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The impoverished République Centrafricaine whose health as a state is attacked from within and without and barely administered with a phantom government is held together by its indigenous language, a lingua franca for most of the population but the first language of a growing number. In this respect it is almost unique on the African continent even when compared with Swahili, Lingala, and Kituba. But like the latter two it owes its existence as a pidginized vehicular language to the spontaneous idioms that arose when Africans from elsewhere arrived with colonization in the nineteenth century and interacted as well as they could linguistically with the people along the banks of the upper Ubangi River. This, at least, is the view that I have expounded during the last two decades, taking care to evaluate other views of the language, all of which are cited below.

Here again I argue my case, because a concerted argument against my view has been made by Charles H. Morrill in Language, culture, and society in the Central African Republic: The emergence and development of Sango. Morrill rejects my view and believes instead that: (a) Sango arose in pre-colonial times when it was already a “vehicular language” (a lingua franca, about which more is said below) and (b) it is unlike a pidgin linguistically. About sixty-one per cent of his work is an argument contra Samarin on these

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1 This thought was expressed in Sango in 1994 when I was energizing the Central African teachers of Peace Corps Volunteers: I said Yanga ti kodoro ake kamba so akanga be ti azo aga ako ‘The language of a community is a cord that binds people’s hearts to become one.’

2 This work, a doctoral dissertation accepted by Indiana University (Bloomington, Indiana) in 1997, is in the public domain, obtainable as an “authorized facsimile.” Ann Arbor, Michigan, UMI Dissertation Services.

3 My view is also dismissed by Thornell, but on different grounds: two decades “seems too short” for a pidgin to develop (1997: 30, without any reference to the literature on the origins of pidgins and creoles). Of my own works she cites only 1967, 1982, and 1989, ignoring all those that might have informed her research and her opinions. For a review of her work see Samarin 1999.
two topics (33). In what follows I respond to this challenge by showing that the alleged linguistic evidence (identified in sub-section headings) is faulty and adding a new perspective and new material. This work should also be considered an expansion of my contention with Marcel Diki-Kidiri, some of whose publications are cited below, and an oblique critique of the views of Jean-Louis Calvet on vehicularization.

MORRILL’S VERSION OF SANGO’S HISTORY

Morrill begins by considering the “theory” of Diki-Kidiri more probable than my own (156, 166). The latter’s views are summarized in the following statements (Diki-Kidiri 1985 : 39, 85-86 ; 1981 : 35, 39 ; 1986 : 87):
- “une forme de sango servait déjà de langue véhiculaire entre les ethnies riveraines depuis le village des Abira jusqu’à Bangui”;
- “la langue véhiculaire dendi a dû servir, le long du cours moyen de l’Oubangui que contrôlaient les Ngbandi, comme langue de marché et d’échange avec les populations banda ... qui bordent l’arrière-pays”;
- “le sango, sous l’appellation ‘dendi’, était déjà une langue véhiculaire avant l’arrivée des Européens dans le pays”.

Morrill, however, ignores Diki-Kidiri’s selection of Dendi, a dialect of Ngbandi with relatively few speakers, as the precursor of Sango (revealed as ill-founded historically in Samarin 1998), but weaves his own historical tapestry with material from both of ours, without giving any reason for preferring his own over Diki-Kidiri’s. In fact, he does not seem clear about the position he adopts. Whereas, on the one hand, he equivocates by saying that “while Sango may not have existed as a distinct variety of Ngbandi in the precolonial period,” it “would have been” used for interethnic communication (89, italics added), so that “there possibly existed [a] well-established, simplified ‘foreigner talk’ variety of Ngbandi before the arrival of the colonial forces” (90, italics added), on the other hand, he is categorical in saying this “foreigner variety of Yakoma

\footnote{All such parenthesized numbers are pages in Morrill’s work. The tables are all mine. My research in Belgium, England, France, the Netherlands, and Portugal on language and colonization in central Africa, with particular reference to the origin and development of Sango, began in 1970 and continued until 1994, approved by the Central African Ministry of Education, and was made possible with grants from the following: the International Studies Program of the University of Toronto, the Humanities and Social Sciences Committee (University of Toronto), the American Philosophical Society, Social Sciences and Humanities Research Council of Canada, the Centre National de Recherche Scientifique (France). Help of different kinds came also from individuals too numerous to cite here, but gratefully acknowledged. Various kinds of help were also provided by Grace Brethren International Missions (Winona Lake, Indiana) and by paid Central African assistants.}

\footnote{Morrill’s historical leaps are illustrated by asserting as a matter of “fact” that Gbanziri was used in the pre-colonial period (158) whereas Marcel Diki-Kidiri had only speculated: “[gbanziri] a dû servir de langue de contact.”}
was in existence before the arrival of European forces” and this was “the ultimate source of Sango” (166). Argumentation in this work for the existence of a Ngbandi lingua franca is fractured, but reasons may be extracted from the text under three topics: trade, slavery (in this section) and language change (in the next).

**Trade.** The first argument is based on the role Ngbandis had in regional trade. (These are the people, known since colonization as Yakomas, Sangos, Dendis, and Kpatiris, who inhabited the banks of the Ubangi River or not far from it and a few other rivers feeding into it, and whose language became what some pidginists and creolists call the base or source of Sango.) Morrill makes sweeping statements, some of which cannot be supported historically: for example, “the Ubangi [river] has been since time immemorial a major artery of migration and trade” (40), where, “before the arrival of Europeans into the region there existed vast kingdoms and complex trading networks” (50), in which the Ngbandi-speaking peoples played “the dominant role” in commerce and trade (38) and “were both economically and geographically at the center of a very extensive trade network” (74), especially because of the production and trade of iron and copper goods.

Some of this perception is not new, because the topic had been lengthily discussed in Samarin 1989, and very little of it is argued by Morrill with any historical rigor. In any case, he assumes, as so many have, that because there is trade, there must be a trade language. See, for example, Harold Fehderau on Kituba (1966).

The second argument is that in pre-colonial Ngbandi society “there was a contingent of foreign-born slaves [who] would have spoken something of their masters’ language,” a variety, it would appear, that was already a simplified “foreigner talk” (90).

**Slavery.** Although slavery was common to the cultures of central Africa, and there are frequent references to slaves in the documents of whites, starting with Alphonse Van Gele, the first one to go up the Ubangi River (adequately discussed in Samarin 1989), not the third one (52) –Morrill makes no attempt to

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6 Morrill errs in equating ‘foreigner talk’ with a ‘foreigner variety.’ The latter is the kind that most people speak (at least for a while) when they learn a foreign language as an adult, but the former is what I consider a hypothetical variety of language created by native speakers to make it more easily understood by foreigners. This alleged idiom (usually associated with the name of our deceased colleague Charles A. Ferguson) has not, I believe, been subjected to the kind of criticism it deserves. Elsewhere Morrill equates a ‘foreigner variety’ with a “vehicular variety of Ngbandi,” such as would have been spoken by these slaves” (165–166). However, according to him Sango “as a distinct variety of Ngbandi” “underwent the process of vehicularization” during the colonial period” (34).

7 I follow the practice of others writing academically in using Ngbandi for both the ethnolinguistic group and its Ubangian language even though the people in the area around Mobaye are referred to as Sangos and those further up-river as Yakomas.
describe who the Ngbandis’ slaves might have been. There is no evidence whatsoever in all that I have read that they were “foreign-born” in the sense of coming from hundreds of kilometres away. Some of them, if not all of them, were most certainly people from the hinterland north of the Ubangi River, probably speaking varieties of Banda, of a different family but in the Ubangian group. It is inconceivable that Ngbandis had Nzakara and Zande slaves in large numbers, if any at all: the latter were from far to the east, and the former – found up the Mbomu River, which flows into the Ubangi – were the trading partners of the Ngbandis.

Based on Morrill’s groundless history, the slaves become critical in the origin and development of Sango, because these slaves are supposed to have spoken “something of their masters’ language” – a simplified “foreigner talk” variety of Ngbandi (90). It is simply assumed that because they were slaves – or because being slaves they spoke so many different languages – they could not learn to speak Ngbandi well at all.

Neither of the above assumptions is justified; it is a fiction that has too often been invoked in alleged histories of language contact and change. The Bandas would not have had to simplify Ngbandi to speak it. Besides, many of them (perhaps most of them) would have been taken captive when they were young, and slaves, as has been well documented by Africanist historians were assimilated into a village community in a way that would have led to their speaking its language very quickly (e.g., Harms 1981). In any case, none of the above allegations is squared with the assumption that “there would have been considerable bilingualism with Ngbandi on the part of neighboring ethnic groups and a tradition of using Ngbandi for interethnic communication” (89). The captives of the Ngbandis would have been, to a degree that Morrill should have considered, speakers of Ngbandi, not a significantly altered form of it.

Some of these slaves, the argument continues (but without any documentation), were purchased by whites from the Ngbandis (Yakomas in particular are cited) for the colonial forces (Belgian apparently) as part of the militia (54). (A whole chapter is devoted to the Belgian militia in Samarin 1989.) Superficial reading and speculation by Morrill also lead to the assertion that in one case when a French force of about 300 troops was on its way to confront the British troops at Fashoda in the Sudan the bulk of them “would most likely have been the ‘liberated’ slaves of a variety of ethnicities” (56) and “would have spoken a reduced variety of Ngbandi,” the lingua franca that was allegedly the antecedent of Sango (57). The step is obviously made that Belgian and French colonial forces simply adopted a simplified vehicular Ngbandi that was spoken by these slaves (90)\(^8\). Despite this characterization of the language Morrill admits that

\(^8\) Morrill implies on this page that I attribute the creation of Sango to the colonial forces. This interpretation distorts my argument.
this vehicular language “may not have existed as a distinct variety of Ngbandi in the precolonial period” (89). (Nothing in the microfilmed Victor Liotard archives I consulted in Paris and in the history of the expedition by Mazières [1982] that deal with this case, but not consulted by Morrill, supports this assumption.)

THE LINGUISTIC ARGUMENT

On this unconvincing history of Sango’s origin, now criticized in print, as noted above, Morrill argues (in a logically circular fashion) a typological classification of Sango among the languages of the world, which makes it a vehicular language, not a pidgin.

Sango is not a pidgin, Morrill declares, because a pidgin is “a totally new linguistic creation that may not be attached to an existing family tree” (35), that is, genetically unrelated to a source language (10, see also 1), and “[t]he sine qua non of what it means for a language to be a pidgin must be genetic discontinuity with any one natural language[,]” Sango “is a direct genetic descendant of Ngbandi” (3). However, Sango is genetically related to Ngbandi, Morrill continues, “in the normal sense of the term” and they “both stem from the same linguistic source” (217); “Sango is the genetic continuation of a single, well-documented natural language” (1). Although Sango, as a vehicular language, is, according to Morrill, restructured and somewhat simplified, it is not a “new language” (35); it is only a “distinct variety” of Ngbandi (34, 350), not “a wholly separate entity from Ngbandi” (92). If, he argues, he can demonstrate that Sango is not a new language, but still “genetically related” to Ngbandi, it cannot be considered a pidgin.9

For the cited criterion Morrill credits Language contact, creolization, and genetic linguistics by S. Thomason & T. Kaufman (1988) as his “general

9 Despite using Salikoko Mufwene’s acronym PC and having heard him give a lecture at Indiana University in 1992, Morrill ignores the fact that Mufwene is vigorously opposed to the view adopted by Morrill. For example: [Pigins and creoles] “do not constitute a formal type of language which may be contrasted with other traditional types ... [They] have so far revealed no property that has not been attested in any non-PC language” (Mufwene 1988: 265).

10 One should compare his views with some of Michel DeGraff’s even though the latter writes about creoles based on European languages: e.g., that people (citing Samarin 1980) have considered them degenerate variants of their European sources, partially, at least, because of having an extremely poor vocabulary and simple morphology (DeGraff 2001b: 497; see also 517; 2003: 394). I am also cited when he rejects the alleged belief that creoles “form a biological/cognitive class of their own,” having certain morphological and lexical characteristics (DeGraff 2001a: 92). However, DeGraff and I have engaged in written discussion about what I take to be a misrepresentation of my views, leading to apologies from him.
theoretical framework of language contact” (4; see also 24). (Elsewhere he credits Calvet 1992 [350] and apparently Salikoko Mufwene [349].) Informed by them, he concludes that in pidginization “a linguistic >compromise< will be reached in which an existing language will be drastically restructured to create a new vehicle of communication” (9, italics added). According to him, the onus is on those who want to classify a language as a pidgin to demonstrate genetic discontinuity (25), also called (but oddly) “non-genetic transmission” (33).

We continue evaluating Morrill’s theses despite the fact that it can be said that it is useless to base one’s argument on a specific definition of a pidgin. Researchers on language contact and its consequences (pidgins and creoles being only some of them) at the time when Morrill was working on this dissertation had yet to agree on a definition of pidgin (and still do not), a fact that is amply documented in interventions and discussions at conferences and in contradictory views that appear in publications.

LINGUISTIC EVIDENCE

Linguistic evidence for Morrill’s claims appear to be four: the vocabulary of Sango is not as mixed as one would expect in a pidgin; one proto-form in Sango reveals that a vehicular language existed a long time ago; the verbal grammars of Ngbandi and Sango are so much alike that they must be considered closely related languages; and Sango and Ngbandi are mutually intelligible.

The nature of Sango’s vocabulary. With respect to lexicon, Morrill argues, what is important is that a pidgin be recognized as having words from different

11 Although Morrill notes that Robert A. Hall, Jr., the pioneer in the linguistic study of Papua New Guinea’s Tok Pisin (Pidgin English) and Haitian Creole, also believed that pidgins “do not represent new genetic creations, but are merely altered forms of existing languages” (19), he does not make anything of this fact. Morrill also ignores the contrary view of a historical linguist who continues to be involved in research on language contact: Pidgins “are wholly new languages, created from materials taken from two or more languages” (Thomason 1982: 480, citing her own work of 1980).

12 Thomason maintains her belief that adults “shift away from their native languages and ‘negotiate’ a new medium of communication by acquiring or maintaining structures that are most readily understood by their interlocutors” in a “genesis process” that is abrupt, citing Thomason & Kaufman 1988 (Thomason 2002: 267). She must be using the verb shift in a particular sense. I had many years ago been led to identify the concept with the spread of a language. In this work she also makes an assertion that Morrill, in retrospect, could have used: “creoles [and presumably pidgins] derive only their lexicon from a single language, while their grammatical structures cannot be traced back directly to any single language” (Thomason 2002: 266). But in an earlier work she makes a distinction between processes of pidginization that do and others that do not lead to pidgins. That Thomason recognizes difficulties is seen also in her comment with respect to Gillian Sankoff’s criteria for identifying a speech form as a fully crystallized pidgin language: “It may be hard in practice to apply these criteria rigorously” (Thomason 1982: 480).
languages. For this criterion he cites the definition of pidgins that was adopted by linguists at a conference in Paris in 1952 on the use of vernacular languages in education (at which I was a guest observer preparing to go to Oubangui-Chari). Although pidgins, other than Tok Pisin, perhaps, were not of special concern to the participants, they adopted as a working definition the following: “[A pidgin is a] language which has arisen as the result of contact between peoples of different language, usually formed from a mixing of the languages” (18, italics added; Morrill elsewhere omits the qualification [21]). In any case, “mixity” (as he calls it) becomes one of the tests to determine what languages are pidgins. He even assumes that it was because of “the mixed nature of its lexicon” that I considered Sango a pidgin in Samarin 1961 (161). This is not true.

The importance of Sango’s lexical similarity to Ngbandi is clearly declared in the following: “It is this overwhelming [elsewhere “striking”] similarity of basic vocabulary that provides the most conclusive linguistic evidence that Sango and Ngbandi both stem from the same linguistic source” (217). Morrill should be quoted fully:

What the early colonialist [sic] and missionaries described as Volapük [an artificial international language] and a mixture of a variety of languages was, in fact, during the period quite pure Ngbandi. If Sango had begun as a pidgin, as Samarin has suggested, one would expect to find from its very inception a heavily mixed vocabulary drawing lexical items from the numerous substrate languages present at the time. The fact that foreign elements came into Sango only gradually over a five decade period [presumably 1900 to 1950] – well after the purported period of pidginization – provides strong evidence that Sango has as its earliest antecedent a vehicularized variety of Ngbandi and not a pidgin (214).

Morrill’s evidence for Sango’s lexical similarity to Ngbandi is based only on his critique of the words I elicited in 1952 from rural speakers of Sango for Morris Swadesh’s 100-word “basic vocabulary” (Samarin 1961). This was the short list; there was also a 200-word list, which he could have used with profit. Its only purpose in those days was to determine degrees of relationship between languages, a procedure that was called glottochronology based on lexicostatistics. (Its nature and use became a topic of animated discussion and serious writing for several years, none of it utilized by Morrill.) As a neophyte linguist (with only a Bachelor of Arts Degree from the University of California at Berkeley when collecting words in 1952 and 1953), my only purpose was to contribute some information about an African lingua franca that had a limited

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13 He might also have used the following: “Pidgins usually involve [among other things] admixture from the native languages of their users” (Rickford 1986: 281, italics added).

14 Basic vocabulary was assumed to consist of those words that were most resistant to replacement in language over centuries and millennia, such as sun, moon, water, fire, man, woman …
vocabulary. I knew nothing about pidgins and creoles except for what I had read in *Language* by Leonard Bloomfield (1923, whose generalization about pidgins I rejected in Samarin 1955); that book and *Language* by Edward Sapir were the only general texts for undergraduate students in linguistics when I was a student in the 1940’s. (Eugene A. Nida and Kenneth L. Pike, however, were writing textbooks for students on morphology and phonemics).

Whereas I found that only 61 of the words I elicited were of Ngbandi origin, Morrill says that 79 were in Sango from the beginning of the colonial period, 18 are derived from Ngbandi morphemes, only one is found in neither language, and only two are borrowed from foreign sources. And whereas I find that ten words were borrowed from other languages, he says that eight of these were adopted during the later colonial period 1911–1950, during which two works by Catholic priests were published (Calloc’h 1911, Tisserant 1950), an era that is important in his chronology but not carefully explained. He is so eager to establish a pure ancestry for Sango that he claims that before the non-Ngbandi words were adopted for *horn* and *tail*, which occur only in Tisserant, Ngbandi equivalents would have existed.

There are three problems with Morrill’s analysis. The first is that his view of pidginization is simplistic. He does not demonstrate in the history of any other pidgin that lexical mixture arises almost immediately. Second, he ignores the fact that some of Sango’s ‘words’ arose in pidginization. It is true, for example, that for ‘who?’ one says in Sango *zo wa* which consists of two Ngbandi words: ‘person where?’. This was, I should think, either extrapolated from an utterance that was not correctly understood by the people who were jargonizing in Ngbandi or is a nice creation based on two words, whose meaning would be transparent for people communicating in a jargon and similar to *la wa* ‘at what time?’ and ‘when?’ in Ngbandi, which means—appropriately for people without clocks—‘where’s the sun?’ (sun [is] where?). The third is that some of his etymologies are erroneous. (a) For example, because Morrill finds the word *mbi* for ashes in Laird 1932, he believes that it must have been “in common usage” until at least then (212); for the same kind of assumption see *horn* and *tail* below. It is more likely that missionaries, needing a word for the translation of the New Testament (see 2 Peter 2:6), borrowed it from Ngbandi, but it was not yet acquired by my sources, who were only partially

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15 The time period is important to him, because he believes, without any cited support in research on language contact, that “borrowing in the initial stages of contact is observed primarily in the area of lexicon” (8).

16 Tone is not marked on Sango words in this article.

17 I would like to use the term *jargonize* (perhaps French *baragouiner*) for attempts on the part of speakers to use whatever linguistic means they have (from as many languages they believe may be appropriate in a speech event) to achieve mutual comprehension – and perhaps more.
literate in Sango. Morrill cannot include the Sango word be as a Ngbandi word for heart, because I was eliciting deliberately and explicitly the name of the organ (as opposed to be ‘liver,’ which is more important in Central African metaphysics). (c) Because my assistants did not know Sango words for claw, cloud, heart, knee, swim, tongue (even though all but claw were in the New Testament) does not say anything about the relationship of Sango to Ngbandi. The word for knee, allegedly likouni (likuni, probably li ‘head’ + ku ‘X’ + determinant), does not occur in Lekens 1958. (d) Morrill’s Ngbandi etymology for ‘root’ as consisting of gu ‘base’ and nda (incorrectly glossed by him as ‘bottom,’ in Lekens derrière ; achterste, uitende), because in Ngbandi gu means ‘souche’ in the sense of ‘famille’ (in Flemish stam, stamvader, familie, oorsprong) not ‘racine’ (Lekens 1958).

A proto-form in the vehicular language. There is, Morrill declares, “solid [linguistic] evidence” in the proto-form ando of Sango (190, 196, 216). According to him this word is used for ‘distant past,’ and the Ngbandi cognate for ‘absolute past.’ What he means is that because ando is obviously a phonologically full form of Ngbandi’s eroded form –´ndo (high tone occurring on whatever vowel precedes it), which must have eroded ever since proto-time, the more complete form ando is older. (On the difference between the free word in Sango and the alleged suffixal form in Ngbandi see below.) One is led to the conclusion that the contemporary form of the Ngbandi language (or just its –´ndo, a “relic of a past stage”) is more recent than the vehicular form of Sango ; “Sango and Ngbandi must have diverged before this grammatical change took place” in Ngbandi (190). This reasoning is not only incredible, it is also unnecessary. If it did not occur in some variety of Ngbandi when Sango was coming into existence, it most certainly could have arisen with the kind of regularization that results when persons learning a language make analyses and interpretations. In other words, ando must have arisen from utterances like the following : lo ga -´ndo (pronounced lo ga ando) > lo ga ando ‘he/she came some time ago.’

18 When American missionaries were establishing themselves at Fort Crampel in the 1920’s, intending to use only Sango in dealing with Africans (except for the few expatriate ones who spoke some English), their leader William Haas went to Ouango (on the lower Mbomu River, for which see Samarin 1989) to fetch a Yakoma to teach Sango to the missionaries, because it was believed that these people were the best speakers of the language.

19 This work of two volumes is absent in what Morrill considers “the full body of published and unpublished materials” on Ngbandi (95) and does not appear to have been used by him. Nor was he aware of Samarin 1953 (available at the University of Toronto Library and at the New York City Public Library), the first work on Sango in which tones for all vowels are marked. Although he cites the French version of my grammar (1970), he is wrong in saying that it “is not merely a translation, but rather an up-dated and revised version of the original” (161, fn 17).
The grammars of Ngbandi and Sango. After providing sketches of the “verbal systems” of Ngbandi and Sango, Morrill concludes that the similarity between the two is “striking” (204; see also 187, 195) and that “all of the verbal categories of Ngbandi are found in Sango” (195); “Sango is the result of processes of grammatical change which pre-existed in the source language and which were intensified as a result of the expansion of the vernacular into a vehicular language” (186). Although Morrill acknowledges “considerable restructuring and simplification” (187, cf. 195), the “processes of grammatical change,” which are not identified, are, he claims, just the kind one finds in ‘vehicularization,’ for which see below.

The following critique and evaluation does not support the conclusion of his dissertation: “I have demonstrated that in all aspects of its [sic] grammar, Sango and Ngbandi exhibit clear genetic relationship [of the kind he is interested in]” (353).

Ngbandi’s verbal system is complex, probably more complex than what is described in a few publications consulted by Morrill. (He says nothing about what research he may have done on the topic.) The only parts of it that concern us, as we try to understand Morrill’s reasons for declaring Sango genetically related to Ngbandi, are those that constitute the TMA (tense-mood-aspect) system of the language. We must note the following: “The free TMA morphemes of Ngbandi [nowhere listed] are found in most instances in Sango with identical form and function” (215, see also 125, 133). He might again have cited Rickford, who noted that among the recognized “pidgin-like features” is the absence of TMA markers (Rickford 1987).

Morrill’s re-analysis of Ngbandi’s verbal system can be summarized by the following categories: 1) Tense: Future (with a distinction between near and
distant future) and Past. 2) Mood: Realis (the default mood), Irrealis (for futures, conditionals, and hypotheticals), and Volative (for desires, orders, and commands). 3) Aspect: The word aspect occurs sporadically only in connection with what are called “progressive aspect” and “imperfective aspect” (126-137 passim). There are no contrastive aspects that would constitute an aspectual system.

There is much that Morrill fails to report about the Ngbandi verbal system (for which see Boyeldieu 1975). In the first place, most of it is expressed (realized) in tone replacements over the significant morphosyntactic unit. In other words, the tones of the pronouns and verbs, for example, are unpredictably variable: thus, the tones (High, Mid, and Low) of the pronoun and verb of ‘I dance’ in the Realis are L M, but H M for Irrealis and Volative; for the first person plural, however, the tones are L H, H M, and H H respectively. To speak Ngbandi correctly—and to make correct sense, of course—one must memorize the appropriate tones. And since there are sequences of syllables differentiated only by tone to make words, learning becomes even more problematic: for example, the same monosyllabic consonant and vowel with low, mid, high, and falling tone are the basic or underlying forms of the verbs ‘shoot,’ ‘dance,’ ‘step,’ and ‘soak (something).’ If one says with the same syllable [d] L L, it means ‘I shot,’ L M, ‘I danced,’ L H, ‘I stepped,’ and L F, ‘I soaked’ (124-125).

Despite the disappearance of tone-based grammar Morrill insists on saying “all of the changes that have taken place in Sango … are not the dramatic nature one would expect from a true case of pidginization” (156).

In the second place, Morrill does not address himself to the question as to whether a single, fully integrated verbal system can in contemporary linguistics consist of different types of semantic units. Without comment, he adds affixes and words to the tonal system as the linguistic means of constructing the system. In doing so, moreover, he makes mistakes, such as the following.

(a) Although the irrealis ‘mode’ that is realized tonally in Ngbandi “conveys the general future tense,” Morrill says that the verbal suffix -ndé “indicates a near future,” and “the free adverbial morpheme” (that is, word) gbanda indicates a distant future (131). The first is not a suffix, because it is an adverb with the variant form ande (Lekens 1958) meaning ‘maintenant, tantôt, tout à l’heure’, and it occurs in other positions in a sentence.

(b) Although past time is also encoded tonally in the realis form of the verb (as in I walked to the market), Morrill says that the verbal suffix -ndo signals absolute past (not relative to the time expressed in another verb in the sentence). According to Lekens, however, this word also occurs in other

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22 Morrill uses the words mood and mode interchangeably, even on the same page (125).
23 "The simple irrealis mode, with no additional aspectual reference, conveys the general future tense in Ngbandi" (131).
positions of a clause, and he writes it as a separate word ’ndo, the same as Sango’s ando (189-190)\textsuperscript{24}.

(c) Although a “progressive aspect” is posited with the use of the alleged verbal prefix ndo-, said to occur in all three moods also expressed tonally (variations of something like ‘be eating’), the analysis is far from complete. (i) The status of this alleged aspect in the grammar of the verb is ambiguous due to the fact that “progressive aspect” is said to be “generally indicated by the bound morpheme” (134, italics added), leaving the possibility that there are other ways to signal this aspect. (ii) Morrill does not demonstrate that this must be analyzed as a prefix, whereas Lekens lists it as a word in his dictionary even though writing it solid with the verb. (iii) The meaning, according to Lekens, is habitual.\textsuperscript{25} (iv) Morrill ignores the fact that the tone of the morpheme and the pronoun subject are variable. (v) Whereas Morrill says that the predicate marker occurs before the ‘prefixed’ verb, Lekens does not illustrate such usage (an example, if it could occur, might be andoli ‘[someone] is doing’\textsuperscript{26}. (vi) Whereas Morrill’s examples have only low and high tones, Lekens’ have mid and high.

Sango’s system, by contrast, is extremely simple. Morrill recognizes that this restructuring and simplification (187,195), but only in that the tone of the verb [for pronouns see below] does not change and there are no bound aspectual morphemes (187). For Morrill this does not mean that Ngbandi’s verbal system had disappeared. Instead, we are informed that (italics being added here):

Sango’s verbal system “has not been reduced (195); “certain aspects of this complex system have been retained in Sango” (195); “all of the temporal and aspectual markers of Ngbandi have been retained in Sango” (195); “all of the verbal categories of Ngbandi” are found in Sango” (195); the verbal system of Sango “shows striking similarity to that of Ngbandi” (187); “The free [tense, mood, and aspect] morphemes of Ngbandi are found in most instances in Sango with identical form and function” (215).

Morrill arrives at this surprising conclusion by adopting (without giving a reason and making some changes) the “best account of the Sango TMA system” namely, that of Diki-Kidiri 1988 and 1995. Whereas the latter creates a grid of three moods (Réel, Virtuel, and Injunctif) intersecting with three aspects (Absolu, Acquis, and Non-acquis), producing nine modal-aspectual categories (significantly different from what exists in Ngbandi), Morrill considers it more

\textsuperscript{24}Lekens provides examples, but does not define the word, saying only “duidt de ver verleden tijd aan,” and refers to his grammar (1958 : 638A).

\textsuperscript{25}“Vóór het werkwoord geplaatst duidt de gewoonte aan of een werk dat bezig is” (Lekens 1958 : 636A). His translations include the word “gewoonlijk.”

\textsuperscript{26}Lekens has an example of ndo in what I call a nominalized verb: ndoengo (ndo + ‘habitual’ + e ‘laugh’ + ngo suffix) ‘laughter’ (Lekens 1958 : 636B).
accurate “to describe distinct verbal constructions which subsume all three parameters” (187).

The confusion here of lexicology and grammar is also illustrated in a paper by Diki-Kidiri on aspect and time, where it is said that *fadë* (adopting his orthography) is one of those temporal adverbs that are semantically and grammatically specialized in a “système de valeurs structurés manifesté par un paradigme singulier de signifiants” for marking time (Diki-Kidiri 1988 : 118 ; a similar view of grammaticalization is adopted in Thornell 1997 ; see also Samarin 1999.) The function of *fadë* and *ânde* stated in that work is to mark “simple” or “ordinary” future. The categories are the following (the itemization being mine): (a) the immediate future, (*yeke* [copula] + verb) with the noun/adverb *fafadesô* ‘right now’, where, it would appear, the copula is optional ; (b) the near future, expressed by *kekereke* ‘tomorrow’ and *mbenî kekereke* ‘day-after-tomorrow’ (literally, ‘another tomorrow’); and (c) the distant future, divided into degrees – *gbândä* “plus tard”, *ânde gbândä* “bien plus tard”, and *gbândagbâ* “tôt ou tard” (Diki-Kidiri 1988 : 118). In other words, the alleged verbal system is really a lexical one based entirely on temporal adverbs. (About variation in their use, of which there is so much, nothing is said.) Elsewhere the explanation of the use of *fadë* is slightly modified by saying that, on the one hand, when it is placed before the subject it is the marker of future tense and, on the other hand, the immediate past when placed after the verb (Diki-Kidiri 1998 : 34). From this same work the definitions and functions of the other words are given as follows : *ânde* ‘later’, said to mark the future, *fadësô* ‘now’ (no grammatical function given ; and *fafadesô* ‘right now’ is not included), *gbândâ* ‘later’ (with no cross-reference to *ânde* ; *gbândagbâ*) (1998 : 10, 34, 40).

Sango’s verbal system, we must note, is clearly based on the way adverbs and adverbial nouns are used in sentences to convey ideas that in Ngbandi are expressed morphosyntactically. An examination of tense illustrates this fact; aspect is no different. See the following table, to which one can add, according to Morrill, recent past with *biri* ‘yesterday’ and near future with *kekereke* ‘tomorrow’ (189).

<table>
<thead>
<tr>
<th>Time</th>
<th>Past tenses</th>
<th>Future tenses</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distant</td>
<td>ando ‘in the past’</td>
<td>gbanda ‘some time’</td>
</tr>
<tr>
<td>Near</td>
<td>giriri ‘long ago’</td>
<td>ande ‘soon’, fade</td>
</tr>
<tr>
<td>Immediate</td>
<td>fade ‘right away’</td>
<td>fafadeso ‘right now’</td>
</tr>
</tbody>
</table>

Table 1. The tenses of Sango according to Morrill

Since the subject of tense marking in Sango has already been published (Samarin 2001), we need make only a few comments to illustrate the way one can argue that the verbal system of Diki-Kidiri and Morrill is not a grammatical
one in the sense this phrase had in the discipline of linguistics when Morrill’s work was being researched and written, adding some remarks about grammaticalization.

All of the words listed above have adverbial meanings, but this is ignored by Diki-Kidiri, who makes *fade* a future marker when placed before the verb and the marker of the immediate past when placed after it. (This explains the two blank spaces in table 2. In Diki-Kidiri 1998 the adverbial meanings are significantly ignored.) The significance given to *fade* in its two positions is not based on an analysis of usage. Calloc’h’s (1911) remarks for the beginning of the twentieth century probably described usage correctly: to express future time one added a time word, including *fade* ‘tout à l’heure, bientôt, vite’.

Some of these adverbs can co-occur, like *ando giriri* (together, as noted by Morrill, or in different places in a sentence) for extreme-distant past (191), which I would translate as ‘a really long time ago.’ Morrill says nothing about the way these words are used in Ngbandi.

Everything said about *fafadeso* is wrong (191). The word is not based on the “post- is based on *fadeso* ‘now’, which can function as a noun or as an adverb. The reduplication of the first syllable is just an instance of a verbal morpheme” (the adverb) *fade* ‘soon’ with the addition of the demonstrative *so*, but process in the language, perhaps on its way to being grammaticalized, that intensifies the meaning of an attributive: also *kekete* ‘very small’ from *kete* ‘small’ and *mimigi* ‘very many’ from *mingi* ‘a lot, many, very’.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><em>lando</em></td>
<td>plus tard, dans l’avenir</td>
<td>plus tard</td>
<td>plus tard</td>
</tr>
<tr>
<td><em>lando</em></td>
<td>récemment, dernièrement, depuis peu</td>
<td>jadis, autre fois</td>
<td>autre fois</td>
</tr>
<tr>
<td>fade</td>
<td>tout de suite, rapidement, immédiatement, sur-le-champ</td>
<td>See the text</td>
<td>tout de suite</td>
</tr>
<tr>
<td><em>fafadeso</em></td>
<td>tout de suite, rapidement, immédiatement, sur-le-champ</td>
<td>See the text</td>
<td>tout de suite</td>
</tr>
<tr>
<td><em>giriri</em></td>
<td>autre fois, jadis, il y a longtemps</td>
<td>autre fois</td>
<td>autre fois</td>
</tr>
<tr>
<td><em>gbanda</em></td>
<td>plus tard, dorénavant, à l’avenir</td>
<td>plus tard</td>
<td>plus tard</td>
</tr>
</tbody>
</table>

Table 2. The meanings of adverbial ‘tense markers’ in three dictionaries.

Grammaticalization (or grammaticization) might be defined as the emergence of grammatical forms with abstract meanings that have arisen out of forms with concrete meanings, a process called desemanticization. In fully accomplished grammaticalization linguistic units undergo loss in semantic complexity, pragmatic significance, syntactic or morphosyntactic freedom (what is also called the autonomy of a word), and phonetic substance in various
degrees, to be sure. Another concomitant of fairly well developed grammaticalization is paradigmatization, where grammaticalized elements are analyzed as belonging to paradigms.

In early grammaticalization, however, the problematic form or construction is not yet obligatory, fixed, and so forth, resulting in considerable variation. Indeed, grammaticalization is always, one has opined, a question of degree; it is not absolute. And “there appear to be no clear ways in which the borders which separate grammatical from lexical and other phenomena can be meaningfully and consistently drawn” (Hopper 1991: 19). Five principles have nonetheless been proposed for selecting “from among the rhetorical resources of texts those recurrent collocations that [are] candidates for being, at least marginally, ‘in’ the grammar of the language” (Hopper 1991: 32), but even they do not identify grammaticalization unambiguously.27

Pidgins and creoles are said to be a gold mine for those who study this phenomenon, and Sango has been used to illustrate it, but the examples are poorly chosen. It is said, for example, that tene a-ga polele ‘The speech became clear’ illustrates the way the verb meaning ‘come’ (ga) became a resultative marker meaning ‘change of state’ (Heine & Kuteva 2002: 75). This cannot be an example of grammaticalization on the basis, first, of the definition given above, because this verb still means ‘come’ also (e.g., mo ga ge [2s come here] ‘come here’), and also that its meaning ‘become’ is inherited from Ngbandi.

Mutual intelligibility with Ngbandi. Morrill starts with the assertion that “it is generally agreed that any definition of a pidgin must include the fact that it is not mutually intelligible with its lexical source” (2, citing Sankoff 1980: 139 [should be 140]).28 ‘Pidginization’ should therefore be limited to cases “where it may be irrefutably established that a break in normal linguistic transmission has taken place” (3); elsewhere “abnormal transmission” (33). This cannot be said of Sango, because, he asserts, “there is partial mutual intelligibility; speakers of Ngbandi who have had no previous exposure to Sango can understand it, while the speakers of Sango can only understand a few simple words and phrases of Ngbandi” (2–3). Since he does not support this claim by any evidence whatsoever, we cannot give it any credence. Because Morrill apparently did not test speakers of Ngbandi who were completely ignorant of Sango, he must have reported what he had been told. I do not believe this assertion. First, since the Ngbandis have been exposed to Sango for over a hundred years in a region where contact with speakers of other languages has

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28 Thomason considers the “lack of mutual intelligibility with any and all of its grammatical and lexical source languages” one of the two criteria for identifying “a fully crystallized pidgin language” as being “eminently sensible” (1982: 479).
taken place, one cannot imagine anyone in the Ngbandi homeland being unfamiliar with Sango. Second, since Morrill carried on his research (and I mine) when Ngbandis were in political control of the nation, they took advantage of any opportunity to identify statehood with their languages (Ngbandi and Sango) with their ethnicity. And since mutual intelligibility is so important in determining what is a pidgin, Morrill should have noted, as did Thomason (1982: 480), that there are “borderline cases.” In other words, intelligibility is not the fine instrument he would make it out to be.

Vehicularization instead of pidginization

Sango is denied an origin in pidginization, as we have just seen, and the changes that make it somewhat different from Ngbandi, already cited, are explained by *vehicularisation*, which means the process whereby a language becomes a *langue véhiculaire* (26), but this I take to be only a synonym for what we call a *lingua franca* (Samarin 1987)\(^{29}\). The adoption of this “category” of language as “distinctive” from other languages, which is promulgated, he notes, in francophone linguistic circles (26, for example, by Calvet 1992 and Manessy 1995), should have been proclaimed a major innovation in the literature on language contact. Of all the types of languages there are, some of them would presumably be the following: Natural / Artificial / Vehicular / Pidgins / Creoles

According to Morrill “Vehicularization may be viewed as the preliminary stage of bilingualism that accompanies most (non-abrupt) (original) language situations” (29-30, parentheses in the original). It “does not represent ‘normal’ transmission” and “is similar to pidginization in that both takes [sic] place in circumstances of intense social upheaval and linguistic contact” (35); a language seems to undergo a process of ‘simplification’” (28), and “difficult (irregular) (original) forms will be eliminated” (30); it is a process that Calvet calls “regularization” (28); the changes are not “dramatic,” as they are in pidginization (156).\(^{30}\) Changes would not be major, presumably because “the

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\(^{29}\) I may err, however, in equating the two, because Morrill says that vehicularization like pidginization “takes place in circumstances of intense social upheaval and linguistic contact” (35). What should be recognized, in any case, is that whereas internationally-known francophone (and bilingual) linguists are familiar with the way the word *pidgin* is used as a technical term, many others (including some lexicographers) use it in other ways: for example, to refer only to varieties of Pidgin English (as if French could never be pidginized) or for hodge-podge, impromptu speech that I refer to as a jargon, in French *sabir* according to some writers. Therefore the translator of Samarín 1984 changed *Pidgin Sango* to *sango sabiresque*.

\(^{30}\) Although Morrill says (without referencing the remark) “Calvet seeks to make explicit the predictable changes in form that result when a language changes function” in vehicularization (28), Calvet actually makes what appears to be a contrary view: with a vehicular language “ce n’est pas la langue qui importe que la fonction véhiculaire” (Calvet...
vehicular variety would remain in contact with the source language which would continue to serve as a target language for the nascent variety” (30–31)\textsuperscript{31}. “It differs from pidginization, however, in the crucial fact that the result is not a new [crystallized] language, but a restructured (and frequently simplified) (original) variety of pre-existing language” (35; also 26). Sango is said to have undergone vehicularization during the colonial period (34), a period, we should note, that coincides with my period for the pidginization of Ngbandi. However, he contradicts himself in a statement that is clear enough: “Sango probably emerged as a distinct variety of Ngbandi before the arrival of the colonial forces” (190); “vehicular Sango was in existence before the arrival of the colonial forces” (196). See also: Awhile Sango may not have existed as distinct variety of Ngbandi in the precolonial period” (89).

One might suggest that what Morrill is trying to describe is the kind of creolization being proposed (again and again) by Salikoko Mufwene (cited above)> mimingi ‘very many.’ Creoles are said to be nothing more than the result of language change in certain kinds of environment, a view taken by Calvet quite some time ago (1981 : 76).

It would appear that Morrill is caught in a quagmire out of which he did not extricate himself.

A SOCIOLINGUISTIC STUDY OF CHANGE

Most of Morrill’s original work is in the part devoted to “Vernacularization and modernization of vehicular Sango” (about 41.0 per cent of the dissertation). Here, with information about linguistic variation and language use, he endeavors to support the position espoused by Jean-Louis Calvet, which was cited above. Morrill was, in effect, undertaking a project in the 1990’s that was similar to my own, launched in 1991 (‘The linguistic consequences of the

\textsuperscript{31} Morrill contradicts himself in saying “Sango was effectively cut off from Ngbandi around the turn of the century” (173). In any case, there is much evidence to support the claim that the Sango of persons of Ngbandi ethnicity is different from that of inlanders in ways that are linguistically significant, if not obvious to Central Africans, from a sociolinguistic point of view. This is one example. Of the sixty-five records of my large corpus of examples of the use of the preposition na of the phrase yeke na (COP PREP) ‘be with, have, possess’ – as in mbi [yeke] na nginza ape (1spn (COP) PREP money NEG) ‘I don’t have any money’ – there are twenty-five in which the copula is missing. Since Ngbandi does not have a copula, one might hypothesize that its speakers would use yeke less than others (Boyeldieu 1998). This is the case with yeke na. Ten of the fifteen speakers who used no copula (for nineteen records or 76.0 per cent) are speakers of Ngbandi and their fellow-riverines the Gbanziris and Ngbakas. The remaining seven records are from four Gbayas, whose speech frequently patterns with the riverines, and one person of mixed ethnicity.

Although we should like to know when exactly Morrill pursued his research in the CAR and how long a period it was, he provides no information. Some time before beginning his doctoral research, it would appear, he had been a Peace Corps Volunteer (dates not given) as a teacher in French in the southwest not far from the town of Mbaiki (257), where I lived with my family in 1953. It must have been at that time that he became a “fluent speaker of Sango.”

The instrument that Morrill used in eliciting information about the use of and attitudes towards varieties of Sango was a questionnaire in Sango that is provided in an appendix. The English equivalents of Sango may not, however, be translations in the strict sense. Some of them are quite different from the Sango they accompany, and the Sango differs considerably from the vernacular that I recorded in the 1990’s. Morrill says nothing about the help he may have had in preparing the questionnaire. It would appear that in every instance he was the interviewer.

The linguistic analyses are based on tape recordings of these interviews, which lasted from ten to thirty minutes. From the participants’ responses to the questions he selected a passage of 350 words for transcription (270). He considers these samples (probably from different portions of the whole interview) to be “natural speech.” Since he claims to have interviewed 118 persons, he must have acquired a corpus of 41,300 words, but Morrill gives no figure. He is also silent about the way he processed the data.

The participants, Morrill claims, were selected so that “the demographic patterns of the sample reflect that of the population [as revealed in the census of 1988]. The selection of participants, therefore, is of fundamental importance in this study” (221); on another occasion he criticized my research for not being based on a similar sample of the Central African population. However, although he uses age, urbanization, gender, and level of education, he gives census figures only for age and education with incomplete figures for urbanization. (One would have thought that the census provided figures for gender.) Ethnicity is ignored without explanation; I have found it a significant variable in my study of variation in Sango.

Morrill cannot boast about his having a sociologically (or sociolinguistically) representative sample of the population, because his participants constituted a “network” of friends or family of his contacts in Bangui; even outside Bangui the persons had “some connection” with his “original circle of contacts in Bangui” (230, 235, 250), therefore, if they were among those considered “rural,” they were likely not as ‘rural’ as farmers are but were in a better socio-economic class. This “circle” took shape, we learn, in
a neighborhood that “is a mix of Sango, Yakoma, and Gbanziri ethnicities” with “also a large number of Ngbaka” the kind of neighborhood he chose to live in, because he wanted to be among people of Sango and Yakoma ethnicity (234). It is not surprising that these riverine peoples were living in what Morrill describes as “the premier middle class neighborhood” of Bangui: André Kolingba was the nation’s president, and many posts in the civil service, education, and military were held by persons of riverine ethnicities.

Morrill analyses the variants and frequencies of three words to measure the consequences of vernacularization of Sango among his participants. They are the preposition (referred to in French as the associative) ti ‘of,’ the copula yeke, and the negative marker pepe, commonly pronounced ape.

Morrill takes considerable pride in having established correlations by statistical analysis, which I have never undertaken to do. (He even criticizes an early work of William Labov for its lack of “sophisticated statistical analysis of the data” [226].) His findings, however, appear to be meaningless: no picture emerges. Had he provided more information, one might arrive at his own explanation. His mathematical analysis fails for two reasons.

(a) Morrill provides in the appendix only the number of times {ti} is used in possessive and attributive constructions (N = 756 and 446; and 1,096 for “reg”). He provides no table revealing significant facts about the speakers, and nowhere do we learn the number of speakers for each variable. I have found that frequently the percentages I arrive at correlate with the number of records I have: a percentage will be greater or smaller with fewer or more records.

(b) Morrill ignores the phenomenon of intra-subjective variation (illustrated in the tape-recorded text provided in Samarin 1997). This refers to the fact that when a number of records of {ti} appear in a text, even with exactly the same phrase, the variants will be different: e.g., mama á mbi and mama ti mbi, etc. One would hypothesize, of course, that speakers have their favorite variants.

For the sake of brevity I discuss only the variable {ti}. As Morrill observes, it appears to be the most frequently used word in Sango because it bears a heavy functional load in the creation of noun phrases, which Morrill calls “compounds” [271]. (It is also used in verb phrases such as gwe ti vo ‘go to buy,’ not discussed by Morrill. Ngbandi uses another construction.) In my corpus of 1962 there were 3,055 tokens (records) of {ti}: that is, 9.05 per cent of all words (Samarin 1967a). And this does not include the number of instances, significant in sociolinguistic calculations, of places where it could have occurred, because its absence is as significant formally as its phonological presence as in the following attested examples: mama ti mbi or mama m mbi, mama T mbi, mama Ø mbi (where m carries high tone and T is high tone on the preceding tone-bearing unit or the lengthening of either a vowel or consonant)
'my mother.' (And there are several other variants in the vernacular that Morrill does not identify. I discovered the phenomenon while engaged in research in Bangui in 1988 and discussed it at a public meeting for linguists at the University of Bangui at that time.)\textsuperscript{32}

The variant of \{\textit{ti}\} that interests Morrill the most is \textit{chi} (as in English \textit{cheap}). (He makes the mistake of calling it an allomorph of \textit{ti} [271]). He finds that “syntactic functions,” age, and urbanity are correlating variables.

(a) Of the two “syntactic functions” (also called syntactic environments [271]) of the “constructions” (that is, noun phrases) in which the variable \{\textit{ti}\} occurs – possessive and attributive (the latter in \textit{melenge ti wali} [child of female] ‘girl’ – it is used more frequently in possessive than in attributive phrases, although \textit{ti} is used more than \textit{chi} in both constructions (272, Figure 26)\textsuperscript{33}. According to Morrill, “the syntactic environment in which the morpheme falls conditions this alternation” (271), and “there is a significant correlation between the choice of variants of \{\textit{ti}\} and syntactic construction” (272).

(b) This variant is also used more frequently in possessive noun phrases by persons in the age cohort 20–34 and by those who are considered urban; this “is not the result of random variation” (271-272).

No explanation is offered for these correlations. Why do young adults use this variant with possessive constructions more than the cohort 10–19, when it is among young people, as he observes correctly, that language change is first seen. And why should this variant occur more frequently with possessives than with attributives? (But see below.)

As for Ø Morrill considers it the “dropping” of the “morpheme” \textit{ti}, the same as the floating tone /\textit{\textasciitilde{}}/ (275). (For an early discussion of the absence of a phonological variant see Samarin 1967a : 132). (The complete absence of the preposition, he does not observe, is grammatically determined in Ngbandi [Boyeldieu 1987]). Much more could have been said of the tonal variant. Not only does it occur on the preceding lengthened vowel but also consonant, and it also replaces the preceding low or mid tone. (Morrill does not mention the latter.) The phrase \textit{da ti mbi} (L H M) with the tonal variant is heard in recordings as \textit{daa mbi} (LH M) ‘my house’ or \textit{da mbi} [H M].) According to him it occurs more frequently with possessives than attributives and only age is a significant independent variable (276), occurring almost as frequently among the cohort 10-19 as \textit{ti} is among those aged 20–34.

The alleged correlation between the variant and the function of the

\textsuperscript{32} Linguistically trained Central Africans first transcribed the tape recordings, and I checked the occurrences of \{\textit{ti}\} minutely once or several times.

\textsuperscript{33} Morrill does not explain the meaning of attribution and does not provide examples. He could have referred to Samarin 1967, where we find examples like these: \textit{tene ti ngia} (affair of pleasure) ‘good news,’ \textit{yaka ti café} (garden of coffee) ‘coffee plantation,’ \textit{maboko ti wale} (hand of woman) ‘left hand.’
variable is not replicated in my corpus of 1263 noun-noun phrases. (In this corpus there are 182 different nouns beginning with a consonant following the variant \textit{ti} and 180 head nouns. With T there are 44 and 45 respectively). In other words, T does not occur in possessive noun-noun phrases more frequently than in attributive ones. (A detailed exposition is beyond the bounds of this paper.) Moreover, in pronominalized phrases (noun-pronoun), where possession is not ambiguous as in many noun-noun phrases, of which there are 718 records (with 102 different head nouns) the most conservative variant \textit{ti} occurs 517 times (72.0 per cent), whereas T occurs only 201 times (28.0 per cent). There are 125 different speakers for the latter and 63 for the former.

That Sango’s grammar is different from that of Ngbandi with respect to the lexeme \{\textit{ti}\} is seen in the preferred use of the variant \textit{ti}, as found in the full form before consonants, with kinship terms (as in \textit{baba ti mbi} ‘my father’). Table 3 demonstrates variation in the occurrence of three variants. The preference for T with ‘sibling, cousin’ and Ø with ‘mother’ is exceptional, and it illustrates the difficulty one encounters in describing variation as it has in the last forty years in literate societies. However, we do find that girls use the non-conservative variants more frequently than boys: in the case of ‘sibling’ 65.98 vs. 34.15 per cent and for ‘mother’ 81.48 vs. 18.52 per cent. Although other data suggest that persons of Ngbandi ethnicity use T more frequently than others, this is not the case here. What may explain these high percentages may be the number of times certain persons used the same phrase. In the case of ‘sibling’ from two to six times, in the case of ‘mother’ two to ten times.

<table>
<thead>
<tr>
<th>Nouns</th>
<th>Total records</th>
<th>\textit{ti} Rds</th>
<th>\textit{ti} %</th>
<th>Ø Rds</th>
<th>Ø %</th>
<th>T Rds</th>
<th>T %</th>
</tr>
</thead>
<tbody>
<tr>
<td>\textit{baba} ‘father’</td>
<td>71</td>
<td>19</td>
<td>26.8</td>
<td>15</td>
<td>21.1</td>
<td>3</td>
<td>4.2</td>
</tr>
<tr>
<td>\textit{ita} ‘sibling’</td>
<td>55</td>
<td>29</td>
<td>52.7</td>
<td>1</td>
<td>.2</td>
<td>41</td>
<td>74.5</td>
</tr>
<tr>
<td>\textit{koli} ‘husband’</td>
<td>53</td>
<td>16</td>
<td>30.2</td>
<td>11</td>
<td>20.7</td>
<td>7</td>
<td>13.2</td>
</tr>
<tr>
<td>\textit{mama} ‘mother’</td>
<td>142</td>
<td>39</td>
<td>27.5</td>
<td>54</td>
<td>38.0</td>
<td>7</td>
<td>4.9</td>
</tr>
<tr>
<td>\textit{wali} ‘wife’</td>
<td>82</td>
<td>31</td>
<td>37.8</td>
<td>10</td>
<td>12.2</td>
<td>11</td>
<td>13.4</td>
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</tbody>
</table>

Table 3. Variation in the occurrence of three variants of the variable \{\textit{ti}\} ‘of’ in possessive noun-pronoun phrases with kinship terms in extemporaneous speech, exceptional cells highlighted.

Morrill does not attempt to explain the origin of \textit{chi}; suggest three possibilities. It is either based on a riverine variety of Ngbandi where \textit{t} is replaced by an affricate before a front vowel (I heard it in the Ngbandi word for ‘tooth’ in the speech of an elderly woman) or the pronunciation by speakers of Banda ethnicity; many (if not all) their dialects have this affricate as a phonological unit. (The latter is invalidated below.) The third explanation is more appealing to me because of the role of style and fashion among urban speakers. I suggest that French-speaking Central Africans acquired the use of
the affricate in the word tu in French and introduced the variant consonant in Sango words. Evidence for this hypothesis comes first from the fact that some people use this variant of tu, perhaps in only some words, in Paris. I heard it three times in October 2007. One speaker was a young female standing almost elbow-to-elbow in a bus; the other also was a female, slightly older, at a table doing most of the talking in a café. In both cases the pronoun was followed by the second-person singular of a verb: as ‘have’ in one case and vas ‘go’ in the other and the young women appeared to be native speakers of French.

Further evidence that the variant is limited to a small number of speakers is that of the several hundred subjects whose speech I recorded from 1988 to 1994 (a corpus of 3587 records, 2934 [81.80 per cent] of phonological variants and 653 [18.20 per cent] of Ø) just one person provided most of the examples. She was of Gbaya ethnicity, sixteen years of age, with minimal education (“Cours Moyen 1”) and spoke no other language but Sango (speaker S11A03 in my records, a free conversation recorded by me in 1994). Of the forty-four times she used a phonological variant, thirty-three of them (75.0 per cent) had ch, only one of which was chi. The syntactic contexts included both verb and noun phrases, and the word classes of the words following the preposition included verbs, nouns, and pronouns. It would appear that if the young woman was not deliberately putting on an act, and there was no evidence that this was so, she had chosen this variant for herself. Indeed, she is so radical that she does not even use a vowel with the consonant. Here are some examples, where ‘_’ marks liaison:

- salte ch_en’ (filth [French salté] of what?) ‘what kind of filth?’
- yanga ch_i (mouth of 1ppn) ‘our mouth’
- bongo ch mo (cloth of 2spn) ‘your dress’
- komase ch tene (begin [French commencer] of say) ‘begin to say’

The others using this variant in my corpus, contrary to what Morrill found, were of Banda ethnicity. There were no Yakomas and Gbanziris, and although there were three Ngbakas, they used tsi (only three times for all of them). The Bandas provide twelve records of chi (eight speakers), but they also use ts and s with a vowel (like tsi and tsu) or ts alone before a vowel. At least twelve of the speakers come from the same part of Bangui: from the Miskine quarter, others recorded at the Koudoukou primary school in what is called the neighborhood of ‘Kilo Cinq’ much less affluent than the city center five kilometres away. Bandas, however, are not characterized by the use of ch. The preferred variant of fifty-one speakers of Banda ethnicity in my corpus is ti (62.67 percent [418 records] of 850 records). Even the eight speakers who used ch used it only 6.5 per cent of the time (11 records of 165), favoring ti at 47.27 per cent (78 records).
CONCLUSION

One should see by now that (a) on the basis of historical evidence and academic historiography and (b) on the basis of a professional standard of linguistic argumentation at the time of the completion of Morrill’s work, and even more on what has been published since then, that (i) there are more reasons to believe that Sango arose in the colonial period at the end of the nineteenth century than that it had some kind of vehicularized antecedent and that (ii) Sango is a pidgin. The latter assumes, of course, that such a kind of language be recognized by linguists. I do so for two reasons. The first is that although some of the features once considered characteristic only (or most frequently) of pidgins can be attested in natural languages, only certain languages have the extremely small vocabularies that all pidgins have by comparison with all natural languages. This is a purely linguistic characteristic. The second is that always when compared to the language on which a pidgin is based (or out of which it emerged, or whatever may be the alleged derivation) a pidgin is reduced, simplified, and regularized by comparison. (The characteristic ‘restructured’ is omitted, because I find this concept too vague.) Although linguists seem to agree that certain kinds of historical events are associated with the rise of pidgins, I see no reason to invoke them for establishing a type of language. Moreover, especially a propos of Morrill’s work, linguistic similarities between a pidgin and its putative source or sources is exactly what we should expect. Pidgins do not come from nowhere. Morrill almost seems to think so, because he depicts a pidgin’s origin with something like my representation in (A), both the absence of connection between the horizontal lines and the double diagonals, presumably representing social upheaval (35). The usual depiction (and my own) is in (B).

A. Natural language —————— > Pidgin/

B. Natural language ————> Pidgin

Finally, a pidgin (or perhaps some pidgins more than others) can be said to be related to a natural language. But what ‘related’ means is something that linguists must decide in the forum of the discipline. We can expect consensus to change over time, and at no time is one person’s opinion necessarily better than anyone else’s.