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Aboh, E.O.

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The emergence of hybrid grammars: A rejoinder to Peter Bakker

Enoch O. Aboh*

Department of General Linguistics/ACLC, Faculty of Humanities, University of Amsterdam, Spuistraat 134, Room 647, 1012 VB Amsterdam, The Netherlands

Peter Bakker's review of my book *The emergence of hybrid grammars: Language contact and change*, published in *Word* 62, 4, 228–243, amounts to a catalogue of misrepresentations and misconceptions of the positions I developed there. These claim 12 of his 14-page review. In what follows, I first summarize the hypotheses I developed in the book, intended now for those who have not read it yet, and then I expose Bakker's inability or unwillingness to interpret the book accurately and critique it competently.

1. *The emergence of hybrid grammars: background information and clarification*

In the abstract of his review, Bakker writes:

Creoles are seen as a result of failed acquisition. It is shown in the article that Bantu speakers were more numerous than Gbe speakers, on the basis of historical, genetic and linguistic evidence. Still, Bantu languages are structurally quite different from the creoles, which can be taken for an argument that Gbe as well only had a minimal influence on the grammatical structures of the creoles. The analytic structures are argued in the article to be as diverse for creoles as they are for the Gbe languages. Some structures are found to be different in all three creoles and five varieties of Gbe. (228)

While this passage summarizes Bakker's own view of how creole languages emerge, it also reveals his misinterpretation of the position I articulate in *The emergence of hybrid grammars*. I do not argue in it that Gbe languages form the grammatical basis of Suriname and Haitian creoles. Neither do I argue that the analytic structures of these creoles reflect influence from Gbe languages. Although there is room for Bantu to have contributed to their divergence from their lexifiers, this is still to be investigated (more on this below). The quotations below demonstrate that Bakker indeed read the book selectively, overlooking the essence of the positions I develop.

The parallels that Aboh draws between Gungbe and sometimes other Gbe or Kwa languages, demand historical proof that Gbe indeed was the language that impacted Haitian and the Surinamese creoles most. If you want to argue that one language, or one restricted group of languages (Gbe and Kwa), was responsible for most if not all of the non-European structures in Atlantic creoles, you need to provide proof based on the facts of the slave trade, with multiple sources of origins and destination, differing over time. (234–235)

*Email: e.o.aboh@uva.nl

Aboh's central claim that the creoles are hybrids cannot be maintained if there were many Bantu-speaking slaves – and there were many of them. The creoles may have close to zero Bantu traits, and certainly several Gbe-like traits. If Gbe is indeed important and Bantu negligible, we should expect close to zero Bantu lexicon and substantive Gbe lexicon. (239)

The historical information Bakker is asking for is provided in Chapter 2, pp. 16–59. The rest of his observations shows that he completely missed the point of the book. One of my central theoretical assumptions is that language acquisition always involves heterogeneous inputs. These inputs are hypothesized to be qualitatively similar to those of the multilingual societies that led to the emergence of certain creoles during the European colonial expansion since the seventeenth century. The book argues that learners cannot reproduce the heterogeneous inputs they are exposed to. Instead, they develop mental grammars that are recombinations of linguistic features selected from the inputs, subject to competition and selection. The result of the recombination is by default a hybrid system. The inputs from which creoles emerged included not only their lexifiers but also the various substrate languages, including Gbe, Bantu, Germanic, Romance, and Amerindian. Together they generated systemic heterogeneity for the language learners. A major aim of my book was to understand how learners develop their grammars out of heterogeneous inputs and why languages change as a consequence of acquisition. This position is articulated at the outset of the book, in the Introduction chapter, which Bakker appears to have skipped. While focusing on structures of creoles, and Gbe versus Bantu influence, he overlooked why the book discusses creoles and the bigger picture I develop about the mechanisms of language acquisition and change.

An obvious question that arises regarding language acquisition and change is how competition and selection account for the recombination of linguistic features. The book addresses this question, showing how the recombination of linguistic features proceeds and what types of linguistic features might be involved in this process. It explores two sets of creole languages: Haitian Creole, lexified by French and Sranan and Saramaccan (spoken in Suriname), lexified primarily by English. Chapter 1 makes clear that the choice of these creoles is not based on a presumed creole typology, but rather on the following facts:

- Creole languages are generally young languages which emerge from the combination of typologically and genetically different languages.
- Though Haitian Creole and the Suriname Creoles have different lexifiers, they have the same substrate languages, which include Gbe (in the Kwa family) and Kikongo (which is Bantu).

The combination of these facts enables us to engage in reverse engineering in order to identify specific features that were recombined during the formation of these creoles. Such a study, however, is only possible if we have the right expertise in the morphosyntax of the languages involved.

The theory developed in my book is neutral as to whether or not a restricted set of languages were the main contributors to the feature pool. Since the book argues that linguistic hybridization is the norm during acquisition, claims about some main contributing languages versus negligible contributors would be self-contradictory.

Chapter 4 of the book argues that the contributor languages serve different functions within the community. As a result of inter-speaker dynamics, any source language that contributed to the primary linguistic data can shape aspects of the emerging grammars. This is consistent with the conclusion of Chapter 3, which shows that monolithic theories that assume one (or a unique major) contributing factor (e.g. superstrate/substrate influence, the Language Bioprogram, Conventionalized Interlanguage) are all inadequate. It is not clear to me how Bakker missed this discussion.

The argument for the recombination of linguistic features guided by competition and selection raises three questions:

- (1) Who were the learners on the plantations in Haiti and Suriname?
- (2) What languages were they native speakers of?
- (3) How did recombination operate on these languages to generate the creoles?

In order to answer these questions in a meaningful way, we need to have a very good knowledge of the socio-historical and political situations in which Haitian Creole, and the Suriname Creoles emerged. We must also have the relevant expertise in the source languages, if we hope to demonstrate that they provided the features that were recombined into the creoles. Without meeting these two conditions, we run the risk of making erroneous claims. In my response to Bakker's column in the *Journal of Pidgin and Creole Languages*, I showed that the error rate in Bakker et.al (2011) data point was up to 34.8% for Yoruba and 39.5% for Ewegbe (cf. Aboh 2016, Fon Sing 2017). The following section also highlights some mistakes in Bakker's analysis of possessive constructions in Gbe. In order to avoid such pitfalls, I made the methodological choice of focusing on Haitian Creole, the Suriname Creoles, their respective lexifiers French and English, and the Gbe languages. I have worked on these languages for the past twenty years and can claim that I have the minimum expertise necessary to engage in a detailed description of their morphosyntax. Accordingly, my book presents a few relevant case studies rather than making general claims about all grammatical phenomena in the creoles and their source languages.

Thanks to detailed studies by the late Jacques Arends, Bettina Migge, John Singler, Norval Smith, Don Winford, and much related work, we know that Gbe and Kikongo-speaking people were enslaved in Suriname and Haiti. What we know less (in creolistics) is how the slave trade was organized in West and Central Africa, and the impacts of this on intra-community dynamics that could have played a role during the formation of the relevant creole societies. My book bridges this gap by showing that while Gbe and Kikongo were present in significant numbers in Haiti and Suriname, the Gbe seemed to have been instrumental in the formation of these creole societies, especially regarding certain aspects of their cultures, religions, and languages. The reason for this seems to be the way the slave trade was organized on the Slave Coast. In a nutshell, Chapter 2 shows that the Gbe people deported to Haiti and Suriname originated in a narrow coastal territory (spanning 200 km inland) sandwiched between several competing Kingdoms. Accordingly, significant numbers of enslaved people, who arguably belonged to related ethnic groups from this area, ended up in plantation camps during creole formation. The enslaved Bantu people, on the contrary, were captured from larger areas, sometimes within a range of 1000–1500 km inland. These enslaved Bantu presumably belonged to different ethnicities. In addition, the enslaved Gbe people were deported in large cohorts

over short periods, while the Bantu seemed to have been deported in smaller ones over longer periods. Because of these differences in timing and in the way slavery was organized on the West African coast and in West Central Africa, the Gbe speakers sold to the colonies came in compact cohorts and could have developed tighter relations allowing in-group communication. Things seem different for the enslaved Bantu speakers who might have formed looser communities. As a result of these differences, the Gbe might have exerted more influence on certain aspects of the emerging cultures (e.g. vodou religion, medicine, etc.). Many of these cultural aspects involve linguistic skills and could have favored some Gbe features during the competition and selection. Other factors that may have favored the Gbe to some extent in this particular ecology are the structural similarities within the Gbe group and between Gbe and other Niger-Congo languages.

As the discussion on page 47 of Chapter 2 and in subsequent chapters in my book shows clearly, all these factors in favor of Gbe should not make one forget that Bantu languages did contribute to these creole languages, as made obvious by Smith's (2009) study on the retention of Kikongo noun classes in Saramaccan, cited on page 121 of the book. In light of this demonstration, it is clear that Bakker's conclusion that Kikongo speakers were present in Suriname and Haiti only restates my own conclusion in Chapter 2. Bakker appears not to have read this and his comment adds nothing that was not already said in my book. If he knows of intra- and inter-group dynamics or other relevant social factors that could have favored the Bantu influence over Gbe influence, he should have articulated them. He simply states that "the creoles may have close to zero Bantu traits" but provides no empirical evidence to support his claim.

According to Bakker's rationale, Bantu speakers had almost no influence on the creoles despite their massive number. Therefore, one must also conclude that the Gbe languages (whose speakers were less numerous than Bantu speakers) could not have had any significant influence on these creoles (see page 228 of his review). The argument is of course futile. As has been known to sociolinguists since the work by William Labov in the 1960s, the number of some learner profiles alone is neither a sufficient nor a decisive factor in determining which linguistic patterns will spread across a community and become conventionalized. For instance, my book shows that, other than from Gbe, Haitian Creole and Suriname Creoles inherited some syntactic properties from French, and English, respectively. The European languages influenced the creoles in many respects, even though the European colonists represented a small minority in plantation colonies. This is unexpected under Bakker's rationale.

In his review, Bakker overlooks my analysis of superstrate-derived properties of creoles, which indicate clearly that I have not focused on Gbe influence only. Bakker is too determined to explain creoles' structures based on the demographic proportions of the populations in contact that he fails to notice the obvious. My book focuses on relevant case studies based on the Gbe languages for which we have precise knowledge of socio-historical as well as linguistic factors. In addition, the book urges specialists of other source languages (including Bantu and Amerindian languages) to undertake similar studies in order to identify which grammatical aspects of these languages were restructured into the relevant creoles. The possibility of structural influence from these languages cannot be dismissed as casually as Bakker would like us to do, without adducing one shred of linguistic evidence to support his claim. One must wonder whether Bakker did not understand the book or was unwilling to consider the merits of a position that is contrary to his.

In focusing on some relevant case studies, the book demonstrates that linguistic recombination is selective: not all features behave similarly during competition and selection. Consequently, the cases for which we can identify the source languages are limited to certain aspects of the creole's grammar only: creole languages select features from their source languages, but these features are systematically adapted to the emerging new grammar. The latter is different from that of the source languages in many ways. Therefore, Bakker's objection that "recombination is shown for a couple of cases" is pointless and exposes his failure to understand the basic approach of the book.

The theory developed in *The emergence of hybrid grammars* predicts that we can show the contribution of certain source languages (i.e. features that are selected by the creole creators) for some cases only. The reasons for this are presented in detail in Chapters 5, 6, and 7, which show how selective the recombination process is. These chapters discuss which types of features are more likely to be selected. This discussion is based on empirical data related to the nominal and clausal left peripheries. It appears that features related to these peripheries (DP and CP) are particularly involved in the process of recombination because these structural domains represent interfaces between the predicate or proposition level and the discourse level. This finding is consistent with observations already made in the SLA literature, which show that interface properties are vulnerable during acquisition. The book therefore takes the analysis a step further in showing that vulnerable features are also those that allow recombination and may underlie typological variation. Unfortunately, Bakker completely overlooks this discussion, although he concedes his unfamiliarity with the framework I use; he concludes that the argumentation is built "on complicated generative analyses." It would have been more honest and courageous of him to decline reviewing a book that he cannot understand (well). His incompetence explains why his discussion of these chapters does not go beyond superficial remarks and why there are so many misrepresentations of my positions, as I show in the next section.

2. Bakker's misrepresentations and misconceptions

For the sake of readability, I present relevant parts of Bakker's quotes only, ignoring many of his unfounded allegations. Quotes from the review are preceded by the expression "Bakker's rendition" followed by the page number in his review. The box following the quote contains my reaction introduced by the expression "my comment" and the actual formulation in my book (*Hybrid Grammars*) including the relevant pages.

Bakker's rendition (229): Aboh claims that all people mix the properties of languages they hear around them. In some cases, these are two or more varieties of English, in other cases for example French and Fongbe, leading to quite different results: some form of English in the first case, or a completely new language, a French-based creole, in the latter.

My comment: Unlike recombination, which applies selectively to linguistic features in the inputs, the term 'mix' implies an unconstrained process that generates varying outputs. Below is what I previously wrote about Bakker's use of the term 'mix' in an earlier article of his.

Hybrid Grammars (47): All these points lead to the conclusion that, morphosyntactically, Kwa languages form a typologically homogeneous group. This means that an enslaved African of Aja descent speaks a language that is typologically very close to the languages of the enslaved people of Nago or Edo descent. Accordingly, though it is often assumed that the slaves were of too diverse origins, this might not be true when considering their linguistic profile and the typological relations between the languages. Based on this, and taking into account the geopolitics of the Slave Coast, it appears that while it might be difficult for an enslaved person of Aja, Nago, or Edo descent to acquire a Central Bantu language readily, this must not have been the case when such enslaved people were confronted with a neighboring Kwa language.

Indeed, traditional considerations of linguistic diversity based on the vast and diverse area from which the slaves were imported do not do justice to the typological homogeneity of some subareas (and to the spread of some areal features in sub-Saharan Africa). Such structural kinship suggests that in a number of ways there were larger proportions of Africans sharing structural linguistic properties than suggested by geography-based demographics alone [...] This is an important factor to keep in mind when it comes to the intra group communicative strategies that the enslaved Africans might have adopted as well as the learning strategies they might have developed in their attempts to acquire European languages.

Bakker's rendition (231): He [Aboh] argues convincingly that the creoles were created among Africans, rather than between Europeans and Africans, and the new varieties were mediating and neutral languages.

My comment: This quote illustrates Bakker's failure to understand the book and the discussion in Chapter 4. A major conclusion of the book is that the creole emerged from an interaction between ALL inhabitants of the colony including slave masters and their slaves (rather than a creation of the enslaved Africans only, as Bakker asserts)! Section 4.1 is devoted to this hypothesis.

Hybrid Grammars (125): Under this understanding, it appears logical to conclude that the creole emerged as a mediation language between the plantation communities where daily interactions are 'negotiated': a contact language. As indicated by DeGraff (2001a: 251) quoting Schuchardt, "[T]he slaves spoke the creole not only with the Whites but also among themselves while their mother tongue was still in existence, the latter being moreover constantly revived to some extent by the continual immigration from Africa." From the perspective of the colonists, the creole therefore represents the language of efficient and orderly management of the enslaved population that guarantees a flourishing business: sugar and tobacco production or gold extraction. For (part of) the Africans, however, it represents the neutral language (just like official languages in modern Africa) that guarantees successful daily interaction with everyone on the colony. Yet, this language was probably not used for other survival purposes such as planning an escape or a rebellion.

Bakker's rendition (232): Again the Gbe languages are compared with Germanic (English) and Romance (French) and the creoles as outcomes of hybridization. He studies the combination of functional categories and syntax. He quotes a number of generative syntacticians who have studied multilingual acquisition, in which reanalysis of syntactic patterns is well documented, and he contrasts these findings with most theories

of creolization. The “extraordinary” (but apparently not exceptional) circumstances of plantation systems with multilingual work forces with a variety of agents, L1 and L2 speakers of several languages, is [*sic*] likened with bilingual acquisition in suburban middle class settings. Nevertheless, only this “extraordinary” situation has led to transfer from competing features, leading to creole languages.

My comment: The discussion does not bear on exceptional vs. unexceptional learning situations. Rather, the relevant point here is that creole languages emerged from an interaction between different learners’ profiles (including the colonists).

Hybrid Grammars (174): Clearly, the socio-cultural, economic, and political context of plantation colonies provides us with an extraordinary context where one finds not only bilingual and monolingual L1 learners forming the cohort of early learners in terms of Weerman (2011), but also child and adult L2 learners representing late learners. Consistent with the contact model described in Section 4.4, this implies that the agents of change in a creole setting possibly include:

- Monolingual L1 learners of the colonial vernacular
- Early bilingual L1 learners of the colonial vernacular and the emerging creole (and/or an African language). This essentially involves the White children, the Creoles, and the enslaved African children.
- Late L2 learners of the colonial vernacular (e.g. the newly arrived bozals)
- Late L2 learners of the emerging creole (both Whites and Black).

Given this description, it is reasonable to assume that the interaction between these different types of learners with their specific L-languages continuously fed into what would be identified later as a creole. Indeed, what is fascinating about the development of creoles is that the context of their birth made it possible for competing acquisition processes to arise simultaneously in relatively isolated communities under significant demographic changes.

Bakker’s rendition (232): Further he deals with topic marking (in Saramaccan *dé/dé* but no morpheme in Gungbe).

My comment: This statement is wrong as indicated by these examples and their discussion.

Hybrid Grammar (250):

(60)	a.	As for the chicken, I cooked it.				English
	b.	[Di gania] dé m̀ì bóí en				Saramaccan
		DET chicken TOP 1SG cook 3SG.ACC				
		‘As for the/this chicken, I cooked it.’				
	c.	Kòkló ló yà ùn ɖà è				Gungbe
		chicken DET TOP 1SG cook 3SG				
		‘As for the/this chicken, I boiled it.’				

The following difference is noteworthy: While Gungbe and Saramaccan exhibit a topic marker, *yà* and *dé*, respectively, which follows the topicalized constituent, English lacks such a marker.

Bakker's rendition (233): According to Aboh, hybridization leads to increased complexity in creoles, a point of view that goes against almost all other observers.

My comment: Note that Bakker does not give a single reference representative of 'almost all other observers', thus making his claim vacuous. More importantly, though, the book makes no claim about a possible correlation between hybridization and increase of overall complexity. Contrary to Bakker's claim, I argue that linguistic hybridization may lead to 'local complexity'. My book makes no claim about the overall complexity of creole languages as compared to non-creole languages (cf. pp. 15, 169, 302, 305). In Aboh & DeGraff (2017, note 6) we insist on "local complexity" to highlight our skepticism vis-à-vis various claims that certain languages can be overall more (or less) complex than others.

Bakker's rendition (234): Aboh hypothesizes that a matrix verb is elided (p. 228). It could be that Saramaccan does not use this deontic marker, or that not all speakers do.

My comment: This statement is inaccurate. In the book, I first report Damonte's (2002) study in which he argues for elision of the matrix V. My own analysis follows in section 6.1.2, on pages 230–232, in which I demonstrate why Damonte's analysis of elision of the matrix verb is misleading and cannot be maintained. I wonder how Bakker missed all such explicit arguments against elision of a matrix verb.

Hybrid Grammars, 229: Damonte (2002) therefore concludes that Saramaccan unique *fu* does not express deontic modality but derives such semantics from the higher selecting verb (e.g. *ábi* or *musu*). Yet, as Aboh (2006b, 2007a) shows, several facts about Saramaccan suggest that Damonte's (2002) analysis and related work are misguided. The following section recapitulates a few shortcomings of this analysis that are relevant to the discussion (cf. pp. 230, 232, 232).

Bakker's rendition (234–235): The parallels that Aboh draws between Gungbe and sometimes other Gbe or Kwa languages demand historical proof that Gbe indeed was the language that impacted Haitian and the Surinamese creoles most. If you want to argue that one language, or one restricted group of languages (Gbe and Kwa), was responsible for most if not all of the non-European structures in Atlantic creoles, you need to provide proof based on the facts of the slave trade, with multiple sources of origins and destination, differing over time.

My comment: I already addressed this point partially in the background section, in which I show that Bakker's claims are misguided. There is absolutely no claim in my book that the "Gbe or Kwa languages are responsible for most if not all of the non-European structures in Atlantic creoles". That is Bakker's own claim, and because he held on to this claim, he failed to see that his objections in which he invoked Bantu or other contexts of creole formation not involving Gbe are irrelevant. The interested reader is referred to Chapters 2, 3, and 4 for the motivation of my analysis in terms of competition and selection. Below is a single quote which should suffice to contextualise my book.

Hybrid Grammar (53): Here, I am particularly interested in whether a Gbe (or any other African language e.g. Kikongo) could have been retained within the Saramaccan society? If so, why were the African languages eventually displaced by the emerging creole (Mufwene 2005a, 2005b, 2008)? I will come back to these questions throughout the book.

3. Revealing Bakker's misleading allegations

Bakker's rendition (235): Aboh conveniently compares the creoles with Kwa languages, in particular Gbe, but is that perhaps a convenient choice in that the typological makeup of Bantu is strikingly far from Gbe and creole languages?

My comment: This allegation suggests that Bakker did not read the book carefully. As I mentioned in the background section above, the methodological motivations that underlie the choice of a comparative method between the Gbe languages and the lexifier languages of Haitian Creole and the Suriname Creoles are explained in the Introduction chapter and further discussed in Chapters 2, 3, and 4. Given that recombination operates on syntactic features, it is pointless to claim that "Bantu is strikingly far from Gbe and creole languages." I mentioned in the book and also in the background section above that Gbe and Bantu do converge on certain aspects (e.g. position of demonstrative, light verb constructions). We do not know the impact of such converging structures on creoles and this is why the book exhorts specialists of other languages (including Bantu) to engage in similar detailed comparative analyses in order to shed light on which features were retained from those languages, and how syntactic recombination proceeds. The choice of Gbe was therefore an informed one, in addition to being a convenient one due to my own expertise about Gbe.

Bakker spent several pages of his review claiming the numerical importance of the enslaved Bantu speakers, and how Bantu languages, Gbe languages, and the creoles are different. He did not provide a single piece of linguistic evidence showing that "Bantu is strikingly far from Gbe", nor did he demonstrate that the theory developed in the book falls apart when one considers syntactic aspects of Bantu. What we still lack in Bakker's review is a sound demonstration that in a learning situation involving Bantu and Gbe on the one hand and the lexifiers French and English on the other hand, the output of recombination under competition and selection must be different from the creole structures discussed in the book and accounted for in terms of hybrid grammar. Only such a work can falsify the theory developed in the book.

Since the book does mention potential contributions of Bantu, I repeat a few passages below which show the vacuity of Bakker's accusations. The reader is also referred to footnote 1 on page 17 and footnote 20 on page 39.

Hybrid Grammars (8): Following Mufwene (1996, 2001, 2005a, 2008), DeGraff (1997, 1999, 2001a, 2001b, 2002) and much related work, this book shows that creoles represent a normal instance of language change resulting from the contact between typologically different and genetically unrelated languages (e.g. Romance/Germanic vs. Kwa/Bantu [Niger-Congo]) that had been geographically far apart (Europe/America vs. Africa).

Hybrid Grammars (119): Indeed, the closeness of the Gbe languages as discussed in Chapter 2 can only favor this mutual learning. If we agree that some slaves (say some Eastern Gbe speakers) may have tried to learn or accommodate with other Gbe varieties (e.g. Western Gbe-like Ewegbe), then there is no principled reason to assume that as normal human beings endowed with language capacity they could not have tried the same with less related languages such as Yoruba or even typologically different languages such as Kikongo.

Hybrid Grammars (121–122): During the plantation phase, there was less direct contact with the colonists and I assume that this may have enhanced Afro-African interactions and hence the survival of more viable African vernaculars whenever possible. For instance in the case of Suriname, it is arguable that the Gbe and the Kikongo kept speaking such varieties in addition to learning the colonial language from each other. For instance, Smith (2009) shows that certain Kikongo words that are retained in Saramaccan display a deviant nominal class. Assuming that the Kikongo speakers were natives, it is improbable that they were the source of such ‘mistakes’ (notwithstanding individual cases of language attrition).

Bakker’s table on page 237.

My comment: This table is misleading because of some confusion Bakker seems to entertain about Kwa languages. In Joseph Greenberg’s classification, which I assumed in the book, Kru, Ewe, Akan, Yoruba, Igbo, Ijò belong to the Kwa branch of Niger Congo. Since Greenberg, there have been a number of classifications in terms of Kwa vs. Benue Congo, Benue Kwa etc. In all current classifications, these languages belong to sub-families of the same typological branch. I showed in Chapter 2 that many of the typological features of Gbe are common to these other Kwa languages as well. Therefore, in Bakker’s table, Kwa, Yoruboid, Ijoid, Edoid, Igboid, Nupoid, and Kru belong to the same typological class. With this in mind, one immediately realizes that the picture depicted by Bakker simply misses the point of the book: it is likely that speakers of these typologically related languages will entertain similar learning hypotheses about the lexifier languages. Therefore, typological similarities between these languages may converge to lead learners to produce similar outputs.

One may disagree with this assumption, but this is not what Bakker does in his review. All we are left with is a list of languages which the author assumes to be genetically different based on their names, but he does not give a single example showing that these languages share no structural property that could induce their speakers to formulate certain common learning hypotheses when confronted with French or English. Likewise, he does not show that structural properties differentiating these languages outweigh their similarities such that their speakers cannot converge to similar learning hypotheses.

Bakker’s rendition (238): Aboh does not provide numbers for Bantu, but his source (Anglade 1998) shows 216 Bantu etymons. This source points to considerable Bantu influence, but much more extensive Kwa and its subgroup Gbe. There are about as many words from Yoruba as from Bantu. These are the only sources where Kwa outnumbers

Bantu, but we have no data on the Gbe proportion among the Kwa words.[...] It should be added, though, that other specialists were rather critical of Anglade's book, and warned that it must be used with caution. Valdman (2001: 199) states that "at least half of the proposed etymologies do not meet minimal standards for reliability".

My comment: Bakker's objection here illustrates another misconception. The discussion about the list of words in Chapter 2 is not meant to evaluate language influence: one cannot deduce grammatical influence by simply counting the number of words retained in a language. This seems to be Bakker's own method, hence his conclusion that "this source points to considerable Bantu influence". Modern English retained an impressive set of French (or Latin-derived) words. Yet, one cannot simply claim on the basis of this fact that French or Latin influenced English grammar the most!

The causal relation between lexical borrowing and grammatical change is intricate. Lexical borrowing however, can be a proxy for the linguistic profile of the speakers of a community at a certain point in time. This is what the list discussed in Chapter 2 is used for: to identify the linguistic profile of the enslaved African populations in Haiti and in Suriname. Because this is only a proxy, the actual text repeated below clearly mentions the limitations of etymology-based hypotheses. These limitations led me to focus on the proportion of Gbe words which I could firmly identify. These words were then compared to the whole sample, regardless of other etymons. It could be that Bakker objects to my figures, if so he should demonstrate why. Unfortunately, no such demonstration is to be found in the review.

Finally, Bakker claims that the sample contains as many Yoruba words as his proclaimed 216 Bantu words. This is false, the source mentions 85 Yoruba words to which we can add 30 items from Nago (a dialect of Yoruba).

The actual text is reproduced below.

Hybrid Grammars (58): With this in mind, it is noteworthy that Anglade's (1998) lexical inventory includes 1,117 lexical items which the author relates to various African languages. As is always the case with this type of work, there may be misanalyses or mistakes here and there, but what is remarkable for our discussion is that 992 of these lexical items originated from Niger Congo. 581 of these items are related to Kwa and are distributed as follows: 356 Fongbe, 85 Yoruba, 44 Ewegbe, 30 Nago, 17 Ashanti, 11 Sefwi/Sehwi, 11 Gungbe, 8 Mina/Gengbe, 4 Agni, 4 Mahi/Maxi, 3 Baoule, 2 Igbo, 2 Fanti, 1 Pédah/Xweḍa, 1 Adja, 1 Nzima. Quite surprisingly, the Gbe languages provided 73 percent of the items on the Kwa list: 356 Fongbe + 44 Ewegbe + 11 Gungbe + 8 Gengbe + 4 Maxi + 1 Adja + 1 Xweḍa = 425. Weighed against the sample of 1,117 items investigated in this work, we reach the conclusion that the Gbe represents 38 percent of the sample. This would mean that the Gbe languages alone provided more than a third of the African lexemes found in these French-based creoles of the Caribbean.

Bakker's rendition, (238): In short, the numbers of slave imports and the numbers of lexical loans point in the same direction: the Bantu speakers were numerically and linguistically dominant in both Haiti and Suriname, but still the resulting creoles do not have noun class prefixes, three levels of time distinctions, agglutinative verbal and nominal morphology, etc., of the kind we find in Bantu languages. In an otherwise carefully argued book, the importance of Bantu is swept under the carpet, or, more concretely, relegated to a footnote (n. 20 on p 39; see also n. 27 on p. 57). The Gbe influence is eloquently argued for in a few areas of the grammar, but the analyticity of the creoles has nothing to do with Gbe influence. It goes back to a stage where Africans created a new language on the basis

of a strategically ideal stripped down version of the lexifiers, and added with amazing creativity the complexities that we can observe in creoles, and which Aboh argued for.

My comment: As I have shown throughout this note, Bakker's insistence on Bantu being ignored stems from his failure or unwillingness to understand the argument of the book. *The Emergence of Hybrid Grammars* is not concerned with which language influences the creole most, but rather which linguistic features were recombined in the creole and where these features presumably originated. As my book shows, language acquisition implies linguistic hybridization, which presupposes multiple linguistic sources.

Bakker's rendition (239): Aboh's central claim that the creoles are hybrids cannot be maintained if there were many Bantu-speaking slaves – and there were many of them. The creoles may have close to zero Bantu traits, and certainly several Gbe-like traits. If Gbe is indeed important and Bantu negligible, we should expect close to zero Bantu lexicon and substantive Gbe lexicon.

My comment: Given my explanations above, based on quotes from my book, it should be clear that this statement is not a reasonable critique of my views. First, the notion of “hybridity” implies that Gbe as well as Bantu, and other relevant source languages contributed to the creoles. This is the central theme of my book, which Bakker seems to have missed. Second, I never claimed in the book that “Gbe was important and Bantu negligible”.

Bakker's rendition (239): Aboh also discusses the complexity issue. Some creolists have claimed that creoles as a set are less complex than non-creoles as a set [...] Aboh is not convinced by their arguments, and claims the opposite: being more hybrid than others, creoles are more complex than non-creoles.

My comment: Wrong! The discussion on complexity is short and is to be found in Sections 8.2 and 8.3. First, I never argued that creoles are more hybrid than other languages: this would be irrational. We have no measure of hybridity. Second, the discussion on complexity indicates that one cannot evaluate overall linguistic complexity in any meaningful way. Instead, I propose to adopt the notion of “local complexity” (see also Aboh & DeGraff, 2014, 2017). Accordingly, it would be illogical to claim that creoles are *overall* more complex than non-creoles: the book makes no claim about creole being overall more complex than non-creoles.

Bakker's rendition (239): How come the creators of the creoles acquired wh-fronting rules even though they failed to acquire subject-auxiliary inversion?” The absence of inversion and the presence of sentence-initial question words in creoles are seen as “failure” in the first case, and success in the latter (308), even though Aboh does not subscribe to the “failure” model (61).

My comment: This statement is misguided. The actual text repeated below argues against an analysis in terms of SLA failure.

Hybrid Grammars (308): Even if we were to grant the view that the syntactic operation underlying constituent or head movement is generally costlier than base-generation of a lexical item, the fact that Saramaccan does display wh-movement in content questions would lead us to a paradox that cannot be handled by theories of creole simplicity grounded in imperfect second language acquisition. Indeed, this language would appear to have developed a complex option for part of the syntax of forming content questions, while the other part would involve a simpler syntax. How come the creators of the creoles acquired wh-fronting rules even though they failed to acquire subject–auxiliary inversion? For the picture to be complete, one would also need to take into account the fact that English subject–auxiliary inversion derives from the presence of auxiliaries in this language as opposed to Saramaccan, where verbal conjugations and their related auxiliary paradigm were replaced by an intricate system of TMA markers.

Bakker's rendition (240): But are Gbe languages really that similar? No, they are not. That is clear from Capo's and Kluge's insightful work on Gbe languages and dialects (Capo 1991; Kluge 2000) [...] Some Gbe languages have plural markers, others have not [...] Creoles and Gbe languages, however, show roughly the same amount of variation in ordering elements in the NP. A simple phrase like "the man's eyes" is to be glossed as "man POSS eye PL" in Agu (Western Gbe), as "man eye(s)" in Ajra (Eastern Gbe), as "man eye(s) POSS PL" in Se (Eastern Gbe) and as "eyes man DET? POSS" in Gbesi (Eastern Gbe) (Kluge 2000: 22). Gungbe (Eastern Gbe) seems different again (man DET POSS eye PL?). Sranan (p. 214) has the orders "DET doctor house" or "DET house POSS DET doctor" and Haitian "eye man". Thus, neither of the three creole constructions are the same as any the five Gbe constructions. With so many Gbe constructions to choose from, why did none of the creoles investigated pick any of them?

My comment: Bakker's insinuation here is that I did not mention obvious variation across Gbe, but the argument falls short and illustrates Bakker's unfamiliarity with Gbe data and morphosyntax.

First, I do not know of any Gbe language which does not have a number marker, but I stand for correction and hope Bakker will reveal which Gbe languages he has in mind. Second, Kluge's (2000, 2005) work is indeed insightful, in showing that while there are notable structural differences across Gbe, these languages share 64–73% lexical similarities. Third, Capo's (1991) work on comparative phonology in Gbe is central to Norval Smith's work on Gbe influence on Suriname Creoles (cf. Muysken & Smith 2015 and references therein). Fourth, VO vs. OV patterns in the context of aspect are discussed in all Gbe studies on aspect that I am aware of including my own (cf. Aboh 2004, 2009, 2015 Chapter 4). Bakker therefore brings nothing new here since relevant variations across Gbe are discussed in Chapters 2, 3, 4, and 7 of my book.

This discussion in my book, and references therein, seems to have escaped Bakker just as he missed the discussion on possessive constructions across Gbe in Chapter 2. Bakker refers to a piece of sentence on page 47 but the conclusion there follows a presentation of several aspects of Gbe grammar including possessive construction. The relevant text to be found on pages 43–44 reads as follows:

Hybrid Grammars (43–44): Possessive constructions generally come in two types: Possessor > Possessed sequences as in (4a) versus Possessed > Possessor sequences as in (4b).

- (4) a. Àyò sín kòkló
 Ayo POSS chicken
 ‘Ayo’s chicken.’
- b. Kòkló Àyò tòn
 chicken Ayo POSS
 ‘Ayo’s chicken.’

While these two options are found across Gbe, the languages differ regarding whether the construction is sensitive to features such as [human], [animate], [alienable], or whether the possessive markers are overtly realized or not.

My comment continued: Not only does the text mention the variation across Gbe, but the generalization presented in my book with regard to the two patterns found in Gbe subsumes what Bakker presents in his review as wide variation across Gbe. Let us consider again the patterns he found:

- man POSS eye PL	Agu
- man eye (PL)	Ajra
- man eye POSS (PL)	Se
- eye man POSS	Gbesi
- man POSS eye	Gungbe

We can focus on the possessor and possessed phrases only because, like many creoles, Gbe languages allow bare nouns and the specificity determiner and number marker are not required in (in)definite or generic noun phrases (cf. Aboh 2004). Ignoring these articles therefore produces the following grammatical patterns for the languages listed in Bakker’s review.

- man POSS eye	Agu
- man POSS eye	Gungbe
- man eye	Ajra
- man eye POSS	Se
- eye man POSS	Gbesi

Presented with this description, any amateur linguist would realize that we have two basic patterns here: Possessor–Possessed vs. Possessed–Possessor; the languages differ as to how the possessive marker is realized. As indicated in the book, and repeated above, overt marking of possession is sensitive to features such as [alienable], a point that is also noticeable in Bakker’s examples involving somebody’s eye. Finally, note that the Possessor–Possessed pattern is what we find in the Suriname creoles (Sranan, Saramaccan), while Haitian displays the pattern Possessed–Possessor. These languages also vary as to how the possessive marker is expressed. Clearly, therefore, the patterns found in these languages are NOT alien to those found in their relevant source languages including Gbe.

In conclusion, what these last facts show clearly is that Bakker’s apparent misunderstanding of the empirical facts alongside analyses based on impressionistic generalizations lead him to fundamentally wrong conclusions. Given his unfamiliarity with generative theoretical principles, Bakker failed to understand the theoretical foundations of my book and its contributions to the debate of language acquisition and change. The African languages and creole languages I discussed in the book are

underdocumented and require us to be extremely careful in our descriptions and rigorous in our analyses. Unfortunately, Bakker's review does not live up to this rigor. He seems to have reviewed a book that argues for Gbe substrate influence as the main factor in the formation of Haitian and Suriname creoles, but that book is different from my book *The emergence of hybrid grammars: Language contact and change*.

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Notes on contributor

Enoch Oladé Aboh is a professor of linguistics at the University of Amsterdam, where he investigates the learnability of human language with a special focus on comparative syntax; language creation, and language change. His main publications include *The Emergence of Hybrid Grammars*, CUP 2015 and *The Morphosyntax of Head-Complement Sequences*, OUP 2004, several edited volumes, as well as dozens of articles in scholarly journals. In 2008, he was a visiting professor at MIT where he taught two courses. He has also taught several summer schools, including, LOT 2005 Groningen, LOT 2010 Nijmegen, LOT 2014 Vrije Universiteit, LSA 2005 MIT/Harvard, and LSA 2015 University of Chicago. As a founding member and co-organizer of the African Linguistics School, he is strongly engaged in working toward a better transfer of knowledge from North to South.

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