

PERSPECTIVES

Diversity driven but cognitively constrained: Boas meets Chomsky (Response to commentators)

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1. INTRODUCTION. We would like to begin by thanking de Reuse, Dryer, and Haspelmath for their thoughtful and constructive replies to our target article (Davis et al. 2014) on how best to uncover linguistic diversity. All three of our respondents point out that there is a considerable amount of common ground between researchers from the generative linguistic tradition and those who fall within the descriptive-typological tradition. We concur, and also agree that since many of the differences are more sociological than intellectual, the field is better served by constructive engagement than by indulging in doctrinal disputes.

Nevertheless, there are still important methodological and theoretical differences between the two traditions. Our main purpose in this final summary is to try to sort out the more substantive differences from those that are largely rhetorical; in doing so, we hope to shed more light on the crucial issues, without at the same time generating more heat.

2. C-LINGUISTS VS. D-LINGUISTS: WHAT IS THE DEBATE REALLY ABOUT? Our original article was partly motivated by the necessity to refute certain widely propagated myths, such as the claim that linguists who work within a ‘Chomskyan’ tradition (‘C-linguists’) choose ‘off-the-shelf categories arising from specific grammatical traditions, and foist them on all languages’, a move that ‘typologists and descriptivists deplore ... not only because it does procrustean violence to the basic data, but also because it pre-empts the discovery of new categories and new patterns’ (Levinson & Evans 2010:2739; henceforth L&E). As we argued previously, this kind of rhetoric does a disservice not only to the many C-linguists who engage in primary fieldwork on underdescribed languages, but also to the discipline as a whole, by widening existing fissures and needlessly exaggerating ideological differences.

In their responses to our article, de Reuse (2014), Dryer (2014), and Haspelmath (2014b) all attempt to repair the damage brought about by such sweeping pronouncements. All three acknowledge that useful crosslinguistic work has been and is being done within both traditions, and all three believe that methodologies from both sides are useful in the discovery of linguistic diversity. They also all agree that scientific hypothesis testing is just as vital a component of research on individual languages for D-linguists as it is for C-linguists, a point that we readily acknowledge.

However, differences remain. Here, we highlight three areas where we think there are genuine points of contention, or at least where the two approaches diverge considerably in emphasis. The first two are methodological: one concerns the role of grammatical intuitions (§3), and the other the right way to construct an effective large-scale grammatical typology, with particular reference to the *World atlas of language structures* (WALS; Dryer & Haspelmath 2013) (§4). The third area concerns the nature of hypothesis testing and the difference between what Haspelmath characterizes as ‘a priori categories’ and his ‘comparative concepts’ (§5). In exploring these areas, we touch upon

most of the specific points made by our respondents; we deal with residual issues in §6, and conclude in §7.

3. METHODOLOGY AND THE ROLE OF GRAMMATICAL INTUITIONS. In our target article, we mention ‘a commitment to the use of grammatical intuitions’ as a key component of the C-linguistic approach to exploring linguistic diversity (p. e182, n. 1). Both de Reuse and Haspelmath take us to task on this point by observing that D-linguists, too, routinely make use of intuitions; they thereby claim that there is no substantive difference between the two camps on this point.

While conceding that in practice most D-linguists do make use of intuitions, our impression is that they do so reluctantly and often only as a last resort, when textual resources fail. In fact, despite de Reuse’s claims to the contrary, some of the more rigid neo-Bloomfieldian descriptivists refuse to employ direct elicitation techniques at all, even to fill in straightforward inflectional paradigms. One notable example among Pacific Northwest linguists is the late Salishan scholar Aert Kuipers, whose grammars of Squamish (Skwxwú7mesh) and Shuswap (Secwepemctsin) are the standard references for those languages (cited, for example, a total of 154 times in *WALS*). In *The Squamish language*, Kuipers (1967:91) supplements some less widely used pronominal paradigms with a number of ‘conjectured forms’—that is, cells that he has filled in himself on the basis of analogy with attested forms. Why did he not simply ask his consultants to supply the missing forms? Because that would have violated his particularly uncompromising brand of neo-Bloomfieldian descriptivism.

Of course, this is an extreme case of allergy to speaker judgments, but it is nevertheless symptomatic of a tendency that is pervasive in the D-linguistic tradition, where judgments are regarded with widespread suspicion, if not outright hostility. L&E make no bones about it: ‘intuitions prove fairly wobbly both across speakers and within speakers across time ... they are off-line metalinguistic judgments, and do not directly reflect online processes and representations actually involved in speaking and understanding’ (L&E:2735). These statements reflect a more general bias—inherited ultimately from the American behaviorist psychology that influenced Bloomfield and his contemporaries—against methods that claim to directly access mental representations (I-language) as opposed to those that involve the observation of various types of linguistic behavior (E-language).¹

De Reuse’s comments on our article rather strikingly reflect the descriptivist unease with grammatical intuitions. When reporting on his own field experience with the grammatical intuitions of speakers of two endangered Athabaskan languages, he notes that ‘speakers change their minds over the years. Not only do they change their minds regarding judgments, but they even provide volunteered forms that directly contradict forms volunteered earlier’ (de Reuse 2014:e227). In other words, he concurs with L&E over the ‘wobbliness’ of judgments both across speakers and times. Later, however, de Reuse mounts a spirited defense of D-linguistic methodology on the basis of the fact that (most of) its practitioners DO employ speaker intuitions. We have no reason to doubt this claim, but it strikes us as odd to decry the reliability of intuitions while simultaneously asserting that they form an integral component of D-linguistic fieldwork.

¹ Boas, often invoked in the same breath as Bloomfield, is largely immune to this criticism. As de Reuse points out—and as any serious student of Northwest languages knows—Boas was not particularly doctrinaire about the text-grammar-dictionary model that is usually ascribed to him, and he made extensive, though unacknowledged, use of elicited examples in his own work.

As C-linguists, we have no such qualms about intuitions, not only because they are indispensable to the work we do on syntax and semantics, but also—as we argued in our original article—because most of the criticisms that have been leveled against them concern issues of implementation; once these have been addressed, we see nothing ‘wobbly’ about judgment-based methodologies.

We should add that in our own fieldwork we have found little change over time in speaker judgments, so it is something of a puzzle that de Reuse’s Athabaskan consultants show such variability. It is unlikely to have anything to do with the number of speakers or the predominance of English, since none of the languages we work on have more than a few hundred speakers (most have considerably fewer) and all of the communities where they are spoken have been English-dominant for many years. Without more information, it is difficult to speculate, but we suspect that one contributing factor may be the different types of data we and de Reuse are eliciting: whereas we are primarily interested in (often rather complex) syntactic and semantic judgments, de Reuse’s main interest is in morphology. De Reuse himself dismisses this idea, remarking that ‘I see no reason why syntactic or semantic complexity would be more amenable to consistency than morphological complexity. If anything, I would expect morphology to be more stable than syntax’ (2014:e227). We disagree, however; and moreover, we think the disagreement itself reveals something deep-seated about the difference between C-linguistic and D-linguistic conceptions of grammar. For de Reuse, the most stable and consistent parts of the system lie at the surface, in line with inductive D-linguistic assumptions about language acquisition: that is, the more frequently a phenomenon is attested in the primary linguistic data, the more stable it will be, both between speakers and over time. Since most of the syntactic and semantic phenomena we investigate lie at some distance from the surface, whereas morphology is by definition ON the surface, it is not surprising that de Reuse expects the latter to show more stability than the former.

We take an almost diametrically opposite viewpoint. For us—in line with the highly deductive structure of universal grammar that is a central tenet of C-linguistics—much of the inaccessible part of the grammar is given in advance, and therefore is expected to show less variation than surface phenomena, which are precisely the areas that can be learned—and unlearned—most easily. This is why we find ‘deep’ variation of the type exemplified by BINDING CONDITION C so fascinating: the data that might trigger a ‘binding parameter’ are not surface-accessible and not obviously linked to particular lexical items, and yet variation in condition C is robustly—if sparsely—attested. Conversely, we expect morphological variation to be both common and frequency-sensitive, precisely since it is learned inductively—and that is exactly the pattern we do find, in language acquisition, language change, and, according to de Reuse, also in language decline.

We conclude this section by remarking on another of de Reuse’s methodological points: his objection to the parallel that we drew in our article between traditional D-linguistic text collection and modern corpus linguistics. Of course, he is correct in denying that there is a historical connection—corpus linguistics only became possible thanks to modern computational power. The point we were making, however, was that as far as small languages are concerned, there is no fundamental methodological difference between modern online corpora and old-style text collections; they are both as accurate as the humans who transcribe and gloss them, and take just as long to compile. So L&E’s claim that larger corpora plus ‘data-mining and data-visualization techniques’ are going to radically alter the nature of linguistic data for understudied languages is at best hyperbolic.

4. *WALS* AND COMPARATIVE LINGUISTICS.² Though our original intention when writing the target article was never to focus on the shortcomings of *WALS*, two of our case studies (on determiners and on modal semantics) ended up challenging conclusions reached in individual *WALS* chapters (by Dryer, and van de Auwera and Ammann, respectively). Given that *WALS* is the most prominent public face of the D-linguistic enterprise, this is perhaps inevitable. Though we are critical of aspects of its organization and modus operandi, we would like to preface this section by acknowledging that *WALS* is a pioneering enterprise that has enhanced the understanding of and appreciation for linguistic diversity well beyond the D-linguistic community who designed and wrote it, and in its online incarnation it has enormous future potential as a resource for comparative linguistics.

We begin the discussion by making some general points about the ‘grammar-mining’ approach employed by *WALS* and some of the pitfalls that it encounters. We then respond to Dryer’s detailed comments on our case study of determiners.

4.1. GRAMMAR-MINING AND THE METHODOLOGY OF COMPARATIVE LINGUISTICS. The principal methodology of *WALS* is ‘grammar-mining’, a technique that involves searching through the grammars of as many languages as possible, from as many different families as possible, using a coding scheme that allows descriptions of particular phenomena to be converted into a small set of values for the data points that then appear on a map.

The first and most obvious point to make about this method is that it can only yield results that are as reliable as the grammars that serve as its source, as readily acknowledged by the authors of *WALS*. We do not see this as necessarily a disadvantage (it is true of any study based on secondary sources), but we do think that *WALS* contributors could greatly increase the reliability of their results by taking steps to minimize errors of interpretation in the source data.

We would like to highlight the necessity of these steps by presenting a mini case study from the Pacific Northwest that illustrates the depth of the problem in a particularly illuminating way. The two Interior Tsimshianic languages Gitksan and Nisga’a are sociopolitically distinct, but mutually intelligible and part of a dialect continuum long recognized in the linguistic literature, where they were formerly grouped together as ‘Nass-Gitksan’. Each has a well-regarded descriptive grammar (Rigsby 1986 for Gitksan, and Tarpent 1987 for Nisga’a); Rigsby and Tarpent communicated extensively during the period in which their grammars were being written, and it is fair to say that they came to a consensus on most major syntactic issues. Both are cited fairly extensively in *WALS*: there are forty-three data points for Nisga’a, and twenty-one for Gitksan, virtually all based on these two grammars. Of these, fourteen are coded for the same feature (that is, data points for both Nisga’a and Gitksan appear on the same map fourteen times).

Interestingly—and disturbingly—the values for these shared features differ 50% (7/14) of the time, as shown in Table 1. In other words, the grammar-mining technique yields only 50% agreement when applied to two almost entirely compatible descriptive grammars of what is for all intents and purposes the same language. Unless the Tsimshianic situation is an isolated aberration, this raises grave concerns about the reliability of grammar-mining over the thousands of languages featured in *WALS* and, therefore, a

² We follow Haspelmath in replacing ‘linguistic typology’ with ‘comparative linguistics’; Haspelmath (2014a, n. 1) remarks that the former is ‘a curious 19th century term (going back to a time when comparative syntacticians wanted to find idealized macro-types of languages) that has no analogy in other fields’.

FEATURE	VALUE FOR GITKSAN	VALUE FOR NISGA'A
SHARED VALUES:		
51A: Position of case affixes	No case affixes or adpositional clitics	
85A: Order of adposition and noun phrase		Preposition
87A: Order of adjective and noun		Adjective-Noun
92A: Position of polar question particles		Final
93A: Position of interrogative phrases in content questions		Initial interrogative phrase
97A: Relation between the order of object and verb and the order of adjective and noun		VO and AdjN
116A: Question particle		Polar questions
CONTRASTING VALUES:		
33A: Coding of nominal plurality	Plural prefix	No plural
37A: Definite articles	Definite affix	No definite or indefinite article
81A: Order of subject, object, and verb	No dominant order	VSO
82A: Order of subject and verb	No dominant order	VS
83A: Order of verb and object	No dominant order	VO
84A: Order of object, oblique, and verb	No dominant order	VOX
95: Relation between the order of object and verb and the order of adposition and noun phrase	Other	VO and preposition

TABLE 1. *WALS* values for shared features between Gitksan and Nisga'a.

fortiori for the comparative generalizations that it supports.³ To be fair, Dryer acknowledges this in his comments on his *WALS* chapters on determiner semantics: 'There is little question that I have probably misclassified many languages, either because my source is inaccurate or misleading or because I have misinterpreted the source' (Dryer 2014:e233). However, acknowledging the problem is not the same as solving it.

Does this mean that grammar-mining is irremediably flawed? Not necessarily, if certain precautionary measures are taken. Most obviously, the mistakes we have highlighted could have been corrected by asking for clarification from the authors of the grammars themselves. Even if this were impossible, consulting areal specialists with the knowledge and background to interpret the grammars of a particular sprachbund or language family would achieve the same result. And failing even this, *WALS* could at least use a more effective self-correction mechanism, whereby feedback is encouraged and promptly followed up on.⁴ Any of these moves would involve some loss of control on behalf of the authors, preferably in favor of a 'crowd-sourcing' model more akin to that of Wikipedia. However, the fact is that the sheer scale of an enterprise such as *WALS* is already beyond

³ In fact, we have reason to believe the inconsistency revealed by the Interior Tsimshianic data is not unique, since we have observed similar problems with *WALS* in other Pacific Northwest language families we are familiar with. To take a Southern Wakashan example, both Kyoquot (Rose 1981) and 'Nuuchahnulth' (Davidson 2002) are represented in *WALS*: however, Kyoquot is a dialect of Nuu-chah-nulth (Davidson's grammar employs data collected by Sapir and Swadesh from the Tseshaht dialect). And just as in Interior Tsimshianic, we find discrepancies in values for features coded in both dialects: of the five shared features, two (40%) differ in value. Thus in map 81A (Order of subject, object, and verb), Kyoquot is ascribed the value VSO and Nuu-chah-nulth is ascribed 'No dominant order', while in map 91A (Order of degree word and adjective), Kyoquot is ascribed 'No dominant order' while Nuu-chah-nulth is described as showing Adjective-Degree word order. As far as we are aware, these differences are not due to dialect variation, but simply to coding discrepancies.

⁴ For example, in November 2011, the first author of this article commented on the discrepancy between Gitksan and Nisga'a for map 37A (Definite articles). Dryer conceded the point in his response, which he ascribed to a programming error. The map, however, remains unaltered.

the capacity of two authors to effectively monitor, and *WALS* would benefit in increased accuracy and scope from a more open and collaborative approach, facilitated by the rapid propagation of information made possible by an online format.⁵

There is another advantage to online dissemination: a greatly expanded capacity to handle both depth AND breadth, rather than sacrificing one for the other. Because of this, there is really no need for Dryer, for example, to have to fit a complex set of coding decisions into a couple of pages, as he ruefully reports: ‘The authors of *WALS* chapters had only two pages in which to discuss various aspects of their typology, and I was forced to discuss my criteria for identifying definite and indefinite articles rather briefly’ (Dryer 2014:e233). Why? A more useful editorial decision would be to allow authors to provide full justifications for their coding (as Dryer does in his response to us, discussed immediately below). Given the virtually unlimited space available for online contributions, we can see no practical reason why this should be impossible, and it would surely also avoid many of the misunderstandings that both Dryer and Haspelmath report in the responses to *WALS* chapters (see n. 5), and therefore greatly improve the accuracy of feedback.

4.2. DRYER ON DEFINITENESS: THE CASE OF SKWŪWŪ7MESH ARTICLES.⁶ In this section, we examine in more detail Dryer’s response to our critique of his *WALS* chapter (Ch. 37) on definiteness, with particular reference to the Central Salish language SkwŪwŪ7mesh (Squamish), which offers a good illustration of the limitations of the *WALS* methodology.

In our original article, we claimed that Dryer had misclassified SkwŪwŪ7mesh by giving it the value ‘definite word distinct from demonstrative’. We showed that on any ordinary definition of definiteness, SkwŪwŪ7mesh (like every other Salish language) only has INDEFINITE articles.

Dryer’s basic point is that in our original critique we were unaware of the methodology behind his coding, which he was unable to elaborate in his *WALS* chapter because of space constraints (see previous section). He claims that once his methodology has been fully understood, it turns out that his characterization of SkwŪwŪ7mesh is correct.

Rather than a simple distinction between definite and indefinite articles, Dryer’s elaborated system is based on the following five-value ‘referential hierarchy’.

- (1) anaphoric definite (AD) > nonanaphoric definite (ND) > pragmatically specific indefinite (PSI) > pragmatically nonspecific but semantically specific indefinite (PNI) > semantically nonspecific indefinite (SNI)

The basic idea is that languages must choose articles that encompass a contiguous sequence on the hierarchy: for example, English chooses the first two values (AD + ND) for its definite article, while Garrwa chooses only the first (AD) and Kokota the first

⁵ Both Dryer and Haspelmath disagree with us on this point. Dryer (2014:e233, n. 3) remarks that ‘our experience with *WALS* is that the vast majority of alleged errors brought to our attention by experts are not in fact errors, but that the experts have not read the defining criteria carefully. Thus, allowing experts to make changes would result, in the majority of cases, in changing correct codings to incorrect ones and would thus decrease the accuracy, not increase it’. Haspelmath (2014b:e254) writes: ‘As for the alleged inaccuracies of *WALS*, a rough estimate is that at most a quarter of all cases where critics have claimed an inaccuracy are due to real errors’. The point is not, though, how accurate the criticisms are, but how accurate the original coding is: unless northwestern North America is particularly poorly served by its descriptive grammarians, the answer to this question—at least for syntax and semantics—is that it is alarmingly low.

⁶ We adopt Dryer’s term ‘article’ rather than ‘determiner’ in order to exclude demonstratives from consideration, as he suggests.

three (AD + ND + PSI), but no language is expected to have an article that, for example, only includes *(AD + PSI).

However, since in practice most descriptive grammars do not contain enough information to accurately calibrate their article systems according to 1, Dryer is forced back to a broader set of criteria, to wit:

[I]f an article is more strongly associated with positions higher on the reference hierarchy, I treat it as a definite article, while if it is more strongly associated with positions lower on the hierarchy, I treat it as an indefinite article. More specifically, I classify articles as follows: (i) if an article is restricted to the first two types on the reference hierarchy (including cases where it is restricted to one of these two types), then it counts as a definite article; (ii) if an article is restricted to the last three types on the reference hierarchy, then it counts as an indefinite article; (iii) if a language has two articles where, in some contexts, one article is more likely to be interpreted as definite and the other as indefinite, then the first article counts as a definite and the second as an indefinite article; (iv) if an article is restricted to a set of types that includes anaphoric definites but not semantically nonspecific indefinites or some subset thereof, then it counts as a definite article. (Dryer 2014:e241)

Skwxwú7mesh counts as having a definite/indefinite contrast on the two weakest interpretations ((iii) and (iv) of these criteria), because (a) ‘there are contexts in which a noun phrase with *ta* is more likely to be interpreted as definite and *k^{oi}* [= *k^{wi}*—HD/CG/LM] is more likely to be interpreted as indefinite’; (b) ‘although *k^{oi}* is not restricted to semantically nonspecific indefinites, only a nonspecific reading is possible in the context of a variety of scopal operators, such as negation and the universal quantifier’; (c) ‘although [*ta*] can be used with narrow-scope readings with respect to negation, it can only be used with wide-scope readings with respect to other scopal operators, like universal quantification’; and (d) ‘in the context given in [2], *ta míχał* “the bear” can only be interpreted as referring back to the bear mentioned in the preceding sentence’ (Dryer 2014:e241–e242).

(2) ... s=ən mən k^wólaš-t ta=míχał.⁷

NMLZ=1SG.POSS just shoot-TR DET=bear

‘[I went hunting. I saw a bear.] Then I shot the bear.’

(Dryer 2014:e242; generalizations and data from Gillon 2013)

Note that this last diagnostic might be taken to indicate that the contrasting article *k^{wi}* cannot be an anaphoric definite. This is false, however, as demonstrated by the examples in 3 and 4, which form part of a narrative sequence.

(3) n=s=na mən q’ánacut-nít-an [k^wəci snəx^wíł-čət] s=mən

1POSS=NMLZ=RL just return-APPL-1ERG DEM canoe-1PL.POSS NMLZ=just

cún-t-an [k^wəs=n-sq^w’úł-t]: ...

təll-TR-1ERG KWLF=1POSS-wife-PST

‘Then I returned to our canoe and told my wife: ...’

...

(4) n=s=na mən q’ánacut nám’ t=ta=n-snəx^wíł, n=s=na

1POSS=NMLZ=RL just return go OBL=DET=1POSS-canoe 1POSS=NMLZ=RL

mən wílq^w-t-an [k^wəs n-sq^w’úł-t]: ‘čəx^w ?ənča-s k^wəci

just ask-TR-1ERG KWLF 1SG.POSS-wife-PST 2SG.S where-CAUS DEM

n-šát?’

1POSS-shot

‘I returned to my canoe and asked my wife: “Where did you put my ammunition?”’

(Kuipers 1967:240–41)

⁷ Transcription and glossing conventions have been modified from Gillon 2013 to conform to the system employed in our original article.

The second use of *kʷəs nsqʷ úʔt* ‘my late wife’ in 4 is certainly anaphoric, and both uses are definite; the fact that *kʷi* can (easily) be used for anaphoric definites is lost when it is described as indefinite. Conversely, there are also many places where *ta* can be used as a narrow-scope, nonspecific indefinite (Gillon 2013). In other words, while there may be a tendency for *ta* to be used for interpretations higher on the referential hierarchy, and for *kʷi* to be used for interpretations lower on the hierarchy, the two articles completely overlap on the dimension of definiteness.

What can Dryer conclude? Minimally, that given the two determiners *ta* and *kʷi*, one is ‘more definite’ and the other ‘less definite’. This is used to justify the claim that *Skw̥wú7mesh* has a definiteness contrast. The problem with this is that it is difficult to say whether ANY two-determiner system would escape being classified along the dimension of definiteness via these criteria.

Even more significantly, because it tries to calibrate all determiners along the scale of referentiality in 1, Dryer’s system misses the most fundamental organizing principle within the *Skw̥wú7mesh* article system (and the one that characterizes the majority of other Salish languages; see Matthewson 1998, Gillon 2013): that is, the distinction between DEICTIC and NONDEICTIC articles.⁸ This is because Dryer assumes that articles cannot have deictic properties: his classification system treats deictically specified determiners (like English *that*) as demonstratives. The latter move is highly problematic for *Skw̥wú7mesh*, which, like other Salish languages, has parallel sets of articles and demonstratives, BOTH of which have deictic features, but that show separate syntactic behavior: for example, demonstratives but not articles can occur in isolation as demonstrative pronouns.

For comparison, the two sets are given in Tables 2 and 3.

	DEICTIC			NONDEICTIC
	NEUTRAL	PROXIMAL	DISTAL, INVISIBLE	
gender-neutral	ta	tí	kʷa	kʷi
feminine	ʔa	tsi	kʷəʔa	kʷəs

TABLE 2. The article system of *Skw̥wú7mesh* (adapted from Gillon 2009:8).

		NEUTRAL, INVISIBLE	PROXIMAL	MEDIAL	DISTAL	
					UNMARKED	INVISIBLE
gender-neutral	number-neutral	kʷiya	tí, tíwa	táy	kʷəci	
	plural	kʷiyáwit	iyá(wit)	íci(wit)	kʷəciwit	kʷáwit
feminine		kʷsá	cíwa	áʔi	kʷəʔi	

TABLE 3. The demonstrative system of *Skw̥wú7mesh* (adapted from Gillon 2009:8).

Notice that the articles and demonstratives are nearly identical in terms of their featural composition: both are distinguished by gender and by location relative to the speaker. In fact, there are only two major differences: the first is that the demonstratives distinguish number, as well as four rather than three degrees of distance; and the second is that there are two nondeictic articles (neutral *kʷi* and feminine *kʷəs*) that lack a demonstrative counterpart.

Now, on Dryer’s classification, nondeictic *kʷi* and *kʷəs* would count as the ONLY articles in *Skw̥wú7mesh*: all of the rest of the elements in Table 2 would be demonstratives, due to their deictic specification. However, this is clearly incorrect: both par-

⁸ The only known exception is the the Southern Interior language Nsyílxcen (Okanagan), whose single determiner has no deictic specification; see Lyon 2013, 2015.

adigmatic organization and syntactic behavior support the division represented in Tables 2 and 3. Treating the deictically neutral articles *ta* and *ta* as nondeictic would increase the number of ‘true’ articles, but would lead to further problems: in particular, it is difficult to see how the deictically neutral DEMONSTRATIVES could then be excluded from the article system.

We conclude that even the more elaborated version of (in)definiteness outlined by Dryer mischaracterizes the Skwxwú7mesh article system. The basic problem is that definiteness (or more broadly, the referentiality scale in 1) is not directly relevant to the system at all; instead, it is deixis—mistakenly rejected by Dryer as a property of demonstratives, not articles—that acts as its major organizing principle.⁹

5. APRIORISTIC HYPOTHESIS FORMATION VS. COMPARATIVE CONCEPTS. In this section, we address what we consider to be one of the major substantive disagreements between (our conception of) C-linguistics and (in particular, Haspelmath’s conception of) D-linguistics: the nature of hypothesis formation.

Again we preface the section with a comment about where we do agree. All three of our commentators correctly point out that hypothesis testing is in no way the preserve of C-linguists; all kinds of linguists, in all kinds of circumstances, engage in the construction and testing of hypotheses as a basic component of linguistic analysis. The debate is only about where those hypotheses come from.

Haspelmath characterizes the intellectual division here as primarily being between those ‘who are committed to an aprioristic approach (working with crosslinguistic categories that are given in advance by the restrictive framework) and linguists who have no such commitment and are open to discovering completely new categories, and who are also open to diverse ways of explaining the generalizations they find’ (2014b:e250). This is indeed one plausible characterization of the C- vs. D-linguist division (especially if one abstracts away from the negative connotations of ‘aprioristic’ and the positive ones of ‘open’), and we take some time to discuss it here.

Haspelmath states that to a large extent, our own work falls into the second, nonaprioristic, category, but he has two main points of disagreement with our position. First, he argues that we incorrectly conflate DESCRIPTIVE and COMPARATIVE hypothesis testing. Second, he proposes that comparative work should make use not of aprioristic crosslinguistic categories that are given in advance by the theoretical framework, but of ‘comparative concepts’ (cf. Haspelmath 2010). (Although Haspelmath writes that the successful parts of our own research make use of such comparative concepts, he also argues that some of our case studies rely on inappropriately eurocentric analyses.)

We disagree with both of these points. As Haspelmath suspects, we do in fact hold that there is no principled difference between descriptive and comparative fieldwork. And we do support the use of categories that are adopted from other languages or are even postulated as universal—but crucially, as we argued in our original article, NOT as ANALYSES about other languages, but only as INITIAL HYPOTHESES, which are often disproven.

Haspelmath’s contention is that ‘[t]esting a comparative or crosslinguistic hypothesis is an entirely different matter from testing a language-specific hypothesis’ (2014b:e252). A crosslinguistic hypothesis necessitates generalizing over more than one language; this is, he argues, strictly separate from the fieldworker’s job of attempting to describe indi-

⁹ As Haspelmath (2014b:e253, n. 6) points out, this is not an inherent flaw in the methodology of *WALS*: it is simply a problem with Dryer’s taxonomy of article systems. Nothing would prevent the addition of deixis as a further dimension of crosslinguistic variation.

vidual languages. Haspelmath says we ‘talk like typical aprioristic Chomskyans’ in that we ‘see fieldwork as part of a crosslinguistic investigation’ (2014b:e251).

Haspelmath has understood our point of view correctly: for us, as for generative linguists generally, fieldwork is certainly part of a crosslinguistic investigation. But similarly, introspection-based research on English or any other language the researcher might natively speak is also part of a crosslinguistic investigation. This is because our fundamental goal is to find out what human language is like. Seen from this perspective, anything we find out about any one language is interesting not only in and of itself, but also because of how it compares to other languages. If language L has property X and this property seems to be shared across languages, that is interesting. If language L has property Y, which seems to differ from most or all other languages, that is also interesting. It seems to us that an approach whereby one constantly compares languages, even while studying individual ones, has a greater chance of leading to useful generalizations about what human language in general can be like.

We go even further in our defense of the ‘aprioristic’ view (which is, as stated above, not actually aprioristic, since one’s initial hypothesis, even if it is drawn from another language, is often rejected in the course of the study). One of the main proposals of our original article was that adopting universalist hypotheses when studying new languages is a useful way to uncover novel, accurate, detailed, and deep language-specific properties.

Haspelmath seems to believe the opposite: namely, that universal hypotheses PREVENT the discovery of novel phenomena and generalizations. He asserts that ‘linguists who conflate descriptive hypothesis testing with comparative hypothesis testing run the risk of ignoring phenomena that have no counterparts in other languages that we know about’ (2014b:e252). However, this claim relies on the false premise that C-linguist fieldworkers limit their testing to phenomena they are already familiar with. On the contrary: precisely because we are interested in human language generally, we are interested in ALL phenomena in ALL languages.

As an example of the kind of phenomena he thinks C-linguists miss because of their aprioristic biases, Haspelmath cites discourse particles in German such as *ja*, *denn*, or *doch*, which have no obvious counterparts in English.¹⁰ In fact, however, discourse particles are by no means unique to German, and are currently under active crosslinguistic investigation by C-linguists, using a procedure similar to that pursued in other areas of the grammar. First, one set of researchers looks at a particle or particles in an individual language and conducts description, generalization, and analysis. These researchers do not worry at first that they do not know about similar phenomena in other languages. They make their findings available for other people to test. Then, when the next set of fieldworkers stumbles across a mysterious particle in another language, they have somewhere to start. If the initial data fit at all, they may adopt an initial hypothesis based on the first researchers’ findings. After testing, they will almost certainly find that their particle shares some similarities as well as some differences with the first-described ones. Those similarities and differences are a large part of what is interesting.

Although space prevents us from giving detailed examples here, there is already evidence that crosslinguistic comparison of discourse particles leads to useful individual language description. For example, Kratzer and Matthewson (2011) compare the Ger-

¹⁰ Although we would never ignore them for THIS reason, it must be admitted that many researchers initially ignore discourse particles for a different reason, namely, because they are hard to test and analyze: as Matthewson (2015) observes, they often remain in the ‘too difficult basket’ for years. However, this is equally true for non-Chomskyan fieldworkers: there is no chapter in *WALS* on discourse particles, for example.

man particle *ja* with the St'át'imcets (Lillooet Salish) particle *qa?*. They argue that the particles are largely similar, but display one critical difference, which follows from an independent difference between the languages. Their unified proposal for the two particles is given in 5.

- (5) If *p* is the descriptive content of a sentence *α* in a context *c*, then St'át'imcets *qa?* *α* or German *ja* *α* in *c* indicates that the speaker of *c* takes *p* to be an established fact, and therefore does not consider the question 'whether or not *p*' to be an issue for either the current or any future inquiry.

This proposal accounts for a range of facts that are shared across both languages. First, both of the particles can be used when reporting obvious and/or mutually known facts, as in 6 and 7.

- (6) ʔaʔ k^w=ən=ləχláχ-s ʔi=w=an ka-háhl-a,
 NEG DET=1SG.POSS=remember-CAUS when=IPFV=1SG.SBJN CIRC-appear-CIRC
 ʔiʔwaʔ=**qa?** t=s=ən=a wáʔ latiʔ
 even=**QA?** DET=NMLZ=1SG.SBJN=EXIS be DEIC
 'I don't remember when I was born, even though I was there.'
 (St'át'imcets; Kratzer & Matthewson 2011)

- (7) An meine Geburt kann ich mich nicht mehr erinnern, obwohl ich **ja**
 at my birth can I me not more remember although I **JA**
 dabei war.
 there was
 'I don't remember my birth, even though I was there.'
 (From the memoir of Angelika Kratzer's grandmother Hertha Dietz)
 (German; Kratzer & Matthewson 2011)

Both of the particles can also be used when asserting propositions that the addressee does not know, as in 8, or even disagrees with, as in 9, as long as the speaker considers the information to be an established fact and not open for further discussion.

- (8) [Context: At a gas station. A stranger tells you 'Your car is shiny.' You reply:]
 a. ʔayɬ=kan=á=**qa?** k^wəz-ən
 just=1SG.SBJ=A=**QA?** polish-DIR
 'I just cleaned it.'
 (St'át'imcets; Kratzer & Matthewson 2011)
 b. Ich hab's **ja** (doch) eben erst waschen lassen.
 I have.it **JA** (doch) just now wash let
 'I just had it washed.'
 (German; Kratzer & Matthewson 2011)
- (9) [Context: You served me a sandwich that I know you think is delicious. After the first bite I say:]
 a. ʔəz=ʔuʔ mútaʔ k^w=ən=s=x^wúʔ čáq^w-ań, kəlaʔ=**qá?**=tiʔ
 NEG=just again DET=1SG.POSS=NMLZ=going.to eat-DIR first=**QA?**=DEM
 təχ
 bitter
 'I'm not eating any more. It tastes awful.'
 (St'át'imcets; Kratzer & Matthewson 2011)
 b. Ich esse keinen Bissen mehr, der schmeckt **ja** scheusslich.
 I eat no bite more that.one tastes **JA** awful
 'I am not eating another bite. It tastes awful.'
 (German; Kratzer & Matthewson 2011)

A further parallel between the particles is that both are infelicitous when the proposition asserted answers the question currently up for discussion. In these discourse contexts, the speaker cannot take *p* to be an established fact.

(10) [Context: You go to the doctor, wondering what is the matter with your leg, and he takes x-rays. After looking at the x-rays, he says:]

- a. #qax^wəx^w-q=káx^w=**qa?**
break-leg=2SG.SBJ=**QA?**
'You broke your leg.' (St'át'imcets; Kratzer & Matthewson 2011)
- b. #Sie haben **ja** ihr Schienbein gebrochen.
you have **JA** your shin.bone broke
'You broke your shin bone.' (German; Kratzer & Matthewson 2011)

The contrast between 8–9 and 10 indicates, according to Kratzer and Matthewson, that *ja* and *qa?* place conditions only on the SPEAKER's beliefs about the status of the embedded proposition, not the addressee's. The proposition may be controversial (as in 9), but it must be established and not up for discussion as far as the speaker is concerned.

Ja and *qa?* are not completely identical, however. Kratzer and Matthewson observe that in out-of-the-blue exclamatives expressing surprise about a fact that is accessible in the utterance situation, *ja* is good but *qa?* is rejected. The St'át'imcets cases seem to require a connection to another salient fact, as indicated by the consultant's comment in 11b.

(11) [Context: You and I have been playing the lottery. We are having dinner together and are talking politics with the TV running in the background. We both happen to look at the screen when the results of this week's lottery drawing appear. I see my numbers and say:]

- a. Ich hab' **ja** gewonnen!
I have **JA** won
'I've won!' (German; Kratzer & Matthewson 2011, modeled after Lindner 1991)
- b. #ʔx^wum=ʔkan=á=**qa?**
win=1SG.SBJ=A=**QA?**
'I've won.' (St'át'imcets; Kratzer & Matthewson 2011)
Consultant's comment: 'That'd be okay if you said something before, like x^wúyʔkan ʔáʔən k^wu sáʔʔkaʔ ['I'm going to buy our food'] or x^wúʔkan zaʔ kaʔáʔa k^wu sáʔʔkaʔ ['I'm going to be able to buy our food after all'].'

Kratzer and Matthewson argue that, in fact, BOTH particles require a causal connection to another salient fact. In German the out-of-the-blue exclamatives are licensed by 'surprise intonation', under the assumption that the other salient fact might be the very act of expressing surprise: 'The reason for my surprise is that I won the lottery'. St'át'imcets *qa?* disallows this usage, as St'át'imcets appears to lack surprise intonation (in line with a general tendency for intonation to be used differently in Salish languages from in Indo-European; see Jacobs 2007, Koch 2008, Caldecott 2009). In St'át'imcets, the missing connection to another fact can be supplied by overt linguistic material, as in the consultant's comment for 11b. This conjecture correctly predicts that St'át'imcets 'surprise' cases become good with *qa?* if an explicit statement of surprise or a previous exclamation is employed.

(12) [Context: We are walking and you see a bear. You say:]

- a. #wáʔ=a=**qa?** ti=míxʔaʔ=a
be=A=**QA?** DET=bear=EXIS
'There's a bear.'
- b. ʔáʔχ-ən! wáʔ=a=**qa?** ti=míxʔaʔ=a
see-DIR be=A=**QA?** DET=bear=EXIS
'Look! There's a bear.'

- c. stúx^wum! wá?=**a=qa?** ti=míχaʔ=a
 careful be=A=**QA?** DET=bear=EXIS
 ‘Careful! There’s a bear.’

The moral of this new mini case study is that contrary to what Haspelmath suggests, it is fruitful to compare even difficult elements like discourse particles across languages. Kratzer and Matthewson’s methodology involved taking results from one language and testing them in another unrelated language. If one is a C-linguist by inclination, one will probably appreciate this attempt to find a unified analysis of particles in two unrelated languages.¹¹ Importantly, however, the enterprise is useful even if one does not care about a unified analysis or about the search for language universals. The enterprise is useful because it led to uncovering language-specific, language-internal empirical results. Knowledge was advanced more quickly and more easily than if one had viewed each language in isolation.¹²

In short, we believe that our job as linguists is to try to understand human language in general. Viewing our enterprise in this light, and using empirical and theoretical results arising from prior work on well-studied languages, leads to better descriptions of individual languages, as well as to—in our opinion—more interesting conclusions. But even those not convinced of the second point should be able to accept the first. Thus, so-called aprioristic views do not mask diversity; they can help to uncover it.

6. OTHER ISSUES. In this section, we deal with comments on two of our other case studies: condition C (case study 1 in our target article), and lexical categories (case study 2).

6.1. FOCUS IN NUU-CHAH-NULTH. Commenting on note 13 (p. e191) in our target article, where we claim that ‘focus has been controlled for’ in our examination of condition C effects in Nuu-chah-nulth, de Reuse asks: ‘But how does one reliably control for focus when eliciting data from elderly speakers of an endangered language?’ (2014: e228). More specifically, he asks why our example 10b, given below as 13, cannot be interpreted as a case of focus-induced coreference, as in (ii), as opposed to the nonfocused translation in (i).

- (13) wawa:ma [ʔanič **Lucy** ɣ̣aɣ̣aɣ̣aɣ̣i:ʔẉiʔas ʔaʔni:ʔik]
 say=3IND [COMP=3SBRD **Lucy** bread-make-ASP tomorrow=FUT]
 ‘Lucy_i said that she_i will make bread tomorrow.’
 (i) ‘She_i said that Lucy_i will make bread tomorrow.’
 (ii) ‘(even/only) SHE_i said that Lucy_i will make bread tomorrow.’

The simple answer to this question (which is given a more thorough treatment in Davis et al. 2007, the primary source for this section of our article) is that ordinary (null) pronouns as in 13 do not allow focus interpretations at all. When Nuu-chah-nulth speakers are asked to translate English sentences with focal stress on a pronoun, such as (ii), they employ instead a special set of inherently focused pronouns (see Davidson 2002:130–31), as illustrated in 14, from Davis et al. 2007:206.

¹¹ This point obviously does not rely on whether Kratzer and Matthewson’s particular analysis is correct.

¹² There are other examples of the fruitful use of crosslinguistic hypotheses in the description of discourse particles. For example, Matthewson (2015) investigates the Gitksan discourse particles *ist* and *qap*. She uncovers similarities as well as differences between *ist* and German/English *verum* focus (as analyzed by Gutzmann and Castroviejo-Miró (2011)), and she analyzes *qap* using tools drawn from studies of English by, for example, Farkas and Bruce (2009).

- (14) ʔanamitwaʔiʃ ʔuh̥ yaačaaʔ [ʔin ʔaačiiʔlit **Bill**
 only=PST=3QUOT 3FOC regret [COMP leave=PST=3ABS **Bill**
 waʔyuuʔakʔi]
 home=POSS=3PS]
 ‘Only HE_i regretted that Bill_i left home.’

Given this, it is straightforward to eliminate a focus-related reading for condition C-violating cases such as 13.

6.2. YET MORE ON LEXICAL CATEGORIES. Both de Reuse and Haspelmath address the issue of lexical categories (our case study 2).

De Reuse’s comment is a reminder that we should cite Jacobsen (1979) on Southern Wakashan. We are happy to acknowledge Jacobsen’s seminal contribution to the debate on lexical category distinctions in the Pacific Northwest, though the focus of our own case study was on the unrelated Salish family, where the debate raged on for another quarter of a century and, as we pointed out, is still frequently misrepresented in the typological literature. And while de Reuse characterizes Jacobsen as a D-linguist (a label that he himself would probably have agreed with), Jacobsen’s main argument for a noun-verb distinction in Makah and Nuuchahnulth is itself a reminder that good syntactic argumentation almost always crucially depends on negative evidence, irrespective of doctrinal adherence. In a nutshell, Jacobsen’s argument runs like this: Southern Wakashan languages have a determiner-like enclitic (= *iq* in Makah, = *ʔi* in Nuuchahnulth) that attaches to argument phrases. This clitic may be omitted on arguments headed by nouns, but is obligatorily present on arguments headed by verbs: hence nouns must be distinguished from verbs. Note that it is the **UNGRAMMATICALITY** of argument phrases headed by verbs without the enclitic that provides the crucial evidence here, evidence that by definition could not be adduced except via direct elicitation with fluent speakers.

Haspelmath’s comments on our arguments for lexical category distinctions in Salish are more critical, and reflect his own commitment to a relativist position akin to that of, for example, Croft (2005). In particular, he characterizes our discussion as a ‘striking case of a eurocentric analysis’ (2014b:e254), on the following basis. As we showed, Salish languages differ from English in showing ‘predicate-argument flexibility’, that is, the ability of lexical categories of any type to occur relatively freely in predicate or argument position. For Haspelmath—essentially following Swadesh (1933, 1948)—this means that there must be a single undifferentiated noun-verb class. As for the category differences we do adduce, Haspelmath relegates them to ‘specific contexts in which two subclasses of the verb-noun class need to be distinguished’ (2014b:e254).

However, here Haspelmath makes exactly the same error as Swadesh (and Sapir and Boas before him): that is, to confuse ‘argument’ with ‘noun’ and ‘predicate’ with ‘verb’. Ironically, this is itself a case of eurocentrism: while in European languages, the relationship between the two is tight enough that it is usually harmless—though incorrect—to conflate them, in the Pacific Northwest, confusing the two leads Haspelmath to the mistaken conclusion that the (robust, crosslinguistically supported) noun-verb distinctions that do exist must be properties of ‘subclasses’ of an undifferentiated supercategory. But these ‘subclasses’ ARE nouns and verbs: Haspelmath’s claim boils down to the vacuous assertion that nouns and verbs are subclasses of ‘lexical category’.

Of course, this is not to say that predicate-argument flexibility is not an interesting phenomenon in and of itself, and one worthy of further investigation. As we suggest in our target article, however, it is not only orthogonal to the issue of lexical category dis-

inctions, but is in fact probably epiphenomenal, consisting of the conjunction of two independently varying properties: the absence of a predicational copula (which accounts for the apparent freedom of any lexical category to occur in predicate position), and the availability of ‘headless’ (*pro*-headed) relative clauses (which accounts for the apparent freedom of any lexical category to occur in argument position).¹³

7. CONCLUSION: WHERE DO WE GO FROM HERE? We are grateful for both the tone and content of the responses to our article by de Reuse, Dryer, and Haspelmath. Their efforts to find common ground are welcome, and clearly show that C- and D-linguists have much to learn from each other, both methodologically and analytically. We want to reiterate in particular that though we have been quite critical of *WALS*, our comments are meant as constructive suggestions for improvement, not as attempts to denigrate the initiative itself, which we think holds great potential as a resource for comparative linguists, irrespective of C- or D-affiliation.

At the same time, we think the responses to our article have helped to clarify some areas of genuine difference between C- and D-linguists. In spite of Haspelmath’s generous offer to include us among the Boasians, we remain unabashed ‘methodological universalists’: we believe the best way to uncover linguistic diversity is (often) to assume uniformity and allow the primary linguistic data to persuade us otherwise—as it frequently does.

It is also worth emphasizing that the discovery of crosslinguistic differences is not the endpoint of the C-linguistic approach to linguistic diversity, but just the beginning. The next step—a more difficult one, of course—is to construct a universal theory that accounts for the observed variation and makes further (testable) empirical predictions. This is simply part of the familiar dialectic between theory and data that characterizes any natural science; we see no reason why this spectacularly successful mode of inquiry should not apply equally well to the investigation of crosslinguistic diversity.

For this reason, we reject Haspelmath’s characterization of our work as ‘very non-Chomskyan in that it does not seem to have any concern for universals of human cognition’ and his claim that we ‘make no attempts to salvage what might be left of the presumed universals, but seem content with recording the differences between the languages’ (Haspelmath 2014b:e256). It is true that in our Perspectives article we focus on exposing the myth that C-linguists are not interested in linguistic diversity, rather than providing fuller, explanatory accounts of the phenomena we discuss. However, to do justice to the latter enterprise would require book-length treatments; those familiar with our research will already be aware that we are actively interested in using linguistic diversity to refine and deepen universals of human cognition, rather than simply assembling a *Wunderkammer* of counterexamples to established generalizations. In short, while, like Boas, we believe that detailed investigation of understudied languages is critical to linguistic theory, we also remain committed to the Chomskyan universalist agenda that has so successfully served linguistics for the last half century.

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¹³ This predicts that languages will exist with just one of these two properties. Interestingly, two languages from opposite ends of the Pacific Northwest sprachbund bear out both halves of the prediction. In the south-east, Ktunaxa (Kutenai; isolate) allows headless relative clauses, but requires an overt copula with nonverbal predicates (Elise McKay, p.c.); in the far northwest, Tlingit (Na-Dene) lacks headless relative clauses, but allows nonverbal predicates without a copula (James Crippen, p.c.).

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