How field linguists have been investigating linguistic diversity:  
Commentary on Davis, Gillon, and Matthewson

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I begin by congratulating Davis, Gillon, and Matthewson (DG&M; 2014) on this clear, eloquent, provocative, and insightful article. There are many points in this article on which the so-called ‘D-linguists’ and ‘C-linguists’ will wholeheartedly agree, regardless of their theoretical backgrounds.

My comments focus on five areas where a more nuanced approach might have been helpful. I conclude with a substantial objection to the article. This objection has more to do with DG&M’s perception of fieldwork as conducted and defined by ‘D-linguists’ than with the investigation of linguistic diversity.

I first consider the following statement in the ‘fieldwork methodologies’ section (p. e189):

Of course, this [reliance on grammatical intuitions of native speakers — WdR] entails a methodological commitment to the validity of such intuitions. In our own experience, we have found that our consultants—many of whom do not read or write their first language, and almost none of whom have any formal training in linguistics—are remarkably consistent in their judgments of very complex structures, over literally years of elicitation.

To evaluate this quote fairly, let me be clear that we agree on the validity of intuitions, and that intuitions are useful in semantic and syntactic elicitation, indeed in any sort of elicitation. What puzzles me in the quote above is the assertion that linguistically unsophisticated speakers are ‘remarkably consistent’ and are this way ‘over literally years of elicitation’. My own experience with speakers of two endangered languages of North America, Western Apache of Arizona (twenty years of fieldwork experience) and Hän Athabaskan of Alaska and Yukon Territory (seven years of fieldwork experience), is surprisingly different. Like the authors, I cultivate and value a close personal and collaborative relationship with the speakers of these languages, but I do not find this remarkable consistency at all. On the contrary, 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. There is not only interspeaker variation, but intraspeaker variation as well. It is not unusual for the same speaker to volunteer a form and, a few weeks, months, or years later, reject it as something s/he would never say. So let me say that I am puzzled by DG&M’s report of the consistency of the Northwest Coast speakers. I must assume that the elders of this area are more native-language dominant and/or have much better language recall.

It may be that the authors report more consistency because their focus is on syntactic and semantic elicitation, whereas most of my Athabaskan fieldwork involves morphological elicitation. But then again, 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. In any case, our contradictory experiences are worthy of mention.

Of course, the degree of language endangerment has something to do with it. Speakers of Western Apache, part of a community of c. 6,000 speakers, are not nearly as in-
consistent in their judgments as are the nine remaining speakers of Hän Athabaskan. It is well known that terminal speakers tend to increase morphological complexity and do so in inconsistent ways, as documented for a Scottish Gaelic variety by Dorian (1981).

My second comment has to with §3.1, ‘Condition C meets Nuu-chah-nulth’. I have no reason to doubt the accuracy of the data or DG&M’s conclusion that condition C does not hold in the Nuu-chah-nulth language (or in other languages). What I am curious about is n. 13 (p. e191), reproduced in full here:

As has been known at least since G. Evans 1980, these judgments can be affected by focus, or more broadly by a distinction between presupposed and asserted content. Focus has been controlled for in the Nuu-chah-nulth examples given here.

But how does one reliably control for focus when eliciting data from elderly speakers of an endangered language? One has to find a way to tell the speaker something like the following (regarding DG&M’s example on p. e192):

Mind you, 10b is not supposed to be translated as any of the below (a–c):

a. She said that she, (i.e.) LUCY, will make bread tomorrow.
b. She said that she, that’s LUCY, will make bread tomorrow.
c. She said that she, we are talking about LUCY, will make bread tomorrow.

Now, are you completely sure that none of the above are the best translation for 10b, and that the best translation of 10b is really just ’Lucy said that she will make bread tomorrow’?

To be sure, none of these English sentences are pragmatically felicitous, but they are all acceptable violations of condition C, allowable because of the strong focus on Lucy. Now this is the elicitation script I would use to control for focus and make sure that there is an actual violation of condition C. But this script is obviously a roundabout and clumsy way of eliciting things. Can we expect the elderly native speaker to understand such subtle pragmatic distinctions, and to translate them accurately?

It would be useful to have a script showing how DG&M themselves manage to control for focus without confusing the speakers. From my experience, this case is one where I would not trust the results of controlling for focus, and where it would be nice to encounter an example in a naturally produced text, just to be sure.

My third comment has to do with the noun-verb distinction in Northwest Coast languages (§4.1) and the methodological conclusion the authors reach (§4.2). DG&M’s discussion of the noun-verb lexical distinction is quite convincing, and it is clear that Salishanists and Wakashanists are a bit more circumspect about this issue than they used to be. However, I disagree with DG&M when they assert that the ‘noun-verb distinction’ hypothesis took a long time to evolve. A brilliant article by William Jacobsen, ‘Noun and verb in Nootkan’ (1979), early on questioned the premise that the noun-verb distinction was not relevant for Wakashan. It did not take ‘C-linguists’ or a special methodology to discover this. Jacobsen, an exemplary D-linguist (or at least certainly not a C-linguist), set the record straight seven years before van Eijk and Hess (1986), and sixteen years before Matthewson and Davis (1995).

My fourth comment concerns DG&M’s opinion that corpus-based linguistics is more problematic with endangered languages than with languages with large numbers of speakers (p. e218). Undoubtedly, a corpus of texts gathered from an endangered language will always be relatively small, and never as representative as a large corpus of the kind easily obtained from languages with many speakers (and even these can be inadequate, when based largely on written samples or formal genres). But this does not, as the authors conclude, imply that targeted elicitation is the methodology of choice in
dealing with endangered languages. As the authors must have noted from their own field experiences, one never can tell in advance the language or language consultant proficiencies of an elderly speaker of an endangered language. On the one hand, a respected elder might be the well-known authority ‘who knows the names of all the parts of the dogsled’, but might no longer be able to put two sentences together. On the other hand, a different elder, who might not be considered an expert and is never asked about his/her language, might turn out to be the one who, to everyone’s surprise, blurs out perfectly natural conversational sentences, or happens to have memorized parts of traditional stories that the recognized experts have forgotten. One never knows. So while a fieldworker would indeed not expect elderly speakers who are no longer dominant in the indigenous language to be good producers of texts, s/he nevertheless needs to check on their ability to produce texts and then record whatever pieces of text, no matter how small, can be produced. It is possible that such pieces will not tell one anything new about linguistic diversity, but one cannot tell this in advance. So I would argue that both corpus work and elicitation are equally important in documenting linguistic diversity, regardless of the endangerment status of the language.

My fifth comment has to do with the implication, throughout the article, that descriptive grammars are insufficient for documenting linguistic diversity because they do not provide negative data, and tend to be text-based rather than based on targeted elicitation. There are references to descriptive grammars such as the following:

In his excellent text-based descriptive grammar, van Eijk (1997) is circumspect about the unmarked word order of transitive clauses with two overt arguments. The reason is simple: his corpus contains exactly six examples, two of them with VOS order and the other four with VSO order. (p.e186)

We very much doubt that the data in 69–73 would have appeared in even the best descriptive grammar. (p.e210)

Descriptive grammars are (naturally) not typically based on targeted fieldwork that tests concrete hypotheses about modal semantics. (p.e216)

We would like to clarify again that the issue here is not with the descriptive grammars themselves, which may be excellent. The problem arises when data and generalizations are used for purposes for which descriptive grammars are inherently inadequate. (p.e216)

As we have shown, reliance on secondary sources designed for other purposes (i.e. traditional descriptive grammars) cannot provide the empirical foundation for a scientific typology in the areas of syntax and semantics. (p.e219)

These statements about descriptive grammars lead me to my last, and most serious, point of criticism. The assumption, mostly implicit, but sufficiently clear if one rereads the quotes above, seems to be that, since descriptive grammars are based on corpora and not on targeted elicitation, they miss an awful lot of things. Actually, I do not disagree that descriptive grammars can miss a lot of things of interest to students of linguistic diversity. What I object to is the implied mischaracterization of D-linguists, further shown in this quote:

Instead, they [Evans and Levinson—WdR] fall back on the use of corpora—the most traditional of all field methodologies, dating back to the text-grammar-dictionary model established by Boas. (p.e218)

The assertion that descriptive grammars are based only on corpora and not on targeted elicitation reflects a misunderstanding of the history of descriptive linguistic fieldwork. It is obvious to anyone who has read a reference grammar written by Boas (Boas & Deloria 1941 is an excellent example) that much of the description did not originate from text mining, but was acquired through systematic or targeted elicitation. The same can
be said of the grammars written by the other founding figures of D-linguistics, Edward Sapir and Leonard Bloomfield.

So, systematic or targeted elicitation was practiced by the founders of scientific linguistic fieldwork right from the beginning. The difference between them and the C-linguists is that they did not promote it as the sole methodology to be used. They saw the efficacy of a methodology that necessitated a constant back and forth between text collection and elicitation, each informing the other. It is well known that both Boas and Bloomfield tended to depart from texts, and data acquired through elicitation and confirmed through occurrence in a text were highly valued. It is unfortunate that these scholars were rather discreet about their elicitation procedures. But there is no doubt that they complemented their text collection with heavy doses of targeted elicitation. Bloomfield even spoke Ojibwa to his consultants to see how they would react to his sentences (Voegelin 1959:114–15). To say otherwise would be rewriting history.

In reality, the methodology of concurrent text gathering and targeted elicitation is the way D-linguists have actually been carrying out fieldwork ever since Boas, and, as argued in Chelliah & de Reuse 2011, it is still the most reliable way to conduct successful descriptive fieldwork.

Thankfully, in n. 1, DG&M mention their awareness that the ‘D-linguist’ and the ‘C-linguist’ labels are caricatures. Their willingness to use D-linguistic methods is commendable, and indeed they are correct in assuming that ‘D-linguists equally employ C-linguistic methods where the need arises’ (p.e181).

Nevertheless, it is not enough to strike this conciliatory tone. I wish DG&M had avoided the strawman ‘of the traditional text-based models (whether in their original Boasian guise, or in technologically enhanced current incarnations)’. I have argued in this response that there has never been such a thing as ‘traditional text-based models’, and that Boas, Sapir, and Bloomfield themselves used C-linguistic methods, anticipating the Chomskyan revolution by many years.

Let me hasten to say that I am aware that certain corpus linguists hold that claims about language are not reliable unless they are found in a corpus and statistically quantified. These claims make sense if one takes the extreme view that only corpus data can provide accurate grammatical insights. Presumably like DG&M, I find such views limiting of scientific inquiry. In any case, corpus linguistics is a methodology totally different from the D-linguistic methods in the Boas, Sapir, and Bloomfield tradition, and one that was developed quite recently. DG&M make the mistake of conflating these two traditions.

To conclude, let me express the hope that the article under discussion, as well as my comments, will be interpreted as a contribution to better communication between C-linguists and D-linguists, in their efforts to document language diversity. On the one hand, D-linguists can learn from the rigorous and hypothesis-driven elicitation as is carried out nowadays by C-linguists such as the authors. On the other hand, C-linguists should come to appreciate that many interesting typological features or parameters of the languages of the world, such as ergativity, evidentiality, head-marking versus dependent-marking, inverse markings of several sorts, mirativity, OVS and OSV constituent orders, split intransitivity, or switch-reference, were discovered by D-linguists, who happened to notice something unusual in the data they gathered.

The point of my remarks in the preceding paragraphs has been to emphasize that the D-linguists did not just sit on the phenomena they discovered, but have used, ever since the beginnings of field research, targeted elicitation to further elucidate the phenomena in question.
REFERENCES


Davis, Henry; Carrie Gillon; and Lisa Matthewson. 2014. How to investigate linguistic diversity: Lessons from the Pacific Northwest. Language 90.4.e180–e226.


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