

# Beware of the emperor's conceptual clothes: General linguistics must not be based on shaky dichotomies

## 1 Introduction

In the field of contemporary typology and general linguistics I consider Martin Haspelmath (henceforth H) as one of the most knowledgeable, interesting, and productive scholars in Germany. And this holds not only because I share many tenets with him, but also because in most cases of disagreement his position seems to me transparent, comprehensible, and well-founded such that attacking it requires careful argumentation. Therefore I happily accepted the invitation to comment on one of his most recent papers, the target paper of this issue, all the more because the idea of learning more about our nature as humans through one of its essential features, language and its universals, keeps fascinating me since the mid-eighties, especially when supported by a digital general comparative data repository. Unfortunately, in the case of the target article the amount of disagreement or perhaps my lack of properly comprehending too many aspects made it somewhat challenging for me to come up with pertinent and constructive comments on the whole article. Therefore I have decided to focus mostly on one selected aspect and make only cursory remarks on other elements that I either fail to adequately understand or disagree with.

The aspect I have chosen for closer scrutiny is the paramount role given by H (and probably together with him the majority of typologists<sup>2</sup>) to the commensurability problem in general linguistics, the issue of comparability between particular languages and consequently of the relation between particular and general linguistics.

But first I would like to take stock of what we seem to agree on (section 2), then I will review some other problems I find with the article (section 3) before coming to my thoughts about what H calls the 'general linguistics paradox' (section 4), which will take us to the core of my criticism, the problematic dichotomies I see behind his proposal for dealing with the comparability problem (section 5). Next I will briefly comment on H's conclusion (section 6) before closing with my own summary (section 7).

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<sup>2</sup> Cf. Evans (2020: 417): "Issues of comparability lie at the heart of linguistic typology."

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## 2 Presumable areas of agreement

According to my conception of what an empirical science should strive for and in view of my experience in the field I can in general agree with the picture of the current situation of general linguistics outlined by H. At least none of the following pertinent views seem to be at variance with his.

First, general linguistics is an empirical field that is concerned with the phenomenon of human language, an abstract entity that is instantiated by particular languages. Ideally, this should hold not only of languages of the present and the past, but also of those of the future. Human language as such is thus by definition (a) an abstract entity and (b) one that is by one instantiation level more abstract than any particular human language. Particular languages in turn are also abstract objects, but abstract to a lesser degree in that they have observable direct manifestations.

Second, and this follows from their assumed empirical nature, all research strategies aiming at describing and explaining human language can be assigned to one of two idealized categories (cf. p. 23): the primarily bottom-up approaches that starting from particular languages generalize to the overall picture by induction ('By assumption, English and Tagalog are human languages, they share the property  $p$ , hence there is non-zero probability that all human language share property  $p$ ), and the mainly top-down approaches that link general assumptions about human language to properties of particular languages by deduction ('By assumption, any human language has property  $p$ ; German Sign Language is a human language; therefore, must have property  $p$ ').<sup>3</sup> In practice, however, this distinction is neither categorical nor time-independently applicable, it rather forms a gradient on which any promising research process will keep moving up and down according to what is currently considered expedient. Still, there can be a conspicuous difference in weight given to induction and deduction in different research programs.

Third, ever since its birth generative linguistics tended to work closer to the deductive end of this scale and most other linguistic schools closer to the inductive end. Still, the difference between these approaches lies arguably less in the relative weight given to their general assumptions than in their content: classical generative linguistics views language and languages as based on innate factors that are specific for the former and that allow for acquisition of the latter (and hence in a sense of both), whereas usage-based approaches see languages as dynamic systems "that are constantly restructured and reorganized under the pressure of domain-general cognitive processes" (Diessel 2017: 1). The latter assumption is compatible with the hypothesis that language-specific cognitive processes do not exist, which it is incompatible with the former.

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<sup>3</sup> Note that there is a principled difference in the kind of falsifiability: only the deductive approach affords hard predictions and thus clearly defined counterexamples; with induction-based findings only the claimed degree of probability can be questioned.

This is of course only a very rough outline of the field and its recent history, but it does not seem to imply any fundamental difference between H's view and mine. Regarding our respective conceptions of the philosophy of science that informs linguistics and related fields the situation seems to be quite different.

### **3 Points of disagreement**

#### **3.1 Irritating terminology and problematic distinctions**

Three of the reasons why I doubt that I understand H's paper correctly occur already in its title and abstract. The first one is to do with irritating terminology: the title addresses the universals that should *serve as the basis* of general linguistics whereas the abstract says that in order to make general claims one has to *study* universals (emphasis mine). What kind of universals does H have in mind, those that underlie (are the basis of) general linguistics or those that grounded on such a basis can be studied, i.e. stated and examined? It cannot possibly be both at the same time: if we understand the former as definitional universals, i.e. properties shared by all language by definition and the latter as empirical universals, the non-definitional features that cluster around the definitional properties (Zaehler & Poeppel 2010: 160) it is obvious that the former cannot be empirically investigated.

Given this and in the light of what follows in the body of the paper I assume that H means the latter kind of universals and that accordingly one should read the postulate formulated in the title as something like 'General linguistics must attempt to find and establish empirical universals.' This is somewhat closer to my view of the field, but I would still prefer a more balanced postulate like the following: 'One of the major goals of general linguistics should be the establishment of linguistic universals.' The reason for this preference is that for a complete picture of human language we have to specify not only the common denominator of particular languages, the set of universals, but with equal weight also the spread or distribution of them across the different dimensions of the typological space and the clusters in some of its regions.

Second, the title suggests that there are two options regarding the choice of a basis for (or rather an object of study of) general linguistics, universals or nonconventional aspects of language. The abstract illustrates the latter concept with reaction times in psycholinguistics. Here I fail to see two things: first, why should linguists make such a choice instead of combining conventional and nonconventional aspects of language, replacing thus the *or* by *and*, and second, why should there be a principled difference between reaction time data in a psycholinguistic experiment, say a lexical decision test, and other elicitation data, say those obtained from a native speaker as an acceptability judgment. Further on in the text H mentions other kinds of nonconventional aspects that are really different from reaction time data, but my lack of understanding the way he characterizes these aspects of language remains.

Third, and still in the abstract H introduces two types of universals according to what they are based on, namely grammatical descriptions in a Greenbergian and a Chomskian tradition, respectively. This leads to the question of how relevant the basis of a universal is for its identity: once a universal hypothesis is stated, to what degree can its origin possibly matter for its investigation? In order to establish that the category systems of all languages include a distinction between nominal and verbal forms, for instance, I cannot think of a single aspect that relies on classifying this universal as belonging to the first of H's types or the second.

Perhaps this classification of universals is motivated by the terminology in which they are stated: comparing the universal claims (a) and (b) below it is not hard to guess which one belongs to which of H's types.

- (a) When any or all of the items (demonstrative, numeral, and descriptive adjective) precede the noun, they are always found in that order. If they follow, the order is either the same or its exact opposite.
- (b) [Dem ... [Num ... [A ... [N]]]] is the (universal) structure of the DP resulting from Merge.

(a) is Greenberg's Universal 20, which has spawned a lot of research since its publication (Greenberg 1963: 52), among many others the paper statement (b) is quoted from (Cinque 2005: 327). But since both aim at capturing the same regularities, I think it is misleading to assign them to different kinds of universals. What can be observed, though, is that (b) is more refined and ambitious, but also more theory-laden.

Of course, there are other and likely more useful distinctions between types of universals, as H is well aware: on page 2 he mentions absolute universals and strong tendencies and would certainly not object to completing the picture by adding implicational universals, obtaining thus four categories. Here are some less well-known examples:

1. Absolute and General: *Person-Number Universal*.  
All languages must at least incorporate a three-way person distinction and a two-way number distinction in their argument DPs. (Tvica 2010: 33)
2. Absolute and Restricted: *Prevoicing and direction of voice assimilation*.  
Languages with prevoicing always have only regressive voice assimilation. (van Rooy & Wissing 2001, quoted after Ringen & Helgason 2004: 53)
3. Statistical and General: *Talmy's Law*.  
Natural human languages tend to co-encode with the concept of motion exactly one additional factor of motion. (Zaeffferer 2002: 33)
4. Statistical and Restricted: *Chinese Weather Event Coding*.  
In Sinitic languages weather events with bigger weather substances and faster weather processes tend to select action verbs with high transitivity. (Huang 2021:

1)

I leave it as an exercise for the reader to find out which one belongs to which of H's types of universals, those based on Greenbergian as opposed to Chomskian descriptions.

As a last and lesser remark on the introductory section where H emphasizes the arbitrariness of linguistic forms I would like to add a point that I deem nonnegligible: investigations into the extent of non-arbitrariness (e.g. Johannson et al. 2020) have developed into a significant part of general linguistics.

### 3.2 Different views of theoretical linguistics

Regarding the concept of theoretical linguistics (section 2) there seems to be a major dissent between our views. Although I agree with H's definition of theoretical (as opposed to applied) linguistics as primarily aiming at understanding or explaining its object of study, his concept of explanation is rather different from mine. His claim that a descriptive grammar or a dictionary "explains the behaviour of speakers [...] and is thus no less explanatory than a general theoretical account of Human Language" (p. 4) is in my view gravely mistaken. I can understand it only as buying into the annoying generative custom of calling syntactic analyses explanations.<sup>4</sup> By definition, a descriptive grammar *describes* regularities followed by speakers. In order to really *explain* these regularities (and not just to provide an elegant description that captures generalizations) one has to go back to its causes.

My view of explanation in theoretical linguistics is stated in (Zaefferer forthcoming) under the header 'Predictive language theories and their moving target' as follows:

Arguably, one of the most fundamental questions in linguistic theorizing is this: 'Why are human languages the way they are?' The correct answer is of course easy to find: 'Because they became to be that way.' But this answer only leads to the next question: 'How did this happen? What are the forces that determined their prehistory and history up to their present stages?'

From this metatheoretical viewpoint H's proposal of a threefold classification of linguistic research (p. 3) seems unsatisfactory and incomplete. Instead I propose the following structure:

- A Particular linguistics
  - (i) DESCRIPTION of particular languages
    - a. Isolated
    - b. Comparative (contrastive)
  - (ii) EXPLANATION of properties of particular languages

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4 Here is an illustration of what I mean: "The analysis proposed here explains the possibility of string-vacuous QM" (Svenonius 2000: 276)

- B General linguistics
  - (i) DESCRIPTION of sets of languages
    - a. Enumerative
    - b. Comparative (typological)
  - (ii) EXPLANATION of properties of sets of languages

Whereas H's trichotomy considers only A(i)a., B(i)b., and B(ii), my proposal provides space also for contrastive linguistics and distinguishes between enumerative and comparative descriptions of multiple languages. Furthermore it adds explanatory studies in the history of particular languages, trying to answer questions like 'Why is Riau Indonesian the way it is?'

In the light of these differences it should be clear that I cannot agree with H's claim that p-linguistics is no less theoretical than g-linguistics (p. 23). Human language in the generic sense is by one level of instantiation more abstract than any of its instances, and in that sense g-linguistics is more theoretical than p-linguistics. But the most relevant distinction in both p- and g-linguistics is between description and explanation: whereas the former can be theory-laden to a lower or higher degree, only the latter is purely theoretical.

## **4 Haspelmath's general linguistics paradox**

### **4.1 Normal science or: What's so paradox about this paradox?**

In order to prepare the ground for my thoughts about the question of commensurability between languages to be presented in the following section, some reflections about what I take to be the central idea of the target paper will be helpful, the idea that there is a major problem general linguistics has to come to grips with, a problem that H calls somewhat dramatically the 'general linguistics paradox'. Reading the formulation used to state it on p. 8 at first glance one tends to agree, to even consider it obvious and unobjectionable:

(1) **The general linguistics paradox:**

We want to explore and understand the nature of Human Language, but what we can observe directly is particular languages.

But on closer inspection it turns out that there are several weak spots in it. First, and somewhat pedantically, one can remark that particular languages cannot be observed directly either. In view of the alternative formulation on p. 2 ("... we want to study Human Language, but all we can observe is utterances of particular languages.") it can be safely assumed that this is what is really meant by 'general linguistics paradox' throughout the paper, even though it is a little less carefully worded elsewhere. Thus the first problem can be interpreted away. Second, again in a slightly fussy spirit, one could object that not only utterance tokens can be directly observed, but also reactions of addressees and other events that indicate linguistic dispositions. Again, applying

the principle of charity I will assume that all observable cases of linguistic behavior are meant.

What we are left with is an uneasy feeling about two further aspects, the use of the term 'paradox' and the somewhat unhelpful restriction of the problem to general linguistics. Beginning with the latter one can see this restriction as completely justified because the paper is about linguistics, but it might still be helpful to widen the scope to other disciplines, a view H obviously shares (cf. section 6.2). As a first step we can apply a substitution test and we get the following result:

(2) **The general x-ology paradox:**

We want to explore and understand the nature of  $x$ , but what we can observe directly is particular instances of  $x$ .

This makes perfectly sense for every scientific investigation of abstract entities with observable manifestations. Given that H and other typologists persistently emphasize the uniqueness of each human language and that the prototype of a unique entity (at least in the Western cultures) is the human person we can make a second step and instantiate the variable  $x$  in the schema above by 'human being'.

(3) **The general anthropology paradox:**

We want to explore and understand the nature of human beings, but what we can observe directly is particular instances of human beings.

Via a third step, modifying two frequently quoted claims we get back to general and particular linguistics:

Despite any similarities by which languages are identified as members of human language, [...] despite any broad or narrow regularities which are involved in any linguistic events, a primal uniqueness always remains, through which every language is a world of its own with regard to other languages.<sup>5</sup>

Every language is in certain respects: a) like all other languages, b) like some other languages, c) like no other language.<sup>6</sup>

H says it somewhat more concisely: "While language structures are often similar, each language is structurally unique [...]" (p. 19) and concludes "These differences mean that language systems are incommensurable, so that making them comparable requires extra effort." (p. 20) And with extra effort H means the development of special concepts he calls comparative and that are strictly separated from descriptive concepts. But if he is right, how could it be that, according to my knowledge, psycholo-

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<sup>5</sup> Famed psychologist William Stern's words are: "Despite any similarities by which persons are identified as members of humankind, a particular race or gender, etc., despite any broad or narrow regularities which are involved in any personal events, a primal uniqueness always remains, through which every person is a world of its own with regard to other persons." (Stern 1923: 7)

<sup>6</sup> Originally: "Every man is in certain respects: a) like all other men, b) like some other men, c) like no other man." (Kluckhohn & Murray 1948: 53; joint work of an anthropologist and a psychologist)

gists never came up with the idea of an analogous effort required to make persons comparable?<sup>7</sup> Of course, to this H could answer 'All the worse for psychology' and similarly for all other disciplines that fail to make such an effort although they fit the schema (2), but this move would assign general linguistics a vanguard role that takes it ahead of all comparable disciplines. I doubt this to be the case, but I admit that my qualms do not prove it false.

The other aspect of H's claim repeated as (1) above that made me feel uneasy is the use of the term 'paradox'. I would concede that the step from, say, Polish to human language can be considered problematic, especially if you are a philosopher, but on the other hand it instantiates a relation that being so ubiquitous and mundane it most of the time escapes attention: the relation of a token and a type, or more precisely between a particular case and a general concept, a relation called instantiation in one direction and generalization in the other. And I fail to see anything paradoxical in the fact that humans all the time move back and forth between the particular and the general via generalization and instantiation.

But as so often what goes without saying in everyday life turns out to be problematic if looked at from a philosophical perspective. So I understand (1) as specializing to general linguistics a problem that all empirical disciplines must come to grips with: the problem of induction and its close relative, the problem of universals.

This is of course not the place for an even minimal review of the ways these problems are discussed in the contemporary philosophy of science. Instead I would like to add some remarks on an often-quoted exclamation about methodology Darwin made in a letter to a friend:

About 30 years ago there was much talk that Geologists ought only to observe & not theorise; & I well remember some one saying, that at this rate a man might as well go into a gravel-pit & count the pebbles & describe their colours. How odd it is that every one should not see that all observation must be for or against some view, if it is to be of any service.— (Darwin 1861: Letter to Henry Fawcett)

The topic Darwin addresses here is the art of scientific discovery, i.e. the interplay of observation, emergence of an educated guess, formation of a hypothesis (or view in his terms), testing it, and building a theory, an art he mastered in such an impressive and exemplary manner.

It is of course theoretically feasible to restrict oneself to mere observation and to collect data without theorizing or looking for anything, but this could go on for centuries without generating any interesting insights. Likewise in linguistics the researcher could content herself with data generation and leave the work of theory building to the

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<sup>7</sup> Interestingly, contemporary psychologists (at least some of them) are well aware of the uniqueness of persons and discuss it under the label person-oriented approach, cf. e.g. Bergman & Trost 2006.



data mining engineer.<sup>8</sup> But that does not obviate the need for linguistic theory, as stated in the following quote from the data mining literature: "Without a hypothesis based on sound theory, it is far more likely that one will find relationships because of [...] chance occurrences [...] So, is discovery-driven data mining voodoo science? In a way it is." (Colonna 2013: 341)

On the other hand, if observation only leads to progress if it either supports or subverts a given view or hypothesis, the speed of scientific progress depends also on the quality of the views that are checked against the data coming from the observations. After all, if a scientific community would review not only the most plausible but all conceivable hypotheses with regard to the fit between what they deductively entail and the data,<sup>9</sup> such a strategy could also take aeons.

But as mentioned above any success-prone research strategy will constantly move up and down the scale of more inductive or more deductive methods according to the actual needs. So what H calls paradox is nothing else than what day-to-day normal science does all the time: generalizing from observation-based data about specific objects of research to corresponding generic concepts in order to gain insights into the nature of what these concepts are to capture.

## 4.2 Three solutions or a triad of strategies?

In order to make progress in practicing normal linguistic science (or in his terms to solve the general linguistics paradox) H describes three research strategies: (a) studying nonconventional constraints on human language, (b) doing comparative investigations of the world's languages and (c) searching for the building blocks, seen as natural kinds, all languages are based on. The way he presents these strategies suggests that he sees (b) and (c) as competitors, and since the latter "has been basically abandoned by many linguists as an explicit goal" (p.13) the reader may be led to conclude that (b) is about to win simply by survival.

Be this as it may, I see no reason to regard (a) to (c) either as disjoint or as jointly exhaustive. It may seem plausible to see (b) and (c) as incompatible in view of the deplorable custom of mutually ignoring work in the other tradition cultivated in both schools, but obviously this compatibility problem belongs to the sociology of science and not to the logic of linguistic research itself. Moreover, there are fortunately also linguists like H who follow what is going on in both traditions. And from an abstract point of view there is no principled incompatibility between the more inductive strategy (b) and the more deductive program (c), as mentioned above. For instance Wilt-schko's (2014) generative proposal of a formal typology provides for language-

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<sup>8</sup> "An advantage of discovery-driven data mining is that an analyst does not need to have any preconceived assumptions about the dataset" (Colonna 2013: 340)

<sup>9</sup> This is the idea of abduction in the sense of inference to the best of a set of competing explanations.

specific categories.

The next section will review what I consider the most problematic aspect of H's paper. It relates to the philosophy of science of linguistics and has inspired the title of this review.

## **5 From language monads via procrustophobia to an untenable dichotomy, or: How to give incommensurate weight to a purported incommensurability**

### **5.1 Human uniqueness and language monads**

There is no doubt that the fine structure of a particular language as a whole is something unique, just like the whole of a person (or of any other sufficiently complex entity in some historical period of time when viewed in sufficient detail). In this section I will argue that this fact entails neither incommensurability nor incomparability. On the contrary, I see no way of proving uniqueness than either by stipulation or by comparison. I submit furthermore that the very ontological status of both human languages and persons requires comparison in one way or the other.

I start by pointing out that the relation between human languages and persons regarding their uniqueness and incommensurability is more than a convenient analogy. In the preceding section I have tried to interpret away the lack of precision in H's statement that 'what we can observe directly is particular languages' by assuming that he uses here 'particular languages' as shorthand for 'instances of linguistic behavior belonging to particular languages', because as abstract entities the latter cannot be directly observed either. And it turns out that this specification, pedantic as it might seem, leads to an important observation: instances of linguistic behavior of a person are always instances of this person's idiolect and thus the concepts of human language and human person are intrinsically interwoven in any kind of inference that links the observable phenomena to the abstract concepts.

Let us understand by idiolect the entirety of linguistic dispositions a person possesses at a given period of time. This includes merely passive dispositions in a prelinguistic infant as well as multiple language dispositions in a multi-lingual person. Therefore, and fieldworkers are well aware of this, inferences from observable manifestations of a native speaker's idiolect to a particular language are far more problematic than the step from a particular language to language per se. In some sense the latter step is trivial since the fieldworker already takes it for granted that the manifestations of linguistic dispositions she observes are instances of a language and thus also of language in the generic sense.

Interestingly, the development of both a human person and her idiolect are essentially

social phenomena in that they require another person and another idiolect. If for instance a leading German psychologist claims that "subjectivity and selfhood are initially perceived as key features of other minds before they then become imported from other minds to own minds" (Prinz 2017: 347), a view known as import theory by contrast with export theories that argue for the opposite, then a transfer to the development of language makes perfect sense (by contrast with the export view): language and speech are initially perceived as key features of other minds before they then become imported from other minds to own minds.

In his recent paper on uniqueness and comparison H admits the great value of comparison for description while denying its theoretical necessity (Haspelmath 2020: 362). In support of this view he makes two claims: (a) language systems are structurally unique and (b) the ancient Greek and ancient Indian grammarians described their languages without any comparative knowledge.<sup>10</sup> As said at the beginning of this section I have no problems with (a), but I strongly reject (b): even under the (utterly unrealistic) assumption of a completely homogeneous variation-free monolingual community, the very fact that idiolects gradually develop over years both requires and offers comparative knowledge in the community: earlier and later stages of language development afford language comparison in every society, since children's utterances are not just a subset of what adults say.

The observation that some linguists regard uniqueness and incommensurability as assets is not new:

As is well known among typologists, there are what one might call familiarizers, linguists who try to show that all languages are basically the same and therefore tend to play down the differences, and there are exoticizers, linguists who try to prove that the language they are working on is incommensurable with all others and who therefore tend to play down the commonalities. Both attitudes are of course exaggerations [...] <sup>11</sup>

When I wrote this (Zaefferer 2007: 195) I did not know that only a short time later H would start to outdistance the exoticizers as far as possible by pushing the incommensurability idea to its extreme. He did so by postulating a strict separation between language-specific and cross-linguistically valid categories, a postulate he kept emphasizing again and again<sup>12</sup> and that reappears in the target paper as the claim that comparative concepts must be different from descriptive categories because 'each language is structurally unique' and therefore 'language systems are incommensurable' (section

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<sup>10</sup> If recognizing systematic variability requires comparative knowledge Pānini falsifies this claim (cf. Kiparsky 1979).

<sup>11</sup> A real-life anecdote illustrates this nicely: a new student once entered my office and wanted me to supervise his PhD thesis. In order to get me interested he said: "I want to work on a language that is completely different from any other you may know. An entirely new set of terms and categories will be necessary to describe it." It turned out that what he meant was Japanese.

<sup>12</sup> See Haspelmath 2010a, 2010b, 2010c, 2011b, 2014, 2016, 2018, among others.

6.3). In a way this suggests that all languages are equally exotic, blurring thus the difference between frequently and rarely occurring patterns. More importantly, making such a strict separation viable presupposes a strongly idealized concept of what particular languages are, a concept that I propose to call language monads in reminiscence of Leibniz's monads, "mind-like entities that do not, strictly speaking, exist in space but that represent the universe from a unique perspective."<sup>13</sup> (Look 2020: 35)

As it turns out, however, this idealized concept of particular languages and the following well-supported statistical universal are strange bedfellows: it is very likely that most currently existing idiolects on this planet are multilingual at least in the weak sense of housing both a standard version and a dialect.<sup>14</sup> So I agree with Höder's observation that every approach that starts from a monadic concept of particular languages is far from being framework-free: "The idea that linguistic knowledge is organised in terms of 'languages' is, in fact, in itself a theoretical framework of sorts: the axiom that what a grammar describes is a 'language'." (Höder 2018: 46)

But the finding that the monadic concept of particular languages is a strong idealization does not mean that it cannot be useful.<sup>15</sup> So let's assume as a working hypothesis that it is at least to a certain extent useful. Then the idea of a cross-linguistic reference grammar organized by a framework for the description of human languages of any type is easier to conceive. Since Wilhelm von Humboldt had already complained about the lack of a general comparative grammar based on 'the firm ground of properly established leading ideas', I have dubbed the idea of filling this gap Humboldt's dream (Zaefferer 2006).

## 5.2 The misrepresentation issue and procrustophobia

The three main problems for Humboldt's dream to come true I have stated there are also addressed in the target paper: (a) comparability, (b) typological bias, and (c) theoretical bias. Regarding (c) H proposes to solve the problem by advocating a framework-free approach, but as mentioned above I am not alone in judging this as wishful thinking. Since H is aware of the tension between admitting that theory-free description is impossible and favoring framework-free descriptions he kindly adds a clarification: "By 'framework-free' I mean that there is no universal descriptive framework for all languages. Of course, one needs general concepts for describing a language [...] and these could be called 'framework' – but there can be a different framework

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<sup>13</sup> Analogous views are also to be found in anthropology: "the idea that meaning is socially constructed [...] is read to imply [...] that human communities are, or should be, semantic monads, nearly windowless." (Geertz 2000: 76)

<sup>14</sup> "The idea that monolingualism is the human norm is a myth." (Thomason, 2001: 31) "We estimate that most of the human language users speak more than one language [...] In quantitative terms, then, monolingualism may be the exception and multilingualism the norm." (Auer&Wei 2008: 1)

<sup>15</sup> In fact there is plenty of evidence that most speakers conceive of their language(s) as something static and categorical instead of dynamic and gradient.

for each language." (Haspelmath 2010a: 14 fn 30)

Unfortunately, this cannot solve the problem for at least four reasons: First, in order to find out if, say, H's admirable description of Lezgian (Haspelmath 2011) is framework-free in his sense or not, the criterion that there can be a different framework for each language is not very helpful, even if one translates framework-free description as arbitrary-framework description; second, although it is obviously possible to use an arbitrarily chosen or even a different framework for each language, this does not entail that such an enterprise is desirable (why could it be?), let alone necessary; third and still under the same interpretation of the term, this would also subsume generative descriptions as framework-free; and fourth, what H himself advocates is not a different framework for each language, but the same Saussurean distribution analysis "based on contrasts, not on substance." (p. 20)

Perhaps what H has in mind is akin in spirit to my observation regarding the relation between description and explanation: strong theoretical assumptions are good for advancing our understanding of languages, but bad as basis for describing them. Whoever ignores runs the risk of becoming an easy prey to what I propose to call the DF:

(DF) Desplanation Fallacy: the assumption that using one's favorite explanatory theory as framework for description gives it an advantage over its competitors.

Although it seems paradoxical, to fall for the DF creates a handicap for any empirical theory because empirical entails falsifiable, and it is impossible to find data that falsify a theory whose assumptions are built into the very description of that data (Zaefferer 2006: 117f.). I think the best way of solving the theoretical bias problem is by avoiding the DF trap.

Whenever we linguists take up the challenge of general comparability, the specter of Procrustes enters the room and scares everybody. I call it a specter not because I fail to see the danger of misdescribing one language by forcing it on the descriptive torture bed made for another one, but because I remember being scared by it myself for a long time. The resulting fear, I call it procrustophobia, is in my judgment quite widespread among typologists. But fear is a bad counselor and so I set out to attack the problem head-on. Generalizing the problem of distorted description I see the challenge as follows: since any extant general framework for language description has started out from a rather small sample of languages it is conceivable that this sample is typologically biased and so descriptions using this framework are better suited to languages of one kind, say SAE, than of another, possibly resulting in a Procrustean treatment of the latter. The proposal for solving this problem I made in Zaefferer (2006: 116) is to select the sample used in developing the framework according to the criterion of maximal typological distance in different dimensions of typological variation. I submit that this would be a neat solution, but obviously also a rather costly one.

Today I think that there are simpler and less effortful ways of avoiding Procrustes effects in language description, prime among them by fostering competing descriptions. Under the header 'Competition is Good for Descriptions' I have argued for a consensus on dissenting entries as desideratum for linguistic database design (Zaefferer 2004). The advantages of digital databases over other forms of storing descriptions include the possible close co-existence of different and thus competing descriptions of the same observation-based data. This way users can always prefer a less-biased description to a more-biased one, their behavior can be recorded and the database administrator can use it to justify updates.

As said I do recognize the Procrustes problem as real, but I conceive it more in general terms such as describing spoken in terms of written language or, and here I see the most urgent need for action, describing signed in terms of spoken language in current sign language grammatography, where the fact "that a (hearing) researcher may be biased by processes that are attested in the spoken language" is regarded as "a common challenge" (Quer et al. 2018: 191).

Finally, in spite of the above, I do not believe that unsatisfactory descriptions have to result in serious, let alone irreparable, damage to progress in general linguistics. The fact that the first grammars of German were modeled on Latin grammars (e.g. Albertus 1573) did not prevent the grammaticography of German from developing into a more and more independent discipline, and analogously the spoken language bias in sign language grammaticography will peter out over time.

### **5.3 From a useful distinction to an untenable dichotomy**

His monadic view of languages, presumably combined with a considerable dose of procrustophobia has led H to a bold postulate: category concepts used in describing a single language are incompatible with category concepts used in describing more than one language, a prerequisite for comparing them. He calls the former 'descriptive categories' and the latter 'comparative concepts', henceforth DCs and CCs, and insists on there being a 'fundamental conceptual distinction' between them (Haspelmath 2018: 83). In order to uphold this claim he has added another daring postulate: each language must be described exclusively in its own purely structural terms. This obviously prevents the Procrustes problem from arising at all, certainly an advantage, but as I will show, this benefit cannot possibly outweigh the costs to be paid. Note that none of the benefits stated in Haspelmath (2016: 300f.)<sup>16</sup> requires more than a simple distinction between language-specific categories and their generalizations.

To show that the distinction is simple and not fundamental, let alone ontological, I

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<sup>16</sup> 1. Liberating language describers; 2. Freeing typologists from need of hitting the 'right' concepts; 3. Permitting partial comparison, not description, 4. Allowing varied kinds of interlinear glosses.

take an example from Haspelmath (2018: 89), the German future tense construction, formed with the auxiliary *werden*. Substituting the variable *L* for German turns a language-specific into a general category. Of course chances are rather dim that another instantiation for *L* yields a non-empty category (mainly because of the special form of the auxiliary), but empirical likeliness and logical possibility are a far cry from being the same thing. And ironically, as Höder has observed, the price to be paid includes treating 'language' as a pre-established category, which arguably is another Procrustean corset.<sup>17</sup>

In order to understand how H has managed to turn a helpful distinction into an impracticable dichotomy, we have to go back in history to the heydays of structuralism. Based on the familiar distinction between phonetics and phonemics Kenneth L. Pike coined in chapter 2 of his monumental outline of a unified theory of human behavior (Pike 1967: 37) the pair of terms *emic* and *etic*, since it "proves convenient — though partially arbitrary — to describe behavior from two different standpoints, which lead to results which shade into one another." He calls *etic* the viewpoint taken when studying behavior "as from outside of a particular system" and *emic* its counterpart, the view "from inside the system." Four pages further ahead, under the heading "Caution — Not a Dichotomy" he emphasizes "that etic and emic data do not constitute a rigid dichotomy".

Nevertheless, and especially in anthropological circles, the distinction had become so prominent that Gustav Jahoda, one of the founding fathers of cross-cultural psychology and its association IACCP, felt compelled to deliver a key note to the IACCP 1982 congress under the somewhat provocative title "The cross-cultural emperor's conceptual clothes: The emic-etic distinction revisited".<sup>18</sup> Talking about "the cross-cultural emperors believing that they are wearing solidly made conceptual emic-etic clothes" he argued that "if not entirely naked, they are at most dressed in ill-fitting rags." (Jahoda 1983: 20).

The reason for this short historical excursion is the fact that H explains in several places his DC-CC opposition with reference to the emic-etic distinction introduced by Pike,<sup>19</sup> seemingly without being aware of either Pike's caveat in its definition or the

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<sup>17</sup> "[...] the habit of treating 'language' as a pre-established category can be described as the 'Procrustean corset' around 'vanilla' grammatical theory" (Höder 2018: 46).

<sup>18</sup> Presumably he was not very happy with the faint response to an earlier paper that ended with the following conclusion: "The pursuit of the emic-etic distinction appears to me a little like the alchemists' search for the philosopher's stone. [...] Meanwhile we should cease to exhort others to engage in a somewhat unprofitable enterprise, to which we ourselves pay mainly lip service." (Jahoda 1977)

<sup>19</sup> "Comparative concepts are etic notions (not part of the language system). Language-particular meanings are emic notions, part of the language system (Etic vs. emic: introduced by Kenneth L. Pike)" Haspelmath 2010c: 73). Cf. also Haspelmath 2016: 302, Haspelmath 2018: 87f.

controversial discussion about its validity as a strict dichotomy.<sup>20</sup> If there is an ontological difference between DCs and CCs, as H claims in another paper<sup>21</sup>, and if DCs are emic categories and CCs are etic concepts, as he also submits, this does not fit well with Pike's definition according to which the results of taking an emic or an etic viewpoint shade into one another. And if outstanding scholars in other fields have a hard time finding clear criteria for a sharp dichotomy, then it is not surprising that also linguists don't feel at ease with H's proposal.

So I am not the first to express reservations about the way H handles the CC-DC difference. Here is a selection of comments: "[The] distinction is valid, but not as a quasi-dichotomy" (Lieb 2015: 482); "We maintain that this claim [that cross- and intra-linguistic concepts are ontologically different] is mistaken." (van der Auwera & Sahoo 2015: 136); "[description and comparison] employ the same conceptual apparatus" (Lehmann 2018: 20); "claims that they [cross- and intra-linguistic concepts] are ontologically different do not stand up to further scrutiny" (Himmelmann 2019: 1); "... there is no a priori requirement for a fundamental descriptive/comparative distinction" (Spike 2020: 465); "[...] we should be careful of erecting a methodological framework based on the notion that somehow language-internal and language-external comparisons are inherently different" (Round & Corbett 2020: 517). Strangely, H feels still entitled to claim that his dichotomy is "not a problematic conceptual distinction" (Haspelmath 2020a:15).

Whereas my own criticism does not fully coincide with any of the quoted views, it certainly goes in the same direction. So far I have already provided some additional arguments against H's claims, but one important point is still missing: H apparently believes to have conceptually immunized his strict dichotomy by the abovementioned postulate that "linguistic categories must be defined in structural terms (with respect to other constructions of the language)" (Haspelmath 2018: 109). He even goes as far as to state a meta-level universal, the claim that this postulate is "universally recognized" (ibidem). But this meta-universal is not easy to prove. On the contrary, if neither the author of this review nor Himmelmann recognizes this postulate<sup>22</sup>, we probably represent the majority view. There are several reasons for that. Without going as far as Himmelmann, who claims that purely distributional analyses are plainly impossible, I submit that they are just unsatisfactory. There are computer programs for the

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<sup>20</sup> Other Linguists who also use the terms etic and emic are much less strict regarding their difference. Nick Evans for instance shows a more laid-back attitude: "A disadvantage of concentrating on the etic is that it overlooks obvious elegances of characterization that appear once one gives emic formulations." (Evans 1995: 510) A difference in elegances of characterization cannot possibly be of a very fundamental, let alone ontological kind.

<sup>21</sup> "Ontological difference[:] Comparative concepts are a different kind of entity than descriptive categories" (Haspelmath 2018: 84)

<sup>22</sup> "[...] there is no current grammatical theory or framework that explicitly subscribes to the idea that linguistic signs can be adequately described exclusively in terms of oppositions." (Himmelmann 2019: 10)



unsupervised learning of hierarchical natural language categories based on Harris-style distribution analysis. For instance the algorithm presented in Klein & Manning 2004 "acquires a substantial amount of correct structure" (p. 418), but not more. This is why another meta-level universal holds: distributional analyses must be supplemented by semantic criteria. And even if purely distributional definitions of all categories would be viable, this would not exclude language comparison: applied to code-mixing corpora of bilinguals (e.g. Çetinoğlu 2017) it could also be used in cross-linguistic constituent categorization.

## 6 Remarks on H's conclusion

Before coming to my own conclusion I want to briefly comment on each of the three points of H's conclusion. First, for reasons laid out above in section 3.2. I don't agree with his claim that p-linguistics is no less theoretical than g-linguistics: a theory-laden description is certainly less theoretical than a (purely theoretical) causal explanation.

Second, his claim that there are two ways of solving the problem of establishing general linguistics (barring psycholinguistics etc.), to follow either Greenberg or Chomsky and Baker seems to suggest that they are incompatible. But apart from terminological barriers they can be combined, at least in principle, in the service of scientific progress.

Third, even if H's impression that particular linguistics in an exclusive sense has lost in prestige is correct I don't consider this a shortcoming. On the contrary, if p-linguistics is more and more done with an eye on both rare phenomena and possible generalizations (and my impression is that this is the case) this is good news for advances in the study of both language and languages. And his postulate that no "linguist can simply pretend that the description of a particular language will automatically contribute to general linguistics" (p. 24) is simply untenable: even if the description is about a single language, it *does* automatically contribute to general linguistics by adding about 1/7000 to its enumerative descriptive subfield. If according to H "it is not immediately clear how one could learn about Human Language in general by studying a particular language" (p. 23), how else could this be done? Certainly, a single language is a rather weak basis for generalizations, but this statistical shortcoming is not H's main concern since he considers the heterogeneity of languages as a principled obstacle.

In the very last sentence H expresses his hope for converging evidence from both the innateness approach and the one he calls non-aprioristic. To this I would like to add that on the more deductive side there are other approaches besides the innateness-based ones, and that the more inductive approaches are neither without any a priori assumptions, nor are their findings in any way more empirical than those of the oth-

ers, let alone the only empirical ones, as H seems to suggest. Besides that I agree that converging evidence is much to be welcomed.

## 7 Summary

In view of the outstanding services H has rendered to progress in linguistic typology and general linguistics (among other fields not at stake here), it is somewhat ironical that at the same time he threatens to hamper this very progress by insisting on the feasibility and usefulness of upholding a watershed distinction between descriptive categories and comparative concepts. With this insistence he has in my view managed to paint himself into a corner. Arguably, the way H pursues his idea of particular languages as monads together with other questionable idealizations can be seen as a curious and instructive exercise in meta-scientific extremism, presumably induced by the noble motive of preventing comparison-based bias in language description. Although I agree that this kind of bias can be a real problem, I hope to have shown that H's attempt to solve it by overstressing an ill-founded dichotomy amounts to building an incommensurability wall between the particular and the general in language and linguistics. The lesson to be learned, I think, is that such a wall is more of a hindrance than a help for linguistics, both particular and general. In order to solve the Procrustes problem there are far more constructive ways than building an obstructive wall. So I will end, somewhat pathetically (and tongue-in-cheek), with a plea that plays with a famous phrase: Mr. Haspelmath, tear down this wall!

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