Magnus Huber & Viveka Villupillai (eds.) *Synchronic and Diachronic Perspectives on Contact Languages*, 2007, xii + 370 pages. Amsterdam - Philadelphia: John Benjamins.

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As stated in the Preface (IX-XII), this volume is a selection of fifteen papers presented at three meetings (2004, 2005, and 2006) held by the Society for Pidgin and Creole Linguistics and which have been regrouped by the editors into three parts, devoted to phonology, synchronically oriented analyses, and diachronic studies, respectively.

Generally speaking, this volume is well edited and formatted. One exception, however, is the treatment of abbreviations. Most of the articles lack a list of abbreviations (either initially or finally, a much appreciated exception being David D. Robertson's study, which offers such a list in fn. 5 on p. 131). Since the authors, as is natural, have different theoretical backgrounds and discuss diverse languages, it is not easy to get acquainted with the various abbreviations they each use. Typically, an abbreviation is introduced, following the *in extenso* expression, somewhere in the article but if you then happen to forget its meaning, you have to visually scan several pages in order to find the first occurrence, which is a bit annoying. Some other abbreviations, coming from such or such linguistic general theory or background, are also deemed by their authors to be immediately understandable to any reader, which does not prove to be the case, at least so far as I am concerned. For example, I did not know what BrACE (British Afro Caribbean English) meant when I began to read Michelle C. Braña-Straw's article and found the first occurrence of this abbreviation on p. 4 (not explained), its *in extenso* meaning appearing only on p. 6. While abbreviations may seem a matter of rather minor interest, the absence of a such a list in almost all contributions to this book inevitably detracts from the reader's appreciation of its contents.

Let me now turn to real scientific matters, beginning with Part I, concerned mainly with phonology. In *Maintenance or assimilation*, Michelle Braña-Straw (3-22) provides us with a fine-grained sociolinguistic study of the realization of /t/ in the English spoken in the town of Ipswich (United Kingdom) by people of Barbadian descent as contrasted with the English spoken in the same place by people of British descent (the so-called "*Anglos*", as defined on p. 3), focussing on the case of the speech of Edward, a Barbadian speaker who arrived in Ipswich at age 9 and who, having married an Anglo woman, has active social interactions with both Anglo and Barbadian communities.

As a descriptivist trained and accustomed to give accounts of linguistic uses at a macro-societal level, I am not very familiar with the type of perspective developed in this article. In spite of that, I must acknowledge immediately that I found her insightful analyses totally convincing. The structure of this article is well-balanced and the argument is clearly explained. The first three sections (*Introduction*, *Developmental factors – Critical period for acquisition*, *Sociolinguistic setting*) allow the reader to understand in detail both the theoretical implications of the research carried out by the author (who seems to have a solid knowledge of previous work on this topic) and the socio-linguistic situation which prevails among the Afro-Caribbean community of Ipswich. Section 4 (*Methodology*) and 5 (*Results*) are equally well presented. From a cognitive point of view, I would just like to point out that in the Figures giving the percentages of diverse realisations of /t/ in Section 5, Barbadian speakers appear before Anglos in Figures 1 to 3, and after Anglos in Figures 4 and 5. This does not appear to be a good choice, even if (possibly) dictated by the value of the results. The fact of belonging to a determined speech-community is a key-parameter in this study and changing the order of appearance of Barbadians and Anglos in the middle of the presentation is rather confusing for the reader.

Section 6 and 7 happily resume the author's findings and, through the study of Edward's case, stress, in a typically Labovian vein, the importance of "*individual access to different models of language behaviour* (p. 18)". The fact that "*Edward exhibits elements of a mixed system, employing the processes of simplification and overgeneralization to accentuate Barbadian patterns in some environments [...] and accentuating Anglo patterns in other environments* (p. 20)" is significant and impressive, all the more so when one considers this speaker may not be fully aware of the overgeneralisations revealed by the analysis of his speech, since people probably cannot really control the average percentage of (non)-glottal realisations of /t/ in their own idiolect. Another positive aspect of this article is that it contains several samples of spontaneous speech (10-11, 18-20), which give an exact idea of the nature of the material on which the author has based her analyses.

In *Universal and substrate influence on the phonotactics and syllable structure of Krio* (23-42), Malcolm Awadjin Finney, a native Krio speaker, ponders the relative roles of substrate (i.e. "*West African languages*" (26, 30)) and language universals (the unmarked features being preferably retained by emerging Creoles, see for example discussion p. 27) in the shaping of certain phonetic and syllabic peculiarities which distinguish Krio from English, "*its superstrate language* (p. 27)" and main lexifier.

Most of this article is devoted to the discussion of Krio examples (Section 4 and 5, 27-39), on which I shall therefore focus my comments. From a technical point of view, the data are nicely presented, with careful phonologic transcriptions which provide the reader with an exact idea of the actual pronunciation of the Krio words—such transcriptions are not particularly frequent in contributions on Creole languages, even in those dealing with Phonology—although there is no mention of stress (which cannot be disregarded, since it would be surprising if stressed and unstressed English vowels received the same treatment in Krio). Furthermore, there appears at least twice in the article some kind of confusion between Krio and English, when the author says that:

- in Krio "*beat/bit [...] are homophones* (p. 33)". Such a formulation is inadequate because *beat* and *bit* are English (not Krio) words. In order to be rigorous, the quoted sentence should read something like: *"*English 'beat' and 'bit' have become homophonous in Krio as [bit]*";
- "*the vowels of some lexical items originating in English have undergone minor pronunciation changes [...]. They include words like: shrimp, shrink, square [...], school, etc.* (p. 37)". If we are dealing with Krio (not English), even if the Krio word is realised the same way as in English, it should be mentioned in the same (phonological) transcription used for the remaining Krio words of this article.

As regards the evolutive trends presented by the author and always from a strictly technical point of view, several inconsistencies can be found in the data:

- p. 30, in order to illustrate the fact that "*Affricates in lexical items of both English and non-English origins for the most part remain unchanged*", the author adduces (among other words) the Krio form /bintʃ/ '*beans*', in which the affricate /tʃ/ actually differs from the final sequence -/nz/ of English '*beans*';

- p. 32, the items /krɔkrɔ/ '*skin disease*' and /sansan/ '*sand*' are given as examples attesting the presence of 'vowel harmony' in Krio. Leaving aside the quite idiosyncratic use which the author seems to make of the very notion of 'vowel harmony' (all his examples consist of items displaying one and the same vowel in all of their syllables, which is altogether different from the +/- ATR (Advanced Tongue Root) vowel harmony found in many African languages: see Creissels 1994:89-103), I cannot help but think that /krɔkrɔ/ and /sansan/ are merely typical cases of reduplication. If you reduplicate a monosyllabic word, it is predictable that both syllables will have the same vowel: therefore, even in Finney's acception of the term 'vowel harmony', /krɔkrɔ/ and /sansan/ are not good examples of what he appears to want to show;

- p. 39, several items aimed at illustrating “a simple CV syllable structure”, seem to begin with a V syllable : /arara/ ‘nothing at all’, /emina/ ‘plant with edible yam-like tuber’, /okuru/ ‘rabies’. Unless the author considers that every initial vowel is reinforced by a glottal attack—and this is nowhere indicated in the article—the examples adduced here again contradict the author’s demonstration.

Coherence and accuracy of data are essential to good linguistic demonstration and, from this point of view, the article does have some serious flaws. However, the above criticism does not mean that this article is unworthy of attention, nor that the data presented therein are unreliable. It is readily apparent that the author does have a deep knowledge of Krio and he displays a genuine linguistic sensitivity by taking care to signal that some items are characteristic of the basilect (e.g. /ton/ ‘stone’ p. 37).

Turning from form to content, this article addresses the general question of the respective influences of both African substrate and linguistic universals in a systematical and objective way for each phonetic or syllabic feature discussed. The author’s conclusion that “in Krio [...] there is no conclusive evidence to support a stronger influence of one over the other” seems to be justified, at least for the linguistic features he considers.

Tone on quantifiers in Saramaccan as a transferred feature from Kikongo (Martin Kramer, 43-66) is a very stimulating study, aiming at comparing some tonal spread rules of certain quantifiers of Saramaccan (spoken in Suriname) with the ones that can be observed in Kikongo, one of its main African substrates. The end of the article, especially Subsections 6.2. and 6.3. and Section 7 (conclusion), is very interesting in that it hypothesizes a three-stage development of the diverse tonal spread rules in Saramaccan. First, a leftward H spread rule applying in particular for a subset of Serial Verbal Constructions; second a more general rightward H spread rule, linked to a Fongbe substrate and, third, a specific spread rule for quantifiers, due to Kikongo influence. This temporal succession is confirmed by and correlated with historical sources which inform us of the ethno-linguistic origins of African slaves arriving at Suriname during the 17th and 18th centuries. This is the first time that I personally have read an analysis proposing a diachronic approach of the different layers of a tonal system. Although I am not fully convinced by the comparisons made with Fongbe (p. 55) and Kikongo (48-49, 59), for they seem to me too limited in scope, this article is undoubtedly an impressive attempt to give new and original explanations of the tone system of an Afro-European language.

From the point of view of the form, I find the account of tone spread rules quite difficult to follow, perhaps because of the inner complexity of tonal phenomena but also because of a relatively hermetic style. If I am not mistaken, there are also several mistakes in the notation of tone which further increase the difficulty for a non-specialist of Saramaccan to fully grasp the significance of the demonstration:

- p. 52, ex. (16d): à fèni fòló búta should probably read *à fèni fòló búta.
- p. 55, end of ex. (23): é nà sà tê should probably read *é nà sà tê.
- p. 57, end of ex. (28): búta à fèsi should probably read *búta à fèsi.

As regards the author’s argument, which tends to attribute the loss of /r/ in Saramaccan, attested in an earlier stage of this Creole language, to a Kikongo influence (p. 63), I would also like to point out that much caution should be taken when dealing with such cases. Language internal evolutions can also account for the loss of /r/ (or other phonemes), at least in certain positions: cf. Souletin Basque /eoi~eoj/ ‘to fall’ vs. Standard Basque *erori* /erori/, where no external influence seems to have motivated the disappearance of intervocalic -r/- in the Souletin variety.

I shall deal with the two following articles, namely *Morphophonological properties of pitch accents in Jamaican Creole reduplication* (Shelome Gooden, 67-90) and *Effort reduction and the*

grammar (Eric Russell Webb, 91-114), together, as both follow a parallel plan.

In their first parts, they present linguistic phenomena:

- adjectival reduplication in Jamaican Creole (Gooden);
- a comparison of the treatment of the reflexes of French liquids /l, r/ in Haitian and St. Lucian (Russell Webb).

In their second parts, they propose an analysis of these phenomena within the framework of the Optimality Theory (OT):

- Section 5 (*Constraint based analysis*), pp. 79-87 for Jamaican.
- Section 3 (*Analytical framework*, where the criteria used for the OT analysis are introduced) and 4 (*Analysis*), pp. 96-111 for Haitian and St. Lucian.

In both articles, the first part contains data which are discussed in an interesting manner:

- for adjectival reduplication, Gooden clearly shows that the analysis of the pitch accent allows one to distinguish on acoustic grounds distributive reduplicated forms (“*with a single stressed syllable [in the last reduplicant] onto which the pitch falls*” (p. 79)) from intensive reduplicated forms (where each reduplicant has a pitch accent).
- for the liquids in Haitian and St. Lucian, the allophony of /r/ and /l/ is presented in an easy-to-understand way and nicely summarized in a Table (p. 95).

Then, to my mind, the real question, in both cases, is the true relevance of the second part (i.e. the introduction of the OT framework) in both articles. I fully appreciate that certain linguistic phenomena can be better accounted for as the result of a succession of hierarchically ordered constraints. However, I am not convinced from what I have read that OT does provide a deeper understanding of adjectival reduplication in Jamaican or the treatment of liquids in Haitian and St. Lucian. More crucially, some essential issues regarding these phenomena are still left unexplained:

- concerning adjectival reduplication in Jamaican, very brief mention is made of “*an epenthetic vowel, [ɪ], which has been shown to be the default epenthetic vowel in J[amaican] C[reole]* (Gooden 2003a), *inserted to fulfill a bisyllabic foot requirement*” (p. 73), but, although the examples with and without [ɪ] alternate throughout the article (compare *dedded*, ‘truly dead’ (p. 80) with *grinigrinɪ*, ‘green all over’ (p. 82)), we are never told why the [ɪ] appears in certain reduplicated forms and not in the others, an uncertainty which in the end seems very unsatisfactory.
- “*the reasons underlying the adoption of different strategies by related grammars*” (fn.7, p. 110) are considered by Russell Webb to be a “*question which extends beyond the scope of the present work*” whereas the reader would have expected precisely this (which is clearly comparative) to give us at least some possible clues about such “*reasons*”.

Lastly, I would like to emphasize the special interest of Russell Webb’s Section 3 (*Analytical framework*) which, in dealing in particular with the notion of effort, a “*categorical and gradient*” parameter (p. 105), introduces us to an original perspective for analysing articulatory changes.

The second part of the book begins with a rather short article, dealing mainly with *Reflexivity in Capeverdean [Predicate properties and sentence structure]* (Maria Alexandra Fiéis & Fernanda Pratas, 117-128), within a generativist framework and with a comparative perspective, most of the (Santiaguense (see fn. 1 p. 117)) Capeverdean data being contrasted with European Portuguese, the main lexifier of Capeverdean.

The presentation of the data and the subsequent proposal (insofar as I can evaluate it, not being myself a generativist), is coherent. In particular, I think the authors are right in insisting on the fact that, with transitive verbal items, the absence of any overt object (see ex. (1) to (5)) favours a reflexive interpretation in Santiaguense Capeverdean and they are probably the first researchers to have shown that so clearly.

I have however two general criticisms of this article. First, I am rather surprised that no account is taken of the majority of previous work on reflexivity in Capeverdean. Baptista (1997, 2002) is the only specialist of Capeverdean explicitly mentioned (p. 118), but what this researcher— notwithstanding the fact that she also works within the same generativist framework—has said on reflexivity (see for example Baptista 2002:55-57) is neither discussed nor analysed. Other works on the same topic are just simply absent (e.g. Lang 2002:XXXIII; Veiga 2000:174-176, 1995:361-362 etc.).

Secondly, the data are not always accurate:

- p. 119, ex. (8.a): the sentence “*Personne ici ne serait capable de me tomber*” is not understandable to a French native-speaker (such as myself, and I also have tested the sentence with several other colleagues, all native-speakers of French), unless perhaps in certain very specific contexts (wrestling ?), not specified in the article;

- p. 117, the only open marking of reflexivity mentioned throughout this article is the construction [DP V POSS *kabesa*] (e.g. p. 117, see also p. 127, *Pedru mata si kabesa*, ‘Peter killed himself’, word by word ‘Peter killed his head’), which, if I am not mistaken, seems to imply that the use of the possessive (POSS) is systematic or compulsory in this construction. However, the possessive is considered as facultative in reflexive constructions by several other available sources on Capeverdean (hence the interest of consulting them): see for example Veiga (2000:175) (*el mata si kabésa* (+POSS) ~ *el mata kabésa* (-POSS), ‘il s’est donné la mort’ = ‘he killed himself’ (translation mine)) or Baptista (2002:55-56) (*no(s) ta trata no(s) kabesa* (+POSS) ~ *no(s) ta trata kabesa* (-POSS), ‘we take care of ourselves’, word by word ‘we take.care.of (±our) head’). As Santiaguense Capeverdean is in functional terms my second language after French, I can hereby confirm that, in my many interactions with (Santiaguense) Capeverdean native-speakers, I have personally regularly observed the same alternation (±POSS) reported by Baptista and Veiga, which leads me to think that discussion of the facultative status of the possessive should have been included in an article whose main concern is precisely *Reflexivity in Capeverdean*.

An additional pronoun and hierarchies in Lower Columbia Chinúk Wawa (David D. Robertson, 129-158), is a well constructed study of the expression of pronominal third-person in Lower Columbia Chinúk Wawa (henceforward lcCW), a now extinct vehicular language once spoken in the Columbia Valley. This article is really fascinating and I strongly recommend its reading to anybody interested in this language or, more generally, in the interpretation of linguistic data.

From a methodological point of view, Robertson’s paper is original—at least in the field of Creole studies—in that it deals only with corpuses collected by previous researchers, “*as virtually no speakers who acquired lcCW as a childhood home language are left*” (p. 133). This somewhat disparate material (coming from various authors, diverse speakers and different historical periods (from the first half of the 19th century onwards, see p. 133)) is set out by Robertson in a near-perfect presentation using four levels, as illustrated by the following example (p. 131):

(1)	<a’lda	ya[-]	i’sgam	ya’xga...>	(original orthography of the source)
	álta	ya	ískam	yáxka	(standardized orthography used by Robertson)
	now	3sg	take	3sg	(word by word morphological gloss)
	‘Then	he	seized	her...’	(English translation)

In addition to these four levels, Robertson gives, wherever possible, the name of the speaker who produced the utterance and the source where it can be found (respectively, Victoria Howard and Jacobs (1936:2) in the above example).

A careful reading of the article reveals that the author has indeed used most (all?) of the available material to construct his analysis. The examples he adduces (Section 3, 134-138) clearly show that the use of the third person pronoun in lcCW is linked to the animate / inanimate status of the referent. These empirical findings are then nicely related to theoretical, more general models taken from the linguistic literature (Section 4, 138-143), after which (Section 5, 143-148) the third-person pronouns of lcCW are analysed through a three parameter (plurality, animacy and definiteness) grid (see Table 1 p. 147), which Robertson uses to refine his hypothesis of the existence of a lcCW null ($\emptyset$) third-person pronoun, coding for inanimate referents and independent of number (see (28) p. 148). In Section 6 (148-152), he shows that “*the lcCW null pronoun is restricted to object position*” and is “*excluded [from] the other syntactically-specified argument positions – subject/agent and possessor.*” Finally, the conclusion (Section 7, 152-156) of this article is a very valid one, in that it proposes interesting suggestions for further research, mostly comparisons with other Native American languages which may have contributed to the null marking of the third-person pronoun in lcCW.

In sum, this article plainly achieves its aim of getting “*closer to descriptive adequacy*” (see p. 156 and previously p. 129). In search of this “*descriptive adequacy*”, the author also demonstrates his honesty, as he does not hesitate to provide us with data which might be considered as a “*contradiction of what [he has] said so far*” (p. 143), thus proving how faithful he tries to remain to the linguistic reality of lcCW.

I would nevertheless also like to comment on a few shortcomings which have come to my attention while reading this excellent study. First, one of the few things which a non-specialist of Chinúk Wawa really misses in this paper is a brief presentation of the language and its environment. In a publication such as the present volume, dealing with very diverse languages, it is not enough to say that “*among the best-documented regional varieties of Chinúk Wawa is that of the lower Columbia River*” (p. 130), for I am sure that many readers of this book have no definite idea of the linguistic nature of the Chinúk Wawa language itself. Of course, it is now possible to find this kind of information on Internet, but a few extra introductory lines would have been most welcome at the beginning of this article. Second, I do not agree with the author when, basing himself on the prediction of Differential Object Marking as established by Aissen (2003), he says that “*it can be hypothesized from (6 a,b) [i.e. one of Aissen’s rules] that any inanimate PNs [proper names] will take null [pronominal reference] as well*”. As good as a theoretical model may be, in linguistics as in other empirical sciences, such a model must always be validated by existing data: descriptive linguistics is based and depends on the real use of the language, not on unattested forms. Therefore, in my view, it would probably have been better not to hypothesize anything for the case of third-person pronominal reference to inanimate proper names and to have merely mentioned that this was not attested in the available corpus. Third, the three elicitation prompts given by Robertson (145-146) in order to “*learn more about how inanimate count-plural objects are pronominally tracked*” strike me as being pragmatically highly improbable. Although I am not a native speaker of English, I have the impression that e.g. the first of these prompts (ex (26a) p. 145), which reads “*I have three cars; do you like them ?*” is not easy to insert in casual, spontaneous speech: few people actually are in possession of three cars at the same time and it is not that easy to imagine a plausible context in which one such person would happen to ask somebody else whether s/he likes those cars or not.

At any rate, the above remarks are clearly secondary and, as a conclusion, I just want to underline the impressively high quality of this study of a particular characteristic of lower Columbia Chinúk Wawa.

In the following article, David B. Frank (159-173) examines *Three irregular verbs in Gullah*, more precisely in the variety of this language spoken on St. Helena Island (fn. 1 p. 159). The structure of this paper is easy to grasp: after a concise but very informative *Introduction* (159-161) to Gullah culture and language, Section 2 (161-164) provides us with *the basic pattern for Gullah*

verbs; then in Section 3 to 5 (164-172) the peculiarities of the three irregular verbs (*the copula, to go and to do*) are introduced, each verb being dealt with in a specific section.

The conclusion (172-173) emphasizes in particular the fact that the author has exclusively focused on the local reality he was able to observe: “*no claim is made here that this snapshot captures the essence of Gullah for all places and all time.*” Notwithstanding this self-restriction, Frank reveals in this study his great sensitivity and ability in language description:

- the distinction between stative and non-stative verbs, so frequent among Afro-European Creoles, is explicitly taken into account (162-163) in the discussion of the basic pattern for Gullah verbs, which allows for comparisons with other languages;
- in the discussion of the *da* form of the copula, I also particularly appreciated the passage in which the author says that “*it is tempting for someone going back and forth between Gullah and English to equate [Gullah] ‘da’ with [English] ‘is’, but the patterns for Gullah need to be appreciated independently of the patterns of English.*” In my opinion, this sentence captures one of the most important challenges of descriptive linguistics which is to give as exact a picture as possible of the linguistic reality (here the Gullah language) despite the influence which other better-known and/or socially more powerful linguistic systems (here English) exert on the mind of the descriptivist and/or possibly, the speaker.

I nevertheless consider that this article has two main drawbacks. First, the Gullah words and sentences are only given in their orthographic form, which, as Frank puts it himself, “*has only a loose relationship with the phonology [of the Gullah language]* (fn. 5 p. 161).” To be fair, I have to admit that it is perfectly possible to follow the whole of his study without any phonologic transcription. However, such transcriptions (which could have stood alongside the orthographic ones) would have been particularly welcome in order to allow people with little or no prior familiarity with Gullah or South-eastern U.S. regional English (whose pronunciation of certain lexical items happens to be “*more or less the same*” as Gullah, see continuation of fn. 5 on p. 162) to have a more precise idea of the acoustic reality hidden behind these spellings. Second, the *References* section is rather reduced, when compared with the average number of publications mentioned by most authors of the present volume. Yet the author, as he says in his conclusion, does not claim to deal with all issues related with Gullah, but only to present the “*three irregular verbs*” he has found in the dialect he is presently investigating: the objective is clear and the result in accordance with that. Indeed Frank’s paper illustrates three fundamental qualities which should be found in any descriptive work: accuracy, clarity and modesty.

John Lipski’s article (175-198) is an attractive and particularly well-balanced presentation of *Afro-Bolivian Spanish [The survival of a true creole prototype]*, “*a unique Afro-Hispanic dialect, spoken in remote areas of Bolivia by descendents of Africans who arrived in the 16th century*” (p. 176). Section 1 (*Introduction*, 175-176) summarizes the question of the scarcity of reliable historical sources bearing witness of early Afro-Hispanic varieties (the so-called *bozal* Spanish), which were probably spoken once in many places of Spanish America. Section 2 (176-179) introduces us to the main characteristics of the Afro-Bolivian community of the Yungas, whose dialect may well be a “[*continuation*] of early colonial ‘bozal’ Spanish”. In Section 3 (179-190), the author provides us with an *Overview of traditional Afro-Yungueño speech*, insisting mainly on its morphological characteristics, both at the Determiner Phrase (DP) level (“*lack of gender agreement*” (180-181) and “*stripped and invariant plurals*” (181-184)), and at the Verb Phrase (VP) level (use of “*invariant 3SG verb forms*” (184-186) and “*possible continuations of ta + infinitive*” (186-190)). The author compares his own findings and observations with diverse data taken from other mixed Iberian varieties, therefore revealing his vast and consistent knowledge of the issue. Section 4 (*Decreolization and implicational relationships in Afro-Yungueño Spanish*, 190-193) takes into account the fact that today’s Afro Bolivian is undergoing a gradual loss of its most salient

features, due to increasing “*exposure to standard Spanish*” (p. 190) and proposes an implicational scale to classify such “*creole-like features*” according to their relative probability of co-occurrence in hispanicized Afro-Bolivian. This classification suggests in particular that in the decreolization process, “*number features are acquired before gender features*” (p. 192), as one can observe Afro-Bolivian instances of Noun Phrases inflected for number and not for gender but not the reverse (inflected for gender and not for number), an inference which might be applied more generally (at least in Romance), as the cross-linguistic references adduced by the author seem to show. The conclusion (193-194) lays the stress on the importance of the study of Afro-Iberian varieties such as Afro-Bolivian or *Helvécia* Portuguese, “*before they succumb to the inevitable threat of mass-produced language globalization*” (p. 194), as these varieties could be the last remnants of “*fully-fledged creole[s]*” (p. 193) once spoken in Latin America and as they represent, at any rate, powerful models for refining the notion of Ibero-Romance Creole prototype.

As regards the contents of this presentation, I have only a few minor observations to make. First, the assertion that “[*Ibero-romance based*] creole languages have eliminated Spanish and Portuguese morphological agreement in favour of invariant forms” (p. 175) does not fully apply to Capeverdean (which is explicitly included by Lipski in his list of Creole languages lacking such morphological agreement, alongside Palenquero, Papiamentu and São Tomense), where some adjectives do in fact exhibit gender agreement (at least with animate referents), even in the most basilectal varieties, such as rural Santiaguense, e.g. *rapás bunitu* ‘nice boy’ vs. *minina bunita* ‘nice girl’ (with *bunitu* = ‘nice’ (masculine) and *bunita* = ‘nice’ (feminine)). Second, I think that the choice of the adjective “*correct*” to qualify those occurrences of “*noun-adjective gender agreement*” in *Helvécia* Portuguese which fit the standard is not very happy, as it implicitly gives the idea that the standard is in some way better (i.e. more *correct*) than the variety under consideration, a judgement value which is clearly inopportune from a descriptive perspective. Third and perhaps more important, I am not fully convinced by the arguments the author uses to support a gerund origin of the invariant verb following the copula *ta*: for instance, when he contrasts (p. 189, ex. (11)) *ta viniendo* ‘he is coming’ and *ta vení* ‘idem’, I do not see how a form such as *vení* could be derived from *viniendo* ‘coming’ through a process of “*phonetic erosion in rapid speech*” (p. 189): first, the fact of dropping the entire sequence *-(e)ndo*, even in rapid speech is not that casual and does not seem to be a general trend in Afro-Bolivian, at least so far as one can judge from the samples given by the author himself; second, the first (pretonic) vowel of *vení* is /e/ (like Spanish infinitive *venir*, ‘to come’) and not /i/ (like Spanish gerund *viniendo* ‘coming’), which clearly favours an infinitive origin of this Afro-Bolivian form.

As regards the form, I have nothing to say but that it is excellent. The examples are clearly presented and John Lipski writes in a beautiful, vivid style, which makes the reading of his English prose a pleasant experience, while at the same time being an instructive one, due to its utmost scientific interest.

Copula patterns in Hawa’i Creole [Creole origin and decreolization] (Aya Inoue, 199-212) is a sociolinguistics-oriented, corpus-based study of the absence of copula in Hawaiian Creole (henceforward HC). After having introduced briefly the question of copula absence in Hawa’i Creole and in English-based Creoles as a whole, the author describes the methodology she has used to collect and classify her data (Section 2, 201-203), which are then presented in Section 3 (203-206), and compared with the results obtained by previous researchers. This part of the article is perfectly clear and very interesting.

I find the remaining two sections (4 and 5) are, however, somewhat fuzzier, in particular the notion of *creole copula*, the exact meaning of which is not easy to grasp through the discussion. Inoue rightly tries to resituate the phenomenon she has studied for HC, namely copula absence, among the wider group of English-based Creoles. However, what obviously poses her a problem is the existence of the relatively rare (see 203, 209) HC copula *stay* (convincingly traced to Portuguese

estar, ‘to be (in a certain place or state)’, see p. 209), which does not follow the general pattern of copula-deletion (introduced p. 200) observed in many English-based Creoles. If I understand her correctly, the actual source of confusion is that she considers *stay* to be *THE* creole copula in HC, whereas the most common copula in that language (as her data tend to show) is the (optionally deleted) reflex of English ‘to be’, which does indeed match the pattern of the creole copula absence. But if the *be*-derived copula were to be considered the (main) creole copula in HC, there would no longer be a mismatch with the general tendencies of copula-deletion found in English-based Creoles. My personal interpretation of the evidence provided by the author is that *stay* should instead be considered as an HC idiosyncrasy whose probable Portuguese origin (i.e. not African nor Afro-European) explains its particular range of use.

Claire Lefebvre and Isabelle Therrien open the third part of this volume with a dense article *On the properties of Papiamentu ‘pa’ [Synchronic and diachronic perspectives]* (215-255). Section 1 (215-217) introduces us to the general characteristics of Papiamentu (an Ibero-romance based Creole language spoken on the islands of Aruba, Bonaire and Curaçao, in the Netherland Antilles), and to the structure of this paper, mainly concerned with the behaviour and origins of the element *pa* in Papiamentu. Section 2 (217-229) presents in detail *the various functions of ‘pa’* in today’s Papiamentu. In Section 3, the authors endeavour to trace back *the source of the properties of [Papiamentu] ‘pa’*, by successively comparing the syntactic properties of this element to the ones of *para* in Portuguese (229-235), one of the main lexifiers of Papiamentu, and then of *nú* and *ní* in Fongbe (236-242), which is considered in this article to be a plausible African substrate for Papiamentu. The results of this comparison are summarized on pp. 242-243, and the conclusion is that “if the label of Pp [Papiamentu] ‘pa’ most probably comes from Portuguese ‘para’ through syllable truncation, its other properties generally come from those of the closest Fongbe lexical items, ‘nú/ní’.” After that, Section 4 offers *a relexification account of the properties of Pp [Papiamentu] ‘pa’* (243-245), while Section 5 (246-250) deals with the *mismatches between the substratum [i.e. Fongbe] and the creole lexical entries*. Such mismatches also make up the main topic of the *Conclusion* (250-251).

After having read this article very carefully, I have to say that I do not agree with the authors and am not at all convinced that the functioning of Papiamentu *pa* can effectively be traced to Fongbe *nú/ní*. Since this review is certainly not the most suitable place for a complete discussion of the arguments I have against their claims, I will limit myself to drawing attention to some of the methodological issues that this article raises:

1. Corpus. It is a striking fact that a great deal of the Papiamentu, Portuguese, and Fongbe data the authors use are “*drawn from written sources*” (p. 217). Although Claire Lefebvre has herself worked on Fongbe and Isabelle Therrien seems to have some practical experience of Papiamentu, the fact remains that they work mostly on second-hand sources, an option which is clearly more economical in terms of fieldwork and data-treatment, but which necessarily leads to a fuzzier perception of the mechanisms at work in the three languages. This relative distance between the authors and the data may well be (one of) the reason(s) for which one can find so many typos in the examples such as:

- ex. (26) p. 224: Papiamentu *ela primintí*, ‘he promised’ instead of *el a primintí*,
- ex. (29) p. 225: Papiamentu *bias*, ‘to say’, instead of *bisa*,
- ex. (39) p. 231: Portuguese *mismo*, ‘self’ instead of *mesmo*,
- ex. (45) p. 232: Portuguese *pagei*, ‘I paid’ instead of *paguei*,
- etc.

The typos, despite their rather high number, may perhaps be considered as an anecdotic type of

mistake. (I nevertheless wonder if a paper containing so many typos in English or French examples—instead of in Portuguese and Papiamentu—would have been considered suitable for publication...). However, the article contains more serious errors in the data. For instance, on p. 232, when the example (43) **Escrevi para pais de Maria*, ‘I wrote for Maria’s parents’ is judged ungrammatical by Portuguese speakers, it might be because of the use of ‘para’, as the authors suggest, but equally because of the fact that the definite article *os*, ‘the’ is missing in front of the substantive *pais*, ‘parents’. The prepositional phrase *para pais de Maria* is ungrammatical independently of whether the preposition *para* “introduce[s] a goal” (p. 232) or not, because in such a context, the only possible form of the noun phrase introduced by *para* is *os pais de Maria* (word-by-word ‘the parents of Mary’) and not **pais de Maria*, as the authors put it. Such an instance is not the only one of this kind in the article. Besides, *escrevi para os pais de Maria* is judged perfectly acceptable by the speakers of Brazilian Portuguese whom I have personally consulted: thus, contrary to what the authors say, *para* can introduce a goal in Portuguese, at least in the Brazilian variety (spoken not so very far from Curaçao...).

Still regarding the data, some of the interpretations proposed by the authors also seem rather problematic. For example, the two examples ((32) and (33) on p. 227) used to illustrate the fact of “‘pa’ occurring as a case marker” seem to have been misunderstood:

- in (32) [...] *padrino di e yu pa mi* is glossed word-by-word as ‘godfather + of + DEF + child + PA + ISG’ and is translated by ‘(I would like to ask you if you would be) [the] godfather of my child’, probably following Maurer’s (1988:365) French translation (“[le] parrain de mon enfant”) of the Papiamentu original. This free translation leads the authors to conclude that “in (32), the complement of ‘pa’ is a possessor”, which is certainly erroneous. A more literal translation of the above mentioned sequence would be ‘(I would like to ask you if you would be) [the] godfather of the child for me (= for my sake)’, i.e. if we look at the Papiamentu elements *per se* (and not at the French or English translation), Papiamentu *pa* here has a benefactive value (already mentioned by the authors in ex. (1) p. 218) and is not a case marker but a preposition (and *mi* is an oblique pronoun—1st person singular—and not a possessive).

- in (33), the fact that *pa* introduces an agent (*studiá pa un religioso*, ‘studied by a man of religion’), together with the (obviously wrong) interpretation of (32), leads the authors (p. 228) to assert that “in the above examples [...], ‘pa’ appears to be deprived of meaning since it is relational” (!?). Well, in (33), Papiamentu *pa* just corresponds more or less to the English preposition *by*, i.e. Papiamentu, encodes most of the semantic content of the English prepositions *for* (cf. p. 228) and *by* (or Portuguese and Spanish *para* and *por*, see the observation to this respect of one of the reviewers of the article mentioned by the authors (p. 245)) with one preposition only (namely *pa*): this happens in other languages of the world, for example in the Occitan dialect spoken in my own family (a Romance variety with no known links with Fongbe), in which the range of meanings of the preposition *per* [per] encompasses most of the meanings of the English prepositions *by*, *for*—and even *through*. To my understanding, such a semantic configuration, be it found in Occitan or in Papiamentu, does not justify qualifying *pa* (or *per*) as a case marker instead of a preposition.

2. In their search for possible sources of the actual use and semantic value of Papiamentu *pa*, Lefebvre & Therrien make some simplifications which seem highly questionable (to say the very least), from a historical and comparative point of view, as they have limited their “comparison to one language in each source” (p. 229) [‘each source’ must be understood here as the African substratum and the European lexifier of Papiamentu respectively]:

- Fongbe is the authors’ choice for the African component of Papiamentu (229-230), although, as they say explicitly, “in no way does this methodological choice implies [sic] that Fongbe was

the only substrate language of PP [Papiamentu]". Fongbe (a Kwa language) certainly has played a role in the formation of Papiamentu (see some examples in Maurer 1988:374, Quint 2000b:181-182), but also Bantu (Quint 2000b:181-182) and Mande (Quint 2000b:159-160) languages. If we take into account Bantu and Mande, I am not so sure that Fongbe "*is the best documented among the PP potential substrate languages*", as the authors assert. Rather, Fongbe is the African language which Lefebvre knows best, as she studied it and published a grammar of it (Lefebvre & Brousseau 2002). However, in the case of Papiamentu (of which I have some academic and practical knowledge), the choice of Fongbe as *THE* African substratum to be checked appears to me as unsatisfactory.

First, from a linguistic point of view, the presence of Bantu and Mande elements suggests that other families of African languages also played an important role as African substrata of Papiamentu. In practical terms, Mande, Bantu and Kwa (the family of Fongbe)—which all belong to the Niger-Congo phylum—are (at least) as different from each other as are Germanic, Slavic and Indo-Iranian within Indo-European. As Parkvall rightly puts it, no linguist would dare "*using Bulgarian or Persian as approximations of the lexifier languages of Jamaican [English] and Haitian [French]*", even if Bulgarian, Persian, English and French "*are, after all, Indo-European*" (Parkvall 2000: 4). So, on typological grounds, it is difficult to believe that Fongbe (a particular language of a particular Niger-Congo family) can account for the diverse linguistic varieties which comprise the African substratum of Papiamentu. Furthermore, even if we assume that the Kwa languages (i.e. only one family) were predominant during the period of formation of Papiamentu in Curaçao, Fongbe is but one of these languages, and what can be observed regarding the Fongbe prepositions corresponding to Papiamentu *pa* does not necessarily match what could be found in other Kwa languages. We have just seen above that the Occitan preposition *per* encodes for both English *for* and *by*, while the—closely related to Occitan—French language uses two prepositions in the same context *pour*, '*for*', and *par*, '*by*'. Therefore, if we had to make a comparison between Papiamentu *pa* and its equivalents in Romance languages, what would apply for French would not necessarily apply for Occitan (and conversely).

Second, from a historical point of view, recent works (see Jacobs 2009:322, 351-370 and the references therein) have shown that, at least before 1677, there was intensive Dutch slave-trade from Upper Guinea (i.e. a zone where Atlantic and Mande languages are spoken) to Curaçao: these historical data, of which the authors might have been unaware while writing their article, underline the importance of the non-Kwa element in the first period of settlement of Curaçao.

- as regards the European lexifier, Lefebvre & Therrien have based their comparison "*on Portuguese, rather than Spanish* (p. 229)". I am personally convinced (and have endeavoured to demonstrate) that Papiamentu is fundamentally an Afro-Portuguese Creole (Quint 2000b:196), so I suppose that I should agree with the choice of the authors. However, here again, to my mind, the fact of selecting one specific language—Portuguese—rather than another one—Spanish—is mistaken: all the researchers (see for example Munteanu 1996: 414-418; Quint 2000b:119-124, 183-196; Zamora Vicente 1989:446) who have studied the Ibero-Romance component of Papiamentu agree in that, in the modern language (the one this analysis deals with), the Spanish element is more important than the Portuguese one, at least in statistical terms. Therefore, it seems unwise to systematically discard any Spanish influence in the present syntactic behaviour of Papiamentu *pa*, not to mention the possible role of Dutch (and possibly English): such influence may as well explain at least some of the discrepancies between Papiamentu *pa* and Portuguese *para*.

3. With regard to the search for the origins of *pa*, another methodological flaw strikes me: the

authors have looked at one possible African substrate language (Fongbe) and at one of the European superstrates (Portuguese), but they have completely disregarded those Creole languages which are historically and typologically related to Papiamentu, such as Afro-Portuguese Creoles and in particular Upper Guinea Creoles. In several of these languages (e.g. Capeverdean (Quint 2000a:204) and Guinea Bissau Creole (Scantamburlo 1999:182; Biasutti 1987:172)), there is also a preposition *pa* encoding for Portuguese *para* and *por* (or English *for* and *by*). If we take into account the fact that some of these languages (in particular Capeverdean: see Jacobs, *forthcoming*; Martinus 1996; Quint 2000b:119-196) are probably genetically related to Papiamentu, a comparison of the behaviour of *pa* in Papiamentu and in these languages could have been fruitful within the scope of this article, all the more so if we consider that, although *pa* behaves in a very similar way in both Capeverdean and Papiamentu, there does not seem to be any Kwa substrate in Capeverdean (Quint 2008, 2006, 2000b:5-66).

4. My attention has also been drawn to the lack of systematicity of some analyses. For instance, the authors explain the passage of Portuguese *para* > Papiamentu *pa*, by the fact that “*syllable truncation is widespread in PP [Papiamentu]* (230-231)”, after which they give other examples of truncation such as Portuguese *cansado* > P *kansa* ‘tired’, etc. First, all the other examples of truncation they give are of the same type, in that they only consist of the loss of final Portuguese *-do* ‘-[du]: such examples are not sufficient to reveal a widespread trend to syllable truncation in the Papiamentu treatment of Portuguese items, rather they merely illustrate the fact that final Portuguese *-do* is regularly elided in Papiamentu. Second, one cannot compare (without due precautions) the syllable truncation observed in an unstressed morpheme such as the preposition *para* with what can be observed in stressed content words such as Portuguese past participle *cansado*.

In the same vein, the fact that the complement of the verb meaning ‘to fear’ is introduced by *nú* (the substrate correspondent of Papiamentu *pa*, according to the authors) in Fongbe (ex (83) on p. 240) and by *di*, ‘of’ (i.e. not by *pa*) in Papiamentu (ex (84) on p. 241) is duly signalled as a difference (see Table 3, 241-242) between Fongbe and Papiamentu as regards the respective properties of *nú* and *pa*. I think that such a lexical idiosyncrasy cannot be treated at the same level as the more general categories (such as ‘for’ *benefactive* or ‘for’ + *Goal place*) which are considered elsewhere by Lefebvre & Therrien.

5. Lastly, the article is also not exempt from a certain dose of ethnocentrism: for example when the authors say that they “*are using subjunctive and indicative following the terminology proposed by Winford (1993: 290) for Caribbean English based creoles*”, this choice may be accurate, but it should be explicitly justified (and not made automatically), given the fact that Papiamentu is an Ibero-Romance-based Creole and that subjunctive is expressed in very different ways in Ibero-Romance and English, respectively. Similarly, I cannot but feel ill at ease when the Portuguese personal infinitive is qualified by Lefebvre & Therrien as an “*unusual infinitive*” (p. 234). I suppose that the fact that what the Portuguese grammars traditionally term ‘*infinitivo*’ (“*infinitive*”) may be inflected for person in some contexts is a bit surprising for speakers of another Romance language (such as French in the case of Lefebvre & Therrien). But it is not because a given linguistic feature in a given language differs from what you say in your own mother tongue that allows you to write in a scientific article that such a feature is ‘unusual’. It could be that, from a typological point of view, the behaviour of Portuguese personal infinitive is cross-linguistically relatively ‘unusual’, but such an assertion should be based on solid references on the topic, which is not the case in this article.

To summarize what has been said above, I think that such issues as the lack of accuracy of the

data and the diverse biases in the analysis significantly weaken the value of this demonstration *on the properties of Papiamentu 'pa'*. It may well be that Fongbe accounts for a lot of syntactic properties of Haitian (a language of which I am not a specialist), as C. Lefebvre says in other publications (e.g. Lefebvre 2004). As regards the possibility, contemplated in this article, of Papiamentu *pa* being a reflex of Fongbe *nú/ní*, I really do not think that the evidence adduced proves anything other than the fact that, in order to be reliable, linguistic comparisons must be systematic and rigorous (as far as the limits of our human minds allow it).

No exception to the rule [the tense-modality-aspect system of Papiamentu reconsidered] (Nicholas Faraclas, Yolanda Rivera-Castillo and Don E. Walicek, 257-278), is another article concerned with the origins of Papiamentu, namely of its TMA (Tense-Modality-Aspect) system and the relationship this may have with other Creole and African languages. The introduction (Section 1, 257-262) presents us with five features of the Papiamentu TMA system which are judged as “*highly idiosyncratic*” (p. 258) in Papiamentu and proposes and justifies a comparison of such features with what can be observed both in a sample of languages of Southern Nigeria and in Nigerian Pidgin. Section 2 (263-266) analyses the results of this “*comparison between the PA [Papiamentu] TMA system and the TMA of NP [Nigerian Pidgin] and West African substrate languages*.” Section 3 (266-274) hypothesises several evolutive paths which could explain the present configuration of the Papiamentu TMA system, assuming the fact that it could be traced back to West African substrate languages. Particularly noteworthy is the importance given by the authors to diachronic data, and the care with which they have examined old fragments (dating back to the 18th century) of Papiamentu texts in order to find intermediate stages between African languages and modern Papiamentu. The conclusion (274-276) stresses the fact that, according to Faraclas, Rivera-Castillo & Walicek, the Papiamentu TMA system is far less deviant from “*both the substratal and ‘universal / prototypical’ creole TMA systems*” than it might appear.

From a methodological point of view, I particularly appreciate the fact that, in this article, the African substrate languages are conceived of as being a plural entity, as appears clearly in the following statement made by the authors: “*we contend that claims which purport to link the Afro Caribbean populations of given islands to a single area of origin along the west coast of Africa at any given time in the history of the slave trade are at best extremely tenuous* (p. 259).” As a matter of fact, it is probably quite reasonable to think that, in many cases (and Papiamentu seems to be one of them, see Quint 2000b:159-160, 181-183), several languages belonging to several groups have participated to the genesis of each of the Atlantic Afro-European Creoles.

I do, however, have some reservations as to the accuracy of the choice made by the authors of an exclusively Southern Nigerian African ‘substrate sample’, which they justify (258-261) as being linguistically representative of the languages spoken by the African slaves who contributed to the formation of Creole languages such as Papiamentu. First, in the same vein as what I said above in discussing Lefebvre & Therrien’s article, the fact that “‘*Akan’, Twi, Gbe/Fongbe, Yoruba, Edo, Igbo, Kikongo, and Mbundu*’ [...] “*not only belong to the same language family (i.e. Niger-Congo), but also to the same ‘macro-branch’ of that family (i.e. Benue-Kwa)*” (p. 260)” is not enough to say that these languages are closely related and that some can be taken as representative for all the macro-branch. In fact, the linguistic diversity within Niger-Congo is probably superior to the diversity within Indo-European and, even if we stay within the ‘macro-branch’ of Benue-Kwa, an isolating language such as Fongbe displays striking typological differences with a noun-class Bantu language such as Mbundu. Second, in the case of Papiamentu, given that this language displays striking similarities with Upper-Guinea Creoles (Jacobs *forthcoming*, 2009; Quint 2000b:119-208), which are much influenced by Atlantic and Mande languages (of which some items are found in Papiamentu, see Quint 2000b:159-160), the sole consideration of linguistic characteristics of the Benue-Kwa grouping is not sufficient for an understanding of the role of the

African substrate in the actual shaping of the Papiamentu TMA system.

That said, this article is a stimulating contribution to the understanding of the affinities which certainly exist between this Papiamentu TMA system and those of several African and Creole languages.

In *A look at 'so' in Mauritian Creole [From possessive pronoun to emphatic determiner]* (279-296), Diana Guillemin provides us with a fine-grained study of the use of *so* (< French *son*, 'his/her/its') in Mauritian from the first attested stages of this language until the present time. In Section 1 (280-82), she first introduces us to the determiner system of today's Mauritian. Then she illustrates the uses of 'so' in early Mauritian Creole (Section 2, 282-284) and shows how this original possessive came to acquire a determinative value (namely [+ definite] singular) at that stage of the language. In Section 3 (284-285), Guillemin conveniently broadens the scope of her analysis by showing that "*the use of the possessive pronoun as a determiner*" [...] "*has been documented in other languages of the world*" (p. 284)." In Section 4 ("*So' versus the null determiner in MC [Mauritian Creole]*", 284-287), the author goes further in her analysis of the use of *so* in early MC: she takes into account pragmatic considerations (contrastive and emphatic functions of *so*) and underlines the fact that, from a syntactic point of view, "*the determinative use of 'so' in the early creole [...] always occurs with genitive constructions*" (p. 287)". Section 5 (287-290) deals with the question of the exact status of *so* in genitive Mauritian constructions of the XIXth century and examines whether, at this stage of the language, *so* can be considered as having really exerted the function of a *definite article*. This is followed by Section 6 (290-293) which presents us with the current use of "*'so' in modern MC [Mauritian Creole]*", where, although no longer used as a determiner in genitive constructions, it has become an "*emphatic determiner*" (p. 293). Finally, in her "*Conclusion*" (293-294), Guillemin provides a summary of the main points of her contribution.

Generally speaking, the argument of this article is easy to follow and understand. The examples which illustrate its diverse developments are carefully chosen and discussed and seem to reveal a deep knowledge of both contemporary Mauritian Creole and the history of the language. Moreover, the typologically relatively rare use of an originally possessive form as an article is a noteworthy feature which certainly deserved to be examined precisely, both from a diachronic and synchronic perspective: this rich and interesting contribution is therefore most welcome, as regards its topic as well as the way it is dealt with.

With "*Chinese Spanish in 19th century Cuba [Documenting sociohistorical context]*" (297-324), Don E. Walicek offers us an article predominantly concerned with history, but whose argument is relevant to the understanding of the formation of several Creole and Pidgin languages. In his introduction (Section 1, 297-298), Walicek reminds the reader of how "*between 1847 and 1874, the international trade of Asian indentured laborers known as the 'Coolie trade' took the first large-scale arrivals of Chinese [i.e. 125,000 people] to colonial Cuba*" and explains that diverse historical records, providing "*a surprisingly detailed micro-level view of the plantation system [where the Chinese had to work]*" are nowadays available. Such documentation is all the more crucial as many linguists studying "*language contact and linguistic creolization in the context of colonialism in the Caribbean*" tend to ignore the work of historians, which leads them to "*purely speculative and largely unscientific*" (Bakker 2002:75) claims about plantation life. Section 2 ("*British Imperialism and the international dimension of the trade*", 298-301) gives a short account of the historical conditions which led to the establishment of the 'coolie trade' and shows how Cuba was the first destination for Chinese indentured laborers in Latin America. In Section 3 (301-303), we discover how "*the forced migration of Asian laborers to Cuba during the period at stake*" (p. 301) was favoured by "*changes in [the island's] social order*" (p. 301), among which the fact that, from 1841 onwards, "*blacks outnumbered whites*" (p. 301) in Cuba and that "*Asians were not considered black*" (p. 303)." Section 4 ("*'Chinese-Cuban Pidgin Spanish' and Afrogenesis*", 303-306) examines Lipski's (2000) discussion of the Cuban *habla del chino* (varieties of Spanish—

attested in written documents—spoken by Chinese people in Cuba), which he suggests may reflect an influence of both Macao Creole Portuguese (supposedly acquired by a considerable number of Chinese indentured workers while “awaiting [in Macao] the departure of ships going to Spanish America (p. 304)”) and Bozal Spanish (the Afro-Hispanic variety spoken by the Cubans of African descent). Walicek finally casts doubts on the testimonial value of the written texts containing samples in *habla del chino* discussed by Lipski: “though they may be characterized by a consistent set of features, it is not clear that these [recurring linguistic] stereotypes are based on reliable accounts of spoken language.”

Section 5 (“the Report”, 306-322) is the main part of this article. It is based on extracts of “*The 1874 Cuba Commission Report*” (p. 306), published by a multilingual international commission which was sent “under orders of the [Chinese] Qing government” in order to “investigate abuses against Chinese by collecting depositions from survivors of the Trade” (p. 306). As regards “the linguistic backgrounds of migrants” (Subsection 5.1., 306-307), it appears from the Report that speakers of Cantonese were probably dominant, but it seems unlikely that a majority of them would have had sufficient access to Macao Creole Portuguese to be able to use this as a lingua franca, because verbal exchanges between the Chinese workers and their (possibly Macao Creole-speaking) guards (in the case of those who embarked from Macao) seem to have been somewhat limited, which runs counter to Lipski’s (2000) views. The two following subsections (5.2. and 5.3.), concerned respectively with “the linguistic difference and the organization of the trade” (308-310) and “the role of interpreters on voyages to the West”, tend to show that many Chinese workers were unable to understand any European language at the time when they were recruited and then shipped to Cuba. As regards their “relations with persons of Spanish decent” (Subsection 5.4., 311-314), we learn from the Report as well as other historical sources that the Chinese indentured workers strove to acquire as quickly as they could a good command of Spanish, the language they were expected to speak with their masters, and that they did not seem to have ever used a Spanish dialect of their own. In Subsection 5.5. (“Relations with persons of African descent”, 314-318), Walicek adduces elements which make it unlikely that the Chinese workers may have chosen Bozal Spanish (spoken by Afro-Cubans) as a target variety, as interactions between the two communities (Chinese and Afro-Cubans) seem to have been rather limited. Subsection 5.6. (318-320) underlines the role of “Chinese-Spanish bilingual workers”, who often acted as intermediaries between the mainstream Spanish-speaking Cuban society and their monolingual, Chinese-speaking fellow countrymen. Subsection 5.7. (“Looking back on the Chinese diaspora”, 320-322) resumes the main arguments developed in the previous subsections and serves as a conclusion for the whole article, laying the stress on the interest of sociolinguistic research and historical documentation, which “can be used to assess the adequacy of theoretical models and ideological assumptions that shape some of the questions linguists ask (p. 322).”

I think that the main interest of this article is the fact that it emphasizes the complementarity of the works produced by historians and linguists, particularly as regards the hypotheses made by the latter about the formation of Creole (or other mixed) languages. This contribution is also important in that it sheds light on a phenomenon (the exploitation of Chinese indentured laborers during the 19th century) which is probably relatively little-known (at least among specialists of Atlantic Creoles) in comparison with the Atlantic African slave-trade.

I personally have a reservation about the passage where the author says the Chinese “indigenous grammatical concepts, ‘shizi’ (“full words”/ words with lexical meaning) and ‘xuzi’ (“empty words” / words with grammatical meanings) can assist linguists in recognizing the significance of these differences” (the ones which exist between the grammar of Chinese languages and a European language such as Spanish). In my opinion, the concepts of ‘shizi’ and ‘xuzi’ have more to do with the Chinese grammatical tradition than with the Chinese grammar itself. It would be perfectly possible to describe Spanish grammar using (and adapting) the concepts of the Chinese grammatical

tradition (for example, a preposition such as *de*, ‘of’ would be an ‘empty word’ and a noun such as *mujer*, ‘woman’, would then be a ‘full word’...) or of another grammatical tradition (e.g. the Arabic one) and conversely I am not so sure that “*the grammar of Chinese languages exposes the inadequacy of traditional Western grammatical concepts*”, although it certainly challenges them.

For the rest, as regards both the form and the contents, this article, through the historical perspectives it provides, is certainly a most opportune contribution to this volume.

“*Comparative perspectives on the origins, development and structure of Amazonian (Karipúna) French Creole*” (325-357), by Jo-Anne S. Ferreira and Mervyne C. Alleyne, is another stimulating article which introduces us to two varieties of Amazonian French Creole (Karipúna French Creole and Galibi-Marwono French Creole), also called *Kheuól* or *Patuá* and spoken natively by Amerindian populations in the Uaçá area of northern Amapá in Brazil. In Section 1 (“*Introduction*”, 325-326), the authors stress the fact that, in Creole linguistics, “*very little attention has been paid to internally motivated changes of the type undergone by all human language* (p. 326)”, and that dialects such as the Amazonian French Creoles “*have preserved older forms which are crucial in the mapping of historical processes and the reconstruction of earlier forms [for French Creole languages]* (p. 327).” Section 2 (327-340) gives details about “*the socio-historical context*” of the Amazonian Creoles, mainly the geographical location of these Creoles, the social history of the Amerindians who came to use French Creoles as their mother tongue and the actual number of speakers of these languages (more than 4.000 people). It also explains the dialectal differences between the Karipúna and Galibi Marwono French Creoles and presents samples of items illustrating the different sources of *Kheuól* lexicon (mostly French, Amerindian languages and Portuguese). Section 3 (340-351) offers a “*comparative perspective*” in which, after a general discussion about the historical processes at work in French Creoles, several grammatical features (morphology of personal pronouns, word order of some determiners and serial verbs) are compared for Amazonian French Creole as well as other French Creoles varieties. The conclusion (351-352) reasserts “*the considerable interest of Amazonian French Creoles for Creole Linguistics in general and for French Creole studies in particular*”, and insists on the contrast between English Creoles, generally characterised by a “*progressive movement of convergence with English*” (p. 352) and French Creoles, which “*have undergone progressive divergence from French*” (p. 352), which leads the authors to question “*the validity of a categorisation of a group of the languages of the world under the rubric ‘creole’.*”

I am very much in favour of the importance given to historical and comparative linguistics throughout this article, and particularly to the mention of the fact that “*as we find in other language ‘families’, creole languages exhibit chronological layers, that is historically based variation which reveals changes that have not (yet) gone to completion leaving older forms side by side with the newer forms* (p. 327).” The taking into account of such chronological layers was the basis of my own diachronical approach of the evolutive trends of Capeverdean Santiaguense Creole (Quint 2000a:53-60), and I therefore subscribe entirely to the authors’ view on this topic.

As regards the details of the article, I have several observations. First, according to the specialists (Taddoni Petter 2009: p.c., 1999, 1998; Vogt & Fry 1996), the *cafundo*, which the authors term “*Portuguese-based creole language*” (p. 328), is a regional form of Brazilian Portuguese with a Bantu lexical element (not a creole). Second, as regards the Amazonian Creole (or *Kheuól*) treatment of Portuguese lexical items, I am somewhat surprised not to find any mention of the possible role of stress in such processes: for example, the fact that Portuguese *depósito* ‘deposit’ has given Amazonian Creole *depós* is described uniquely in terms of syllabicity (“*apocopation of V.CV*”, p. 340), whereas the fact that the last two syllables are unstressed in Portuguese may have considerably favoured this apocopation. Third, I disagree with the authors’ statement that “*in creole languages, although both processes are present, nasalization is the rule and denasalisation the exception* (p. 343)”. This may apply to French Creoles (of which I am not a

specialist) but certainly not to several Portuguese-based Creoles, where denasalization is a recurrent trend: see Portuguese *mão*, *bênção* ‘hand, blessing’ > Santiaguense *mó* [‘mo], *bénsu*, where the central nasalized Portuguese vowel (written {ã}) is consistently denasalized in Santiaguense Capeverdean, see also Portuguese *comprar* [kõ’prar], ‘to buy’ > Principense *kopa* (Maurer 2009: 223), etc. Fourth, there is no comment in the main text on Table 11 (“*Reflexive pronouns*”, p. 348), and the data given therein thus remain unexploited. Fifth, regarding the fact that “[*Amazonian French Creoles*] are a probably unique case of a creole language having been adopted by an indigenous population as a second language and then becoming the native language of that population (p. 351)”, I personally know of at least one comparable case, namely the rural communities of Casamansese Creole speakers, in Southern Senegal, who have retained their ethnic identity (they belong to the Nyun group) but adopted a Portuguese-based Creole as their native language (Biagui *forthcoming*).

In a more general way, I feel that this article, in spite of its many positive qualities, perhaps loses some of its demonstrative force in trying to present at the same time linguistic and socio-historical details about the Amazonian French Creoles together with general considerations about the historical processes at work in French Creoles and the validity of the very notion of ‘Creole’, in such a way that the reader is somewhat torn between this alternation of particular and universal perspectives.

To sum up, this volume is certainly a valuable resource for Creole studies, in that it involves many different Creole languages and also many different approaches: this stimulating, healthy diversity is undoubtedly one of the most positive features of *Synchronic and Diachronic Perspectives on Contact Languages*.

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