

Mythomania?
Methods and Morals from
'The Myth of Language Universals'

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Abstract

This paper takes heed of *The Myth of Language Universals*' (Evans and Levinson 2009) warning that tenets can leave theoreticians "partially immunized against ... proper consideration" and applies this admonition to the article itself. From an extremely modest focus of just one sentence and its subsequent defence, the paper reveals 27 failings of "proper consideration", rising 34 if given slightly wider focus. This high number covers a range of defects (from miscitation to errors in logic and misrepresentation of data, debates and theories) and affects material written at different times, on different themes, and in different organs. The paper urges, in consequence, that the field reconsider whether *The Myth of Language Universals* should be considered adequately to have argued its case.

Keywords Evans and Levinson 2009, Kiowa, linguistic methodology, *New Scientist*, Universal Grammar

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1. Introduction

Evans and Levinson’s recent article on language universals and Universal Grammar has attracted much attention, both professional and popular.¹ A problem that *The Myth of Language Universals* (henceforth *Myth*) identifies at the outset is that of being “partially immunized against ... proper consideration” (page 432), that is, of holding theoretical convictions with such force that one fails “honestly [to] confront” (page 429) data on which one’s case is founded or on which it might founder. The contention of this paper is that this problem applies nowhere with greater force than to *Myth* itself and to subsequent related work by the same authors. Indeed, the problem appears to be so thoroughgoing that it is doubtful whether *Myth* can be cited in good faith.

The current paper proposes to demonstrate this in a very simple way. We will pick just one sentence from *Myth* and count the objective failures of basic research methodology in it and arising from it in subsequent discussion. The total reached on a conservative count is 27. An alternative count, arising from an attempt to defend *Myth* by considering slightly more material, puts the number at 34.

The single sentence to be examined is:

So Kiowa, instead of a plural marker on nouns, has a marker that means roughly ‘of unexpected number’: on an animate noun like ‘man’ it means ‘two or more,’ on a word like ‘leg,’ it means ‘one or more than two,’ and on ‘stone,’ it means ‘just two’ (Mithun 1999, p. 81). (page 435)

This sentence, alone, breaches sound research practice in seven distinct ways. The sense in which further errors arise from it is as follows. Harbour (2009), in addition to pointing out core factual errors, argued that the real facts, properly analyzed, strongly support the notion of universals and of Universal Grammar. The attempted rebuttal of that response (pages 473, 475, 481 of the same issue) adds to the seven previous errors, and attempting further

¹Respectively, the commentaries in original edition of *Behavioral and Brain Sciences* and a special issue of *Lingua* (volume 120, issue 12, December 2010, pages 2651–2758); and *New Scientist*, ‘Language lessons: You are what you speak’ by Christine Kenneally (cover story, volume 206, issue 2762, 26 May 2010, pages 32–35), with further discussion in letters to the editor (volume 206, issue 2766, 26 June 2010, pages 29–30), more on which below.

defence by returning to the original article increases the total further. It is consideration of these points arising from attempts to counter problems with *Myth*'s claims that pushes the total up to 27. (The higher number of 34 arises from attempting yet further defences based on the original article. That is, the more charitably disposed we are to the defence of the position, the more improper its consideration it turns out to be.)

Even more problematic than the high number of the errors is their pervasiveness. The fact that attempted defence based on wider scrutiny of the article unearths more errors shows that 'immunity from proper consideration' affects not just a single sentence but is recurrent across disjoint parts of the original. The fact that such errors occur in the later responses to commentators shows that 'immunity' is not an one-off aberration but is consistent over time. And the fact that the errors occur in different, unrelated portions of the original shows that 'immunity' is constant across treatment of different themes. Thus, 'immunity from proper consideration' is restricted neither to a single place, nor to a single time, nor to a single topic, but is, it appears, a recurrent feature of the work.

In fact, *Myth*-ic 'immunity' appears even to have spread to another organ of publication. The *New Scientist* acknowledges that its coverage of *Myth* repeated one of its errors and has published a letter from me pointing this repetition of the error out.² However, at the same time, the *New Scientist* also published a response from the editor which, I contend below, could only have come from Evans and Levinson. The letter misrepresents *Myth*'s claims, falsifies one of its claims, and fails to exemplify the point under discussion.

What emerges from this assessment of *Myth* and its methods may, I think, without bias, be described as quite an indictment (and a true shock to anyone who, like me, had come to admire Evans as one of Australia's preeminent fieldworkers and an eloquent, insightful advocate for "endangered languages and what they have to tell us", to borrow the subtitle of Evans 2010). If the multiple misrepresentations of fact, of theory, and of argument are indeed as universal to the article as their locational, temporal and thematic distribution suggests, then what the authors of *Myth* have presented us with is not a piece of scientific research that "promises us a much better understanding of the nature of language and the cognition that makes it possible" (page 445) but

²Christine Keneally, who wrote the *New Scientist* article, informs me that Evans and Levinson attempted to prevent reproduction of the error, but the article had already gone to press by then.

a web of language myths which researchers in cognition should avoid and the field as a whole should reappraise with the utmost promptness and accuracy.

2. Errors of a single sentence

The single sentence with which our examination of *Myth* begins concerns the portrayal Kiowa as an example of languages that make “semantic distinctions we certainly would never think of”:³

So Kiowa, instead of a plural marker on nouns, has a marker that means roughly ‘of unexpected number’: on an animate noun like ‘man’ it means ‘two or more,’ on a word like ‘leg,’ it means ‘one or more than two,’ and on ‘stone,’ it means ‘just two’ (Mithun 1999, p. 81). (page 435)

The major errors this sentence contains concern which language is intended, how the affix in that language is distributed and whether even superficial inspection warrants characterizing its meaning as “of unexpected number”. Several of these, in turn, comprise various subsidiary errors, leading, even at this preliminary stage, to an error count of seven.

The first, and simplest, error is that sentence has confused Kiowa with Jemez. Both belong to the Kiowa-Tanoan family, which is the object of Mithun’s discussion. But all her examples are from Jemez rather than Kiowa. №1

Second, however, taking Jemez as the object of discussion does not make the quoted description true. Sure enough, unsuffixed ‘man’ means “one” and suffixed ‘man’ means “two or more”. However, suffixed ‘stone’ does not mean “just two”, because ‘stone’ never takes any suffixes at all (Spratt 1992:281): as is common in the language, class membership in this case is determined on the basis of verb agreement, not suffixation (*ibid.*:95, Mithun 1999:82). №2

Furthermore, the portrayal of ‘leg’—unsuffixed ‘leg’ means “just two”, suffixed ‘leg’ “one or more than two”—is doubly incorrect. First, this again miscites Mithun. She actually says that “‘leg’ is in Class II” and “Class №3

³The sentence naturally attracts my attention because I have been researching Kiowa, both empirically and theoretically, for a decade now. It is the only mention of Kiowa in the article and, so, constitutes, more or less, a topic, and hence a sentence, chosen at random.

II nouns show *-sh* in the singular and dual”,⁴ not, as *Myth* reports, in the singular (“one”) and plural (“more than two”).

Moreover, this error does not merely mix ‘leg’ up with some other word of Jemez. Rather, it invents a pattern that is attested nowhere in the language, as the only two extensive published studies of the language—the dissertations, Sprott 1992 and Yumitani 1998—and, indeed, Mithun, make clear. Ironically, the only member of the family to exhibit the pattern is Kiowa, the language that the cited passage accidentally names (Harrington 1928, Wonderly, Gibson, and Kirk 1954, Takahashi 1984, Watkins 1984, Harbour 2007). However, the error cannot be undone by appealing to Kiowa, because ‘leg’ is not in the class in question and because no noun in Kiowa exhibits the patterns of suffixation found for Jemez ‘man’. So, in fact, the system of suffixation that the passage describes is not true for any member of the Kiowa-Tanoan family.⁵ №4

Third, and most crucial, is the assertion that the suffix in question means “of unexpected number”. Here again, it does not matter whether one takes Jemez or Kiowa. The claim is in both cases clearly false. Applied to Jemez, it entails that one is an unexpected number of noses, that two is an unexpected number of legs, and that three or more is an unexpected number of ants. Applied to Kiowa, it entails that one is an unexpected number of heads (and penises), that three or more is an unexpected number of buffalo or locusts, but that two is the expected number of apples and hairs. (In fact, the “just two” class containing ‘apple’ actually excludes such inherently paired entities as eyes and ears, horns and hands, halves and twins.) It is not that the examples just given are exceptions or that the languages are as unsystematic as the gender systems of French, German, Russian, and the like. The semantic systematicity of noun classification in both languages is discussed in detail in Harbour (2010c). №5

⁴Actually, Mithun too has made a slight mistake, when she writes that “*hǫ* ‘leg’ is in Class II, while *hǫ* ‘bone’ is in Class III”. In fact, they are the other way round (Sprott 1992:281). Though this is of no concern to the general purpose of her encyclopedic tome, the correct facts are important to assessment of *Myth*’s claims (see №5 – №7) and highlight a general shortcoming of the article, namely, a tendency to draw data from secondary sources, rather than original, documentary research.

⁵See, for instance, Speirs (1966) on Rio Grande Tewa, Gardiner (1977), Allen and Frantz (1983) and Allen, Frantz, Gardiner, and Perlmutter (1990) on Southern Tiwa, Zaharlick (1977) on Picuris, Trager (1954) on Taos, Leap (1970) on Isletan.

Again, *Myth* misrepresents Mithun here. She does indeed say that the “number suffix ... marks nouns in the ‘unexpected’ or inverse number”. However, the scare quotes around ‘unexpected’ cannot simply be ignored, because she crucially attenuates this claim, stating that “semantic differences between [Classes] II and III is not obvious” and citing the distribution of body parts across the two classes in evidence. Mithun’s mention of body parts is important for two reasons. First, where Mithun uses body parts to caution against semantic oversimplification, *Myth* uses body parts for the opposite purpose, to push an oversimplified generalization. Second, the page that mentions the problematic nature of body part classification presents a flat counterexample to the claim that suffixation occurs for “unexpected number of”: ‘arm’ is suffixed only when it means “just two”, which, according to *Myth* (N^o5), should mean that “just two” is an “unexpected number of” arms. Ignoring Mithun’s attenuation turns a broad-brush heuristic into a claim with many counterexamples, thus failing “honestly [to] confront” either the language or Mithun’s balanced discussion of it. N^o6
N^o7

These last three points (N^o5 – N^o7) are the crux of the matter: the first two sets of errors (N^o1 and N^o2 – N^o4) are perhaps slovenly, but it is the last that determines whether Kiowa-Tanoan presents us with “semantic distinctions we certainly would never think of” and so bolster *Myth*’s case by threatening the concept of universals or the research enterprise of Universal Grammar. Clearly, the facts do not bear this out—a point made both by Harbour (2009) and Pesetsky (2009). Considering how the authors of *Myth* responded to the identification of these problems leads us into:

3. Initial errors in Evans and Levinson’s response

Evans and Levinson write:

Harbour reproaches us for attributing the “unexpected number” facts to Jemez rather than Kiowa; in fact, the languages are related and both exhibit similar phenomena (Mithun 1999, p. 81, and personal communication). We thank Harbour for picking up the factual errors he points out, but for our part would like to correct his mischaracterization of this case as our “prime example” of “something we would never think of” – it was one of many, and the rest still stand. (page 481)

The use of the past tense (“[the example] was one of many”) and saying the “rest still stand” implies that acknowledgement of error. But which error?

To say that the authors were “reproach[ed] us for attributing the ‘unexpected number’ facts to Jemez rather than Kiowa” misleads the reader and misrepresents the crux of the dispute: it wrongly implies that there are such facts and that nothing more went awry than the misnaming of a language. №8
The statement that “the languages are related and both exhibit similar phenomena” is true but irrelevant and so has the feel of a rhetorical device (as does the citing of “personal communication” from Mithun): it diverts the reader from the substance of dispute and does nothing to explain that, in fact, no Kiowa-Tanoan language exhibits the pattern the *Myth* advertizes. №9⁶

The overall attempt is to reduce the objections to a mere quibble about facts and names. Indeed, the quoted counter occurs in the section “Appendix: Disputed data and generalizations”. Nowhere is there explicit acknowledgement that Kiowa-Tanoan “unexpected number of” facts are not misattributed but nonexistent. Nowhere—more importantly—is there acknowledgement that there would have been no need for retraction if sound research methods had been adopted, namely, reading work by those with expertise and experience in the phenomenon in question, rather than relying on a one-and-a-half-page summary in a book itself reliant on other published sources. №10⁷
And almost nowhere is there acknowledgement that the factual corrections were the least of the seven concerns that Harbour (2009) communicated in the mere thousand words at its disposal as a *Behavioural and Brain Sciences* commentary.

Table 1:
Points contra Evans and Levinson (2009) in Harbour (2009)

In relation to Kiowa-Tanoan (primarily Kiowa):

(H₁) Correction of gross errors of fact

Demonstration that just two primitives suffice to explain two systems “we would ... never think of”:

(H₂) Seemingly singular *me+you* in Winnebago

(H₃) Crosscutting of singular and plural to make the dual in Hopi

Derivation, from the same primitives, of two universals:

(H₄) Implications between number values

(H₅) Status of ‘trial’ as the highest exact number expressible without numerals

In relation to Kiowa-Tanoan, and again using the same primitives:

(H₆) Explanation of how Kiowa (and Tanoan) number works and illustration of their similarity to languages like French (*i.e.*, not so never-think-of-able after all)

All of which undermine Evans and Levinson’s approach from a methodological perspective:

(H₇) Superficial inspection of unanalyzed data cannot be the basis for dismissal of claims about universals or Universal Grammar. Such cases, properly analyzed, provide robust evidence both for universals and for Universal Grammar.

4. Further errors in Evans and Levinson’s response

This last point bears further discussion, given the attenuating ‘almost’. Evans and Levinson do pick up on some of the points Harbour (2009) raised. However, examination of these responses again reveals the problem of ‘immunity from proper consideration’. The seven points Harbour (2009) intended to make are laid out in table 1. As is clear, factual corrections were a comparatively minor concern, just one of seven. The real substance concerned abstract analysis of apparently exotic data and the expressive power and predictive scope of a theory constructed on that basis. The general weakness of Evans and Levinson’s interaction with points (H₂)–(H₇) is what leads me to say that they reacted to them ‘almost nowhere’. Let’s consider them in turn.

With regard to (H₂), the authors of *Myth* write:

More importantly, further cross-linguistic data disputes his claim that “singular ‘we’ arises because Winnebago uses only [\pm augmented].” The use of “because” here illustrates the fallacy of inferring cause from single cases. Harbour’s formulation predicts that if a language uses a more elaborated grammatical number system than just [\pm augmented] it should not treat “1+2” as singular. Yet there are many languages which have a three-way number system and which nonetheless treat 1+2 in the same series as the singulars, like Biniñ Gun-wok (Evans 2003a). (page 481)

This attempted rebuttal in fact reveals three further instances of ‘immunity from proper consideration’.

That “many languages which have a three-way number system and which nonetheless treat 1+2 in the same series as the singulars” is a fact of which

⁶Incidentally, what I meant by “prime” was simply ‘very first’. Owing to the tight 1000-word limit on commentaries, I opted for the shorter phrase. (For the same reason, I omitted acknowledgements and, to save even on single words, went so far as to shorten the phrase “we would certainly never think of” to “we would ... never think of”, whenever I cited it.) I regret any misunderstanding that this ambiguous phrase has caused. The issue is, however, irrelevant, as the claim that *Myth*’s other examples of “distinctions we would certainly never think of” “still stand” is wrong; see section 6.

⁷My work would have been found simply by googling ‘Kiowa’, or ‘Kiowa-Tanoan’, or ‘Jemez’, and ‘number’—or by remembering that I had told Evans about how similar I had found Kiowa to be to a system that he had presented in a talk in London. Harbour 2010a and (2010c), though in press or under review at the time, were available online.

I am well aware. The authors of *Myth* would themselves have been aware of this if they had followed up on the references in commentary: Harbour (2010a, see note 7) explicitly derives such systems, using as an example a language related to Bininj-Gunwok, which the passage mistakenly regards as problematic. I do not at all mind that *Myth* did not cite my work on Kiowa-Tanoan number before making claims about the family (though, as said, I believe they should have consulted some such research rather than relying on a one-and-a-half-page, secondary summary). However, to continue to ignore such work when it is cited in a published commentary seems little short of laziness. And simply to make up claims about what the theory predicts when the cited works clearly demonstrate the opposite is, I believe, reprehensible.

Nº11

Nº12

Furthermore, consulting Harbour (2010a) would have staunched another error of the cited passage: the analysis of “singular ‘we’” is no “fallacy of inferring cause from single cases” but is based on, and generalizes over, a robust and wide typology (e.g., Corbett 2000, Cysouw 2003).

Nº13

Even if the authors of *Myth* had not wanted to consult the referenced papers, the commentary itself contained adequate material for anyone confident in their basic mathematical abilities to derive the systems that cited passage claims as underivable. (Lack of space prevented inclusion of sketched derivations in the commentary itself, though this was attempted in an earlier draft.) Specifically, unit augmented, the number that mention of Bininj-Gunwok alludes to, corresponds to [+augmented –augmented]. The interpretation of the feature combination, in conjunction with a third feature, [–singular], was discussed in the derivation of (H₄) and (H₅).⁸ And the discussion of Winnebago (H₂) made clear that some languages may choose not to use this extra feature. To make matters clearer, note 1 included an informal definition of [±augmented]. The response to Harbour (2009) gives no indication of having attempted to use its formal apparatus, and so, in addition to ignoring cited works relevant to their assertions and to making up claims about what these references can or cannot derive, the authors have ignored the parts of the actual commentary that were sufficient to disprove their claims.

Nº14

This last instance of ‘immunity from proper consideration’, that is, ignoring pertinent points, is repeated with regard to (H₃). The authors of *Myth* simply make no comment about it at all.

Nº15

(H₄), too, is ignored. However, it might be argued, in Evans and Levinson’s defence, that they felt that *Myth* had dealt with implicational universals

⁸These are referred to as (U₁) and (U₂) in the commentary (Harbour 2009:457).

between number categories. Examination of this attempted defence reveals yet more problems of ‘immunity’. The relevant passage reads:

(12b) IF a language has a trial number, THEN there is also a dual. IF there is a dual, THEN there is also a plural.

... Nevertheless, there is at least one language that counter-exemplifies: Basic verbs stems in Nen are dual, with non-duals indicated by a suffix meaning ‘either singular or three-or-more’, the singular and the plural sharing an inflection!

This discussion, and the failure to revisit it, is objectionable for seven reasons.

First, and most obviously, there is a failure to establish relevance. Statement (12b) concerns whole languages. The purported counterexample concerns only a subpart of a language, its system of verb stems. No mention is made of agreement, clitics, pronouns, demonstratives, or nominal number, any of which might obey (12b). Without further information, there’s no reason to consider this a counterexample and to do so represents a simple failure of logic. №16
Second, even if this oversight slipped through in the original article, the commentary, by pointing out the error, offered the opportunity for correction.⁹ This, again, is a point that response to the commentaries ignores. №17
Most distressing, however, is that it is impossible for the interested reader to verify whether Nen is a genuine counterexample, because *Myth* does not give any references about the language. Moreover, googling ‘Nen’ and ‘language’, or ‘Nen’ and ‘grammar’ yields nothing. Again, Harbour (2009), by drawing attention to the omission (note 3, page 457), would have allowed for inclusion of a reference in the responses, but this too was ignored and the interested reader is still none the wiser. №18
№19

Matters are made even worse by the editor’s comments in the *New Scientist*, which followed the published version of a letter (Harbour 2010b) pointing out the repetition of some of the Kiowa-Tanoan errors in coverage of *Myth*. The editor writes:

⁹Specifically, note 3, page 457, pointed out a subpart of a language may ‘violate’ (12b) though the language as a whole obeys it. The example given was for Kiowa ‘hair’: the “basic stem[]” (ᓵ) is dual, with “the singular and the plural sharing an inflection!” (ᓵ-ᓵ > ᓵᓵᓵ), in a language with a robust singular/dual/plural distinction. For reasons of space, note 3 was admittedly terse. However, it made explicit mention both of Nen and of the property of Kiowa just mentioned.

Evans and Levinson have published a correction to the Kiowa example in the same issue of *Behavioural and Brain Sciences* (vol 32, p 472). The general point about plurals not being straightforward still stands, and can be exemplified with another language: Nen from Papua New Guinea – also cited in the original article. In Nen you take duals (for two) as the basic stem, with a derived non-dual (anything other than two), while the affixing system distinguishes singular versus non-singular (two or more). Plurals are then composed by combining non-singulars with non-duals (numbers for which there is neither one nor two).

With all due respect to the editor, I suspect that this response is again Evans and Levinson's, not his. First, with no locatable sources, the editor would have faced a problem of tracking down the language “cited in the original article” (*N^o18*). Second, the editor is not a linguist and surely, with a full-time job on his hands, is not going to delve into the grammar of Nen only in the attempt to balance a letter to the editor. Third, the phraseology is so deft and so effortlessly information-rich that one suspects the hand of a well-seasoned linguistic professional. Though I await confirmation (from an enquiry sent 9 July 2010), I believe that we should scrutinize this text just as much as other attempts to redress the original error.

Of the multiple failures of good research methodology that the passage presents, the first is again misrepresentation, though not of Mithun or me, but of *Myth*. As we have seen, they did not “publish a correction to the Kiowa example”, as this would have involved saying, at a bare minimum, (*N^o4*) that no Kiowa-Tanoan language exhibits the pattern of suffixation advertized and (*N^o5*) that the suffix in question cannot mean “unexpected number of”. All the response did was acknowledge a mix up in names, citing relatedness and similarity as defence, and then imply retraction by stating that “the rest [of the examples] still stand”. *N^o20*

Second, the letter also misrepresents the debate in which the citation of Kiowa-Tanoan arose. The point was to illustrate “semantic distinctions we would certainly never think of”. Here, however, the “general point” is said to have been “plurals not being straightforward”. These are clearly not equivalent: plural formation may be *morphologically* unexpected in Nen,¹⁰ *N^o21* but nothing about the distinctions that Nen makes appears *semantically*

¹⁰As such, Nen might serve the same purpose as Hopi in my commentary (H₃).

unexpected, just singular, dual, and plural.¹¹

Third, the new data that the passage gives about Nen shows that its use in *Myth* is flawed in the precisely way that was suspected (*N^o16*). The claim was that Nen counterexemplifies the implicational universal “IF there is a dual, THEN there is also a plural”, but *Myth* gave only data about verb stems, which, I said is only relevant if the language as a whole makes no further number distinctions. The *New Scientist* now informs us that Nen perfectly conforms to the universal in question: it has singular, dual, and plural after all.¹² *N^o22*

Moving on to other points in table 1, (H₅), again, is ignored. *N^o23*

With regard to (H₆), as already noted, Evans and Levinson seem content to imply that they no longer take Kiowa or Jemez to be amongst the examples that “still stand”, but do so only in the context of admitting a minor factual slip, not the existence of theory that adequately explains the surprise presented by Kiowa-Tanoan (and Winnebago and Hopi) using resources that also derive at least two universals (exemplified, inter alia, by erstwhile counterexample Nen).

So, let us finally turn to (H₇). This last is also one of the points on which rebuttal is attempted. The relevant passage reads:

A number of commentators (Baker, Harbour, Nevins, Pesetsky) felt that we were unwilling to entertain the sorts of abstract analyses which allow us to find unity in diversity. But we are simply pointing out that the proposals on the table haven’t worked. Abstractness has a cost: the more unverifiable unobservables, the greater the explanatory payoff we expect. ...

... For this reason Harbour’s commentary misses the target – of course some middle level generalizations about the semantics of grammatical number are valid in any framework (although his account of the plural seems to not generalize beyond three participants, and there are additional problems that we discuss in sect.

¹¹The ‘editor’ also writes: “In a further twist, you get exhaustive plurals by combining the dual with the singular”. Whether “exhaustive plurals” are semantically unexpected cannot be assessed without a definition and some examples—or a bibliographic reference. If they are just plurals of magnitude, then they are hardly novel (see, e.g., Corbett 2000).

¹²*N^o16* and *N^o22* are related but distinct errors: if Nen really did counterexemplify the implicational universal, then *Myth*’s error would have been only one of logic (*N^o16*), not of misrepresentation of data (*N^o22*).

R6.4). The art is to find the highest level generalization that still has empirical ‘bite’. (page 475)

This attempted rebuttal rather unconvincing. Consider first the claim that “the proposals on the table haven’t worked”. At least one of the proposals on the table, namely Harbour (2009, 2010a), works so well that it has forced retraction Kiowa-Tanoan number as an example of the unimaginable and confronted the authors of *Myth* with two more surprises and two universals that they have been unable to refute—the “additional problems that we discuss in sect. R6.4” were dealt with under (H₂) above (№11–№13).¹³

In this light, the second paragraph of the cited passage presents three distinct instances of want of ‘proper consideration’.

First, given the number of points in Harbour (2009) that are simply ignored, the authors’ response has not succeeded in depriving the commentary nor the broader theory of its three surprises and two universals. The question is, how much “empirical bite” do the authors of *Myth* expect in a mere thousand words? And, recall, the real proposal is not in the commentary but in the references that have demonstrably been ignored. As such, the suggestion that the work in question lacks “empirical bite” does not seem to rest on “honest[] confront[ation]”. (And, to reiterate, *Myth* is the proposal that has lost “empirical bite”, given that Kiowa-Tanoan number no longer “stand[s]” as a “semantic distinction[] we certainly would never think of”; see section 6 on the standing of *Myth*’s other examples.) Similarly, given the failure to refute any of the universals and surprises that Harbour (2009) contrived to derive in a mere thousand words from just two primitives, the admonition that “the more unverifiable unobservables, the greater the explanatory payoff we expect” also has the feel of rhetoric, rather than reason.

№24

Second, the claim that “[Harbour’s] account of the plural seems to not generalize beyond three participants” is unsubstantiated. Not only is the intended meaning of the claim not explained (and I honestly don’t know what it is meant to mean—surely not that the features used cannot capture pluralities greater than three!), but no indication is given as to why this “seems” to be the case. Given that the “additional problems that we discuss in sect. R6.4” are dismissed simply by reading the materials referenced in the commentary, one wonders whether there is a factual basis for the claim that

№25

¹³Given that №8–№10 have already been counted, the statement that “the proposals on the table haven’t worked” is not added to the tally of lax methodology, even if more thorough consideration on Evans and Levinson’s part would surely have stanchied it.

the account does not generalize—especially when Harbour (2010a) shows the opposite.

Third, no explanation is offered as to why the universals and surprises in Harbour (2009) count as mere “middle level generalizations”. Kiowa-Tanoan number did not count as a “middle level generalization” when it was originally offered as a “semantic distinction[] we would certainly never think of”, so why its explanation should be so counted, or why derivation of further surprises and universals from the same primitives should, is unclear. More importantly, these do not count as “middle level generalizations” within Universal Grammar, as understood by researchers within that framework. As one researching Universal Grammar, I am interested in just two universals: the algorithms that build and interpret syntactic structure, and the set of primitives over which these algorithms operate. The two primitives proposed in my commentary and the mechanism of feature recursion (e.g., [+augmented –augmented]) are therefore universal in just the sense Universal Grammar requires them to be. If the authors of *Myth* think that something like that must be “valid in any framework”, then they endorse precisely what they purport to attack, the reality of universals and Universal Grammar.

N^o26

5. Interim summary

In considering the extent to which *Myth*, and its subsequent defence, reveals its authors to be “partially immunized against the proper consideration” of data, argument and theory, we have so far maintained a very narrow focus: just one sentence in the original article, eight sentences in the authors’ response, one more sentence in the original that might be called on their defence and four sentences published as an editorial response in the *New Scientist*. Only one assertion has so far escaped scrutiny, namely, that, even without Kiowa-Tanoan number, “the rest” of the “semantic distinctions we would certainly never think of” “still stand”. Anticipating the result of the next section—that the claim rests on data given cursory, complacent handling and not subjected serious, scholarly scrutiny—the count of methodological failings rises one higher (or seven higher, to 34, if we also count the individual failures of “proper consideration” that comprise *N^o27*). So, before we widen our focus slightly, to consider methodological standards elsewhere in the article, let us take stock of the failings that have emerged just from this very narrow focus.

N^o27

The 27 failings are categorized in table 2. As can be seen, they range

from the simple and careless (e.g., mixing up names, *N^o1*, and omission of references, *N^o18*), through the mildly improper (e.g., failure to engage with research, *N^o10–N^o11*, and use of rhetoric in lieu of argument, *N^o24*, *N^o26*), to the very grave (e.g., ignoring of criticisms by commentators, *N^o13*, *etc.*, and failures of basic logic, *N^o9*, *N^o15*) and end at the frankly reprehensible (e.g., failing to consult and making false assertions about cited work, *N^o12–N^o13*, and retracting an example without acknowledging that the real reason for retracting is not a minor nomenclatural mix up but the existence of a robust theory of universals and Universal Grammar, *N^o8*).^{14,15}

Faced with this ratio—scrutiny of one sentence, and minimally connected material, discovery of 27 errors—the question is: how much of *Myth*'s scholarship *can* we trust? The answer is probably: not very much. After all, it would be a remarkable coincidence if its were close to impeccable everywhere and that all failures of “proper consideration” had randomly clustered around the single sentence, and related material, that a specialist in Kiowa-Tanoan would happen to be drawn to. On the contrary, there is much to suggest that ‘immunity from proper consideration’ is a pervasive trait of *The Myth of Language Universals*. First, it has clearly arisen at distinct times (when writing the original article, the response, and, in all likelihood, the editorial comments in the *New Scientist*). Second, it arises with respect to different themes (“semantic distinctions we would certainly never think of” re Kiowa-Tanoan, implicational universals re Nen). And it also arises in different places within the original article (the same examples again).

Thus want of “proper consideration” arises in ways that vary in nature,

¹⁴Various failings have been crossclassified. For instance, *N^o1* is both an error of citation and an error of fact, and *N^o12* is both a failure to engage with research and an unsubstantiated assertion.

¹⁵It will be noted that ignoring is the most common methodological failing. The uncharitable interpretation is that the authors were hoping that the reader would fail to notice this. A more charitable interpretation is that by attempting to find fault with some points, the others might also be taken to have been dealt with. But this would be another error of logic: ‘some are wrong, so the rest are’ is just bad induction, and is, besides, if the rest were wrong, there would have been no need to retract the Kiowa-Tanoan example. Alternatively, they might simply have had too great a task in responding to all commentaries and so have chosen to deal only with some points. But this would be a fair defence only if it had been noted that ‘Harbour also raises five other points (*viz.* ...) but we have not had time to devise responses to these’—a weak response, but one fair to the commentary and to the reader interested in measuring up the results of the debate. Besides, such overstretched authors could surely have used their time better than picking at “prime” (see note 6).

Table 2:
Errors ramifying from a single sentence

Nature of error	N ^o
Errors of statement fact	1, 2, 3, 4
Erroneous or misrepresentative citation	1, 3, 6
Improper examination of data	4, 5, 7, 13, 22, 27
Misrepresentation of commentary or debate	8, 9, 12, 13, 20, 21
Failures of basic logic (incl. irrelevant statements)	9, 14, 16, 21
Failure to engage with research	10, 11
Unsubstantiated assertion	12, 13, 24, 25
Ignoring of commentary	14, 15, 17, 19, 23
Failure to give references	18
Rhetoric in lieu of argument or	9, 24, 26

in severity, in time, in theme, in locus and even in organ of publication.

6. Broader focus

That want of ‘honestly confront[ing]’ evidence or giving it “proper consideration” are thoroughgoing throughout *The Myth of Language Universals* is confirmed by some casting around for other points to scrutinize. Clearly, this task cannot be taken too far within the confines of the current article (given that examination of one sentence has so far taken 16 pages), but some brief consideration can be undertaken. Take, for instance, the only aspect of the domain of narrow focus not examined above, namely, that, though Kiowa-Tanoan number is no longer a “semantic distinction[] we certainly would never think of”, “the rest [of the examples] still stand” (page 435). These other examples are:¹⁶

In many languages, all statements must be coded (e.g., in verbal affixes) for the sources of evidence; for example, in Central Pomo, whether I saw it, perceived it in another modality (tactile, auditory), was told about it, inferred it, or know that it is an

¹⁶I confine myself, for reasons of space, to examples in the same paragraph as Kiowa.

established fact (Mithun 1999, p. 181). Kwakwala insists on referents being coded as visible or not (Anderson & Keenan 1985). Athabaskan languages are renowned for their classificatory verbs, forcing a speaker to decide between a dozen categories of objects (e.g., liquids, rope-like objects, containers, flexible sheets) before picking one of a set of alternate verbs of location, giving, handling, and so on (Mithun 1999, p. 106 ff.). Australian languages force their speakers to pay attention to intricate kinship relations between participants in the discourse – in many to use a pronoun you must first work out whether the referents are in even- or odd-numbered generations with respect to one another, or related by direct links through the male line. On top of this, many have special kin terms that triangulate the relation between speaker, hearer, and referent, with meanings like “the one who is my mother and your daughter, you being my maternal grandmother” (Evans 2003b). (pages 435–436)

Of the four properties mentioned, none deserves to be called something “we would certainly never think of” when “honestly confront[ed]”. Indeed, many similar phenomena are to be found no further afield than English and German. Thus, again, we find *Myth* not giving its data due thought and too hastily founding its conclusions on too insecure a basis. Furthermore, inspection of the individual cases highlights other now familiar failures of research standards, such as eschewing of primary research in favour of secondary summaries. Consider the languages in the order in which the passage presents them.

At the moment of writing this paragraph, I am in Germany and my lunchtime reading was a German broadsheet. The first two articles I read contained the following sentences:¹⁷

Mehr als sieben Jahre lang **will** *das FBI die Gruppe beobachtet haben,*
 for more than seven years wants the FBI the group have watched
die im Rahmen des „Illegalen-Programms“ des russischen Geheimdienstes
 which in the scope of the “Illegals Program” of the Russian secret service
tätig gewesen sein sollen.
 have been active should

¹⁷ *Die Frankfurter Allgemeine Zeitung*, Wednesday, May 30, 2010, page 5.

‘For seven years, the FBI claims to have watched the group, which, it claims, was active in the Russian secret service’s “Illegals Program”.’

Die Sicherheitskräfte hätten das Feuer eröffnet, weil sie
 The security forces have.SBJNCT opened fire because they
von den Demonstranten mit Steinen beworfen worden seien.
 by the demonstrators with stones thrown at were.SBJNCT

‘The security forces [reportedly] opened fire, because they [reportedly] had stones thrown at them by the demonstrators.’

These sentences show three distinct ways of “cod[ing] ... for the sources of evidence”. In the first sentence, the verb ‘want’ in the first clause indicates that the FBI is the source of a statement about itself, while ‘should’ in the second clause indicates that the FBI is the source of a statement about another entity. In the second sentence, both verbs are marked one of German’s two subjunctives which indicates that the reporter did not witness the events described (but, in contrast to the earlier sentence, no other source of evidence is implied). By any reasonable standard, something that you encounter with ease in a lunchtime flick through the papers in a major European language does not count as a “semantic distinction[] we would certainly never think of”. Central Pomo may differ from German in terms of the precise sources of evidence that they have grammatical means to express and in terms of the obligatoriness of such marking in day-to-day speech. However, in terms of the broad phenomenon of evidential marking, Central Pomo no more deserves the label of something “we would certainly never think of” than German does. (N^o28)
 So, again we find *Myth* embracing the exotic giving giving the data in question the thought they deserve. And, again, the style of research that led to the erroneous claims about Kiowa-Tanoan has reappeared: relying on (N^o29)
 a summary in a secondary source rather than consulting detailed, original descriptions.

In light of German and Central Pomo, the subsequent example, Kwakwala’s insistence on “referents being coded as visible or not”, is also far from compelling. In Central Pomo, according to *Myth*’s citation of Mithun, “whether I saw it” is specially encoded source of evidence. Now, an eye-witness to an event must eye-witness the entities it involves (otherwise, the person interpolates, rather than eye-witnessing, them). So, saying that an entity involved in an event is invisible is again a way of marking your source

of evidence, witnessing versus inferring. The difference is that what Central Pomo does on verbs, Kwakwala does in the domain of nouns. Thus, the label of something “we would certainly never think of” again looks like the result of too cursory an appraisal of the data. That said, all I have done is present a sketch of what might be going on; one would want to return to the compendious works of Boas, both descriptive grammar and glossed texts, or later sources, to confirm that we are dealing with a form of evidential marking. Instead, *Myth* again eschews original sources and relies on a two-page summary in survey article. (N^o30)

To see that Athabaskan verb classifications, the passage’s third example, are also far from unimaginable, we need venture little further than English. There, we find sentences like *A glass stood in the corner* and *A plate lay in the corner*; that is, verbs can be used to distinguish the prototypical differences between the kinds of positions that glasses versus plates assume. Two minor changes to English bring us directly to something just like Athabaskan (as I understand it): (N^o31)

1. Eschewing of a generic verb of position (like *was* in *A glass/plate was in the corner*).
2. Extension of the *lie/stand* distinction to other classes of object (e.g., liquids and ropes) and other types of action (e.g., handling and transfer).

However, neither of these moves can seriously be called something “we would certainly never think of”. With regard to the first, languages are known to vary in terms of which generic verbs they have at their disposal: English, for instance, has a generic verb *go*, where Russian forces its speakers to decide on the mode, regularity and completedness of the going before a verb can be chosen. So, the change involves making English more like Russian. With regard to the second change, relevant distinctions already exist in English: *I stood the glass in the corner* versus *I lay the plate in the corner* show that distinctions active in one domain (stative verbs of position) can be carried into another (transitive verbs of placement). Athabaskan merely extends these further, to verbs of giving and handling. Similarly, to deal with liquids and ropes and other nouns that require specialized verbs in Athabaskan, we need only *pool the liquid* or *coil the rope in the corner*, rather than simply *putting* them there.¹⁸ Thus, Athabaskan, far from being something “we would (N^o32)

¹⁸A third change might also be necessary to turn English into Athabaskan, but it, again, is already present in English. We can swap the verbs *lay* and *stood*, as in *A glass lay* or

certainly never think of”, looks similar to English, provided one gives the phenomenon in question a modicum of thought.

The final example in the quoted paragraph comes from kinship systems. The complexities of such systems have been the object of ongoing research since at least the 1871 publication of Lewis Henry Morgan’s *Systems of Consanguinity and Affinity of the Human Family*. So, it is hard to think of such data as constituting “distinctions we would certainly never think of”, especially given that much of interest lies on our linguistic doorstep (think of the detailed Latin system). The unthinkable step cannot be the addition of “even- and odd-numbered generations”, because one of the major roles of kinship systems is to determine who is to rely on or support whom and whose group membership excludes them from marriageability; if generation number determines this, then its reflection in a kinship system is not particularly surprising. Nor can the unthinkable step be the entry of such distinctions into the pronominal system, because pronominal systems frequently mark social status (think of the royal *we*), and kinship relations, as just mentioned, can encode who is to defer to or assume responsibility for whom. So, really, like all the preceding examples, the data has not received “proper consideration” before unwarranted labels of “semantic distinctions we would certainly never think of” have been applied. (№33)

The only part of the cited list of examples that gives me pause is the existence of “triangulate[d]” kinship terms: “the one who is my mother and your daughter, you being my maternal grandmother”. The day before writing this paragraph, I was asked by my mother whether I had seen ‘gran’, that is, her own mother. Similarly, my grandmother will refer to my mother, her daughter, as ‘mum’ when addressing me. These are almost the cases that the cited passage describes and ever since I was young, I have been struck by the oddity of such uses of kinship terms. Nowadays, in fact, I only use ‘mum’, say, as a vocative or when talking about my mother to my brothers or

plate stood in the corner, to indicate a non-prototypical position. If Athabaskan does likewise, then nothing more need be said. If it does not, then we need to add a prototypicality condition, namely, that English verbs of position, in order to be Athabaskan, are determined on the basis of the prototypical position, not the position that the object occupies on a particular instance of usage. This is precisely what we find we certain *-er/-or* nominals in (e.g., my father’s dialect of) English: a female *waiter* is a *waitress*, but a female *loiterer* is not a *loiteress*, and a female *actor* is a *actress*, but a female *(re)enactor* is not a *(re)enactress*. English verbs, then, need only to be poured into the mould of English nouns like *loiterer* and *enactor* in order for the required invariance to obtain.

spouse; under most other circumstances, I use the expression ‘my mother’ (a fact, to my surprise, in evidence in an earlier essay of mine on ‘family values’, Harbour 2006). What I personally find striking then about the systems that the passage describes is not their unthinkability, but the fact that I have unthinkingly felt their lack in my own language.

The foregoing discussion of *Myth*’s other “semantic distinctions we could certainly never think of” serves several purposes. First, it shows that the assertion that Kiowa, or Jemez, whichever was intended, “was one of many [examples], and the rest still stand” does not stand at all. Second, it reinforces that the same ‘immunity from proper consideration’ that led to various errors in the discussion of Kiowa-Tanoan (not the mere mixing up of names) are pervasive in *Myth*’s treatment of data. And third, and most importantly, it shows the unproductiveness and scientific inutility of *Myth*’s approach to languages: if one’s mindset is never to look beyond the surface variation between languages, then all one has is a *Wunderkammer* (to resume Pesetky’s 2009 use of term), an approach that cannot deliver “a much better understanding of the nature of language and the cognition that makes it possible”. The more inconsistent the approaches which different descriptions use, the better, as superficial discrepancies and disparities disguise underlying unity; they provide yet more *Wunder* for the *Kammer*. In particular, this approach clouds mythicists vision to the extent that *Myth* misses the similarity between, say, Athabaskan and its authors’ mother tongue or between Central Pomo and one of Europe’s major languages. What chance then of showing, as Harbour (2009) did, that:

To explain Kiowa requires just two primitives (rooted in Thomas 1955), and these same primitives derive two robust Universals ... and two semantic Surprises ... from unrelated languages.
(Harbour 2009:457)

That is, if one’s mindset and research agenda prevents one from perceiving the unity between languages even at the level of superficial, pretheoretic description, what chance does one have of discovering whether any abstract theory of universally available primitives and algorithms will capture cases that are superficially even more divergent?

As already shown, such imperspicuities of thought are not confined *Myth*’s treatment of data. The article also makes very basic errors of logic. Recall, for instance, statement (12b), cited above: to refute the claim that languages

with dual must contrast singular and plural, *Myth* adduces a language which, apparently, does not do so *in its verb system*. Elementary logic alone tells us that this fails to falsify (12b). And like all the other methodological errors reviewed above, these are repeated beyond the domain of narrow focus in the sections 2–4. For instance, continuing with the *Wunderkammer*, a criticism levelled by Pesetsky and by Tallerman, we find that Evans and Levinson write the following:

... the reader should note the argumentation of these rejoinders: that we, Evans & Levinson (E&L), have cherry-picked exotic facts about language A, but look, language B yields to the normal universal analysis, so there’s no reason to take A seriously. Since absolute universals can be falsified by a single counterexample, it is a logical fallacy to defend a universal by adducing facts from some other language which happens not to violate it. (page 476)

The last sentence is completely true—universals are false if there is even a single counterexample—but the authors of *Myth* err in their application of this basic fact however. If languages A and B exhibit similar phenomena irreconcilable with a “universal analysis”, and if careful work has shown B to be in fact amenable to such analysis, then A loses its force as a counterexample: anyone who wishes to claim it as such must show that no B-like analysis is available for A. Or, in other words, the universal statement that there can be no analysis for facts like those in language A is falsified if there exists a single “universal analysis” of any similar data set, like B.¹⁹ (№34)

In such cases, the burden of proof falls equally to both camps. For the universalist camp to be right, analyses must be adduced for all, or a representative sample, of the data. For the mythicist camp to be right, data must be presented for which it can be demonstrated that no “universal analysis” can be adduced. Of course, both strategies can only be evaluated over the long term: universalists’ theories develop in the light of recalcitrant data and only when such theories have developed can mythicists determine whether there

¹⁹The phrase “yields to the normal universal analysis” is also misguided. Analyses of language B’s generally require considerable ingenuity, both theoretical and empirical, not mere rote application of preexisting templates, as the phrase “the normal universal analysis” suggests to me. For this reason, again, superficial treatment of data *à la Myth* simply cannot serve to advance the debate: what is required is effort, time, and care: giving due diligence to data and competently drawing out the analyses’ logical consequences.

are data that are, in principle, incapable of analysis. Whether the authors of *Myth* have the clarity of logic and the propensity for “proper consideration” required to make this assessment is a matter on which readers individually must decide.

7. Conclusion

One could clearly continue to raise the tally of errors and failings in *The Myth of Language Universals* by subjecting yet more sentences to individual scrutiny. I, for one, do not see the point. Examination even of very limited material reveals that the article and its subsequent defence is more than “partially immunized against the proper consideration of language diversity” and the theories that account for it: from omission of references and errors in citation, to misrepresentation both of data and theory and of other researchers, the article’s commentators and the authors themselves, to ignoring of inconvenient counterarguments and making of errors of basic logic, and ending up at blatant falsehoods about analyses which, apparently, have not been read before errant judgment has been passed on them. Such failings are a pervasive feature of *The Myth of Language Universals*, affecting material written at different times, in different sections, on different data, on different themes, and in different organs of publication. And the superficiality that characterizes *Myth*’s handling of all the data discussed above strongly suggests that this will be an unproductive, unrevealing, and unscientific approach to “understanding of the nature of language and the cognition that makes it possible”, failing, as it does, to find unity even when this involves looking no further than a major European language or indeed the authors’ native tongue. It behoves the field to consider whether *The Myth of Language Universals* can be assumed, in good faith, to have exercised the “proper consideration” it urges and whether its case should be regarded as having been made adequately, if at all.

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