, a process or a property (= a **root**)¹³
 b. **minimorph** = a morph with a meaning that is normally omitted in translation into some other language without significant loss of content (i.e., a meaning that could easily be inferred from context, or a meaning that makes a small, subtle contribution)

All languages would seem to have elements that are not clearly plenimorphs or minimorphs (e.g., *every*, *here*, *ouch*), but this is not a problem as there is no need for comprehensive coverage. We need concepts for sets of elements that we can generalize over, and while we can generalize over plenimorphs and minimorphs, I know of no generalizations over the elements that fall in between these categories.

Many languages lack articles, case forms such as accusative or genitive, bound person markers, tense forms such as future or past, complementizers, and coordinators like ‘and’, and when translating texts from languages with such elements they do not insert anything else. Thus, all these elements are

¹³ I defined the term *root* in this way in Haspelmath (2012). Thus, the term *plenimorph* is not really necessary. However, in the context of *minimorphs*, it is useful to have a similar term that is in a way the opposite.

minimorphs. In other words, minimorphs are elements that are not essential, because their meanings could be inferred from context or are treated as marginal to the message to be conveyed. By contrast, elements such as numerals, demonstratives, and interrogative pronouns, which are also often treated as grammatical elements, are not minimorphs, because they cannot be inferred from context and must have counterparts in translations. That minimorphs are defined with respect to translation into other languages is not ideal, but it is the most practical solution. Minimorphs are essentially what Bybee, Perkins, and Pagliuca (1994) refer to as *grammatical morphemes* (or *grams*), but these elements are not clearly defined. It is true that Boye and Harder (2012) have recently proposed a very elaborate account of how one could define “grammatical morpheme” (in terms of ancillary discourse function), but a lot more needs to be said before this can be applied concretely.

On the basis of these two comparative concepts, we can now formulate universal generalizations, such as the following:

Universal I: In all languages, plenimorphs are longer on average than minimorphs. (In fact, almost all languages have many plenimorphs that are bisyllabic or longer, and all have many minimorphs that are monosyllabic or shorter.)

Universal II: In all languages, plenimorphs show greater ordering variability than minimorphs. (In fact, all languages have some minimorphs whose ordering is strictly fixed with respect to a related plenimorph.)

Universal III: In all languages, the coalescence properties of minimorphs (prosodic dependency, adjacency, narrow scope, shape idiosyncrasy) correlate strongly with each other: If a morph is more coalescent than another morph with respect to one of the properties, it also tends to be more coalescent with respect to the other properties.

Universals I and II are somewhat trivial, but it is still worth stating them, because from a logical point of view, things could easily be different. These are highly interesting general properties of human languages which we will need to explain, and which are rarely discussed by theoretical linguists. They do not concern only clitic phenomena, but all kinds of bound elements, including “typical affixes.”

Finally, Universal III goes to the heart of the issue of clitics. While I have great confidence in the truth of Universals I and II, even in the absence of systematic evidence and just based on my impressionistic observations, I have no

clear intuitions whether Universal III is true.¹⁴ However, linguists have generally assumed implicitly (or explicitly) that it is true.

Let us consider a few examples of correlations between coalescence properties. In Hungarian case suffixes and postpositions, which otherwise behave very similarly (Creissels 2006; Trommer 2008), we see a correlation between prosodic dependency (with respect to vowel harmony) and narrow scope over coordination: only case suffixes like *ben/ban* exhibit vowel harmony (cf. (21a) and (21b) vs. (21c) with the non-harmonizing postposition *mellett*), and only case suffixes do not allow wide scope over coordination (cf. (22a) and (22b) vs. (22c) and (22d)).

- (21) a. *a ház **ban*** (Hungarian)
 the house in
 'in the house'
- b. *a kéz **ben***
 the hand in
 'in the hand'
- c. *a ház **mellett***
 the house beside
 'beside the house'
 (cf. É. Kiss 2002: 184)
- (22) a. *a ház **ban** és a garázs **ban***
 the house in and the garage in
 'in the house and in the garage'
- b. **a ház \emptyset és a garázs **ban***
 'in the house and the garage'
- c. *a ház **mellett** és a garázs **mellett***
 the house beside and the garage beside
 'beside the house and beside the garage'
- d. *a ház \emptyset és a garázs **mellett***
 'beside the house and the garage'
 (cf. É. Kiss 2002: 184)

A correlation between strict adjacency (cf. (23a) vs. (23b)) and narrow scope (cf. (24a) vs. (24b)) is found in German infinitival *zu* (which thus contrasts in two ways with its English counterpart *to*):

14 If it is true, then the idea of somehow distinguishing between highly coalescent minimorphs ("affixes") and weakly coalescent minimorphs ("clitics") begins to make sense after all. One could perhaps say that a minimorph is an affix if it is coalescent on more than 80% of the coalescence properties, and clearly a clitic if it is coalescent on 30–50% of the properties, even though this would of course be very non-traditional. However, dividing the phenomena into just two classes would still be arbitrary (cf. section 4).

- (23) a. *gründlich zu erklären* (German)
 thoroughly to explain
 'to thoroughly explain'
 b. **zu gründlich erklären*
 'to thoroughly explain'
- (24) a. *zu kommen und zu helfen*
 to come and to help
 'to come and help'
 b. **zu kommen und Ø helfen*
 'to come and help'

A third example is the correlation between shape idiosyncrasy and prosodic dependency in person forms in modern Greek. Subject indexes, illustrated in (25), are prosodically dependent in that they are part of the stress domain of verbs, and they show some shape idiosyncrasy (especially in the first and third person singular). By contrast, object indexes (in (26)) are prosodically less dependent (though they do not have stress of their own and actually modify the stress of the host), and they also show no shape idiosyncrasy.

- (25) subject indexes (Modern Greek)
- | | present tense | past tense | 'speak' |
|-----|-----------------|-------------------|---------|
| 1SG | <i>milá-o</i> | <i>mil-ús-a</i> | |
| 2SG | <i>milá-s</i> | <i>mil-ús-es</i> | |
| 3SG | <i>milá(-i)</i> | <i>mil-ús-e</i> | |
| 1PL | <i>milá-me</i> | <i>mil-úsa-me</i> | |
| 2PL | <i>milá-te</i> | <i>mil-úsa-te</i> | |
| 3PL | <i>milá-ne</i> | <i>mil-úsa-n</i> | |
- (26) object indexes
- | | | |
|-----|----------------------|--------------------|
| 1SG | <i>vlépondás me</i> | 'seeing me' |
| 2SG | <i>vlépondás te</i> | 'seeing you', etc. |
| 3SG | <i>vlépondás ton</i> | |
| 1PL | <i>vlépondás mas</i> | |
| 2PL | <i>vlépondás sas</i> | |
| 3PL | <i>vlépondás tus</i> | |

Universal III does not claim that the correlation is perfect, only that all languages show some evidence of the general correlation. An exception in Polish that was highlighted by Aguado and Dogil (1989) is shown in (27). Here we see that the subject index with past-tense verbs is not strictly adjacent (it can occur on a preceding word as in (27b) and (27c)), but nevertheless, when it occurs on the verb, it causes shape idiosyncrasy (cf. the vowel alternation between *pomogł* [pomogw] and *pomógł* [pomukł]).

- (27) a. *Kiedy Janek-owi pomógł-em ...* (Polish)
 when Janek-DAT helped-1SG
 b. *Kiedy Janek-owi-m pomógł ...*
 when Janek-DAT-1SG helped
 c. *Kiedy-m Janek-owi pomógł ...*
 when-1SG Janek-DAT helped
 ‘When I helped Janek ...’
 (Aguado and Dogil 1989: 191)

The Polish example shows that the correlation between the various coalescence properties is not perfect, but a single example that goes against the trend does not invalidate the trend; statistical universals are just as interesting as absolute universals (or more interesting; see Dryer (1997b)), and need to be explained. So Universal III may well turn out to be true. This is an interesting empirical question that is well worth investigating.¹⁵

The next step is to ask why the universals should hold. Proposed answers to this ambitious question are difficult to evaluate, but we should nevertheless attempt them.

Universal I is the easiest to explain. We can observe that minimorphs tend to be frequent in language use, and frequent elements tend to be shortened (Zipf 1935; Haspelmath 2008b). Alternatively, we can say that minimorphs are short because they express relatively unimportant meanings that can be easily inferred from the context – in fact, so easily that there are languages where they normally lack an overt counterpart. Here I do not try to choose between these two explanations (which may ultimately be mutually compatible).

Universal II, about the ordering of plenimorphs and minimorphs, can perhaps be explained with respect to the way in which the two types of morphs are processed. According to Givón (1989: Ch. 7), they reflect a contrast between “attended processing” and “automated processing,” and Lehmann (1993: §3.4)

15 Thus, I do not share Spencer and Luís’s pessimism, who say with regard to these Polish cases (2012: §9.1):

“The most promising way of establishing a typology of clitics in the classical sense would be to set up an implicational scale of some sort. [...] Cases such as Polish show that there is little chance of finding a set of implicational scales for clitics and affixes. The Polish floating inflections show typically clitic properties of wide scope and (completely) promiscuous attachment. On the other hand, unlike special clitics in most languages they don’t really show any special syntactic behaviour. [...] However, they trigger idiosyncratic allomorphy on their hosts in the manner of a typical, highly morphologized affix. Indeed, they are more affix-like in this regard than are the regular English verb suffixes *-s*, *-ed*, *-ing*.”

As noted in the main text, if there are exceptions to an implicational hierarchy, this does not invalidate the hierarchy.

draws a similar distinction between “conscious” and “unconscious” processing. Plenimorphs are more subject to the free manipulation by the speaker, while minimorphs are processed automatically, and hence must generally be processed along with a host element which they are semantically related to.

Finally, I would think that the tendency for minimorphs to show coalescence on several coalescence dimensions simultaneously (Universal III) derives from the fact that they all relate to frequency of cooccurrence (Krug 2003; Bybee 2002): if an item usually cooccurs with some specific other item, it tends to form a processing unit together with it (cf. Bybee (2010) on the psychological process of chunking): The minimorph tends to become obligatory (require narrow scope in coordination), it tends to be prosodically dependent on the host, it tends to develop shape idiosyncrasies, and it tends to be very limited in its ordering possibilities.

These explanations are very tentative and are primarily intended to illustrate the kind of approach that one would take in a non-aprioristic study. By contrast, a restrictivist might propose a universal such as the hypothetical “Universal IV” (invented by me for the sake of illustration):

“Universal IV” (hypothetical): In all languages with clitic movement, it takes place in the syntax, not in the phonology.

This would also be a universal claim, but it has a very different character from Universals I–III. It is not clear that it is an empirical claim, because it is unclear what kind of empirical data might falsify it. It depends on the concepts “clitic,” “movement,” “syntax,” and “phonology,” which are cross-linguistic categories that are not defined and not definable, but can only be diagnosed on the basis of the researchers’ intuition and experience.

8. Conclusion

This paper has used the empirical domain of clitic phenomena to illustrate the difference between the restrictivist (generative) approach to morphosyntax and the non-aprioristic approach. The two approaches make use of taxonomies and categories in very different ways. I have belaboured this point because it seems to me that it is not widely recognized by linguists.

In the non-aprioristic approach, categories are strictly defined by classical necessary and sufficient conditions, and there are two different types of categories: descriptive categories for language-particular analysis, and comparative

concepts for cross-linguistic generalizations.¹⁶ These cross-linguistic generalizations are then explained with reference to more general factors of cognition and language use.

In the restrictivist approach, by contrast, taxonomies, categories and features that are of theoretical interest are assumed to be innate (or at least universally available) cross-linguistic elements that cannot be defined. They can be diagnosed using a range of tests, but there is no clear-cut methodology for assigning phenomena in a given language to these universal abstract entities. As a reviewer observes, my argumentation in principle affects all frameworks that make use of universal categories and features, not only those that assume that these are innately given, and there are some categorial universalist approaches that reject Chomsky's philosophical nativism. I have focused on the explicitly nativist approaches here because only these seem to have a good reason for adopting a restricted ontology of features and categories: an explanatory account of language acquisition and typological generalizations.¹⁷

I have made it clear that I favour the non-aprioristic approach over the restrictivist approach, but progress would already be made if it were recognized more widely that the two approaches work in very different ways.

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¹⁶ See Klockmann (this volume), who on the basis of her analysis of numeral categories in Polish also arrives at the conclusion that in many cases it is not possible to find a set of linguistic tests that will clearly identify a particular linguistic category cross-linguistically. She shows that the different numeral categories identified in her paper for Polish “are wholly language-dependent, and not expected to appear in an identical way in other languages.” She points out, however, that “while the categories themselves are not universal, the building blocks (phi-features, specifiers) may very well be, with languages varying in their manipulation of these primitives.”

¹⁷ HPSG linguists may for convenience work with a limited set of recurring features and categories, but if they do not assume that these are innate, then they cannot argue that they have to be restricted or that the restrictedness of existing categories explains language acquisition or helps explain typological distributions. And in fact, typically HPSG researchers refrain from such claims (Ivan Sag, p.c., 2005), and when they make them *en passant*, one senses a certain incoherence of assumptions. Thus, while HPSG seems to be categorial universalist in its more superficial practice, it is not truly restrictivist at a deeper level.

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