The evidence for word order correlations

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1. Introduction

Dunn et al. (2011) argue for the following four theses. First, they argue that there is no evidence of a crosslinguistic word order correlation between certain pairs of elements for which a correlation has been claimed in the previous literature. Second, they argue that although some correlations do exist, they are specific to individual families and not universal. Furthermore, they argue that, at least for the data they consider, these correlations are never found in more than half of the families they examine and, in the majority of cases, in only one family. Third, they argue that there is evidence in some families for correlations that they claim the existing literature predicts should not exist. Fourth, they argue that cognitive factors do not play a major role in determining linguistic structure, at least as far as word order is concerned, and that (in their words) "cultural evolution is the primary factor that determines linguistic structure, with the current state of a linguistic system shaping and constraining future states".

For reasons of space and time, I will only be able to address some of their claims in this commentary and will address other issues elsewhere. I will argue in Section 2 that Dunn et al. are mistaken in the first of their claims; I will argue that there is good evidence for at least two crosslinguistic correlations; data in Dryer 1992 provides evidence for other correlations. I will also argue in Section 2 against their third claim: I will argue that the correlations which they find and which they claim contradict existing claims in the literature are in fact fully consistent with what is claimed in the existing literature. I will argue in Section 3 that Dunn et al. in fact cannot account for the existing data on word order correlations. In Section 4 I will address their second claim and will argue that the number of families they claim exhibit a correlation, as defined by their methodology, is approximately what we would expect under the view that

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these correlations are crosslinguistic. Detailed discussion of their methodology is beyond the scope of this commentary, but in Section 5 I will discuss one problem with it and will argue that the differences they find among families may be due entirely to random variation. In Section 6, I will discuss very briefly three issues I cannot discuss at length here. The first of these is that the role of contact in influencing changes in word order presents a serious problem for Dunn et al.'s methodology and conclusions. The second of these is problems with the very notion of lineage-specific correlations, a notion which is central to their claims. And the third is that, to a large extent, I believe Dunn et al. are correct in their general claim, that factors other than cognitive ones indeed play a larger role in determining word order.

2. The evidence for correlations

2.1. *Preliminaries*

For reasons of space, I will discuss only five word order correlations here; in three of these cases I will present evidence for a crosslinguistic correlation. The evidence in this section is evidence that I have discussed elsewhere (especially in Dryer 1992), but it is evidence that Dunn et al. do not discuss; in fact, they provide no account at all of previously published results.

The data in this commentary are taken from my current database, which is very close to the data in Dryer 2011a, b, d, which is itself an update on the data from the same chapters in Haspelmath et al. (eds.) 2008 (Dryer 2008b, c, d). The data in these chapters from Haspelmath et al. (eds.) 2008 is the basis for most of the data on which Dunn et al. base their claims. The exception is Indo-European, where they have supplemented my data from other sources considerably.

The method that I use for testing for correlations, discussed in detail in Dryer 1989a, is first to count the number of genera containing languages of each type under investigation (where genera are language groups roughly comparable to the familiar subfamilies of Indo-European) within each of six roughly continental-sized areas of the world and then to compare what I call proportions across these six areas (as explained below). The rationale behind this method of testing for correlations is an attempt to address the problem of independence. As discussed in Dryer 1989a, there are large scale areal phenomena covering wide areas within continents that mean that different language families are often not independent from each other within the same area.

2.2. *The correlation between the order of verb and object and the order of adposition and noun phrase*

The data for numbers of genera containing each of the four types defined by the two possible orders of verb and object and the two possible orders of adposition

and noun phrase is given in Table 1. The larger figure for each area for the two orders of adposition and noun phrase is in boldface. For example, the "22" in the lefthand column of Table 1 under "Africa" means that there are 22 genera in my database that contain OV&Po languages (i.e., languages that are OV and that have postpositions) and it is in boldface because it is greater than 3, the number of genera in Africa that contain OV&Pr languages. The second to last column of the table gives the total number of genera of each type. The last column gives the total number of languages of each type.

Table 1. *Data on the correlation between the order of verb and object and the order of adposition and noun phrase, in terms of number of genera*

	Africa	Eurasia[a]	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Total	Number of languages
OV&Po	**22**	**29**	**14**	**70**	**28**	**40**	203	472
OV&Pr	3	2	1	3	0	1	10	14
Prop Po	.88	.94	.93	.96	1.00	.98	Average = .95	
VO&Po	8	6	0	3	4	6	27	42
VO&Pr	**28**	**8**	**35**	**15**	**23**	**12**	121	456
Prop Po	.22	.43	.00	.17	.15	.33	Average = .22	

a. The term "Eurasia" as used here does not include Southeast Asia, which is treated as part of the third area, Southeast Asia and Oceania. See Dryer 1992 for more detail.

We can see that in Table 1, in all six areas, there are more OV&Po genera than there are OV&Pr genera and there are more VO&Pr genera than there are VO&Po genera.

The general statistical test, however, involves comparing proportions. The two lines labeled "Prop Po" (i.e., proportion of genera containing languages that have postpositions) in Table 1 give the proportion of genera in each area based on the two lines preceding that line. The proportion of genera is the number on the first line as a proportion of the sum of the numbers on the first and second lines. For example, for OV languages in Africa, this proportion is .88, computed as 22 out of 22+3 or 22 out of 25. The data from these two lines showing proportions is shown again in Table 2, where now it is the higher of the two proportions for each area that is in boldface.[1]

1. Because occasionally two types are represented in the same genus (e.g., both OV&Po and OV&Pr types are found in Iranian), the proportions in Table 2 are not exactly proportions

Table 2. *Proportions of genera that contain languages with postpositions*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Average
OV	**.88**	**.94**	**.93**	**.96**	**1.00**	**.98**	.95
VO	.22	.43	.00	.17	.15	.33	.22

The general test, based on these proportions of genera, is as follows: whenever the proportions of genera are greater in all six areas for one type, then we have evidence for a correlation. Table 2 shows that the proportion of genera containing languages with postpositions is higher among OV languages than it is among VO languages for all six genera, so we can conclude that OV languages exhibit a greater tendency than VO languages to employ postpositions rather than prepositions.

It is important to emphasize that this test does not assume that the genera in each area are independent, only that the six areas are independent of each other. The test is thus equivalent to flipping a coin six times and getting six heads. Just as there is only one chance in 32 of getting six heads or six tails in six coin flips, there is only one chance in 32 that one proportion will be higher than the other in all six continental areas.[2]

In fact, as discussed in Dryer 2009, one can also apply the test by counting languages rather than genera within each area, since the method makes no assumption about independence within areas. The relevant data of this sort is given in Table 3, with the comparison of proportions (in this case the proportion of languages) given in Table 4.

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of genera as one might understand this expression. Rather, as implied in the main text, they represent the number of genera that contain OV&Po languages as a proportion of the sum of the number of genera that contain OV&Po languages and the number of genera that contain OV&Pr languages.

2. More specifically, there is one chance in 64 that one of the proportions will be higher in all six areas and one chance in 64 that the other proportion will be higher in all six areas. One chance in 32 is less than .05, the most frequently used cutoff point for statistical tests. Note that by this criterion, finding one proportion to be higher in five of the six areas is not statistically significant since there are 14 chances in 64 of one proportion being higher in five of the six areas.

Table 3. *Data on the correlation between the order of verb and object and the order of adposition and noun phrase, in terms of number of languages*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Total
OV&Po	**46**	**97**	**90**	**114**	**49**	**76**	472
OV&Pr	5	4	1	3	0	1	14
Prop Po	.90	.96	.99	.97	1.00	.99	Average = .97
VO&Po	15	7	0	3	5	12	42
VO&Pr	**171**	**30**	**159**	**21**	**59**	**16**	456
Prop Po	.08	.19	.00	.13	.08	.43	Average = .15

Table 4. *Proportions of languages that are postpositional*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Average
OV	**.90**	**.96**	**.99**	**.97**	**1.00**	**.99**	.97
VO	.08	.19	.00	.13	.08	.43	.15

Again, Table 4 shows that the proportion of languages which are postpositional is higher among OV languages in all six areas.

Counting genera and counting languages each have their own advantages. Counting languages has the advantage that one does not have to address the issue of what counts as a genus. The classification of languages into genera is my own and is based on what are at best half-educated guesses. Counting genera has the advantage that occasionally, the large number of languages in one or two genera can skew the numbers of languages in their areas in a way that distorts the picture. This arises most commonly due to Bantoid in Africa and Oceanic in Southeast Asia & Oceania: each of these genera contain close to 10 % of the languages of the world. For most typological parameters and for most genera, all languages in the same genus are the same, so counting genera gives a more accurate picture. For the other correlations I discuss below, I will only look at numbers of genera for the reason just given. But I will provide the total number of languages of each sort in the rightmost column, as in Table 1.

2.3. *The correlation between the order of verb and object and the order of noun and relative clause*

We now turn to the correlation between the order of verb and object and the order of noun and relative clause. This correlation is different from the correlation involving adpositions discussed above, which involves two language types which are common (OV&Po and VO&Pr) and two types which are not common (OV&Pr and VO&Po); the correlation involving relative clauses involves three common types (OV&RelN, OV&NRel, and VO&NRel) and one uncommon type (VO&RelN). As discussed below, a correlation where three of the four language types are common presents a challenge to Dunn et al.'s method in a way that one which involves only two common types does not.

Tables 5 and 6 give the data showing a correlation between the order of verb and object and the order of noun and relative clause. Table 5 shows numbers of genera exhibiting the various orders. The crucial result is given in Table 6, which shows that the proportion of genera containing languages with RelN order is higher among OV languages than among VO languages in all six areas. On the basis of the figures in Table 6, we can conclude that there is a correlation between the order of verb and object and the order of noun and relative clause, that OV languages have a statistically greater chance of employing RelN order than VO languages do.

Table 5. *Order of verb and object and the order of noun and relative clause*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Total	Number of languages
OV&RelN	6	**21**	**11**	11	3	7	59	132
OV&NRel	**21**	5	3	**17**	**15**	**9**	70	113
VO&RelN	0	0	3	0	0	0	3	5
VO&NRel	**40**	**9**	**29**	**13**	**18**	**9**	118	415

Table 6. *Comparing proportions of genera that are RelN as opposed to NRel among OV and VO languages*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Average
OV	**.22**	**.81**	**.79**	**.39**	**.17**	**.44**	.47
VO	.00	.00	.09	.00	.00	.00	.02

As suggested by Greenberg (1963), correlations like this which involve three common types are best thought of as reflecting two competing motivations, one a harmony principle, the other a dominance principle. A harmony principle is one that relates two typological parameters and favours two of the four language types, one of which is the opposite of the other, such as OV&Po and the opposite combination VO&Pr.[3] In the present instance, the harmony principle is one that favours OV&RelN and its opposite, VO&NRel. A dominance principle is one that favours one value for a single typological parameter, independently of other typological characteristics. In the present instance, the dominance principle is one that favours the order NRel over the order RelN. Both harmony principles and dominance principles must reflect some underlying causal principles.[4] Figure 1 shows which of the four word order types the relevant harmony and dominance principles favour for the pair of word order parameters under discussion. As shown in Figure 1, in VO languages the two principles work together with the result that most VO languages are NRel. In OV languages, in contrast, the two principles are in competition since the harmony principle favours OV&RelN over OV&NRel, while the dominance principle does the opposite. As a result, both orders of noun and relative clause are common among OV languages. The rarest type, VO&RelN, is rare because it satisfies neither principle.

In the supplementary materials to their article, Dunn et al. report on their examination of the correlation between the order of verb and object and the order of noun and relative clause and find no evidence for it in any of the families they examined. But this is simply because their method ignores the possibility of dominance principles and treats all instances of OV&NRel languages

3. In other words, given two typological parameters P and Q, where P has values p_1 and p_2 and Q has values q_1 and q_2, then a harmony principle will favour the two language types p_1&q_1 and p_2&q_2 and disfavour the two types p_1&q_2 and p_2&q_1.
4. Dryer (1992) proposes a processing explanation for the harmony principle in Figure 1. Hawkins (1994) proposes a processing explanation for both principles.

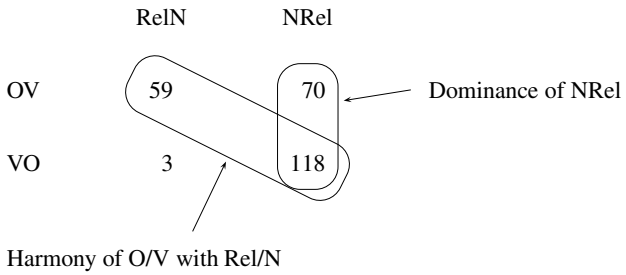

Figure 1. *Harmony and dominance principles governing the order of relative clause and noun*

as counting against there being a correlation in that family. But it is explicit in both Greenberg 1963 and Dryer 1992 that in claiming the existence of a correlation, there may be a dominance principle interacting with a harmony principle underlying any given correlation. In other words, a claim that there is a correlation is simply a claim that there is a harmony principle at work. The existence of a language type that is inconsistent with the harmony principle provides no evidence against the harmony principle, if it reflects an interacting dominance principle. Thus the high frequency of "inconsistent" language types like OV&NRel is fully consistent with the claim that there is a correlation. The method that I have used in the current paper factors out the possible effects of a dominance principle, while Dunn et al.'s method does not.

2.4. *The correlation between the order of verb and object and the order of verb and subject*

The second correlation that Dunn et al. present a full tree for in their supplementary materials is that involving the order of verb and object and the order of verb and subject. They claim that a correlation between these two pairs of elements is found in only one family. However, this case is similar to the preceding one: there is a dominance principle favouring subject-verb order so that one gets many instances of SVO languages. I will not go through the data here, but it is analogous to the one discussed in the preceding section: once one controls for the interacting dominance principle, there is clear evidence for a crosslinguistic correlation. That Dunn et al. did not find a correlation in more families is due in part to their not controlling for interacting dominance principles.

2.5. *The correlation between the order of adjective and noun and the order of relative clause and noun*

In this section, I discuss a correlation between the order of noun and adjective and the order of noun and relative clause which is mentioned in Dryer 2008a and which is implicit in the universals of Greenberg 1963 and Hawkins 1983. Dunn et al. observe the existence of this correlation in some language families and claim that this contradicts the predictions of Dryer 1992. However, their claim that this correlation contradicts the claims of Dryer 1992 is mistaken.

Tables 7 and 8 give the evidence for this correlation. Like the last two correlations discussed, this is an asymmetric correlation in that three of the four types are common (AdjN&RelN, NAdj&NRel, and AdjN&NRel), while only one is uncommon (NAdj&RelN).

Table 7. *Evidence for the correlation between the order of adjective and noun and the order of relative clause and noun in terms of numbers of genera*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Total	Number of languages
AdjN&RelN	6	**20**	**6**	**7**	2	**4**	45	96
AdjN&NRel	**8**	11	5	5	**16**	3	48	91
NAdj&RelN	0	2	12	4	0	2	20	33
NAdj&NRel	**50**	**4**	**26**	**23**	**18**	**15**	136	427

Table 8. *Comparing proportions of genera that are RelN as opposed to NRel among AdjN and NAdj languages*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Average
AdjN	**.43**	**.65**	**.55**	**.58**	**.11**	**.57**	.48
NAdj	.00	.33	.32	.15	.00	.12	.15

The fact that the proportion of genera which contain RelN languages is higher among AdjN languages than among NAdj languages in all six areas, as shown in Table 8, is the basis for concluding that there is a crosslinguistic correlation

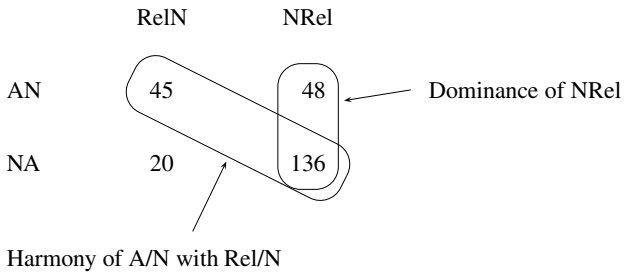

Figure 2. *Harmony and dominance principles governing the order of noun and adjective and noun and relative clause*

between the order of adjective and noun and the order of relative clause and noun.

As with the correlation between the order of verb and object and the order of noun and relative clause discussed in Section 2.3, we can account for the pattern in terms of two competing motivations, as shown in Figure 2. The dominance principle in Figure 2 is the same one we saw in Figure 1, favouring NRel order over RelN order independently of other word order characteristics. The harmony principle favours language types where the adjective and relative clause occur on the same side of the noun. This harmony principle is apparently motivated by the semantic similarity between the relationship of adjectives to nouns and the relationship of relative clauses to nouns. Again, we account for the lower frequency of NAdj&RelN by saying that this type satisfies neither principle.[5]

As noted above, this correlation is discussed in Dryer 2008a and is implicit in the universals of Greenberg 1963 and Hawkins 1983. Why then do Dunn et al. think that it is a problem for the predictions of Dryer 1992? The reason they give is based on a distinction I made in that paper between two sorts of pairs of elements, correlation pairs, which are pairs of elements which correlate with the order of object and verb, and noncorrelation pairs, which are pairs of elements which do not. For example, the pair of elements adposition and noun phrase is a correlation pair because the order of adposition and noun phrase correlates with the order of verb with object, while the pair noun and adjective is a noncorrelation pair, since there is no correlation between the order of noun

5. Greenberg (1963) proposes that there is also a dominance principle favouring noun-adjective order. This would account for the fact that the least frequent type in Figure 2 is more common than the least frequent type in Figure 1. But if there is such a principle, it is weaker than the other two principles since the NAdj&RelN type is the least frequent type in Figure 2.

and adjective and the order of verb and object, as shown by Dryer (1989a, 1992). Dunn et al. seem to think that my paper implies that there should not be correlations between correlation pairs and noncorrelation pairs. Since noun and relative clause is a correlation pair and noun and adjective is not, they think that this predicts that there should not be a correlation between noun and adjective and noun and relative clause.

It is not clear why Dunn et al. think that there should not be correlations between correlation pairs and noncorrelation pairs. My hunch as to what leads them to this idea is that they are assuming that "correlate with" is a transitive relation, that if the order of a pair of elements A_1 and A_2 correlates with the order of a pair of elements B_1 and B_2 and if the order of B_1 and B_2 correlates with the order of a pair of elements C_1 and C_2, then the order of A_1 and A_2 must correlate with the order of C_1 and C_2. Applying this to the correlations in question, if the order of verb and object correlates with the order of noun and relative clause and if the order of noun and relative clause correlates with the order of noun and adjective, then the order of verb and object ought to correlate with the order of noun and adjective. But, as noted above, it does not.[6]

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6. Perhaps one source of the problem is that what is generally known as a correlation in typology is strictly speaking an association rather than a correlation in the conventional terminology of statistics. However, this should be clear from Dryer 1992. It is well known that association is not a transitive relation.

   Another possible source of confusion is that I do not mention correlations between pairs of noncorrelation pairs or between correlation pairs and noncorrelation pairs in Dryer 1992. But this is not because I was not fully aware of them. The reasons can be best understood historically. Until Dryer 1989, 1992 it was generally believed that all the noncorrelation pairs were actually correlation pairs, that, for example, the order of noun and adjective, the order of noun and demonstrative, and the order of noun and numeral all correlated with the order of verb and object. Until these two papers of mine, what was understood by the "Greenbergian word order correlations" was the correlations of various pairs of elements with the order of verb and object, and it was these correlations that were the focus of my 1992 paper. Prior to my 1992 paper, the correlations between pairs of correlation pairs and between correlation pairs and noncorrelation pairs were thought to be instances of the Greenbergian word order correlations, but once I demonstrated that a number of pairs of elements were not correlation pairs, correlations involving them were no longer instances of the Greenbergian word order correlations and therefore outside the scope of my 1992 paper. I believe that the explanations for these other correlations are very different from the explanations for the Greenbergian word order correlations. While I have argued that the latter are motivated at least in part by processing considerations, I believe that the former are motivated largely by semantic considerations. Namely, the fact that the order of most modifiers of nouns relative to the noun correlate with each other is based, I believe, on the fact that broadly speaking there is a semantic commonality associated with the syntactic relation of modifying a noun (although there are also significant semantic differences). The fact that these other correlations have a very different explanation in my view is another reason I did not discuss them in my 1992 paper.

To see why "correlate with" is not a transitive relation, consider the three pairs of elements in question. Although one can have a lack of transitivity with symmetric correlations, if they are weak enough, it is particularly easy to have a lack of transitivity when the two correlations are both asymmetric. It is perhaps easiest to see this by expressing the asymmetric correlations in the form of unidirectional implicational universals. The correlation we have with the order of verb and object and the order of noun and relative clause is expressible by either of the two equivalent unidirectional implicational universals in (1).

(1) a. If VO, then NRel.
b. If RelN, then OV.

Because these universals are unidirectional, neither of the implications in (2) is true.

(2) a. *If NRel, then VO.
b. *If OV, then RelN.

The very frequent type OV&NRel makes the implications in (2) false. Similarly, we can express the asymmetric correlation between the order of noun and relative clause and the order of noun and adjective by either of the equivalent unidirectional implicational universals in (3).

(3) a. If RelN, then AdjN.
b. If NAdj, then NRel.

And again, because these universals are unidirectional, neither of the reverse implications in (4) is true.

(4) a. *If AdjN, then RelN.
b. *If NRel, then NAdj.

To see why we cannot get from the implicational universals in (1) and (3) to any correlation between the order of verb and object and the order of noun and adjective, all we need to do is show that we cannot derive any implicational statement relating these two word order parameters from the implications in (1) and (3). Suppose we start with VO. Then we can predict NRel, from (1a). But from NRel, we cannot predict the order of noun and adjective from either statement in (3); we could if (4b) were true, but it is not. Suppose instead we start with OV. Then we cannot predict the order of noun and relative clause and so we cannot predict the order of noun and adjective. Suppose we start with NAdj. Then from (3b), we can predict NRel. But we cannot predict the order of verb and object from NRel. Finally, suppose we start with AdjN. But from this we cannot predict the order of noun and relative clause and hence we cannot predict the order of verb and object. In short, we cannot predict the

order of noun and adjective from the order of verb and object and we cannot predict the order of verb and object from the order of noun and adjective. In other words, from the fact that the order of noun and relative clause correlates with both the order of verb and object and the order of noun and adjective, there is no reason to expect that there should be any correlation between these last two pairs of elements. So the correlation between the order of noun and relative clause and the order of noun and adjective presents no problems for any previous claims in the literature. In fact, this is just one of a number of correlations involving a correlation pair and a noncorrelation pair that Dunn et al. found in one or more families that they claim Dryer 1992 predicts should not occur; for similar reasons, nothing in Dryer 1992 predicts that any one of these correlations should not occur.

2.6. *The order of verb and object and the order of noun and demonstrative*

Dunn et al. claim that two of the families they investigate show a correlation between the order of verb and object and the order of noun and demonstrative, despite the fact that I claim in Dryer 1992 that the order of noun and demonstrative does not correlate with the order of verb and object. They consider their result to be evidence against this claim of mine.

There is something odd, however, in their logic. Dunn et al. argue that their evidence that the order of adposition and noun phrase correlates with the order of verb and object in only some families is evidence against my claim that there is a crosslinguistic correlation between these two pairs of elements. So it seems odd to argue that their evidence that the order of noun and demonstrative correlates with the order of verb and object in only some families is evidence against my claim that there is NOT a crosslinguistic correlation between these two pairs of elements. Surely if their results provide evidence that there is no correlation in the case of adposition and noun phrase, their similar results also provide evidence that there is no correlation in the case of noun and demonstrative.

However, it may be significant that in every other instance in which Dunn et al. found a correlation in some family, it was a correlation that I have claimed (or would claim) to be a crosslinguistic correlation. A correlation between the order of verb and object and the order of noun and demonstrative in some families would be the only instance of a correlation found in some families that I have claimed is not a crosslinguistic correlation.

While I did treat the order of noun and demonstrative as a noncorrelation pair in Dryer 1992, it is actually a borderline case. Since my database has grown over 150 % since my 1992 paper, it is worth revisiting this pair of elements. The relevant data is given in Tables 9 and 10, with the comparison of the proportions of genera in each area given in Table 10.

Table 9. *Order of verb and object and order of noun and demonstrative*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Total	Number of languages
OV&DemN	13	**28**	**12**	**33**	**23**	**30**	139	317
OV&NDem	**16**	1	5	41	5	5	73	141
VO&DemN	9	**9**	14	**11**	**24**	**10**	53	154
VO&NDem	**38**	1	**28**	9	9	4	43	375

Table 10. *Proportions of genera that are DemN as opposed to NDem among OV and VO languages*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Average
OV	**.45**	**.97**	**.71**	.45	**.82**	**.86**	.71
VO	.19	.90	.33	**.55**	.73	.71	.57

In Dryer 1992, the proportion of genera that were DemN was higher among OV languages for only four of the six areas. However, using my current data, Table 10 shows that there are now five areas in which the proportion is higher among OV languages. In the only area that does not fit this pattern, the difference in the proportions is higher among VO languages by only 10%. Although this case falls short of statistical significance, it is possible that there is a weak crosslinguistic correlation.

Does this support Dunn et al.'s claim? Strictly speaking not, since they claim that if a correlation is found in only some families, then there is no crosslinguistic correlation. On the other hand, it may mean that the order of noun and demonstrative should be considered on a par with clear cases of correlation pairs, and in so far as Dunn et al. claim that, they may be right.

3. How would Dunn et al. explain the existing data?

Let us return to the correlation between the order of adposition and noun phrase and the order of verb and object. Although Dunn et al. argue against a crosslinguistic correlation between these two pairs of elements, it is not clear how they would account for the data that overwhelmingly suggests that there is indeed a correlation. Quite apart from the data I have published (Dryer 1992) and the data in Section 2.2, one only has to look at the numbers of languages in Dryer 2011d, given in Table 11, to see that postpositions overwhelmingly outnumber prepositions in OV languages by 472 to 14 while prepositions overwhelmingly outnumber postpositions in VO languages by 456 to 42.

Table 11. *Numbers of languages with the two orders of verb and object and of adposition and noun phrase*

	Po	Pr
OV	472	14
VO	42	456

How could we get such a difference between OV and VO languages without there being a crosslinguistic correlation between these two pairs of elements?

If there is not a crosslinguistic correlation here, what would Dunn et al. propose as an alternative? Could the differences between OV and VO languages in these numbers be due simply to random variation? Could it be perhaps that early human language had typological features that are common today only because early human language had these properties accidentally? Or do these numbers arise because there are some language families in which there is a correlation and there are enough of these families that we get the numbers in Table 11? Is it possible that if human history had been different we might not have had so many families and thus we would not have had numbers like those in Table 1? Or is there some other explanation? While we might wait to see what Dunn et al. say about this, none of the alternatives I have suggested seems plausible. The possibility of random variation seems ruled out by the statistical evidence presented in Section 2.1.

A somewhat more plausible explanation might be that the numbers are a reflection of certain language types being accidentally more common at early stages of human history and that this has had an impact on numbers today. Let me discuss an example where this possibility is real. Suppose we were to ask the question of whether it is more natural for a language to employ clicks or not to employ clicks. Table 12 gives the relevant data (in terms of number of languages) based on Maddieson 2011, showing languages with clicks versus language that do not have clicks.

Table 12. *Languages with clicks versus languages without clicks*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Total
Clicks	10	0	0	0	0	0	10
No clicks	**119**	**89**	**96**	**83**	**88**	**79**	546
Prop clicks	.08	.00	.00	.00	.00	.00	Average = .01

Table 12 shows that in all six areas, there are more languages WITHOUT clicks than there are languages WITH clicks. Now suppose someone were to say "We have more languages without clicks in all six areas than we have languages with clicks. Therefore, there is a universal linguistic preference in language not to have clicks."

The problem with this argument is the following. All ten languages with clicks in Maddieson's sample are not only spoken in Africa, but in Africa south of the equator, seven of them in the traditional grouping of Khoisan, two of them Bantu languages of southern Africa, and one of them, Dahalo, a Cushitic Language spoken in Kenya. In fact, seven of these ten languages (other than Dahalo and two languages in Tanzania traditionally classified as Khoisan, namely Sandawe and Hadza) are much further south from the equator, in South Africa, Namibia, and Botswana. Apart from the case of Dahalo,[7] the other languages are all one of two sorts. Some of them (those traditionally classified as Khoisan) have apparently descended from languages that were already in Africa south of the equator before the expansion of Bantu. And the others are Bantu languages that appear to have acquired clicks due to contact with languages of the first sort.

But it is not just that all of the languages with clicks are concentrated in one geographic area. What is significant is that this area is very roughly the area in the world that is south of the area where man is believed to have originated. In fact, we can say that clicks are found in most languages that have descended from languages spoken in this area prior to the Bantu expansion. Thus one possible scenario is that as man was evolving, clicks were used by those people who moved south of the place of origin but not by people who moved north and west (or who stayed in the region). Now suppose the physical geography of the

7. I will ignore Dahalo in this discussion since it is not clear how it acquired clicks. I suspect that it is also due to contact with a language with clicks that descended from languages that were already in Africa south of the equator before the expansion of Bantu, but I am not aware of any clear evidence one way or the other.

world had been different, that there was no land mass connected to northern Africa but there had been land connections to the rest of the world from south Africa, and that northern Africa had been much smaller than it is. Then, if the world has also had the properties that most languages spoken by people who descended from people who moved south from the place of origin had clicks, we would find clicks in the vast majority of languages of the world. And if we then applied the same reasoning given above to claim a universal preference not to have clicks, then we would have to conclude, in this alternate world, that there is a universal preference to HAVE clicks. What this shows is that it is possible that there are some typological features whose distribution across the six areas is such that the six areas are not independent, in the sense that the distribution across areas reflects early migrations of man and not a universal preference for some linguistic type. Hence there exist at least some cases where my methodology would apparently lead to a conclusion that there is a universal preference for some linguistic type when in fact there is none.

It is worth pointing out that if the distribution of clicks is strongly influenced by the physical geography of the world in this way, it may be a good example where Dunn et al.'s landscape metaphor is particularly apt ("future states lie in an evolutionary landscape with channels and basins of attraction that are specific to linguistic lineages"). Most if not all languages spoken by those people who descend from humans who moved south of the point of origin have clicks and most if not all languages spoken by people who descended from people who moved north or west of the point of origin lack clicks. This suggests that it is very difficult for a language without clicks to develop clicks or for a language with clicks to lose them. To use Dunn et al.'s landscape metaphor, having clicks is like a deep ravine that it is difficult to get out of, but NOT having clicks is also like a deep ravine. If this is true, then the fact that some of the Bantu languages, such as Zulu and Xhosa, have developed clicks, apparently due to contact with Khoisan languages, shows how strongly contact changes the landscape: when not in contact with a language with clicks, not having clicks is like a ravine surrounded by high escarpments, but when a language is in contact with a language with clicks, not having clicks is like a ravine with a gap on one side that allows easy escape.

We see therefore, that, in at least some instances, we cannot tell from the low frequency in all parts of the world of a particular language type, like languages with clicks, that there is some dispreference for this language type. That raises the question of whether perhaps the same applies to the correlation between the order of verb and object and the order of adposition and noun phrase. Could it be that the two infrequent types OV&Pr and VO&Po are infrequent only due to an accident of history? The answer is that there are reasons to believe that this is unlikely. There is a crucial distinction here between testing for a preferred value for one typological parameter and testing for a correla-

tion between two parameters. There is reason to believe that the problem with applying the methodology to test for a universal preference for one value of a single typological parameter does not apply to correlations between two parameters. To see why this is so, let us see whether the fact that OV&Po and VO&Pr are more common than OV&Pr and VO&Po might be attributed to the accident of history. Suppose that due to the accident of history, two typological features, say OV and postpositions, became widespread so that OV&Po languages became very common, but that OV&Po languages were common only because OV was very common and Po was very common, not because of any dependency between these two features. How then would we account for the fact that there are so many VO&Pr languages in the world today but relatively few OV&Pr or VO&Po languages? Either there have been many changes directly from OV&Po to VO&Pr or, more likely, changes to OV&Pr or VO&Po that were eventually followed by changes to VO&Pr. But this is unlikely in the absence of a dependency between these two word order parameters. There is no apparent way in which VO&Pr could have become so common without OV&Pr or VO&Po becoming common as well, without there being a dependency between these two word order parameters.

Early in this section I mentioned the possibility that the numbers in Table 1 might arise from there being a correlation in only some language families but enough of these families that we get the numbers in Table 1. In order to discuss this possibility, I believe it is necessary to distinguish two senses in which there might be a crosslinguistic correlation between two typological parameters. I will refer to one of these notions as a crosslinguistic correlation and the other as a universal bias towards two language types that contrast in the values of two typological parameters, like OV&Po and VO&Pr. What I intend by this distinction is best explained by an example. Suppose it were the case that there were some correlation in the majority of families in all six areas of the world, but not in all families. Then it would be possible in principle for my test (described in Section 2) to show evidence of a crosslinguistic correlation even when there is not a correlation in all families, only because the majority of families in each area showed a correlation, because the proportions of genera or languages of one type might be higher in all six areas compared to the proportions of genera or languages of a second type. What my test tests for is crosslinguistic correlations.

On the other hand, the claim of there being a universal bias towards two types that contrast in the values of two typological parameters is a stronger claim. It claims that there is a bias of this sort in all languages and that this bias influences the transition probabilities of particular word order changes for all languages. It is possible for there to be a crosslinguistic correlation without there being a universal bias, if the crosslinguistic correlation arises only because the bias is found in most but not all families.

Now it is clear that when Dunn et al. claim that a correlation is found in at most half the families they looked at, and usually less than half, they are denying not only that there is a universal bias, but also that there is not even a crosslinguistic correlation. The evidence presented in Section 2 (and in Dryer 1992) provides evidence for a crosslinguistic correlation for the different pairs of word order parameters examined, and thus provides evidence against Dunn et al.'s claim that there is no crosslinguistic correlation. But it does not show, without further evidence, that there are universal biases in these cases. Dunn et al. might concede that there are crosslinguistic correlations in some cases, but claim that there are no universal biases involving word order parameters. The evidence presented in Section 2 does not provide evidence against such a claim.

However, it is not clear how we test for universal biases. It is not easy to show that there is a universal bias, if only for the fact that it is in general difficult in science to show the truth of propositions that involve universal quantification. Because of this, I claim that the burden of proof is on those who deny a universal bias to show that the bias is not universal. Since Dunn et al. deny that there are universal biases in the realm of word order and claim to have evidence that there are not universal biases, all we can really do is examine their evidence for this claim. I will devote my attention to the correlation between the order of verb and object and the order of adposition and noun phrase and will argue that there is no evidence against a universal bias towards the two consistent types and that the available evidence suggests that the bias is indeed universal.

Because this requires examining Dunn et al.'s evidence, we must first discuss what Dunn et al. mean when they say there is or there is not a correlation in a family. I will argue that given what they mean by this, evidence that only a minority of families exhibit a correlation (in their sense) does not provide any evidence against a universal bias towards the two consistent types.

4. Dunn et al.'s notion of a correlation

4.1. *Three types of families in Dunn et al.'s typology*

The approach of Dunn et al. is to examine individual language families and identify whether one finds a given correlation (in their sense) in that family. They examine the distribution of word order types in the phylogenetic tree and infer from this the most likely changes in word order to have occurred. They conclude that no correlation is found for any pair of elements in more than two of the four families they examined, and in the majority of cases, in either one or no family. From this they conclude that there are no correlations that are crosslinguistic, but only correlations that are specific to particular families (or lineages, as they call them). They also claim that existing theories of word

order correlations predict that most families will exhibit a correlation and that this prediction is false.

There is little question that examining individual language families, especially from a diachronic perspective, is important and can yield insights into word order that typological studies which simply examine the synchronic distribution of word order types, like Dryer 1992, may miss. However, we also have to be aware that when Dunn et al. talk about correlations in individual families, they are not using the term "correlation" in the same sense as that used by typologists.

I will argue in this section that existing theories of correlations do not, contrary to Dunn et al., predict that most families will exhibit a correlation in their sense, and that the number of families in which they find correlations for various pairs of elements is exactly what we would expect under existing theories. I will argue that the number of families exhibiting a correlation in their sense is fully consistent, not only with there being a crosslinguistic correlation, but even a universal bias towards the two consistent types.

It is useful to distinguish three types of families defined by Dunn et al.'s method. The first type is families where all of the languages in the family are of the same language type. Of the four families they examine, Bantu is an instance of this type of family as far as the order of verb and object and the order of adposition and noun phrase are concerned, since all the Bantu languages in their sample are VO&Pr. I will refer to this type of family as a ONE-TYPE FAMILY.

The second type of language family that can be defined in terms of Dunn et al.'s method is one in which two language types occur that are the opposite of each other in the sense that the two types differ in the values of both parameters, but in which the other two types are relatively infrequent, if they exist at all. My characterization "relatively infrequent" is vague, but Dunn et al.'s method provides an explicit characterization of what that means. It does not refer to absolute numbers of languages, but, roughly speaking, to branches in the genealogical tree and also, if I understand correctly, to the length of different branches in the tree. I will refer to this type of family as a CORRELATING FAMILY. Of the four families they examine, two, namely Indo-European and Austronesian, are correlating families with respect to the two typological parameters under discussion. In both families, the language types OV&Po and VO&Pr predominate, while the other two types are relatively infrequent. Note that in principle a correlating family could be one where the two inconsistent types OV&Pr and VO&Po predominate, but it is clear from the data in my database that no family of this sort exists.

The third type of language family are ones which do not meet the criteria defining the first two types. I will refer to these as NONCORRELATING FAMILIES. These are families in which one of the inconsistent types is relatively frequent

relative to one of the consistent types. For example, a family will be a noncorrelating family if one of the inconsistent types OV&Pr or VO&Po is relatively frequent relative to one of the consistent types OV&Pr or VO&Pr. Among the four families they examined, Uto-Aztecan is an instance of a family of this type, since VO&Po occurs relatively frequently compared to VO&Pr.

Dunn et al. consider a family to be one in which there is a correlation only if it is a correlating family in the sense just defined. They consider a family to be one in which there is not a correlation if it is one of the other two types, i.e., a one-type family or a noncorrelating family. It is intuitively obvious why a correlating family counts as one in which there is a correlation while a noncorrelating family counts as one in which there is not, but it might be less obvious why a one-type family counts as one in which there is not a correlation. Why, one might ask, does Bantu count as a family in which there is not a correlation, when all the languages are the consistent type VO&Pr? The answer is that one-type families show no dependency between the two parameters. For example Bantu languages are consistently VO as well as being languages with four or more noun classes. But this doesn't mean that there is any dependency between order of verb and object and the number of noun classes. Rather, these are presumably two features of Bantu languages that accidentally co-occur in this family. Just looking at Bantu, there is no reason to see the fact that they are all VO and prepositional as being any different.

But while it may make sense to classify one-type families as ones not exhibiting a correlation, one cannot conclude, as Dunn et al. do, from the fact that only a minority of families are ones exhibiting a correlation in their sense that there is not a universal bias towards the two consistent language types. The claim that there is a universal bias towards two consistent types does not predict that most families should be correlating families. Rather it predicts that most families should be one of the following three sorts: (i) one-type families of the consistent word order type; (ii) correlating families where the two word order types are consistent ones (where the putative crosslinguistic correlation defines which word order types are consistent); or (iii) noncorrelating families in which the number of languages which are consistent "outnumber" the number of languages which are not consistent (though the relevant notion of "outnumber" should be weighted according to the genealogical classification within the family rather than simply according to raw numbers of languages). It predicts that we should not find many families of the following three types: (i) one-type families of the inconsistent word order type, or (ii) correlating families where the two word order types are inconsistent ones, or (iii) noncorrelating families where inconsistent languages "outnumber" consistent ones. For example, the claim that there is a correlation between the order of verb and object and the order of adposition and noun phrase, in the sense that there is a universal bias towards the two consistent word order types, predicts that most

families should be one-type families where the one type is OV&Po or VO&Pr, or correlating families where the two dominant word order types are OV&Po and VO&Pr, or noncorrelating families where languages which are OV&Po or VO&Pr "outnumber" languages which are OV&Pr or VO&Po (even though one of these last two word order types may be relatively frequent relative to the less frequent of the first two types).

One way to see why the claim that there is a universal bias towards the two consistent word order types does not make the prediction that Dunn et al. claim it does is to consider the hypothetical case of a world in which every family was a one-type family, where in some families the one word order type was OV&Po while in the other families, the one type was VO&Pr. In such a case, every language would be OV&Po or VO&Pr and no language would be OV&Pr or VO&Po. Clearly in such a case, there would be a perfect correlation between the order of these two pairs of elements. But what would Dunn et al.'s logic say in this case? It would say that since no family was a correlating family, there would be no evidence for a correlation between the order of these two pairs of elements. But surely this is counterintuitive.

4.2. *Testing the predictions*

In the preceding section, I outlined what types of language families should be frequent and which types should be infrequent if there is a correlation between two word order types in the sense that there is a universal bias towards two opposite word order types. Let us now re-examine the putative correlation between the order of verb and object and the order of adposition and noun phrase in terms of these predictions, using the data in my database. First, consider one-type languages. Table 13 gives the relative frequency of each of the four word order types of one-type families in my database, classified by the one word order type found in that family.

Table 13. *One-type families by word order type*

	Po	Pr
OV	88	0
VO	3	26

Table 13 shows that the vast majority of one-type families are one of the two consistent word order types, OV&Po or VO&Pr. Only three out of 117 one-type families are instances of one of the inconsistent word order types, VO&Po. All three of these are either language isolates or small language families represented by only one language in the sample and all three are in northern Peru within 500 km of each other: Peba-Yaguan (represented by Yagua), Zaparoan

(represented by Iquito), and Taushiro. Clearly the numbers in Table 13 support the claim of a crosslinguistic correlation and a universal bias towards the two consistent types and show how Dunn et al.'s considering one-type families as not supporting the claim of a crosslinguistic correlation makes little sense.

Consider now the families which are not one-type families. Table 14 gives the data from my database for these families. The first four columns show the number of languages of each of the four word order types; the fifth column shows the number of consistent languages, the sum of the first and fourth columns; the sixth column shows the total number of languages in the family contained in this sample; and the last column shows the percentage of the languages that are one of the consistent types, i.e., the fifth column as a percentage of the sixth column.

Table 14. *Families which are not one-type families, with numbers of languages of different word order types*

Family	OV&Po	OV&Pr	VO&Po	VO&Pr	Consistent (OV&Po or VO&Pr)	Total	% consistent
Mixe-Zoque	0	0	1	1	1	2	50
Uralic	8	0	6	0	8	14	57
Arawakan	2	0	5	5	7	12	58
Solomons East Papuan	2	0	1	0	2	3	67
Tupian	11	1	3	0	11	15	73
Khoisan	4	0	1	0	4	5	80
Uto-Aztecan	10	0	3	4	14	17	82
Torricelli	1	0	1	4	5	6	83
Tucanoan	6	0	1	0	6	7	86
West Papuan	0	1	0	7	7	8	88
Australian	16	2	1	5	21	24	88
Niger-Congo	19	0	11	97	116	127	91
Nilo-Saharan	12	1	3	27	39	43	91
Indo-European	26	4	1	30	56	61	92
Afro-Asiatic	11	4	0	46	57	61	93
Austronesian	12	1	0	132	144	145	99

There are two initial observations to be made about Table 14. The first is that there is no family in Table 14 in which consistent languages constitute

a minority: the percentage of languages that are consistent (shown in the last column of Table 14) is at least 50 % for every family and for 11 of the 16 families, this percentage is 80 % or more. The absence of any family where the percentage of consistent languages is a minority of languages in the family is evidence that there is not only a crosslinguistic correlation between the order of verb and object and the order of adposition and noun phrase but also a universal bias towards the consistent word order types.

The second observation that can be made from Table 14 is that there is a positive correlation between the number of languages in the family and the percentage of languages that are consistent: the five families with the most languages in the sample, those with over 40 such languages, are exactly the five families with the highest percentages of languages of one of the two consistent word order types. This is expected under the view that there is a universal bias towards the two consistent types. Under that view, we expect to get occasional instances of inconsistent languages. If these arise in a small family, they may constitute a larger percentage of the family. If they arise in a large family, they are likely to be greatly outnumbered by consistent languages.

We cannot tell in general which of the families in Table 14 would come out as correlating families and which would come out as noncorrelating families by Dunn et al.'s method, without applying their method. At the very least, we would need to know how the inconsistent languages are distributed though these families, and simply adding up languages of each word order type is a crude measure at best. Nevertheless, the data in Table 14 provide a basis for making an initial prediction as to which families might be correlating families and which might be noncorrelating families. In Tables 15 to 17, I have divided the families listed in Table 14 into three groups. Table 15 contains those families which are apparently noncorrelating families because there are inconsistent languages and one of the two consistent types is absent altogether. Table 16 lists those families which are probably correlating because there are less than half as many languages with inconsistent word order as there are of those with the less common consistent word order type. For example in Afro-Asiatic, there are four languages with inconsistent word order type, but this is less than half the number of those with the less frequent consistent type, OV&Po, of which there are 11 languages. I can describe these families only as probable instances of correlating families because, without actually applying Dunn et al.'s method, I cannot be sure. However, two of the four families in Table 16 are ones that Dunn et al. conclude are in fact correlating families, namely Indo-European and Austronesian.

Finally, Table 17 lists the remaining families. It is more difficult to predict whether these would be correlating or noncorrelating families: there are enough inconsistent languages compared to the less frequent consistent type that they might be correlating or they might be noncorrelating. However, one of these,

Table 15. *Apparently noncorrelating families*

Family	OV&Po	OV&Pr	VO&Po	VO&Pr	Consistent (OV&Po or VO&Pr)	Total	% consistent
Mixe-Zoque	0	0	1	1	1	2	50
Uralic	8	0	6	0	8	14	57
Solomons East Papuan	2	0	1	0	2	3	67
Tupian	11	1	3	0	11	15	73
Khoisan	4	0	1	0	4	5	80
Tucanoan	6	0	1	0	6	7	86
West Papuan	0	1	0	7	7	8	88

Table 16. *Probably correlating families*

Family	OV&Po	OV&Pr	VO&Po	VO&Pr	Consistent (OV&Po or VO&Pr)	Total	% consistent
Nilo-Saharan	12	1	3	27	39	43	91
Indo-European	26	4	1	30	56	61	92
Afro-Asiatic	11	4	0	46	57	61	93
Austronesian	12	1	0	132	144	145	99

Uto-Aztecan, is one that Dunn et al. have concluded is noncorrelating, based on their methodology.

Now the numbers of families in each of these three preceding tables could be taken to support Dunn et al.'s view. They might argue as follows. Because there are seven instances of apparently noncorrelating families in Table 15 plus one family from Table 17 that they have shown to be noncorrelating (Uto-Aztecan), there are at least eight families that are noncorrelating. Even if all four of the remaining families in Table 17 turned out to be noncorrelating, there would still be at least as many noncorrelating families as there are correlating families. And if even one of these four families in Table 17 turned out to be noncorrelating, then there would be more noncorrelating families than there are correlat-

Table 17. *Families which might be correlating or might be noncorrelating*

Family	OV&Po	OV&Pr	VO&Po	VO&Pr	Consistent (OV&Po or VO&Pr)	Total	% consistent
Arawakan	2	0	5	5	7	12	58
Uto-Aztecan	10	0	3	4	14	17	82
Torricelli	1	0	1	4	5	6	83
Australian	16	2	1	5	21	24	88
Niger-Congo	19	0	11	97	116	127	91

ing families. Dunn et al. might reasonably argue that the claim that there is a crosslinguistic correlation or a universal bias towards the two consistent types surely predicts that at the very least there should be more correlating families than there are noncorrelating families, but it appears that this is not the case.

In response to this, I would first point out three other facts already mentioned that argue for a universal bias towards the two consistent types. The first is that the vast majority of one-type families, 114 out of 117 families, exhibit one of the consistent word order type. The second is that in every family which is not a one-type family, at least half the languages are consistent. And the third is that there is a positive correlation between the number of languages in a family and the percentage of languages in the family that are consistent.

There is another factor to consider: the claim that there is a universal bias towards the two consistent word order types predicts that inconsistent types are less stable than consistent types. Evidence for this is presented in the next section.

4.3. *Evidence that the inconsistent word order types are less stable*

When a language with consistent word order in terms of the order of verb and object and the order of adposition and noun phrase (i.e., OV&Po or VO&Pr) changes to become the opposite consistent word order type (from OV&Po to VO&Pr or vice versa), it is usually the case that one pair of elements changes order first and the other changes later. And with these two pairs of elements, it is most common for the order of object and verb to change first and for the order of adposition and noun phrase to change later. The apparent reason for this is the following. A language that changes from OV to VO, for example, involves an earlier stage in which both orders occur but in which OV is more

frequent in usage due to the pragmatic factors governing the order of object and verb. When the language becomes VO, what happens is that the pragmatic factors governing the order of verb and object change so that VO becomes the more frequent in usage. This seems to happen easily, especially in contact situations. Changes in the order of adposition and noun phrase typically do not work this way. The reason is that when languages change from one adposition type to the other, it is generally not the case (as observed by Dunn et al. in their supplementary materials) that individual adpositions change from one type to the other but rather that adpositions of the new type arise due to grammaticalization and that at least some of the adpositions of the old type fall out of use.[8] We therefore expect, under the assumption that there is a universal bias towards the two consistent types, to find various instances of inconsistent types that are in the midst of a change from one consistent type to the other.

Small noncorrelating families, therefore, are something we expect to find under the thesis that there is a bias towards the two consistent types, given that changes from one consistent type to the opposite consistent type typically pass through a stage where the language is inconsistent with respect to the word order parameters in question. On the other hand, Dunn et al. might justifiably argue that in general we have no evidence that any particular instance of an inconsistent language is in the midst of a change towards a consistent type. However, the thesis that many inconsistent languages are in the midst of a change towards a consistent type does make a prediction about their distribution. It predicts that in general, inconsistent languages remain inconsistent for shorter periods of time than consistent languages remain consistent. And we do have a way of testing this. It predicts that in genera containing inconsistent languages, we will often find consistent languages as well, as long as there is more than one language in the genus (since if there is only one language in the genus which is inconsistent, we have no way of knowing how long it has been inconsistent).

8. Even in the minority of cases in which we get a change in the position of a particular adposition relative to its noun phrase, where there is a stage at which both orders are possible for particular adpositions, as was the case with Middle Iranian (Brunner 1977) and is the case with Papago (Dryer 1989b), the alternation is typically not pragmatically governed. When the adposition type changes, what happens is that one of the two positions of the adposition drops out of usage or that a second position emerges that was not possible before. Both of these scenarios are relevant to instances of inconsistent types in Dunn et al.'s data. Some of the Iranian languages are instances of the inconsistent OV&Pr type. But, at least in the case of Persian, adpositions that could previously be used either prepositionally or postpositionally are now exclusively prepositions. And in the case of Papago, what were originally specifically postpositions can now be used either way.

Table 18 provides data from my database on all genera containing languages of the inconsistent types OV&Pr or VO&Po. The first column gives the number of genera of each of the two inconsistent types where the genus contains only one language in my data; the second column lists the number of genera of each type in which all of the languages are inconsistent; and the third column lists the number of genera containing both consistent and inconsistent languages.

Table 18. *Types of genera containing inconsistent languages*

	Sole language in genus	All languages in genus inconsistent	Some languages in genus are consistent
OV&Pr	1	0	9
VO&Po	9	2	14
Total	10	2	23

As already noted, the genera containing a single language in the sample tell us little in that we have no way to determine how old the word order type in that genus might be. What is important is the numbers of genera of the other two word order types. What Table 18 shows clearly is that the vast majority of genera containing inconsistent languages and containing more than one language in the sample, 23 out of 25 genera, also contain consistent languages. There are only two genera containing more than one language in the sample which only contain inconsistent languages, namely Balto-Finnic and Kwa (in the Uralic and Niger-Congo families respectively). What this means is that with most inconsistent languages, one of two situations obtains. Either the proto-language for that genus was consistent and the inconsistent languages are a recent development. Or the proto-language for that genus was inconsistent and some languages in the genus have become consistent. Instances of the former case clearly support the idea that there is a universal bias towards the consistent types since the inconsistency has lasted for only a short period of time. In the latter case, the inconsistent type has indeed lasted longer, but the fact that some languages have changed to a consistent type means that for them, at least, the inconsistency was short-lived, which again supports the idea that there is a universal bias towards the consistent types.

Furthermore, neither of the two genera containing only inconsistent languages is a straightforward case. In the case of Kwa, all of the languages in the dataset are of the inconsistent type VO&Po. Now for reasons I will discuss below in Section 5, I have generally excluded from the data for this commentary languages which are OV/VO or Po/Pr, languages in which both orders of one of these pairs of elements occur without evidence that one order is dominant. However, it turns out that Kwa contains two types of languages where

one of the pairs of elements is of this sort. In (5) are listed the Kwa languages for which I have data on these two word order parameters, along with their word order type.

(5) VO&Po: Baule, Nkonya, Lelemi, Ewe, Adioukrou
VO&Po/Pr: Akan, Fongbe, Gungbe
OV/VO&Po: Ajagbe

This shows that Kwa is not a genus in which all languages are straightforwardly VO&Po. And if we say that proto-Kwa was an instance of the inconsistent type VO&Po, then three of the languages have moved part way towards a more consistent type in that VO&Po/Pr is closer to the consistent type VO&Pr than VO&Po is (though see below for the danger in assuming that a VO&Po/Pr language is necessarily distinct from a VO&Po language). In fact, there is still further information about these languages that it is important to know. Namely some of the VO languages in this genus also allow OV as an alternative order in certain constructions (Ameka 1991: 45, 49 and Nurse (no date) for Ewe; Aboh & Essegbey 2010: 48 for Kwa in general), so that while I have decided that VO is the dominant order in these languages, OV (and more specifically SOV) also exists.[9]

Nor is Balto-Finnic a straightforward case of a genus in which all languages are VO&Po. Though I classify Finnish as postpositional, it actually has both prepositions and postpositions. I classify it as postpositional because it has more than twice as many postpositions than prepositions: Karlsson (1999: 221–225) lists 50 postpositions and 15 prepositions in Finnish. Similarly, Estonian has both postpositions and prepositions (Tauli 1983: 184). And for Votic, for which I lack data on the order of object and verb, Ariste (1968: 108–110) lists 28 postpositions and 16 prepositions. Because the number of postpositions in Votic outnumbers the number of prepositions by less than two to one, I classify Votic as Po/Pr. Thus the Balto-Finnic languages are not quite as inconsistent as might appear at first. The number of prepositions in Balto-Finnic languages appears to reflect an ongoing change from postpositions to prepositions.

The fact that most genera containing inconsistent languages also contain consistent languages supports the idea that the inconsistent types are less stable. Examination of apparently noncorrelating families also supports this conclusion. In five of the seven apparently noncorrelating families in Table 15, there is

9. The fact that these languages have constructions that are specifically SOV rather than OSV is important. Most SVO languages appear to allow OSV word order in special circumstances (as in English *Violence, kids see on television every day*), a feature that doesn't mean that we find OV characteristics in the language. But when SVO languages allow SOV as an alternate word order, it is much more common to find some OV characteristics. The Sinitic languages are examples of this (Dryer 2003).

only one inconsistent language and the remaining languages are all consistent. This, along with the data in Table 18, suggests that the one inconsistent language represents a relatively recent change. For example, among the Khoisan languages for which I have data on these word order parameters, there are four OV&Po languages and one VO&Po language. The most plausible scenario (assuming that Khoisan is a valid family) is that proto-Khoisan was OV&Po and that the one VO&Po language, Ju|'hoan, has changed from OV to VO. Now Ju|'hoan is one of the more northerly Khoisan languages and the likely scenario is that the change from OV to VO represents the effect of contact with VO Bantu languages. But since the Bantu expansion was relatively recent, that means that Ju|'hoan has probably been VO&Po for a relatively short time.[10] The number of noncorrelating families thus does not really raise concerns for the claim that there is a universal bias towards the consistent types.[11]

Let us now return to a question I raised earlier: is it possible that there is no universal bias towards OV&Po and VO&Pr types and that the numbers in Table 11 above in Section 3 arise because there is a correlation in some but not all families, but in enough of these families that we get the numbers in Table 11? One problem with this idea is that the vast majority of families are either one-type families of a consistent word order type or correlating families. The overwhelming numbers in Table 11 reflect both one-type families and correlating families and in addition some families for which I have said we do not have enough evidence to provide a basis for predicting whether they would be correlating or noncorrelating, in particular Niger-Congo, where 91 % of the languages are consistent. And crucially, there is no family other than the three small one-type families containing a single language in the sample in which a majority of languages are inconsistent. In short, the numbers in Table 11 give

10. By relatively short time, I mean no more than 2,000 years. This may seem like a very long time, but it counts as relatively short compared to the much longer periods of time that languages often remain consistent. If, as I suggest below, the factors driving languages towards consistent word order are relatively weak, then it is not surprising that languages can remain inconsistent for thousands of years.

11. In fact, contact plays a major role in understanding the inconsistent types in general. Though it is beyond the scope of this paper to discuss this matter in depth, there is reason to believe that contact plays a role in explaining most if not all of the noncorrelating families. To cite one more example, Mixe-Zoque, the family with the smallest percentage of consistent languages in Table 14, with one VO&Pr language and one VO&Po language, is squarely within the Meso-American linguistic area (Campbell et al. 1986) and appears to have earlier been OV&Po but has changed to VO in both languages, and to Pr in one of the languages, due to contact with other VO&Pr languages in Meso-America. As discussed briefly in Section 6.1 below, the role of contact is more powerful than the forces behind the word order correlations and can easily change a consistent language into an inconsistent language.

us every reason to believe that there is a universal bias towards the consistent word order types.[12]

4.4. *A comparison to a case where there is no crosslinguistic correlation*

For the sake of comparison, I would like now to examine a pair of elements which do not correlate in order with the order of verb and object, to see how the distribution of the three types of families is very different from the distribution we have just seen for the order of verb and object and the order of adposition and noun phrase. This is the relationship between the order of verb and object and the order of adjective and noun (Dryer 1989a, 1992, 2011e). Table 19 gives the relevant data and is analogous to the data in Table 13 above, giving the number of one-type families of each of the four possible word order types.

Table 19. *Number of one-type families involving the order of verb and object and the order of noun and adjective*

	AdjN	NAdj
OV	28	55
VO	14	15

Unlike Table 13, where only two opposite word order types were common. we find all four types well-attested in Table 19, the two lower numbers both associated with VO order, reflecting the fact that there are fewer families that are exclusively VO than there are families that are exclusively OV.

Table 20 shows data analogous to Table 14, but for the order of adjective and noun and the order of verb and object, showing all the families which are not one-type families and listing the number of languages in each word order type from that family.

12. For reasons of space, I have focused on one particular correlation, between the order of verb and object and the order of adposition and noun phrase, and concluded that here there is not only a crosslinguistic correlation but also a universal bias towards the two consistent types. Whether examination of other correlations would yield the same conclusion is not clear without looking at each of these cases in detail. But again, I emphasize that showing conclusively that there is a universal bias is in general difficult if not impossible. And since we expect a certain number of noncorrelating families even when there is a universal bias, simply due to random variation, showing the existence of noncorrelating families does not disprove claims of there being a universal bias.

Table 20. *Families that are one-type families for the order of verb and object and the order or noun and adjective, with numbers of each word order type in each family*

Family	OV& AdjN	OV& NAdj	VO& AdjN	VO& NAdj	OV&AdjN or VO&NAdj	Total	% OV&AdjN or VO&NAdj
Australian	3	39	9	3	6	54	11
Macro-Ge	0	8	0	1	1	9	11
Trans-New Guinea	10	41	0	0	10	51	20
Cariban	1	4	0	0	1	5	20
Tupian	0	8	0	3	3	11	27
Penutian	2	0	5	0	2	7	29
Northwest Caucasian	1	2	0	0	1	3	33
Solomons East Papuan	1	1	1	0	1	3	33
Sino-Tibetan	39	52	3	4	43	98	44
Guaicuruan	0	0	1	1	1	2	50
Nilo-Saharan	1	17	4	35	36	57	63
Indo-European	27	5	19	14	41	65	63
Uralic	10	0	6	0	10	16	63
Sepik	5	2	0	0	5	7	71
Arawakan	1	2	2	9	10	14	71
Torricelli	0	1	1	7	7	9	78
Niger-Congo	1	22	13	135	136	171	80
Afro-Asiatic	11	10	3	41	52	65	80
Uto-Aztecan	11	2	2	5	16	20	80
Austronesian	0	15	13	126	126	154	82
Khoisan	3	1	0	4	7	8	88
Oto-Manguean	0	0	2	19	19	21	90
Altaic	25	1	0	0	25	26	96
Austro-Asiatic	6	0	1	17	23	24	96

In contrast to the case involving the order of adposition and noun phrase, shown above in Table 14, where every family with more than one word order type had at least 50 % OV&Po and VO&Pr, the distribution shown in Table 20 is relatively symmetric: there are 9 families in which the percentage for OV&AdjN and VO&NAdj is less than 50 % and 14 families in which the percentage for OV&AdjN and VO&NAdj is more than 50 %. The obvious explanation for this difference is that for the order of adposition and noun phrase there is a universal bias towards OV&Po and VO&Pr, but with the order of adjective and noun, there is no bias towards any combination of the two orders. Nor does there appear to be any relationship between family size and percentage of languages that are OV&AdjN or VO&NAdj. If we look at families with 50 or more lan-

guages in Table 20, we find the percentages that are OV&AdjN or VO&NAdj widely distributed, with percentages of 11 %, 20 %, 44 %, 63 %, 63 %, 80 %, 80 %, and 82 %.

5. Are the differences that Dunn et al. find among families due to random variation?

As mentioned above, I will address a number of issues surrounding Dunn et al.'s method elsewhere. Here, I will discuss just one problem: I will argue that there is no evidence that the differences among families that Dunn et al. document are not due entirely to random variation. Under any theory, we expect some differences among families to be due simply to random variation.

An analogy may be useful here. Suppose we were to observe a group of people playing poker for an evening and suppose that one of the players is dealt a large number of good hands while another is dealt very few good hands. We would normally attribute the difference between the hands dealt to these two players as due to random variation, rather than concluding that the probabilities of being dealt good hands were different for the two players. Similarly, where there are differences among families of the sort that Dunn et al. report on, the probabilities of finding certain combinations of word order characteristics may be the same for all language families, with the differences among the families arising only because of random variation. Is there some reason to believe that the differences Dunn et al. observe are in fact due to anything other than random variation?

There is a simple way to get an idea about this. In general, if removing two or three tokens from a sample makes an effect disappear completely, then that strongly suggests the difference could be due to random variation. If we exclude from consideration languages which are OV/VO or Po/Pr and restrict attention to those languages which have a dominant value for both of these two parameters, consider the one case of a noncorrelating family among the four families examined by Dunn et al., Uto-Aztecan (one I have discussed in Dryer 1989b). Figure 1 is a copy of the tree from their paper, altered in that (i) I have crossed out the languages which are coded as lacking a dominant order since they need to be excluded for reasons given below; (ii) I have added the word order type after the language names; and (iii) I have added numbers as labels for three subgroups I need to refer to, where the number is placed immediately to the right of the node in the tree defining that group.

If one examines the tree in Figure 1 phylogenetically (and ignores the languages which are crossed out), then one plausible scenario is that Proto-Uto-Aztecan was OV&Po and that three changes occurred: (i) a change to VO&Po in group 1 (the Tepiman languages); (ii) a similar change in Cora, the upper branch of group 2; and (iii) a change to VO&Pr in group 3 (the Aztecan lan-

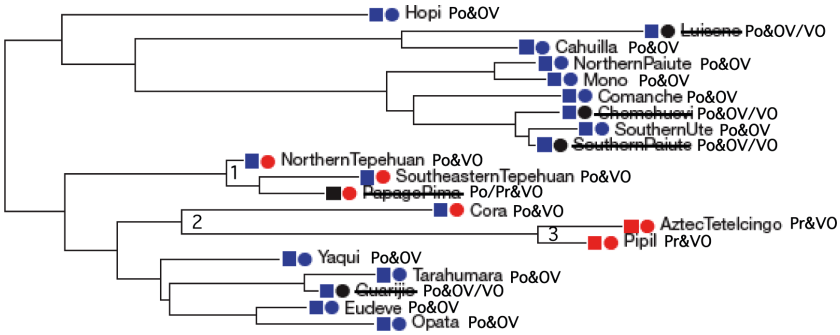

Figure 3. *Dunn et al.'s tree for Uto-Aztecan with some annotations*

guages). Note that the first two of these changes are changes to an inconsistent word order type while the third is a change to a consistent type.

Now what if we were to remove the two branches that involve a change to an inconsistent type? If one removes these two branches, the family becomes a correlating family, since all the languages in the family will be OV&Po or VO&Pr. If we then further remove the one branch which is VO&Pr, the family becomes a one-type family. Since removing two or three branches can change the family from a noncorrelating family to a correlating or one-type family, we have no reason to believe that the differences between Uto-Aztecan and the other families is due to anything other than random variation.

Conversely, consider the case of Austronesian, which is a correlating family. Most Austronesian languages are VO&Pr. The OV&Po languages are all found on two branches both of which contain only languages of Papua New Guinea, one with Manam and Kairiru, the other with Motu, Mekeo, Gapapaiwa, Kilivila, Saliba, Bwaidoga, and Dobuan, except that Kilivila is VO&Pr. If we make the simplest assumption phylogenetically that this group was originally OV&Po and that there was a single change in Kilivila, then we have three changes from one consistent type to the other, the change from VO&Pr to OV&Po in the two branches just described, plus the change from OV&Po back to VO&Pr in Kilivila. If we remove these three cases, Austronesian becomes a one-type family. Thus the differences Dunn et al. observe might be due entirely to random variation.

Now the argument that Dunn et al.'s results might be due entirely to random variation is based on excluding languages which are OV/VO or Pr/Po, languages which allow both orders of one of the two pairs of elements without either order being dominant (in the sense explained in Dryer 2011f). It is not clear whether Dunn et al. included these in their calculations, but I assume they did. If we do include these languages, then it is probably more difficult to

attribute the differences to random variation, though to what extent this is the case is not clear from Dunn et al.'s results.

However, there is a problem with including these languages, at least for the languages that come from my *WALS* chapters (Dryer 2008b, c, d), which is their source for the word order data for the majority of languages in their data in the three families other than Indo-European. As explained in Dryer 2011f, I code languages as lacking a dominant order if my source says that both orders are possible without indicating that one is more common than the other. What this means is that two descriptions of the same language might lead to two different codings. One description might describe a language as allowing both orders of verb and object without indicating whether one is more common. The second description might describe the same language as allowing both orders but say that OV is more common. If my source was the first of these, I would code the language as OV/VO. If my source was the second, I would code the language as OV. This issue manifests itself most strongly with Uto-Aztecan, where Dunn et al.'s tree shows four languages coded as OV/VO where the most closely related language is coded as OV. But many of the Uto-Aztecan languages (like many languages in North America) show considerable freedom of word order and the differences here in the coding of the order of verb and object among the Uto-Aztecan languages are at least partly if not entirely due to different sorts of information in my sources. For example, Chemehuevi and Southern Ute are dialects of the same language, but Chemehuevi is coded as OV/VO since Press (1979) implies that both orders are possible without information about whether one order is more frequent, while Southern Ute is coded as OV since Southern Ute Tribe 1980 (written by T. Givón) states explicitly that OV order is more common than VO order. So there is actually no reason to believe that there is any difference in word order between Chemehuevi and Southern Ute, despite the fact that I code them differently. Using differences in word order based on these cases of two values for a parameter without one order being dominant will lead to artificial results: Dunn et al.'s tree implies that there was a change in word order in the Chemehuevi dialect from OV to OV/VO; but there is no reason to think that any such change occurred. If we exclude these languages, then the difference between Uto-Aztecan and the other families can be attributed entirely to random variation.[13]

13. The difference we find between Papago-Pima, which is coded as Po/Pr&VO, and the Tepehuan languages, which are coded as Po&VO, probably reflects a real difference. This would mean a fourth change in Uto-Aztecan from Po to Po/Pr. But this does not affect the argument given in that first, there are still only three changes distinguishing Uto-Aztecan from a correlating family and second, without knowing on a case-by-case basis which instances of differences between languages coded as having two orders and ones coded as having one order are real differences, including these languages is not legitimate.

6. Further issues

In this section, I will discuss briefly further issues which await elaboration at a later date. I will make a set of assertions which ultimately require further evidence or argumentation but which I believe, for completeness, should be stated here.

6.1. *The role of contact and the use of biological models*

First, while I have argued that some of the differences among families that Dunn et al. report on may be due entirely to random variation, I would also argue that any other differences are probably due to different contact situations. There are, in fact, a number of different ways in which contact presents a problem for them. Especially with word order, typological features are often borrowed from languages in one family to geographically adjacent languages in a different family. One of the primary theses of Dryer 1989a was that there is evidence of large scale areal phenomena covering much of entire continents. Further evidence for this claim is presented in Dryer 2010 for sub-Saharan Africa, where it is shown that postnominal modifiers are more common throughout sub-Saharan Africa than elsewhere in the world. The problem is not simply that contact-induced word order change creates a problem that Dunn et al. have not controlled for. It is more severe than that: for many typological parameters, including many word order parameters, the distribution of language types is primarily areal, not genealogical, in that languages in different families that are closer geographically are more likely to be similar typologically than languages in the same family that are more distant from each other. One of the reasons I proposed using genera as a useful genealogical grouping for typological studies in Dryer 1989a is that there is little reason to believe that genealogical groupings deeper than genera are relevant in understanding the distribution of typological characteristics, either synchronically or diachronically. My claim is that in constructing realist diachronic models of typological change, genealogical groupings beyond the level of genus will often be irrelevant. Where there are similarities shared among different genera in the same family, the primary explanation will often be similarities reinforced by contact, not genealogy. I recognize that I would need to provide evidence for these claims, but examining families divorced from their geographical context, as Dunn et al. attempt to do, makes no sense and their results are an artifact of that.

Dunn et al. employ methods from biological evolution, but I believe that language evolution is so fundamentally different from biological evolution that these methods are simply not applicable in linguistics, at least as far as typological features are concerned. Imagine if biology had been different. Suppose it had been the case that 5 % of the DNA which individuals passed on to their off-

spring had been DNA that they had not been born with but DNA that they had somehow acquired from other species in the environment, often species that were very different from them genetically. If that had been the case, biological phylogeny would have been very different from the way it is. And biologists would never have developed the methods that Dunn et al. use because those methods would simply not have fit the nature of the data. But since the distribution of typological characteristics in the real world is somewhat analogous to biology in this hypothetical world, the method that Dunn et al. employ cannot be used to study the distribution of typological features.

Greenhill et al. (2009) address the question of contact or borrowing indirectly, where they argue that horizontal transmission (i.e., borrowing) does "not invalidate a phylogenetic approach to cultural and linguistic evolution". However, the evidence that they present in that paper does not affect the problems that contact provides for the methods used in Dunn et al. 2011. Greenhill et al. consider two possible scenarios of borrowing. The first is that "languages are equally likely to receive a particular trait from any other contemporaneous language", but they ignore this as implausible. The second is that "a language can borrow only from those lineages with which it shares a common ancestor within a specified time period". But this second scenario ignores the most familiar type of borrowing that occurs with word order, namely borrowing from an unrelated language, for which evidence in the literature abounds. Dunn et al. focus specifically on word order changes, but it seems very clear from the evidence that I have elucidated in various publications (Dryer 1989a, 1992, 2003, 2008a, 2010) that the majority of such word order changes occur due to contact with unrelated languages. What is particularly problematic for Dunn et al. is that languages on distinct branches of the same family may all acquire the same features due to contact. Dunn et al.'s method would see these acquisitions of the same feature as the same or similar change occurring in independent branches of the family and their methodology would then lead them to conclude that there was a dependency between the traits, when the traits might simply be borrowed features that accidentally co-occur due to borrowing of the same feature by a number of the languages in a number of branches of that family.

6.2. *The notion of lineage-specific dependencies*

What could it mean for dependencies between the word order of two pairs of elements, like the order of verb and object and the order of adposition and noun phrase, to be lineage-specific? It is clear what it means in statistical terms. But what could it mean in terms of the real world? What exactly would be going on in the different lineages such that there might be a dependency in one lineage but not another?

Let me first address a related but distinct question: what sources might there be for the differences among families that Dunn et al. report on? Two sources I have already discussed: random variation and different contact situations. In the case of the correlation between the order of verb and object and the order of adposition and noun phrase, I suspect that random variation and different contact situations are the primary sources of the differences they report. But there is another possible source of differences, one that I believe is more likely to play a role with some of the other correlations where Dunn et al. find a correlation in some but not all families. And this is grammatical differences between families other than the word order parameters under investigation.

An example of this may be various differences Dunn et al. observe among families in terms of dependencies involving the order of numeral and noun. As discussed by Dryer (2007, 2011c), there is considerable variation among languages in terms of the sorts of constructions that languages use to express what is expressed in English with a numeral modifying a noun. In some languages, the numeral seems clearly to be modifying the noun, while in other languages, the construction has properties that suggest that it is possibly the numeral which is head and the noun a dependent. This includes cases where the construction resembles to some extent the genitive construction, where the numeral resembles the head noun in the genitive construction and the noun that denotes whatever is counted by the numeral resembles the possessor noun phrase. A special case of this is languages with numeral classifiers, where it is not really the numeral that is the head, but the classifier. But even in some cases of this, the numeral + classifier combination seems to function as the head, again with the noun as a dependent of this numeral + classifier.

This may account for the fact that there are large-scale areal differences in the way the order of noun and numeral interacts with other word order characteristics. In light of the possibility raised in the preceding paragraph, that the numeral + classifier combination may be the head of the noun phrase and the noun a dependent in some languages, I would like to examine the relationship between this pair of elements and the order of noun and genitive, since in some languages, the numeral + classifier combination behaves somewhat like the head noun in the genitive construction. Tables 21 and 22 give the relevant data in the same form used in Section 2 of this article, although in this case, the numbers in Table 21 represent numbers of languages rather than numbers of genera.

Table 21. *Order of genitive and noun and order of numeral and noun: numbers of languages*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Total
GN&NumN	15	**92**	25	23	**37**	**51**	243
GN&NNum	**56**	0	**92**	**90**	21	17	276
NG&NumN	20	**22**	**70**	1	**37**	**7**	157
NG&NNum	**162**	0	38	**10**	0	0	210

Table 22. *Proportions of languages that are NumN*

	Africa	Eurasia	Southeast Asia & Oceania	Australia-New Guinea	North America	South America	Average
GN	**.21**	1.00	.21	**.20**	.64	.75	.50
NG	.11	1.00	**.65**	.09	**1.00**	**1.00**	.57

The first thing to observe is that comparing proportions of languages in Table 22 shows that there is no evidence of any crosslinguistic correlation between the order of these two pairs of elements: there are two areas where NumN order is more common among GN languages, three areas where NumN order is more common among NG languages and one area (Eurasia) where all the languages are NumN (and hence no difference in proportions). The second thing to observe is that Table 22 shows that the order of NumN correlates more strongly with area: there are three areas in which the proportion of genera which are NumN is .64 or more both among GN languages and among NG languages, and two areas that are very different, namely Africa and Australia-New Guinea, where the proportion of languages which are NumN is .21 or less among both GN and NG languages. In only one area do we find a pattern that looks like a possible dependency between these two pairs of elements, in Southeast Asia and Oceania, where the proportion of languages which are NumN is noticeably higher among NG languages (.65) than it is among GN languages (.21).

I suggested above that the construction with numerals and nouns may resemble the genitive construction in some languages with numeral classifiers. This would actually predict a possible dependency between the order of noun

and numeral and the order of noun and genitive in Southeast Asia and Oceania, because this is the one area of the world where numeral classifiers are found in the majority of languages (Gil 2011). In other words, it would predict that we should get NumN more often among NG languages than among GN languages because the Num in the NumN combination is like the N in the NG combination. And this is exactly what Table 22 shows we find in Southeast Asia and Oceania.[14]

What this suggests is that Dunn et al. are right in one of their basic claims: it is possible to have a correlation within one family without there being any universal bias. However, this conclusion must be tempered by a number of considerations. First, because of the problems of the possibility of random variation and the influence of contact, we cannot tell from their results which differences among families that they report on might be due to random variation or different contact situations and which might be due linguistic considerations of the sort illustrated by the case involving orders of genitive and numeral with noun just discussed. Second, this example suggests that the dependencies are not associated with lineages but with grammatical types. In other words, languages with different grammatical properties but in the same family might differ in terms of whether there is a bias towards a particular dependency (because of differences in their grammatical properties). And languages with similar grammatical properties but in different families might be similar in terms of whether or not there is a bias towards a particular dependency because of their properties. Hence the differences among families that emerge using Dunn et al.'s methods may be epiphenomenal: they may simply reflect grammatical types that are more common in one family than another.

Furthermore, testing these relationships between word order parameters by examining individual families, as Dunn et al. do, is of limited value in determining whether the differences might be due to factors like those involved with the order of numeral and noun. Rather we need to examine languages throughout the world to identify relationships between grammatical types and word

14. One of the strongest dependencies that Dunn et al. report on is a dependency between the order of noun and genitive and the order of noun and numeral in Austronesian, which is within the Southeast Asia and Oceania area. But curiously, unless I misunderstand their coding, the dependency they claim is one whereby GN order correlates with NumN order, precisely the opposite of what I have suggested for this area in the main text. I suspect that this discrepancy arises because of the extreme variability in the order of numeral and noun in Austronesian: under one set of assumptions, implicit in Dunn et al.'s methodology, the history of these orders in Austronesian involves more changes from NG&NumN to NG&NNum than the reverse, while under alternative assumptions, consistent with the parallelism of numerals to head nouns in genitive constructions, there have been more changes from NG&NNum to NG&NumN. Under the former set of assumptions, we would get a positive correlation (as they report), while under the alternative set of assumptions we would get an inverse correlation (as the hypothesis I discuss and my data would suggest).

order dependencies because it is only by examining a large number of families that we can determine such relationships. The evidence above suggesting that there may be a dependency between the order of noun and genitive and the order of numeral and noun in languages with numerals classifiers but not in languages in which the numeral is more clearly modifying the noun is based, not only on languages in Southeast Asia, but in languages throughout the world as a whole.

6.3. *The role of cognitive factors in governing word order change*

Dunn et al. claim that cognitive factors do not play a significant role in determining word order in languages. I believe that they are largely correct about this. Both Dryer (1992) and Hawkins (1994, 2004) have proposed that the word order correlations reflect the nature of sentence processing. But if these processing factors were very strong, it would not be possible for languages to change from consistent word order types to inconsistent types. But it is clear that changes of this sort do happen, and not infrequently. And while there are various explanations for how these changes happen, by far the most important explanation is contact. It is very common for contact-induced change to change a consistent language into an inconsistent language. For example, a number of Uralic languages have apparently changed from consistent OV&Po order to inconsistent VO&Po order due to contact with VO Indo-European languages. Clearly, then, contact is a more powerful determinant of word order than cognitive factors. And in so far as the factors that lead to contact and the factors that determine the precise nature of the contact situation are part of cultural evolution, broadly construed, Dunn et al. are right that cultural evolution is more important than cognitive factors in governing word order change.

There is no reason to believe that there are any combinations of word order that are impossible. Most generalizations about word order are statistical in nature and those that are not are most likely accidental gaps in the data. What this means is that there cannot be powerful cognitive constraints on word order. The sort of cognitive explanations offered by Dryer (1992) and Hawkins (1994, 2004) are offered as explanations for relative numbers of languages. Explanations for relative numbers of languages in terms of cognitive factors necessarily involve relatively weak factors.

An analogy from phonology is useful here. Maddieson (1984) reports, based on a study of 317 languages, that most languages have stop phonemes in the labial, dental/alveolar, and velar positions, and most languages have nasal consonant phonemes in the labial and dental/alveolar position, but that only about half the languages in his sample had phonemic velar nasals. Ohala (1975) offered an explanation for this: because the nasals all share additional acoustic features due to their nasality, the perceptual distance between nasals is less

than the perceptual distance between stops. Because the contrast between [n] and [ŋ] is harder to hear than the contrast between [t] and [k], fewer languages utilize that contrast. But while the difficulty hearing the differences between [n] and [ŋ] must be strong enough to account for the fact that only half of the languages in Maddieson's sample have a phoneme /ŋ/, it must also be weak enough so that half the languages of the world have such a phoneme. If the difficulty hearing the contrast were very great, it would not be the case that half the languages of the world have such a phoneme. The situation with cognitive explanations for word order correlations is no different: they must be strong enough to cause differences in numbers of languages of different word order types, but weak enough so that many languages exist despite these cognitive factors.[15]

7. Conclusion

I have argued that there are many problems with Dunn et al.'s paper. All of their results that they claim are a problem for existing claims are in fact fully consistent with existing claims. The number of language families that correlate in their sense is quite consistent with the claim that the word order correlations are universal. In some cases, the number of families exhibiting a correlation in their sense is small only because they fail to consider interacting dominance principles.

The differences they report among different families for various correlations provide no evidence that the word order correlations are lineage-specific. Some if not all of the differences appear to reflect nothing more than random variation. Some of the differences arise due to different contact situations. The role of contact-induced word order change presents a serious problem for their methodology, especially where similar changes in different branches of a particular language family appear to be independent changes phylogenetically, when in fact they all reflect the same contact situation (Section 6.1). Examination of the distribution of inconsistent word order types with at least one of the correlations provides evidence that the inconsistent types are short-lived, apparently reflecting the fact that it is normal for changes from one consistent type to the opposite consistent type to pass through an inconsistent stage (Section 4.3). And while there may be some instances of what appear to be real

15. On the other hand, I believe that there are many other domains where the impact of cognitive factors is greater than with word order. For example, the generalizations in terms of markedness discussed by Croft (2002) are generally exceptionless or close to exceptionless, such as the fact that there is no language in which singular and plural forms of nouns always differ in that the singular involves an affix while the plural is formally unmarked. I assume, however, that Dunn et al.'s claims are directed specifically at word order and that they would admit that there are some domains where the impact of cognitive factors is overwhelming.

differences among lineages, these differences may actually reflect differences among grammatical types, not differences among lineages (Section 6.2). On the other hand, their claim that cognitive factors do not play a major role in word order change appears to be correct.

It is not clear, however, what exactly Dunn et al. intend when they claim that cultural evolution is the primary factor that determines linguistic structure. Since, in my view, the primary factor that determines word order change is contact and since contact is part of cultural evolution, I have found an interpretation of their claim that I can agree with. But is that what they intend?

Let me give an example to illustrate an interpretation that I hope that they do not intend. In Section 2.3, I have argued that there are two competing motivations that result in both orders of noun and relative clause being common in OV languages, a harmony principle favouring OV&RelN and a dominance principle favouring OV&NRel. One hypothesis for what underlies these two principles is that the harmony principle is ultimately motivated by the nature of sentence processing, as argued for by Dryer (1992) and Hawkins (1994), but the dominance principle is motivated by the nature of sentence production. I will not try to justify this latter suggestion here, since I am only using this example as part of a hypothetical example to illustrate what I hope Dunn et al. do not mean.

Now suppose someone were to propose that languages like Japanese are OV&RelN rather than OV&NRel because it is a feature of Japanese culture to be unselfish, and placing relative clauses before nouns makes sentence processing by the hearer easier but production slightly harder. And suppose they were to propose that languages which are OV&NRel are such because it is a feature of the cultures of speakers of such languages to be selfish and placing relative clauses after nouns makes sentence production easier, at the cost of greater difficulty for the hearer to process sentences.

I hope that this is not the sort of relationship between language type and culture that Dunn et al. have in mind since I do not think there is any reason to believe that there is any relationship between the word order of a language and properties of the culture of speakers of that language, and it has been a fundamental principle of modern linguistics that language and culture are not related in this way. However, such an interpretation is not only consistent with what Dunn et al. say but it is my impression from communications from some nonlinguists that this is exactly how some nonlinguists have interpreted their *Nature* article. Hopefully, Dunn et al. will clarify just what they intend.

One thing that I hope occurs as an outcome of Dunn et al.'s article is that more attention will be paid to examining word order from a diachronic perspective. Although my work on areal phenomena with word order is implicitly diachronic, my quantitative work has been purely synchronic. Ultimately, the

synchronic distribution of word order types reflects the nature of word order change.

And detailed examination of particular language families can be fruitful, but only if one examines them not only in terms of their genealogy but also in terms of their geography. Much of Dunn et al.'s word order data comes from the *WALS* atlas (Haspelmath et al. (eds.) 2008). But the *WALS* atlas is, after all, an atlas. There are three types of data in the *WALS* atlas: typological, genealogical, and geographical. Using this data to achieve significant results requires using all three of these types of data. The largest problem with Dunn et al.'s work is their failure to use all three.

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