

The BK expansion — grammar, demography and lexical tone*

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The comparative linguistics of the Niger-Congo languages is important primarily for the light it sheds on the ancestral subgrouping of the languages. The tentative nature of Greenberg's subgrouping must always be borne in mind; his main concern was with the membership of the Niger-Congo family and not with the subgrouping... If, of course, there was a specially close relationship between the Kwa languages (which include Akan) and the Benue-Congo languages (which include the Bantu languages), the latest common ancestor of Akan and the Bantu languages would not be Proto-Niger-Congo... A later common ancestor than Proto-Niger-Congo would in fact seem likely in view of the nature and extent of the resemblances...
(Stewart 1971a, 90, 92)

1. A problem in the natural history of languages

As is long known and often admitted *sotto voce*, the traditional “Bantu” area recognized by Doke, Meinhof and Guthrie lacks a Niger-Congo-internal (i.e. northwest) boundary that’s definable in linguistic terms (Gerhardt 1980, Williamson & Blench 2000, 35, Nurse & Philippson 2003, 5). Based on morphological similarities of nouns, Bleek the Ur-Bantuist even stretched the zone’s left edge to include “portions of Western Africa as far west as Sierra Leone” (1862, 2), inducting himself thereby into the club of taxonomic “lumpers” (www.darwinproject.ac.uk/entry-2130). This view is independently supported by Mukarovsky (1965). The “splitters” replied with the *ad hoc* category “Semi-Bantu” (Johnston 1917, Talbot 1926, 87 and maps *infra*). Despite well noted misanalysis of Tiv and nearby languages (Abraham 1933, 1940b, Greenberg 1949b, Winston 1970, Gerhardt 1982), “Semi-Bantu” extended its half-life in synonyms like “wide Bantu” (Williamson 1971, 248) and “Bantoid” (Greenberg 1963, 9, Williamson & Blench 2000, 18).¹ By this terminological dodge, linguists continue positing a “reconstructed Proto-Bantu” (Schadeberg 2003, 143) and historians keep referencing “the original Bantu language” (Vansina 1995, 186) with no empirical reason to exclude Akan, Gbè, Yorùbá, Nupe, Èdó, Ìgbo and Ídòmà — not to mention the “wide Bantu” clusters of Plateau and Cross — from the descendants of the linguistic ancestor in question.

The mistake goes beyond the philologist’s precinct. It invalidates historical demographic inference, because identification of a set of ancestral traits is prerequisite to deciding how much of the present local diversity of the “Bantu” population arose through drift by migration (Mayr 1959), also known as *demio diffusion*, as opposed to the null hypothesis, random diversification *in situ*, also called *isolation by distance* (IBD).² The reference set for “Bantu” reconstruction has no reason to exclude the seven aforementioned clusters spoken in between “Semi-Bantu-stan” and eastern Cote d’Ivoire.³

Wherever archaeological and textual evidence is scarce, the best graph of large-scale human events on millennial timescales confronts genetic and linguistic signals. The two types of information follow different paths through time: languages aren’t coded in genes, but the mental ability of human language is itself biologically enabled and constrained (Chomsky 1986) and both kinds of demography respect equivalent logics of historical naturalism — respectively called *phylogenetic cladistics* and *the comparative method*, as elaborated in tandem a century and a half ago by Darwin and the Leipzig Neogrammarians (Timpanaro 1981, Alter 1999).⁴ *Ceteris paribus*, stable gene-language correlations are expected (Nichols 1997, Cavalli-Sforza 2000), but only if each taxonomy can stand on its own. A recent genomic survey finds modestly confirmatory echoes of Ehret’s Eastern (“Savanna”) clade, but no trace of the geographically complementary Western branch, whose existence is entailed by the traditional “Bantu” edge (de Filippo & al. 2012, cf. discussion in Ehret 2011). The disappointing result is no surprise. In tropical Africa, most computations of linguistic distance still rely on impressionistic word counts, sometimes supported by *argumentii ex silentio* listing “lexical innovations” (Meeussen 1956, Williamson 1971, 253, Ehret 1999). These methods are famously unreliable: they overlook small phonetic shifts, are vulnerable to big slips of translation semantics and can’t distinguish archaisms (inherited similarities) from secondary convergences of neighborly borrowing (Armstrong 1983, 146f.; Bennett 1983, 1989; Williamson 1989, 249, Campbell 1998, 186, Embleton 2005). Lexical classification is biased to detect recent areal relationships — historically important in their own right (Möhlig 1979, 1981, Güldemann 2011) but younger than the era of allele divergence.⁵ In the biologists’ words, “lexical data might be very susceptible to contact pressure, while structural features are considered to be more stable” (2012, 3262). Without reconstructible traits more durable than lexical accidents, it’s irresistible to fill up the blank spaces of the prehistorical map with exotic and untestable *Mischsprachen* (Hieda & al. 2011). *Africa ulterior bestiis et serpentibus plena* (Miller 1895, 35)!

2. Borrowing, inheritance and coincidence

Linguistics textbooks praise the 19th century discovery of regular sound equations between Sanskrit, Ancient Greek, Latin, Gothic etc., such as Grimm’s and Verner’s Laws (Pedersen 1931, 245, Collinge 1985, cf. Paul 1880). The *élan* is justified by astonishing proofs, such as de Saussure’s 1879 ‘internal’ reconstruction of ghost consonants behind Indo-European *Ablaut* vowel inflection (e.g. English *sing~sang~sung*). These were confirmed by Anatolian cuneiform glyphs deciphered only in 1902 and analyzed much later (Kuryłowicz 1935). But this particular success drew attention to a chronological puzzle: whether Anatolian held a relationship of sister, daughter or aunt to the other, previously established IE branches (Sturtevant 1938). The problem is quite general: reconstructions aren’t intrinsically timed, so it’s easier to prove shared heritage between two languages than to determine branching order when a third descendant is added to the graph (Anttila 1972, 109). Schematically, cognation A::B entails reconstruction of *AB, but subsequent expansion to A::B::C needs more information to know how C connects. Because

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1. On partly demographic grounds, Heine & al. relabel Greenberg’s “Bantoid” as “Proto-Bantu” *tout court* (1977, 61).

2. A different concept of drift, viz. as structural teleology, is presented by Sapir (1921).

3. Tentatively excluding Kru (western Côte d’Ivoire/eastern Liberia) and Iẓõn (colonial “Ijaw”, Niger delta), cf. Greenberg (1963, 39 *fn* 13).

4. For the biologists’ terminology see en.wikipedia.org/wiki/Cladistics.

5. Notwithstanding the tendency of *glottochronology* to overstate divergence time-depths several-fold, by overlooking many modern cognates.

the sequence of pairwise reconstruction is arbitrary, every IE branch faces this indeterminacy unless the protolanguage split ten ways, into Celtic, Germanic, Italic, Greek, Balto-Slavic, Albanian, Armenian, Anatolian, Indo-Iranian and Tocharian, *simultaneously* (Garrett 2006, cf. Watkins 1966). The same specter haunts Greenberg's Niger-Congo: his initial results of largescale *inclusion* (lumping) are robust, but his *exclusive* sorting (splitting) of Niger-Congo's diversification (and thus, demographic dispersal) into a chronological (branching) order of events has not endured.⁶

In situations where branching is undefined, a workable alternative is to disaggregate macro-differences into *isoglosses* — waves of micro-changes diffusing through contiguous populations (Schmidt 1872). These need not bundle together so long as the speakers remain in contact. The wave model resolves the IE branching problem (Meillet 1908, Porzig 1954, Antilla 1972, 305) but carries a glitch of its own: microparameters treat languages as gradualist aggregates, but in known cases with contemporaneous documents — with caveats about timelag between speech and writing — the tempo of change is uneven: abrupt speciation events are separated by long static stretches (Dediu & Levinson 2012, cf. Labov 1971). This pattern — effectively phasing in and out of conformity with the tree model — recalls the paleontologists' "punctuated equilibrium", an idea outside the ken of Darwinian gradualism (Eldredge & Gould 1972). Thus Middle/Chaucerian English of the 14th century closely resembled the Anglo Saxon that was already in place before 1066, whereas its mutation to the Modern/Shakespearean form was relatively quick (Jespersen 1905, Baker 1976). Moreover, the outcome was not more French-like, even though external influence came from Norman French. Stability/abruptness alternation of this kind presumptively reflects a change located in the aforementioned "structural features" which, being more abstract are less susceptible to direct imitation, given that syntactic rules unlike lexical choices run mostly below the threshold of consciousness. Syntax is rigid but brittle, restructuring occurring when accumulated complications in the parents' grammar exceed recoverability limits (Andersen 1973, 767; cf. Lightfoot 2006, Mufwene 2007). Mass lexical replacement, more granular in nature, follows on the heels of syntactic change at least as often as it precedes (Atkinson & al. 2008).⁷

The abstract and quantal nature of syntax however makes it hard to distinguish a shared, inherited property from one obtained by accident. Thus Japanese and Iẏon are closely isomorphic, with prenominal modifiers, the finite verb at the end of the sentence and various similarities of prosody and inflection (cf. Williamson 1965, Kuno 1973), but lexical comparison assures that these languages are non-cognate and their morphosyntactic match has no historical significance. Many applications of structural typology in historical classification turn out to be chance effects, even among adjacent languages. Westermann for example defined the "Kwa" group solely by the absence of polysyllabic roots and of nominal inflection:

In den Kwa-Sprachen herrschen die Stämme in einfachster Form vor. Ihre charakteristische Eigenart besteht in den vokalischen und nasalan Nominalpræfixen, die von Ansätzen abgesehen keine Klasseneinteilung bewirken, wohl aber wortbildend sind. (1927, 20).

[In the Kwa languages, roots mostly appear in bare [= monosyllabic] form. They typically carry a vocalic or nasal noun prefix which no longer expresses an original nounclass distinction, but which is nevertheless still segmentable morphologically.]

Both definitional traits crumbled in the 1970s, when "Kwa" evidence of prefixal number inflection — regarded by Meinhof and Guthrie as a core Bantuism — as well as polysyllabic roots (Elugbe & Williamson 1977, cf. Williamson 1989, 12) was widely observed (de Wolf 1971, Williamson 1971, Welmers 1973). Greenberg had parenthetically suggested that "Kwa and Benue-Congo are particularly close to each other and in fact legitimate doubts arise concerning the validity of the division between them" (1963, 39 *fn.* 13, cf. Stewart 1971b, 206, Williamson 1971, 252), and as soon as this hunch had been substantiated,

De Wolf (1971, 180) combined them [*sc.* Kwa and Benue-Congo] into one branch for which Hyman (*pers. com.*) suggested the name 'Benue-Kwa' (Williamson 1989, 16)

The Benue-Kwa node became the null hypothesis (Givón 1975, Elugbe & Williamson 1977, Bennett & Sterk 1977a, Gerhardt 1978, Stewart 1983, cf. Mukarovsky 1976, Mische 1991) and "the Benue-Congo Working Group renamed as a consequence of the abandonment of the Kwa/Benue-Congo dichotomy" became "the Benue-Kwa Working Group" (Williamson 1989, 17).⁸

3. Statistical presentism, alias "He Miss Road" (Kúti 1975)

Neither Greenberg's doubts about Kwa, nor the mentioned arguments for Benue-Kwa, were statistical in character, so conflict was inevitable once the followers of Swadesh (1952) got into the act. In 1977, Bennett & Sterk redivided Benue-Kwa anew, hiving off the two westernmost neighbors Àkan and Gbè together into a novel subgroup which they eventually called Western SCNC, and amalgamating the rest of Kwa — Yorùbá, Nupe, Èdó, Ìgbo and Ìdòmà — into a sister node, Eastern SCNC, along with all of Benue-Congo. That they considered this to be their main result, is shown by how they edited the title of their paper between the conference presentation (1977a) and the published version (1977b) to expunge the term "Benue-Kwa" because

...we feel that the term "Benue-Kwa" still incorporates the traditional division between "Kwa" and "Benue-Congo" which we suggest should be abandoned. Hence: South Central Niger-Congo or SCNC. (Bennett & Sterk (1977b, 241 *fn.* 1)⁹

Their evidence for the new division includes the statistical measurements sampled below, plus four consistent lexical differences which they tendentiously call "isoglosses" (1977, 253) although that term is normally reserved for *regular* innovations not singular lexical events. One out of the four strains credulity: Western SCNC **tá* 'three' is trivially cognate to Eastern SCNC **tát(ə)* 'three' and to analyze it a morphological innovation on the eastern side only proves how the authors were struggling to make their case.

6. Lumping is not infallible, having failed in other phylogenies, whether Greenberg's own "Amerind" (1987) or the super-stocks proposed by his latter-day proteges (reviewed by Campbell & Poser 2008). And even if the success of Niger-Congo is due to sheer luck, it's not undermined by the failed cases, unless we would insist on inductive "discovery procedures" — a "questionable" scientific goal (Chomsky 1957, 56).

7. See below for a Niger-Congo example.

8. De Wolf's coinage had been the less euphonic but more descriptive "B[enue-]C[ongo]-Kwa" (1971, 180). The renaming was accepted at a working group parasession that I was lucky to attend during the 15th West African Languages Congress at the University of Port Harcourt, April 1982. Stewart's equivalent is "Tano-Congo" (1983, 20), one level below his "Volta-Congo" (1976, 7) which also includes Gur.

9. Maybe the name change also alluded to *South Central L.A.* (as in, Crips and Bloods), given that West Africa had proved to be a tough neighborhood in which to calculate "nearest neighbor" and "farthest neighbor"...

Another idiosyncrasy is their calculation procedure. To reduce the Swadesh tendency to miss cognates through translation error (colonial languages being ill-suited to glossing indigenous terms), Bennett & Sterk allow multiple entries for a single gloss so as to avoid a false negative. This is *contra* standard Swadesh procedure (1952, 457), which explicitly rejects just this kind of expert philology. The attempt backfires: thanks to the well-intentioned multiple glossing, the reference set of English items yield more translations in some languages than others, due to a host of unrelated factors not excluding modern accidents like the strength of indigenous scholarship in each language community. The resulting one-to-many word alignments create incommensurable asymmetries, depending on which language of a pairwise comparison is taken as the denominator of the fractional percentage. Bennett & Sterk declare this muddle forthrightly, without admitting how it exercise undermines as a whole.

Consider languages A and B with $n=87$ and $n=112$ glosses respectively, responding to an 87-word English list, where the philological adequacy of B's documentation yields 25 additional translations over the whole list, whereas the coders of A manage just to elicit one indigenous gloss for each item. If then the coders identify 23 lexical items as cognate between the two, A's share of B is 21% but B's share of A is 26% (cf. 1977, 247). I concocted this example to mimic the outcome that Bennett & Sterk report for the lexical intersection of Twi (€ Àkan) and Èwè (€ Gbè), the two clusters that Bennett & Sterk claim are historically closer to each other than either is to anything further east in the Benue-Kwa/SCNC area.¹⁰ In the same way, each language pairing in their study returns two values. By convention below, the number reported before the comma is the proportion of B found in A, with the reverse after the comma. Because the second values for TwiÈwè is greater, we know that Bennett & Sterk tapped into better quality translations of Èwè than of Twi because they find that Èwè contains a higher proportion of Twi than Twi does of Èwè. Similarly for all the other pairings.

TwiÈwè 21,26%	TwiÈwè Yorùbá 24,26%	TwiÈwè Nupe 20,<18%	TwiÈwè Èdó 20,18%	TwiÈwè Ìgbo 24,21%	TwiÈwè Ìdòmá 27,24%
	ÈwèÈwè Yorùbá 23,20%	ÈwèÈwè Nupe 20,<18%	ÈwèÈwè Èdó 25,19%	ÈwèÈwè Ìgbo 26,19%	ÈwèÈwè Ìdòmá 29,21%

Historical reasoning from statistics of this quality is obviously worthless.¹¹ Discrepancies between the paired figures show the vulnerability of the supposedly precise method to accidental gaps in translation knowledge. What's the point of the exercise if a difference of 8% can arise for ÈwèÈwè Ìdòmá simply by the luck that Èwè's lexicon is so much better described, whereas the "strong border" (1977, 253) which Bennett & Sterk believe to separate the Twi and Èwè clusters on the one hand from Yorùbá, Nupe, Èdó, Ìgbo and Ìdòmá on the other rests on much less than an 8% overall differential? Secondly, no historical linguist can credit the counterintuitive disaggregation of what should intuitively be a reciprocal relationship: is French synchronically more distant from Italian than Italian from French? Is Paris further from Rome than Rome from Paris? A third indefensible point is the authors' stipulation of a cutoff of 18% — "chosen rather arbitrarily" as they confess — below which they won't mention a result because they consider it "meaningless" (1977, 244). Can the corresponding claim of *meaningfulness* above 18% be defended in terms of ordinary statistical significance, when the slim percentage differences are calculated on from baseline $n < 100$?

Schadeberg gives a more standard lexicostatistical analysis of Bennett & Sterk's raw scores.¹² The format of his "similarity matrix" (1986, 82) has no place for relativized percentages, and inspection shows that he chose to discard their *lower* value for each pairing, although he doesn't say why and the reason does not seem to follow by necessity. For convenience I repeat the above tabulation only with the numbers that Schadeberg feeds into his own phylogenetic automaton.¹³

TwiÈwè 26%	TwiÈwè Yorùbá 26%	TwiÈwè Nupe 20%	TwiÈwè Èdó 20%	TwiÈwè Ìgbo 24%	TwiÈwè Ìdòmá 27%
	ÈwèÈwè Yorùbá 23%	ÈwèÈwè Nupe 20%	ÈwèÈwè Èdó 25%	ÈwèÈwè Ìgbo 26%	ÈwèÈwè Ìdòmá 29%

He concludes by endorsing Bennett & Sterk's proposal to separate Àkan and Gbè from the rest of Benue-Kwa, but how he gets there is scarcely clearer. After his own algorithm yields three alternative graphs, requiring intuition to adjudicate, e.g.:

[L]et us accept the nodes that the B[ranch] A[verage] tree shares with either the F[urthest] N[eighbor] or the N[earst] N[eighbor] tree than are not strongly contradicted by the opposite tree. (1986, 73)

The picture would be no less equivocal if Schadeberg had opted to use the *lower* values from Bennett & Sterk's material:

TwiÈwè 21%	TwiÈwè Yorùbá 24%	TwiÈwè Nupe <18%	TwiÈwè Èdó 18%	TwiÈwè Ìgbo 21%	TwiÈwè Ìdòmá 24%
	ÈwèÈwè Yorùbá 20%	ÈwèÈwè Nupe <18%	ÈwèÈwè Èdó 19%	ÈwèÈwè Ìgbo 19%	ÈwèÈwè Ìdòmá 21%

Schadeberg's other lasting impact on this debate is terminological confusion. As already noted, Bennett & Sterk had changed "Benue-Kwa" to the anodyne "SCNC" in order to distinguish their classification from prior uses of "Kwa" and "Benue-Congo" in the literature, but instead of contributing to history, Schadeberg preferred to rewrite it. Thus for him,

"(New) Kwa" represents Bennett & Sterk's "Western SCNC" i.e. the old Western Kwa. "(New) Benue-Congo" represents Bennett & Sterk's "Eastern SCNC", i.e. old Eastern Kwa plus Benue-Congo. (Schadeberg 1986, 73)

Williamson's Niger-Congo reclassification in her chapter for the SIL handbook, endorsed not only Bennett & Sterk's revision to the Benue-Kwa family tree, but also the precious parentheses in Schadeberg's "(New)" designations (1989, 17). In the process, all the phonological and morphosyntactic evidence collected in the preceding 20 years was ditched in favor of lexicostatistics not worth the floppy disks they were magnetically imprinted upon.

In sum, Bennett & Sterk and Schadeberg's cumulative interventions sent Niger-Congo historical research on a road that has yet to reach any destination that can be corroborated by independent evidence. The best possibility in this regard is scant indeed:

10. Without Bennett & Sterk's raw data, I don't know the *actual* sampling n for each language, just that some rates differed on the order of 20%.

11. Or at least "useless" — as the leading historical linguist currently working in Ghana said publicly in an Africanist conference in Berlin, 2005. Similar comments were made by late R. Armstrong (p.c.) in 1983 while preparing his published critique the same paper (see above).

12. Schadeberg obtained Bennett & Sterk's raw data from Kay Williamson in "a file which she had been given by Jan Sterk" (1986, 69 *fn* 1).

13. "The lexicostatic calculations used for this paper were carried out with the program LEXISTAT. ... I would be happy to share this program with anyone who is willing to compensate me for the price of the diskette plus postage" (Schadeberg 1986, 71 *fn* 1). Computational phylogeny has grown massively in sophistication since 1986 (cf. Nakhleh & al. 2005, Warnow & al 2006) but the quality of translated lexical datasets in Niger-Congo, considerably less so!

the most scrupulous imaginable review of sound shifts pertaining strictly to “(New) Kwa” = Eastern SCNC, conducted by the most eminent authority after Stewart, identified only two potential candidates: one supported by “a rather small amount of data” and the other being a generic simplification of a complex onset phonation type which “undoubtedly happened elsewhere” such that it doesn’t clearly count “as an exclusively Kwa [= SCNC] innovation” (Kropp Dakubu 2012, 32*f*).

Yet more confusion on the same point should have been impossible, but Leiden still had one more mischevous card to play. Toward the close of her career, apparently discouraged by the failure of “(New) Kwa” = Eastern SCNC to find a firm empirical foundation, Williamson subtly restored the 1971 position. Her (co-authored) classification chapter for the Cambridge handbook rejoins “Kwa” and “Benue-Congo” (meaning by then “(New) Kwa” and “(New) Benue-Congo”) such that *all* the subgroups of “East Volta-Congo =Proto-Benue-Kwa” are connected with a *doubled* line (Williamson & Blench 2000, 18). The special marking is painstakingly explained as follows:

The divisions of Volta-Congo are shown with doubled lines, following a convention... to indicate the diversification of a dialect continuum. In the case of Gur and Adamawa-Ubangi... it has been argued that these form such a continuum and a similar situation seems to occur with Kwa and Benue-Congo. (2000, 17)

It’s hard to understand why Williamson left her 1989 labels (i.e. those of Schadeberg), intact in the 2000 graph — minus the disambiguating but embarrassing “(New)” prefixes — whereas their historical content was completely evacuated by the concept of “dialect continuum” that was invoked in the same chapter as an annotation to the same graph. In a better world, Williamson & Blench would have spared a word or two to spell out *why* the Benue-Kwa continuum should be restored at the eleventh hour, but meanwhile 1989 labels — minus the “(New)” prefixes — have become entrenched by the institutional clout of SIL, the 1989 book and the current owner of the West African Linguistic Society after West African universities had been destroyed by neoliberal policies of disinvestment in public goods.¹⁴

More mischief unrolled a few years later when, as Kay’s health had entered its terminal decline, Schadeberg employed a handbook chapter of his own to waft more smoke around the visual representation of historical events. His graph (2003, 155) entitled “The position of Bantu within Niger-Congo” is annotated with the legend “Source: adapted from Williamson & Blench (2000)” but despite that vague authority claim, Schadeberg has erased any hint of the Benue-Kwa “dialect continuum” which the ostensible “source” had taken pains to annotate. He then goes further to insert an intermediate node “Benue-Congo” separating his “Kwa” (i.e. the old, “(New) Kwa”) from the rest of “Benue-Kwa” (without the “(New)”), although this additional structure has no origin in the cited “source” even though the existence of just this node was the main point of contention of all the lexicostatistic literature previously mentioned. Under such circumstances, a deceptive intention is difficult to ignore .

The story helps to explain why John Stewart would have personally

regretted the fact that so few people joined him in the strict application of the comparative method to the reconstruction of West [sic] African languages (Mous 2007, 72)¹⁵

The negative value of lexical statistics for historical reconstruction in this particular case recalls Abraham’s verdict on Harry Johnston’s effort about a century ago to defend the indefensible Semi-Bantu dodge:

His classification was on vocabulary alone, often a broken reed, but the only means at his disposal; we are now, however, in a position to examine the grammatical features... (1933, 6).

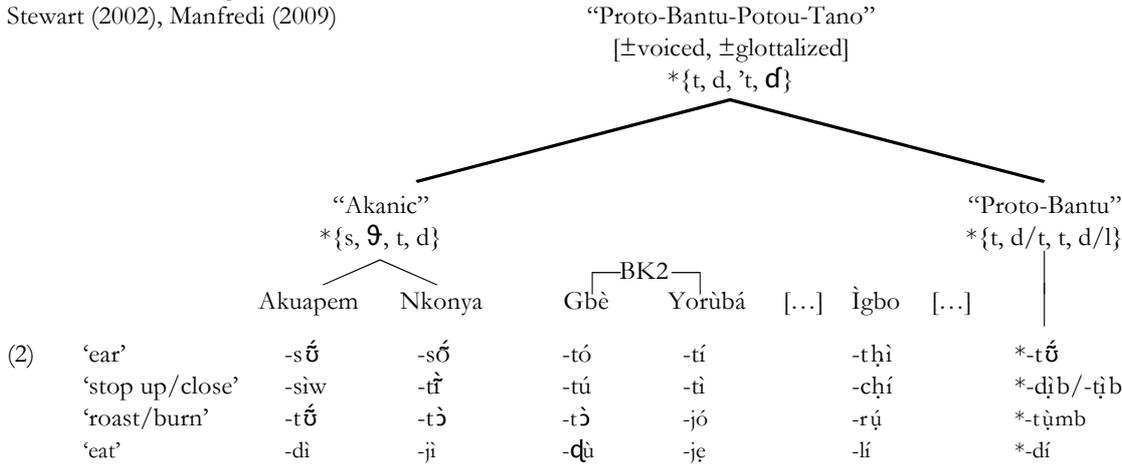
[continuous text ends here]

14. The hubris of the “(New)” nomenclature of 1986 is the refusal to anticipate further revision, which would then logically demand recursive “(New (New (New...)))” labels self-multiplying until the end of time like Ptolemaic epicycles. Such logic being is scarcely human, the default situation has been reversion to “Kwa” and “Benue-Congo” *tout court* without disambiguation, leading to bordellos like the one observed by Kropp Dakubu (2010, 442*f*). If hubris begat original sin, then the testament of that sin is the 1989 handbook, edited by SIL’s John Bendor-Samuel, with its own history of deception. The project had been originally pitched as part of Werner Winter’s eminent series *Trends in Linguistics* (Mouton, Berlin) at the last possible moment, after all the contributions were in hand, production was smoothly switched to University Press of America, a quick-print house near Langley, Virginia offering their lowbrow imprint in exchange for the customer’s prepayment of “85 copies” (JBS letter of 5 May 1989). The switcheroo was excused to the authors as a cost-cutting measure, then when the hard copies books eventually appeared they not only had the ‘wrong’ publisher but carried the SIL logo on the title page! Bendor-Samuel wrote to the authors sheepishly a second time (6 July 1989), “expressing surprise and concern” and soliciting “any objection to the book going out as co-published by UPA and SIL” while leaving no doubt that this was a *fait accompli* because any other outcome would have meant that SIL “paid for the first printing in full. Frankly I don’t have that sort of money.” His claim of penury was itself surprising, coming from the *caudillo* of the world’s best-funded (and least transparent) pentacostal missionary enterprise, and given that the offending logo appeared on a book edited and produced by SIL, half of whose contributors were SIL, in an ongoing commercial arrangement whereby “UPA has co-published a number of books with SIL” as JBS’s second letter also confessed. As scrambling graduate student, I despaired of legal remedies and let the matter drop after circulating a futile page of protest.

15. Mous goes on bizarrely to describe Stewart’s contribution “as an extension of the work by Guthrie” (2007, 72) although it’s obvious that Stewart refuted Guthrie’s basic premise: the independent historical origins of “Bantu” and West African languages.

4. BK comparative grammar

Stewart (2002), Manfredi (2009)



Against “(New)” Kwa: labialization of velars in Gbè and Yorùbá,

	BK2				BK1			
	Gbè	Yorùbá	Nupe	Ìdòmà	Àkan	Ìgbo	Èdó	“Proto-Bantu”
(3) ‘hunger (v.)’	-wù		-gùn	-ɲmú		-g(h)ú		* ^h -guid ‘seize’
‘hunger (n.)’		ebi			òkòm	ág(h)uú/ó		
‘journey’		ebi	ezi	èyè		íj(h)è		* ^h -gend
‘bend/bent’	-bò	-wó			kòtów	-gó	-gò	* ^h -gòb
‘needle/thorn’	àbí	àbẹ́bẹ́	èkin	ìgyẹ́		àg(h)íg(h)á		
‘pierce/split/sew’		-bẹ́		-gá	-chwá	-g(h)á	-gia	

Manfredi (2005)

- (4)a. A finite eventive predicate with minimal inflection is either present-perfect or past.
 b. Aspectually unrelated events are excluded from a single clause.
 c. Minimal finite inflection is an aux/proclitic particle, not a suffix or root-borne tone pattern.
 d. At least three surface tones contrast on roots of the same category.

- (5)a. 4 minus settings: { [Kru (?4a)], Àkan, Èdó, Ìgbo, Bantoid... } = BK1
 b. 4 plus settings: { Gbè, Yorùbá, [Nupe (?4a)], [Ìdòmà (?4b)]... } = BK2
 c. mixed settings: { = Ø }

	Yorùbá (BK2)		Ìgbo (BK1)
(6)a.	<p>̀Ngìgè-é bi Ìgè (*jádèè). N.-FIN ask I. exit ‘̀Ngìgè (has) asked Ìgè (*and then left)’ LLL-H M LL (*HLM)</p>	(6)a.	<p>̀Ngìgè ju-ru Ige (fù-ó). N. ask.FIN-CL I. (exit-AFF). ‘̀Ngìgè asked Ige [about something] (and then left)’ LLL L-L LL (L-H)</p>
b.	<p>̀Igè-é gbé agbòṅ (báàyií). I.-FIN lift basket thus ‘̀Igè (has) lifted [a/the] basket (now)’ LL-H H ML (HLLH)</p>	b.	<p>̀Igè vu-ru ábọ (*ùgbú à). I. lift.FIN-CL basket now ‘̀Igè lifted [a/the] basket (*now)’ LL L-L HH (LH L)</p>

5. Proto-tone or pitch accent?

Ternary tone contrasts on roots are confined to BK2, cf. (4d) above. Binary lexical pitch contrasts, against which locally circumscribed changes played themselves out, are reconstructible before “Bantu”.

A comparison of Èfík with related Proto-Bantu forms shows a high degree of correspondence in tones, about as great as that exhibited by most contemporary Bantu languages to the reconstructed forms. ... We also have Abraham’s tonal material for another Semi-Bantu language, Tiv. Here again there is, in general, excellent agreement with reconstructed Proto-Bantu tonal forms. Most striking of all is the following. In Bantu, the nominal prefixes have low tone. However a small number of Bantu languages agree in having high tone except for Meinhof’s classes 1 and 9 (the singular of the personal and animate classes) for certain forms in concord with the noun. These include one or all of the following: adjective prefixes, prefixes of numerals, genitive particle including the possessive adjectives and pronouns and verb subject pronouns. Thus for all classes except 1 and 9 which have low tone, these concord elements have high tone as against low tone for the nominal prefix. This precise irregularity occurs in Tiv where the “long form” of the pronouns as subject of certain verb forms classes 1 and 9 have low tone and the remaining pronouns have high tone. A more intimate point of contact or one less likely to be borrowed is difficult to imagine. (Greenberg 1963, 33-35)

Many eastern BK languages lack freely contrasting root patterns of 2 tones x 2 syllables = 4 (Meeussen 1976), cf. (7). Luba “tone reversal” (Greenberg 1948, 1963, 41 *fn* 32) is historically impossible, entailing absolute neutralization as an intermediate stage, cf. (8). Holding number of syllables constant — but cf. Meeussen (1979), Phillipson (1999) — either tone contrasts were reduced in some areas (Goldsmith 1984a,b) or inversely the protolanguage had no free tonal contrast (in theory-neutral terms) and some daughter languages acquired it by independently known phonation and phrasing effects plus the banal process of listin, Clark (1988) and (9). In fact the supposed reduction is found in noncontiguous areas (western BK plus eastern/Savanna BK) whereas full binary tone on lexical roots is geographically confined to Guthrie’s western area. Demographic reasoning a.k.a. drift therefore favors tonal innovation not reduction, otherwise it was implausibly reduced twice in the same way independently in noncontiguous areas. Tone reduction is moreover a nonrestrictive or null hypothesis, risking circularity by reconstructing all modern irregularities to the protolanguage (cf. Greenberg ed. 1963).

(7) Ganda (...Bemba, Shona, Nguni...)

	Guthrie	Meeussen	
	*HH	*HH	*nyésé ‘cricket’
	*HL	*HH	*lúmé ‘husband’
	*LL	*LL	*lenzi ‘boy’
	*LH	*LH	*golé ‘bride’

(8) Luba

LL < Guthrie “Common Bantu” *HH
 LH < *HL
 HL < *LH
 HH < *LL

Nande accentual listing of superficial 4-way tone contrast (Manfredi 2014)

			Kambale (1978)	Valinande (1984)	Mutaka & al. (2008)	Jones (2008) <i>n</i> = 1178	
(9)a.	<i>unaccented</i>	[_n P ku [√ gulu]]	‘leg’	group A	[√ LL]	[√ ∅∅]	297 = 25%
b.	<i>accented</i>	[_n P kú [√ boko]]	‘arm’	group B	[√ HL]	[√ H∅]	179 = 15%
c.	<i>double-accented</i>	[_n P ká [√ húka]]	‘insect’	group C	[√ LH]	[√ HH]	63 = 5%
d.	<i>opaque, accented</i>	[_n P kíkere]	‘toad’	group D	[√ HH]	[√ HL]	221 = 19%

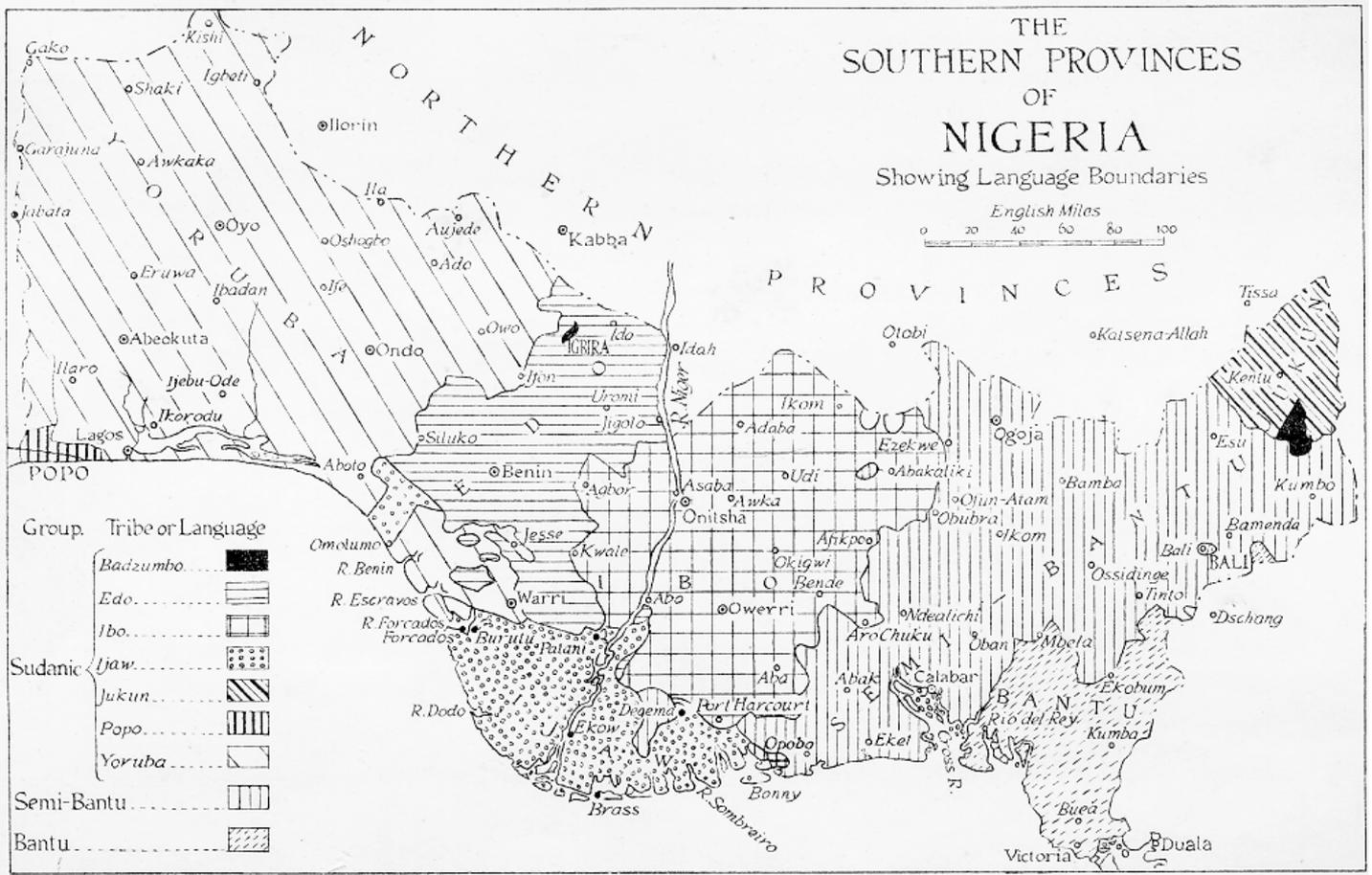
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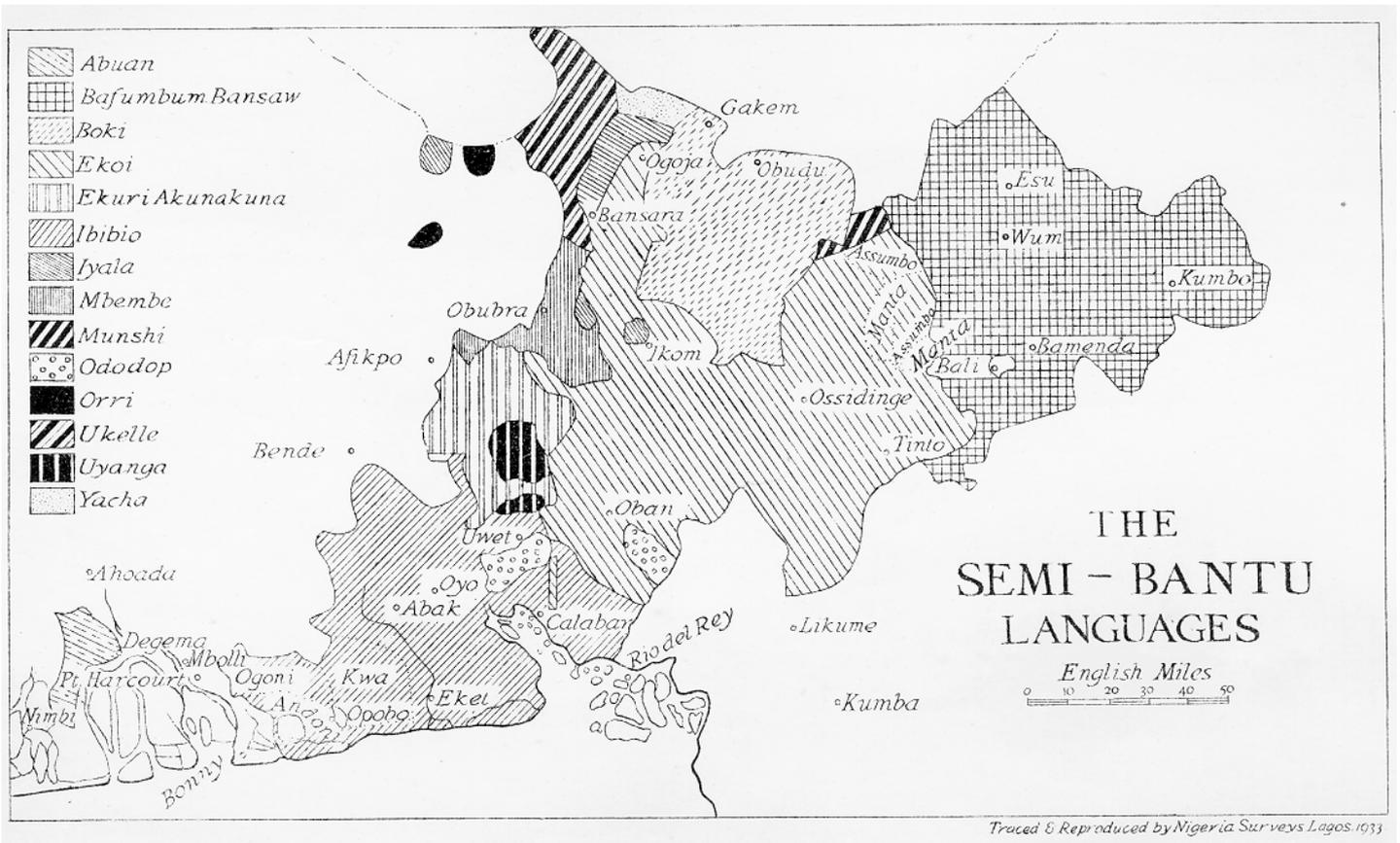
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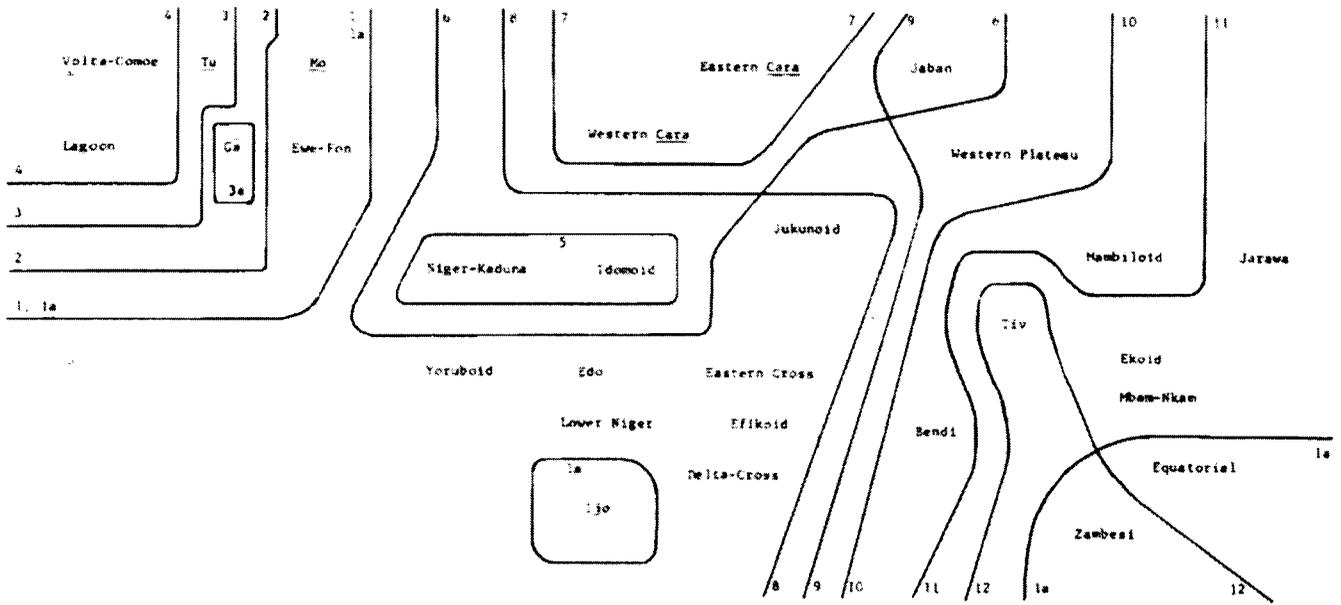
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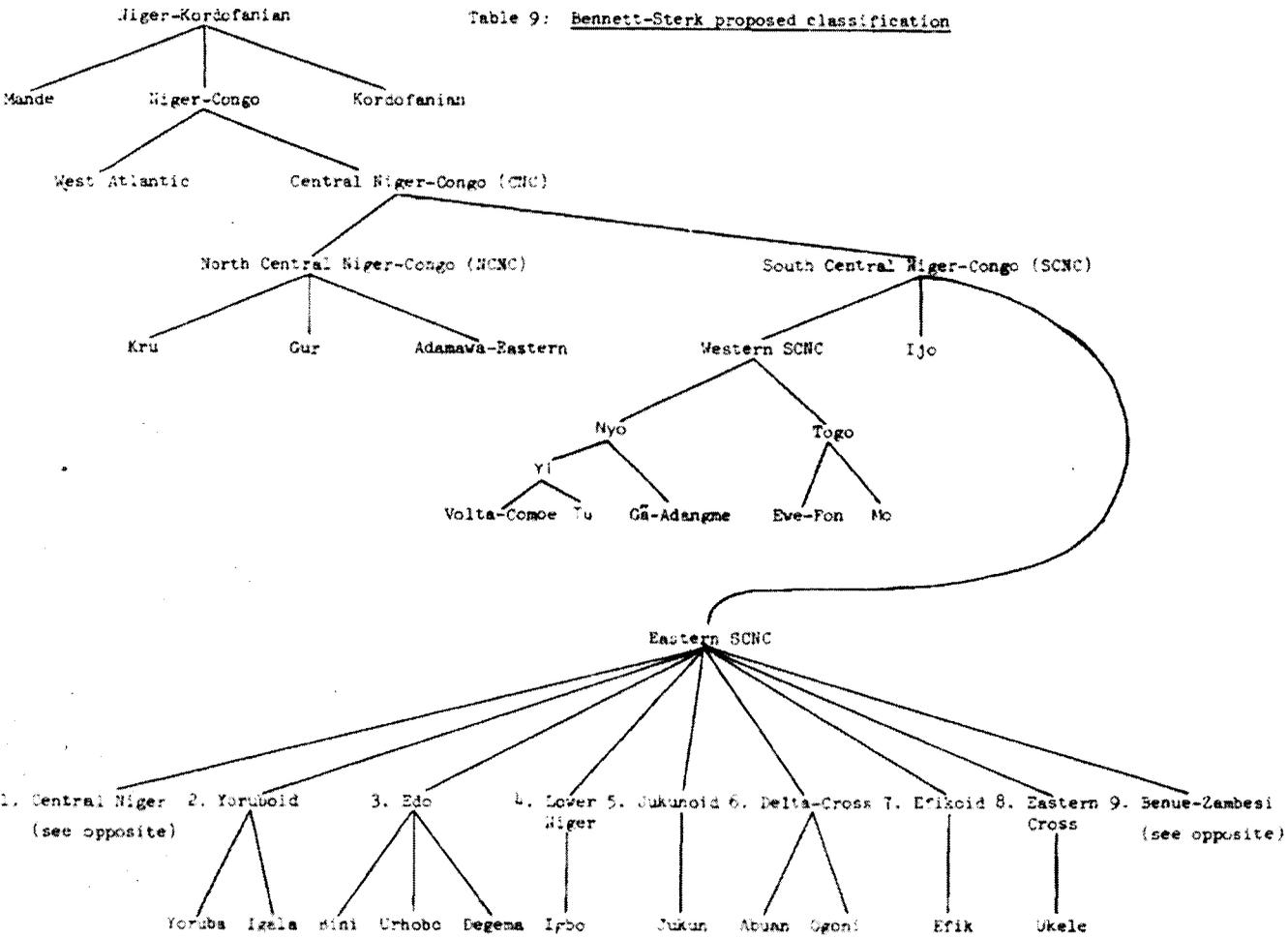
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