The Interplay between Universal Grammar, Universals, and Lineage Specificity: Some reflections raised by Dunn, Greenhill, Levinson and Gray 2011

Mark C. Baker

Rutgers University

The prominent publication of Dunn, Greenhill, Levinson, and Gray's (DGLG) (2011) article in *Nature*, together with the associated media attention, gives typologists and theoreticians a welcome opportunity to clarify and debate what we mean by universals, and what evidence one should expect to find for or against such universals. In these remarks, I take advantage of this opportunity from a formal-generative-Chomskian perspective.

It is well-known that within the Chomskian research tradition the central theoretical notion has not been "(language) universal" but rather "universal grammar". This has been consistently defined over many years as whatever knowledge or biases human children use to interpret the linguistic data they experience, contributing to the shape of the linguistic knowledge they end up with when they mature (Chomsky 1965, Chomsky 1981). This is assumed to be essentially uniform across the human species, apart from genetic abnormalities (e.g., Specific Language Impairment); hence the "universal" in "universal grammar". This assumption of universality can be justified empirically quite apart from any results of typological research per se. It is grounded by the simple fact—uncontested as far as I know—that children from all genetic stocks seem to acquire indistinguishable competence in whatever language they grow up being exposed to. For example, living in the suburban USA, it is easy to observe children from

East Asia, South Asia, Africa, Native American groups, and so on all acquiring perfect American English.[1] It is possible that careful inquiry will show that this is something of a simplification, because there are after all subtle differences in the final linguistic competences that different children arrive at, or in the stages that they go through to reach those competences. Such results could be very interesting. But it seems that any such effects must be quite small, if they exist at all, given our extensive practical experience with immigration and its effect on language in the modern world.

What is somewhat less certain is the relationship between this notion of Universal Grammar and "language universals" in the Greenbergian sense (Greenberg 1963). My own impression is that some such connections can probably be found, and that it is desirable to do so. However, it is not logically necessary that there be such a connection, and it is not surprising if any relationship is a noisy one. The connection is likely to arise if one makes the additional assumption—as generativists typically do—that Universal Grammar makes a relatively large contribution to a normal speaker's final linguistic competence. If Universal Grammar provides a largish percentage of each person's mature linguistic ability, *and* if Universal Grammar is (approximately) invariant across the human species, *then* it is reasonable to expect that this could be observed by comparing the mature linguistic abilities of speakers who grew up in different cultural and linguistic environments, the way a typologist might. Any commonalities discovered in this way might be described as Greenbergian language universals. If so, there can be a

[1] This uncontroversial fact does not yet prove that there is much substance to Universal Grammar , only that whatever there is is effectively invariant across the species. It could be that UG is universal but effectively empty, or nearly so. Showing that there is significant substance to UG depends on taking into account more controversial observations about poverty of the stimulus (or on observing language universals); see below for some discussion. (I thank an anonymous reviewer for helping to clarify this point.)

productive and supportive relationship between Chomskian syntax and Greenbergian typology, in approximately the ways they are usually understood.

It is important to clarify, however, that there is no reason to expect a simple, linear relationship between the primary linguistic data of particular languages and Universal Grammar. If the relationship were a linear one, then it would be relatively easy to subtract out the influence of the particular language being learned and everything that is left should be pure Universal Grammar, essentially identical across languages. For example, one might imagine that the competent Mohawk speaker controls some 10,000 constructions (in something like the Construction Grammar sense), of which 40% are specific to Mohawk and 60% come from Universal Grammar. Then one could find the true Greenbergian universals simply by ignoring the first 40% and concentrating on the second 60%, being sure to compare those constructions to the corresponding constructions used by (say) a Chichewa speaker, or a Mapudungun speaker.

But we can be pretty sure that the true situation is not always going to be that simple. Generative linguists do not conceive of Universal Grammar as a stock of complete constructions that speakers draw on to fill out their language. Rather it is the lens through which they interpret any patterns they hear in the language around them, influencing what they internalize from those patterns. Given this, we should not expect to find that, for any given language, 40% of its constructions are language particular and 60% are universal. Instead we expect to find that 100% of the constructions of a language are a function of both what speakers hear said around them and their innate expectations and biases—perhaps in a 40-60 ratio. On this view, it can still make sense to try to quantify the relative contributions of a particular culture and linguistic experience on the

one hand and of innate knowledge and expectations on the other hand, but it will not be by simple subtraction and direct point-by-point comparison.

Speaking autobiographically, my fullest opportunity to think through these issues was my effort to analyze Mohawk and other polysynthetic languages, comparing them to configurational languages like English (Baker 1996; see Baker 2001a: ch. 3 for a simplified popular version). What I found was the following. When one compares Mohawk to English construction-by-construction, sentence-by-sentence, or paradigm-by-paradigm, one finds extensive differences. These differences involve not only word order and morphological marking, but even question formation patterns and patterns of pronominal anaphora, which generativists expect to be relatively universal. However, I also found some systematic patterns in the differences. It turned out that these patterns could be captured by saying that the abstract grammatical principles that (in a generative approach) define what constructions are possible for a Mohawk speaker are extremely similar to those that define what is possible for an English speaker. The best overall analysis is not that pronouns work differently in Mohawk from in English, and that question words move differently, and that complex words are formed differently, and that words are grouped into phrases differently. Rather, the best overall analysis is that there is one important difference in the syntactic structures of the two languages: roughly, the direct object is inside the verb phrase in English, but loosely adjoined to the clause as a whole in Mohawk. Since the structure of the clause is different in this one well-defined way—*and* since how linguistic phenomena like anaphora and question formation work are defined in structural terms within Chomskian-style research[2]—the same principles

[2] The structure-dependence of Chomskian syntax need not be a sticking point here, however. The same conceptual point can be made within other representational systems, which express approximately the same

give different patterns of observable phenomena in English and Mohawk. In short, fixed grammatical principles plus a specifiable difference in the base structure that those principles operate over yields systematic differences in the observed linguistic patterns. I thus concluded, paradoxically but (I believe) soundly, that the surface facts of Mohawk are so different from the surface facts of English precisely *because* Universal Grammar is rich and invariant. That means that a small difference in the right place can have large consequences throughout the language. To my mind, this illustrates the nonlinear relationship that one must be prepared to find between the surface patterns of a language and Universal Grammar—complicating the task of finding language universals in the standard sense, but not rendering it impossible. And this seems to me fairly typical of what I have found on smaller scales in twenty-five years of analyzing languages from different parts of the world.

For purposes of comparison with other typologists, then, let us ask how these notions of the core theoretical concepts interact with what BGLG have done in their article, and with similar recent proclamations by contemporary typologists who are heavily influenced by evolutionary models and new statistical techniques.

First, I want to make clear that I have no deep disagreement with what BGLG are trying to do in a local sense. I think it is true that, if the sense of universals that I care most about is correct (Universal Grammar), then one should expect to see that different features of a language will change in linked ways over time. Finding out whether this actually happens in large and well-studied language families would be a welcome new method, supplementing the Greenbergian method of comparing unrelated languages. So

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fundamental relationships in different ways, not in terms of phrase structure per se. See Baker 2001b for some discussion of what the real issue of phrase-structure dependence amounts to.

is using statistical techniques in an effort to discern which differences are truly linked and which just happen to cooccur sometimes by accident, which is part of what I take DGLG to be doing. Chomskian views about Universal Grammar and parameterization do not come with any official model of language change (although some people have developed models within these terms), and such studies could be very valuable in helping us to develop such models, or to make whatever adjustments in our foundational assumptions might be required in order to flesh out the theory in these directions.

That being said, I do find troubling two features of the DGLG article, and since those features are shared with other prominent work, they are worth some discussion. One is the bold and (over)sweeping conclusions drawn from what is in certain respects a rather small and modest study. The other is the recommendation of increased methodological sophistication along one dimension (statistical modeling) while apparently being content with a low level of sophistication along other dimensions (like bread and butter grammatical analysis). I explain in brief.

First, since the Chomskian notion of Universal Grammar concerns relatively abstract aspects of linguistic knowledge, it is potentially misleading to group it together with Greenbergian typology in the way that DGLG do. This was no doubt convenient for expository purposes in a short paper, and there may be an element of truth in it, but it may mislead as much as help. In particular, it affects the way DGLG's results can be interpreted more broadly. For example, word order is *not* the domain of universals par excellence for the Chomskian. On the contrary, the paradigmatic Chomskian universals concern matters like structural dependence, island conditions restricting movement, conditions on the coreference of NPs, and so on. Chomskian Universal Grammar is

posited initially and primarily to address "poverty of stimulus" issues, where people's demonstrable grammatical behavior seems to be underdetermined by the data they have plausibly been exposed to. But there may be no "poverty of stimulus" issue when it comes to basic word order properties of the kind DGLG have studied. For each of the eight features they consider, any child in a normal speech environment probably hears hundreds of exemplars in any given week, so they could plausibly learn these features from positive evidence. As a result, word order (together with related areas of grammar, like agreement and case marking) are arguably the last place one would expect Universal Grammar to play an important role, hence the last place that you might expect it to induce observable typological universals.[3] This has indeed given word order universals a special interest to me, for one, largely because it provides a kind of a fortiori argument. To the extent that there are some (statistically) valid Greenbergian universals in this area (and I still find some of them rather impressive), even where one has the least reason to expect them given general considerations, that suggests that the influence of Universal Grammar on particular languages very broad indeed. But some of my generative colleagues think that I am too easily impressed, and if there turn out to be no significant universals of

[3] But there are caveats to add here, which may be important. Implicit in the Greenbergian word order universals, and explicit in their generative reconstructions, is the claim that the order between two expressions is fixed *when they are contained in the same phrase*. For example, a preposition must precede an NP in English *when they form a PP* (not otherwise); a verb must precede its object in English *when they form a verb phrase*. So the word order generalizations are only defined relative to a notion of phrase structure (or its formal equivalent, such as dependency relations), and these phrase structure prerequisites may not be so transparently available to a child learning the language. If so, then poverty of stimulus considerations might be expected to apply in the area of word order after all. I believe this simple fact is underappreciated. It is a potential illustration of how hard it is to get away from a degree of what is called abstractness in linguistic analysis, and how important the need for some innate presuppositions is even for matters that look straightforward.

In a similar vein, two anonymous reviewers point out that the word order universals also presuppose the ability to discern abstract categories and word classes—such as recognizing that a given set of sounds is an adposition or a verb and inferring that all other members of these categories will pattern similarly. Those are far from trivial matters as well, not obviously presented in the surface facts. So there may be additional poverty of stimulus issues here, hence additional need for Universal Grammar.

surface word order, very little about the paradigm would have to change to preserve its viability and intellectual honesty.[4]

Now the line of thought that underlies BGLG's more sweeping claims seems to depend on the idea that word order universals are the best-supported and least controversial of the linguistic universals. Therefore, if those fall before their style of inquiry, then all universals are likely to fall with them. That may or may not be a reasonable posture to take with respect to Greenbergian typology, but it certainly does not carry over to the generative version. (For a much better test case for or against the generative view, consider what I call "the verb-object constraint" in my (2010) commentary on Evans and Levinson 2010.)

To illustrate how the statistical sophistication of DGLG belies a certain conceptual and methodological naivety in other respects, I comment briefly on some of their specific results. First, they claim that some of the expected word order linkages are not in fact found in the majority of language families. Provocatively, even the implicational relationship between adposition-noun order and verb-object order—perhaps the strongest of the Greenbergian universals according to Dryer (1992, 2005), and the one with the most straightforward interpretation in generative terms—is supported in only two of their four language families (Indo-European and Austronesian), they claim. But this has every appearance of being an artifact of choices that they presumably made for

[4] While I and others have not used Greenbergian word order phenomena as a paradigmatic illustration of Universal Grammar, I have sometimes used it as a paradigmatic illustration of the notion of a parameter (e.g. Baker 2001a). Parameters project roughly onto the Greenbergian idea of implicational universals, as opposed to absolute and statistical universals—with the same caveats about abstractness and nonlinearity. But the scope of these remarks does not permit me to unpack all these points in detail, and parameters in this sense are a less deeply embedded feature of the generative approach than Universal Grammar is.

reasons of convenience or feasibility, rather than because those choices approach the methodological ideal.

Why is no linkage between these features found in the Bantu family? Because every Bantu language has both verb-object order and adposition-noun order! That may be a valid statistical reason for throwing out the Bantu family, if one's statistic is designed to measure linked variability and there is no variability to measure. But it seems bizarre in a larger context to then imply that there is no evidence of a link between P-N and V-Object orders in Bantu. On the contrary, it seems that the link is so strong that it has helped the Bantu languages resist change in this area up to this time depth. If so, then Bantu ultimately supports the putative link between these word orders in Universal Grammar. At the very least, if we throw out Bantu entirely, we should say that the link exists in two families out of three (not two out of four), which is a majority.

The issues posed by the Uto-Aztecan family could prove more interesting. But here the statistical issue that could arise is that this is by far the smallest of the four language families considered. It also happens to have several languages with postpositions and verb-object order, which we know from Dryer 2005 is quite rare worldwide. Presumably the fact that this combination happens to exist in larger than normal numbers (three languages) in a smaller than normal family contributes heavily to the fact that no linkage is detected here—even though the family does have both consistent head-final and consistent head-initial branches.

Overall then, despite their apparent statistical sophistication in comparing languages within families, the authors show little sophistication for comparing across families—and that sort of comparison is a crucial part of their argument. If they

expanded their study beyond Bantu to the larger Niger-Congo family, they would encounter the Ijoid languages, which are postpositional and object-verb (Williamson 1965). Then they would have variability to measure, and they would (probably) find linkage between adposition-noun order and verb-object order in that family too. And if they expanded their study beyond four language families to include enough families so that we could compare them statistically, they might well find that Uto-Aztecan is (interestingly) anomalous, not reasonably typical. I am sure that these upgrades would be very challenging to carry out, but until one can do a better job on this, the authors' bold negative conclusions are not very convincing.

Similar points can be made for the strong linkages that DGLG claim to find internal to some families, which (they say) traditional word order typology does not expect, such as adjective-noun order being linked to numeral-noun order in Austronesian (only) and adjective-noun order being linked to genitive-noun order in Indo-European (only). From a generative perspective, I find it not all surprising that these family-specific linkages should exist. To explain why, let me discuss instead the conceptually similar case of demonstrative-noun order—another of the "messier" Greenbergian universals, included in DGLG's analysis but outside the blue region in their diagrams. Now it seems clear that, syntactically speaking, there are at least two things that demonstratives can be: they can be members of the same category as articles and some quantifiers ("determiners"), as in English, or they can be a subcategory of adjective, as in the Nigerian language Edo. It is not hard to see the difference, at least in this pair of languages. In English, articles and demonstratives appear in the same position relative to other material in the noun phrase, and they cannot appear together: *that big book*, *this big*

book, **the that big book*, **that the big book*. In Edo, the definite article appears before the noun, demonstratives are like adjectives in appearing after the noun, and the two can appear together perfectly well (*néné èbé* 'the book', *èbé nàà* 'book this', *néné èbé nàà* 'the book that' cf. *èbé negedegbe* 'book big') (Agheyisi 1990: 81-85). Now if there are at least two things that demonstratives can be crosslinguistically, a subtype of adjective or a subtype of determiner, then the traditional class of demonstrative is not a theoretically significant natural kind, and it should not treated as an independent data point. However, in language families that have demonstrative adjectives, we should expect demonstrative-noun order to be linked (tightly) to adjective-noun order, whereas in language families that have demonstrative determiners we should expect demonstrative-noun to be linked (perhaps more loosely) to verb-object order. (This assumes that determiners take NPs as their complements in much the same way that verbs take objects as their complements—the standard generative analysis since Abney 1987.) This will look like linkages that are strong in one family and absent in another family, just as the authors found. However, such cases would tend to support rather than undermine Universal Grammar and language universals when they are properly applied (i.e., when the true categories of the lexical items are properly identified before counting them).

I thought I would be able to make the same point about DGLG's figure 3, where they try to show that, even when there seem to be linkages in different families, the underlying dynamics of that linkage can be quite different, calling into question whether it should really be considered the same thing. In broad terms, this need not be a surprise to the generativst. As discussed above, the language competence someone acquires is a function of both the linguistic input they experience and their Universal Grammar. So if

different speakers hear different inputs (perhaps subtly different), Universal Grammar could lead them in quite different directions, in a nonlinear fashion. This could conceivably be what figure 3 is showing (but see below).

In this regard, it is potentially relevant that there are two ways that one might get postpositional order in an otherwise head-initial (hence verb-object) language, given the standard generative theory of phrase structure. First, there could be an ordering principle that insists on complement-P order inside PP, overriding the general rule of head-complement order that applies elsewhere in the language, giving the structure [$_{PP}$ [$_{P'}$ NP P]]. Second, the "object" of the P could be in the specifier position inside PP rather than the complement position, giving the structure [$_{PP}$ NP [$_{P'}$ P --]]) (cf. Kayne 1994). In contrast, only the analog of the first option would be plausible as an analysis of a prepositional object-verb language: saying that the "object" of P is really in its specifier position will never put it after the P given that specifiers (aka "subjects") overwhelmingly come before heads in languages of the world. So there are two easily available generative analyses for the first kind of language, and only one for the second kind. That might correspond to the fact that the first kind of mixed word order is noticeably more common than the second (found in 38 languages as opposed to 10, according to Dryer 2005). It might also explain why the natural paths of historical development for a postpositonal verb-object language are more varied than those for a prepositional object-verb language. Different phrase structures underlying this single superficial type might naturally evolve in different directions. If so, the apparent lineage-specificity would again have a natural explanation in universal terms, once one distinguishes the two distinct sources of N-P order.

But as I thought further about the prospects for developing this, I realized that I do not have the slightest idea from DGLG's article what figure 3 is supposed to mean. I assumed that it meant that postpositional verb-object languages tend to develop into normal head initial languages in Austronesian, but tend to develop into normal head final languages in Indo-European. But comparison with figure 1 proves rather mysterious in this regard: figure 1 displays only a single postpositional verb-object language among the Indo-European languages (Kashmiri), and not a single one among the Austronesian languages. So how can one make a reliable claim that such languages develop along different paths in the two families? There may be something I simply do not understand here. But it raises in my mind the possibility this is some kind of statistical artifact produced by DGLG's methodology, with no natural language meaning. (I rather hope not: the situation I thought was described in figure 3 is much more interesting!)

In conclusion, I find some aspects of this target article to be promising if not fully realized, others unsurprising, and still others problematic. Its more sweeping conclusion about linguistic diversity being purely the result of local cultural factors and "not seeming to be tightly constrained by universal cognitive factors" does not seem to me to follow from their study at all. Finally, to my mind, this and similar work does little to illustrate the real opportunities posed by larger linguistic databases and more sophisticated use of statistics. At least as urgent for the field is more sophisticated use of traditional grammatical analysis—those techniques that will show us whether a demonstrative is a subtype of determiner or a subtype of adjective in language X, whether the dependent of an adposition is its grammatical "subject" (specifier) or its "object" (complement) in language Y, and whether the object of a clause is in the verb phrase or not in language Z.

At least some of these issues will be familiar to typologists, but they are often not taken into account seriously enough. If we apply powerful statistics to noisy data, which has not been "filtered" in the relevant ways, we run the risk of magnifying the noise until we are no longer able to even detect the signal. I fear that this is part of what is happening with DGLG's negative results, and those of other works in the same genre.

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