

Review of Salles (2023) on Pirahã Grammar¹

DANIEL L. EVERETT

deverett@bentley.edu

Pirahã grammar is discussed in several places, e.g. *inter alia*, Everett (1983, 1984, 1986, 1987, 2005). I will therefore not repeat earlier descriptions here, except to quote from more recent work when necessary to clarify a criticism. Superficially Pirahã's grammar is relatively simple.^{2,3} It has been described by me as lacking recursion.⁴ I am going to assume hereafter, following, *inter alia*, Pullum (2020; 2024) and Derbyshire (1979) that the absence of recursion is unremarkable, although this particular claim has been met with numerous attempts at reanalysis, sprinkled with vitriol.

As Pullum (2020) discussed the lack of novelty of my claim that Pirahã lacks recursion: "The claim that natural languages could lack features like hypotaxis was not novel or unprecedented."

Although this issue has been discussed in great detail over the years, I want to review here a new description of the language by Salles (2023), and, to a much lesser degree, on a hypothetical consideration about quantification.

What I show in this section is that Salles's description of Pirahã either largely repeats claims already made by previous researchers (usually me) or fails to argue for her deviations from that scholarship. Moreover, she fails to interact with any descriptive or theoretical work that challenges her conclusions and never once explains either her decision to omit reference to the large majority of the existent literature on Pirahã nor to argue for her own positions, especially when those positions have been falsified by earlier argumentation and experiments.

In any scientific work, responsible scholarship requires paying attention to and interacting with previous analyses of the phenomena about which one is suggesting new insights or analyses. When a researcher enters a field of study where there is already a substantial body of work, that researcher incurs an ethical obligation to demonstrate to their readers an understanding of earlier research, especially when that work impinges upon the topics the new work is addressing. This is so even though the strongest reasons for publishing new research might be to correct earlier analyses or to supplement the earlier work with new data. But although Salles (2023)'s analyses in many places diverge dramatically from previous work, she fails to offer any arguments as to why her analyses should be accepted over those previous analyses, nor does she even mention that her work diverges from any previous work. Salles's work throughout cherry-picks references to earlier descriptive and theoretical work.

¹ Salles, Raiane. 2023. Pirahã. In: Patience Epps and Lev Michael (eds.) *Amazonian Languages, Volume 2 Language Isolates II: Kanoé to Yurakaré: An International Handbook*. Berlin, Boston: De Gruyter Mouton, 2023. pp957-995. Because this is an excerpt from a monograph in progress on the conceptual foundations of linguistics, I have not included references here, focusing exclusively commentary.

² Futrell, et. al. (2016, 18ff) give a regular grammar for the language as: $S = \text{NP}_{\text{topic}} > \text{NP}_{\text{topic}} > \text{NP}_{\text{voc}} > \text{NP}_{\text{subj}} > \text{NP}_{\text{subj}} > \text{NP}_{\text{tmp}} > \text{NP}_{\text{loc}} > \text{NP}_{\text{obj}} > (\text{JJ}_{\text{obj}} \mid \text{NP}_{\text{obj}} \text{NP}_{\text{obj}}) > \text{NP}_{\text{obj}} > \text{V} \text{JJ}_{\text{obj}} > \text{NP}_{\text{voc}} \text{NP}_{\text{topic}}$ (where '>' = 'precedes')

³ Also see both Everett (2009) and Sampson, et. al. (2009). I don't believe in fact that statements about relative complexity of languages have much to contribute to the semiotics of language nor to the theory of syntax. I discuss this matter in Everett (2017, 374ff).

⁴ For a partial list of papers by me on Pirahã recursion, see some of my papers on this topic at: <https://daneverettbooks.com/collection-of-most-of-dans-papers-on-recursion>.

In fact, perhaps especially, the ethical obligation to interact critically with earlier work and to justify new proposals rather than merely assert them, is just as important when there is *no* disagreement with previous work. In such cases of agreement, the earlier work must be acknowledged as holding precedence. Otherwise, a later work largely repeating earlier work without acknowledgement could be mistaken at best for lazy research and at worst for plagiarism. The fact that this has been allowed to occur in the current work is a problem for both the author of the piece, as well as the editors of the volume in which the description is found. In this section I discuss these shortcomings in Salles's work and why the work falls short descriptively and theoretically.

Moreover, Salles's (2023) claims, like all linguistic descriptive work, must be evaluated in part by assessing not only the findings but also the methodology employed by the researcher, the amount time the researcher spent living in daily contact with native speakers, how that daily contact was used (how many hours a day of elicitation, analysis, etc.), the researcher's fluency in the language, and - perhaps most importantly - their understanding of the semantics of the language under study (Sakel and Everett 2011). Unfortunately, Salles does not address such questions in her description. Nevertheless, there is information available both in her own account as well as in descriptions by others that raise serious doubts about both the nature and the findings of her research.

For example, Salles could at most be claimed to speak Pirahã at a rudimentary level (see below). In a monolingual setting, this is an extremely serious impediment to research involving meaning and non-ostensive behavior, see (Everett and Gibson (2020)).

Beginning with Salles's description of Pirahã segmental phonology, her work is frequently nearly identical to my own, though without acknowledgment of this. She omits most discussion of prosody beyond minimal phonotactics (thus offering no description of syllable structure, stress, tonal perturbations, etc), although omissions of finer details in a short description are not all that serious, many aspects of Salles's phonological analysis raise concerns about the quality of the linguistic analysis.⁵

To begin with a simple example, she claims, based on early work by Arlo Heinrichs, that glottal stops are predictable word-initially:

"Glottal stop insertion before word-initial vowels also occurs predictably (Heinrichs 1964). Rule and examples are given in (4) and (5):

(4) Rule: /0/ → ? / # \bar{V}

- (5) a. /ibogi/ [ʔi.bo.'gɪ] 'milk'
b. /aáháih/ [ʔaáhájhɪ] 'sugar'"

But this is bad analysis. This is so because it treats one consonant, the glottal stop, as though it were fundamentally different phonologically than other consonants. Though it might be the case that non-phonologists (perhaps phoneticians) think that glottal stops do not fit among the regular consonants of a language (of course, in some cases they do, in other cases they do not), this depends on the phonological analysis. But for Pirahã, it simply will not do to claim that glottal stops are predictable word-initially. In fact, that would be equivalent to saying that /k/ or *any other* Pirahã consonant is predictable word-initially, since *all* Pirahã words begin with a

⁵ As mentioned, her bibliography is very limited. When citing me, she restricts herself almost exclusively to work of mine nearly forty years old, e.g. Everett (1986), prior to my re-analysis of Pirahã as lacking recursion. This earlier work of mine, which is acceptable in the generative community, for its mistaken view that the language has recursive structures. She entirely ignores my later work, which goes into detail about why the language lacks recursion. Moreover, she never offers arguments for her assertions on any aspect of the grammar.

consonant. For example, if all Pirahã consonants were to be represented word-initially (including glottal stops), except /k/ the same conclusion would follow:

0 --> k /# __ V

[ohoáipihái] 'eat' --> /kohoáipihái/, etc.

Such rules are misleading because they claim that native speakers know a rule that is entirely unnecessary, thus ignoring old debates in linguistic theory on parsimony, what speakers are purported to know, and elegance that a published linguist or editor should be aware of. But more importantly, if one were to adopt this rule, as Heinrichs (1964) did (but Heinrichs had no linguistic degree and only minimal linguistic training from SIL), or as Nimuendajú did almost everywhere (also no serious linguistic training), then, again, the rule would claim in effect that /ʔ/ is not a full-fledged consonant in the language, which is false.

For her account to be convincing, Salles would have to *argue* why glottals are phonologically - but not phonetically - omitted word-initially although no other consonants are. She would also have to explain why this rule is necessary.⁶ The ʔ-insertion rule produces an unacceptable segmental phonological complication of the segmental analysis.

Second, when Salles (959) compares Mura and Pirahã she largely repeats Everett (1979), which she cites elsewhere, but not as clearly in this case: "The most widely accepted contemporary classification, though, is that Mura is an isolate language and Pirahã is the last surviving of its numerous dialects (e.g., Nimuendaj. 1948; Aikhenvald 2012). See Table 21.1 for a comparison between Mura words documented by Hanke (1950) and Pirahã words, which convincingly shows that Pirahã should be classified as Muran."

But this is exactly the conclusion of Everett (1979, 6), where I report that:

"No seu trabalho "Alguns Aspectos da Ergologia Mura·Pirahã" (1977: 1) Rodrigues e Oliveira citam Amazonas (1852: 207), que diz que os Mura·Pirahã poderiam ser "oriundos do Peru, d'onde emigrarão, ressentidos da legislação dos Incas.

Isso poderia explicar porque a família Mura (que inclui os Mura, os Bohurá, os Yaháhi e os Pirahã (Loukotka 1968:95,96)) não tem qualquer vinculação óbvia com os demais grupos indígenas do Brasil. De milhares de pessoas (Ribeiro 1977:39) os Mura tem sido reduzidos a um pequeno grupo às margens do Rio Maici. Esse grupo, os Pirahã, dividiu-se em duas aldeias, cuja população global abrange aproximadamente cem pessoas."

Moreover, in her comparison she fails to note that there are conclusions we can draw from Nimuendajú's word lists, such as the fact that proto-Mura *d became /g/ in Pirahã and /d/ in Mura (Everett (1979, 126-128). But, again, this is not a major criticism, just a puzzle as to why she included somethings, omitted others, failed to cite the relevant sources in many cases discussing the phonology, and erred seriously in other cases, all without any attempt to learn from, criticize, or otherwise interact with the previous literature.

As mentioned earlier, a striking characteristic of Salles's chapter is that a great deal of it can be found in my own works (see the references at the end of this study), though she does not cite me as a prior source for most of the identical descriptive assertions she makes in her own chapter that I made in some cases more than forty years ago (from Everett (1979) onward). This is unacceptable. Another example comes from her citations of Sândalo and Abaurre (2010), as though this were independent of other research. Yet what she cites them as saying is similar to statements in my 1979 MA thesis (moreover, one of the authors, Sândalo, was my student at the

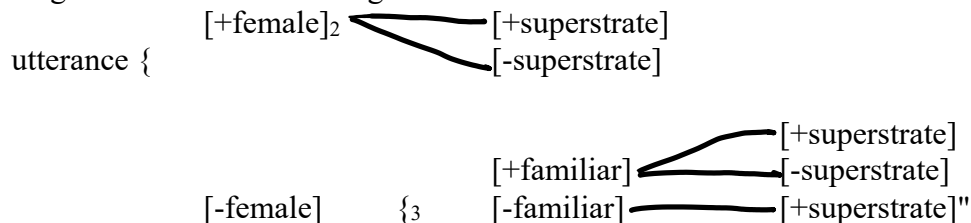
⁶ For a list of all SPE-type phonological rules I was able to discover for Pirahã during my initial field research with them, all of which I believe are necessary in some sense, see Everett (1979, 128-135).

undergraduate and graduate levels and all of her data was collected with me in Pirahã villages, under my supervision, with me translating).

Salles, likely unknowingly, even provides evidence indirectly that she does not speak the language fluently and is not fully accepted as a social intimate of the Pirahãs when she claims (p962) that: "Heinrichs (1964) and D. Everett (1979, 1982) also noted variation between [b]~[β] and [g]~[ɣ]. Although not observed in my fieldwork, anecdotal observations by my consultants suggest that older Pirahã generations used to have [β] and [ɣ] in their inventories."

But in her footnote 11 (p962) she immediately remarks that: "D. Everett (1979, 1982) argues that these sounds are only produced around people with whom the speaker is familiar. Keren Madora (p.c., July 2017) still observes speakers producing those sounds." In other words, if speakers are still using the sound around Keren Madora, then Salles is not considered an intimate member of the group and the sound is not used around her for the reasons given in Everett (1982, 1984, and 1985), in which these rare sounds only appear in familiar environments, i.e. when talking to other Pirahãs (see also Everett and Ladefoged (1996); or she doesn't speak the language at all). As I say in Everett (1985, 252ff):

"The circumstances surrounding the usage of the segments [β] and [ɣ] may be diagrammed in the following fashion:



There are thus five basic options as to the type of socio-dialogic situation involved, each of which is relevant to the production of the vibrants. When a female is speaking, she may use freely either the superstrate sound [g] or [b] or the substrate sounds [β] and [ɣ]. If a male is speaking in a familiar environment (i.e. to those accepted as social intimates) he may use either superstrate or substrate sounds. But when speaking to someone not considered a social intimate, he will use only the superstrate sounds. This is very important for identifying degrees of acceptance among the Pirahãs and it is unfortunate that Salles omits the very telling likelihood that this is why she does not hear these sounds - she is not part of the familiar (linguistically fluent in particular) environment.

Moving on to her (p963) section 2.3., Salles's discussion of Pirahã phonotactics is either wrong or she has a new, unpublished analysis that she fails to discuss here. In any case, she simply ignores a large body of work - Everett (1979), Everett and Everett (1984), Everett (1988), Keren Everett/Madora (1998), Gordon (2005), among others which directly contradict her findings on Pirahã phonotactics:

"Pirahã syllables have a (C)V(V)(G) shape, where G is a glide (/j,w/).¹² This representation also accounts for long vowels (VV), which are bimoraic (D. Everett 1979). Examples of each syllabic pattern are given in (8).

- (8) a. VV.CV
[aa.gɪ]
'coati'⁷

⁷ Here Salles violates her own glottal insertion rule. By her own rule there should be a [ʔ] in [aagi].

Apart from glides, no other consonant can function as a coda, except the possible exception of the animal classifier *ʔ.s*, which is pronounced as [ʔɪ] when preceding a consonant-initial word and [ʔɪs] when followed by a vowel-initial word. There are no complex onsets, and the sequence */ki/ is not attested (see also Heinrichs 1964; D. Everett 1979; K. Everett 1998)."

But this is *not* the syllable structure of Pirahã. Everett (1979, 35-48) offers an in-depth analysis of syllable structure in Pirahã, based on Autosegmental Phonology (Goldsmith (1976), as well as the model of Chomsky and Halle (1968) and many others cited there. Everett and Everett (1984) offer a fuller account of syllable structure and stress placement in Pirahã, where they claim (p706), that the basic syllable structures and weights of Pirahã are: CVV > GVV > VV > CV > GV. In that article they claim that C = voiceless consonant; G = voiced consonant; and V = vowel, where a voiceless consonant, C, is longer than a voiced consonant, G, and a V, vowel, is longer than both. So the hierarchy of syllable weight follows. K. Everett (1998), under the orientation of Peter Ladefoged provides a detailed phonetic analysis of these facts.⁸ Moreover, D. Everett (1988) provides an in-depth discussion and analysis of metrical constituent structure in Pirahã more generally. Both K. Everett (from a phonetic perspective) and D. Everett (from phonology) argue at length that there are *no* phonological glides in Pirahã. That is, Salles offers us an analysis that is at variance with detailed previous analyses by Ladefoged, K. Everett, and D. Everett. But she does this, once again, without referring to any of these analyses or the crucial facts of those analyses. The phonotactics section of her paper seems to be little more than her raw impressions, uninformed by the phonetics or phonological literature more generally as well as the extensive literature on the phonology of Pirahã more specifically.

When she moves on to verbal structure, the same problems reoccur. So, for example, Salles states (p964) that: "The language tends towards an agglutinative structure, and it prefers suffixes. Tense, aspect, mood, and evidentiality (TAME) marking is realized through verbal suffixes, while arguments are realized as independent words and not marked on the verb, as in (10).

- | | | | | |
|------|-----------------------|--------|--------|------|
| (10) | ti | kapí | kaaipí | -haí |
| | 1.pro | coffee | make | -fut |
| | 'I will make coffee.' | | | |

However, in her glosses she confuses free translations with the more nuanced meaning of the suffix, **-haí**, completely disregarding Everett (1986) in which I argue that **-haí** is not a future marker, but part of the evidential system, indicating 'relative certainty'.⁹ It is entirely possible (as for anyone) that I am wrong. But I give arguments for my conclusions and she gives none nor does she respond to any of mine. In fact from this point on, on the syntax, Salles begins a long series of errors in which she appears to confuse the free Portuguese translation with the Pirahã meaning (unfortunately, a common error of both seasoned and novice researchers).¹⁰ Sheldon and Heinrichs might have called **-haí** a future tense in Pirahã, but it is not. And this is quite easy to find out if one is asking the right questions. For example:

⁸ Ladefoged accompanied K. Everett and me to the Pirahãs in 1995, where he personally verified all the acoustic correlates discussed by K. Everett, as well as the stress-placement and syllable weight analysis of Everett and Everett (1984) and Everett (1988).

⁹ This is a manifestation of the "translation fallacy," as is discussed below.

¹⁰ In other words, if someone gives you a construction in Piraha and says that this can paraphrase an utterance in Portuguese, that does not mean that the relevant constructions have the same structures. For example, a middle construction in Portuguese might be translated as a passive, etc. in English but that doesn't make it a passive in Portuguese.

Hi soʔóá kah - á -pi- **haí** ʔahoahíai. Hiagía hi kahápi - ta- hiaba piʔái
He already go - relative certainty another day. Thus he go-repetitive-not now.
"I believe he already went yesterday. Therefore, he is not going now."

The suffix **-hai**, is not a future marker or tense of any kind, as predicted by Everett (1986), but not by Salles's analysis. One could provide more details of Salles's ignorance or disregard of previous analyses, but that would produce diminishing intellectual returns.

On pages 971ff, Salles's account of numerals is self-contradictory: "Quantities are expressed by three *numerals* (emphasis mine, DLE): hói (híio) 'one', hoí (hio) paucal, and báagiso 'many'. These words are used for approximation; that is, they refer to an approximate amount and not an exact quantification, which is a cross-linguistically rare phenomenon (see P. Gordon 2004 and C. Everett & Madora 2012 for experimental work on Pirahã numerosity)."

But, as many works establish, work that she fails to acknowledge or make use of, these are *not* numerals (and the material she adds in ()s in the examples is the same underlying indexical phrase in both cases "he there" **hi** - **íio**, not the two different forms she lists, for which she has confused simple phonetic variation). She might have had a better grasp of these facts had she read more carefully.

For example, in a very well-known article on Pirahã numeric cognition (Frank, et. al.2008) that she *fails* to cite it is claimed that: "We show that the Pirahã have no linguistic method whatsoever for expressing exact quantity, not even "one."¹¹

That is, her decision to refer to these as "approximate numerals" misses the crucial fact that while the words indicate proximate *quantities*, they are not numerals (they do not denote numbers - counts of things). Her analysis exactly misses the fact that this is the only language known to lack numbers entirely and the important conclusion cited from Frank, et. al. (2008) that numbers are a cultural invention - perhaps the most important result of *all* the research into Pirahã numerical cognition. Once again, she fails to cite other references that could elucidate what she is trying to describe or that disagree with her description. Of course it is fine and healthy for her to disagree with those studies, but she needs to say why she disagrees. Simply failing to cite them diminishes the quality of her work.

In her discussion of mood and aspect, p977ff, she gives several glosses that deviate from those given in previous work. But she offers no explanation of what tests or data she used to warrant these changes. And because TAME morphology requires access to the semantics of the language (i.e. speaking the language in a monolingual situation), it is especially important for someone to either identify their level of fluency in the language or to offer some acknowledgement of and explanation for their deviation from any previous analysis of such subtle semantic facts. TAME analyses require nuanced and subtle understanding of the language in question, or access to bilingual speakers (but there are none in Pirahã. See Sakel (2012)).

When she moves to the discussion of relative and correlative clauses in Pirahã, she takes her data largely from Everett (1986). But she omits all subsequent discussion by Everett of these structures. Now the primary difference between Everett (1986) and later works (see the long list of works arguing against any form of embedding or recursion in Pirahã listed here:

¹¹ The authors go on to say that "Despite this lack, when retested on the matching tasks used by Gordon, Pirahã speakers were able to perform exact matches with large numbers of objects perfectly but, as previously reported, they were inaccurate on matching tasks involving memory. These results suggest that language for exact number is a cultural invention rather than a linguistic universal, and that number words do not change our underlying representations of number but instead are a cognitive technology for keeping track of the cardinality of large sets across time, space, and changes in modality."

<https://daneverettbooks.com/collection-of-most-of-dans-papers-on-recursion/>) is that the later works argue in considerable detail for conclusions incompatible with both Everett (1986) and Salles (2023). But she just ignores these works. For example (Salles (2023, 987ff)):

"At least two complementation strategies have been identified in Pirahã: nonfinite and nominalized complements, the latter of which is a common strategy for complementation in Amazonian languages. For instance, non-finite clauses occur as complements to desiderative predicates with the verb **ʔogai** 'want', as in (123). The verb of the complement clause can also be nominalized, as in (124). Notice that the complement clause may precede the matrix verb or be postposed to it, as in (123) and (124), respectively.

(123) ti pi itaip. ʔog-abagai

1.pro water drink want-frus

'I'd like to drink water.'

(124) ti ʔog-abagai pi itai-pi-sai

1.pro want-desid water drink-nmlz

'I'd like to drink water.'

Purposive clauses may also use either a non-finite complement, as in (125), or a nominalized complement, illustrated in (126). In (125) the non-finite complements may also be postposed to the matrix verb.

(125) hi bagia-.ʔio hi ʔao ʔagaoa kobai

3.pro come-decl-ingr 3.pro poss canoe see

'He is coming to show his canoe.'

(adapted from D. Everett 1986: 265)

(126) ti ʔi ʔiga-.ti ʔagaoa kaiti-sai

1.pro 3inan.pro take-imp canoe bore-nmlz

'I must take it to bore the canoes.'

(adapted from D. Everett 1986: 265)

After close examination of all of the relevant Piraha data, contrary to Salles, Futrell, et. al. (2016, 19) conclude that: "Our analysis has failed to find strong support for syntactically embedded structures in Pirahã. We emphasize that any conclusions that can be drawn from this corpus evidence must be highly tentative, due to the difficulty of working with a language whose speakers are so difficult to access, as well as the computational challenges of characterizing linguistic complexity. Our hope is that the analysis presented here, along 756 with the release of the annotated corpus, will promote further investigation into the formal properties of natural languages and help to push the debate towards testable empirical claims."¹²

Moreover, in an earlier study of recursion in Pirahã, Sakel and Stapert (2010, p15) conclude that: "In conclusion, the Pirahã structures we have looked at in this paper have shown no evidence of being syntactically recursive. Instead, Pirahã appears to make use of juxtaposition and morphological complexity to express complex concepts. Our conclusion is hence very similar to Everett's analysis (2005)." ¹³

¹² Notice that if there were recursion in Pirahã, multiple embeddings should be possible. No one in the long debate on Pirahã has ever shown any evidence for multiple embeddings. The debate has centered on whether it has even a single level of embedding. The works linked to earlier in this paper argue at length that the language has no embedding whatsoever.

¹³ And they go on to say that: "We have discussed a number of constructions in which even syntactically complex languages prefer non-recursive structures to recursive ones. It is possible that what other languages have as an option is the default in Pirahã. Further support comes from the fact that Pirahã is an exclusively oral language.

Where Salles's analysis suffers most from her cherry-picking of previous work to cite, however, begins on her section 7, pp986ff: "Clause linkage, information structure, and discourse." So consider her examples (120) and (121), (p986):

- "(120) [ipóihí [tábo ʔapó ʔabaipi sigiái-hi]] ti ibais-ág- áhá
 woman bench on sit same-int 1.pro wife-cop-decl
 'The woman (who) is the same sitting on the bench is my wife.'
 (121) ti baosá pisí ʔog-abagai [giai go -ó baosápisí bigaobá ʔai sigiái]
 1.pro hammock want-frus 2.pro int-loc hammock show be same
 'I wanted the hammock which is the same you showed me.'
 (adapted from D. Everett 1986: 276)"

Although she acknowledges that at least the second example is taken from my work, she presents these as examples of embedding. That is not a terrible first guess. Again, Everett (1986), which she cites, analyses them as she does. However, in much subsequent work (producing one of the most acerbic controversies in linguistic history that has now continued for nearly 20 years (see Pullum (2024)), I have argued in detail (in many papers and books, see above), against analysing such examples as embedding. But Salles cheerfully ignores those arguments. Moreover, in her example (120) she fails to relate the second sentence to the concept of clarification that she mentioned earlier and which it illustrates, leading to her misanalysis or unsupported analysis of recursion.

Consider her sentence (120) in more detail. In this sentence, **ti ibais-ág- áhá**, literally means "I certainly have a partner." It is a separate, clarifying assertion. In fact, the preceding sentence she gives (minus her brackets and with my analysis) is:

ʔipóihí tábo ʔapó ʔabaipi sigiái -hi
 womanboard on top of sit same -INT.

"(The) woman sits on top of (the) board the same one [an example of clarification/afterthought as originally described by Everett (1983), DLE]."

Ti ibais ág -á -há.
 I partner have/is - remote -complete certainty
 "My partner is for sure."

This is not syntactic subordination in my analysis but semantic subordination (as described in many many works, though see Everett and Gibson (2020) for more argumentation). Further, in my analysis, the material **sigiái-hi** 'the same one' in particular fails to show embedding, but rather illustrates appositional clarification. As I have argued in many publications (see above) and as McCawley (1982) argued many years ago, appositional material of this type in any language should in general not be analysed as determinative of the sentence structure in which it is found. She should say why she doesn't discuss my earlier analysis here, especially, if she is going to (in)directly contradict it. She also says (p990) that the postverbal material can indicate "afterthought:"...For instance, a rightward topicalization that D. Everett (1986) describes as a clarification topic could be more precisely described as an afterthought..."¹⁴ It is not clear that this (afterthought vs. clarification) is a difference with a difference, but the

Spoken language and predictable content are exactly the instances in which non-recursive structures are preferred in other languages such as English. Hence, there is no apparent functional need for recursion in Pirahã syntax."

¹⁴ This is all discussed at length in Everett (1983, 1986). Desmond Derbyshire (a member of my 1983 dissertation committee) in fact asked me about these very constructions at my thesis defense.

"afterthought" here in fact clarifies what the speaker has said. Indeed, this is a common type of structure in Amazonian languages, which Salles fails to note.¹⁵

Now let us consider her examples (123) and (124), p987, which purport to show embedding:

- "(123) ti pi itaipi ʔog -abagai
 1.pro water drink want -frus
 'I'd like to drink water.'
 (124) ti ʔog -abagaipi itaipi -sai
 1.pro want -desid water drink-nmlz
 'I'd like to drink water'

There are several problems, once again compounded by Salles's failure to consult or otherwise interact with the abundant literature on this language. First, she ignores pro-drop in Pirahã, though this is described in Everett (1984) (to put it in the terms of one sector of formal linguistics), failing to indicate the understood subjects of the clauses to the right of the main clause. Second, ignoring the work of Everett and Oliveira (2010) as well as Sakel and Stapert (2010), she glosses **-sai** as "nominalizer" rather than "old information." That once was a plausible analysis, before all the research cited here was conducted. This was, after all, my own analysis for many years, from Everett (1983) until Everett (2005). However it is incorrect, as the sources just cited make clear (Sakel and Stapert (2010, p2).¹⁶ At the least a counter analysis must be argued for not merely given by ignoring previous work.

¹⁵ Everett (1983, p53) cites for example:

(40) paigí hi ob -áaxái paigí
 nome próprio 3 ver-muito nome próprio
 "Paigí sabe muito, Paigí"

Uma explicação possível para exemplos desse tipo é que a segunda ocorrência de paigí é apenas uma sentença ou sintagma elíptica, ligada parataticamente a sentença matriz. Segundo D. Derbyshire (comunicação pessoal) esta estrutura seria comum nas línguas amazônicas sendo o elemento nominal à direita um tipo de esclarecimento e não o tópico."

¹⁶ To cite at length from Sakel and Stapert (2010, p2ff): "2. The suffix **-sai**

The suffix **-sai** figures prominently in recent discussions of recursion. Everett (1986: 277) classified it as a nominaliser and an obligatory indicator of embedding in conditional clauses (1986: 264). In his later approach (2005: 21), however, he argues that **-sai** does not mark syntactic subordination. We have studied this marker's functions, in particular with respect to whether it is an obligatory marker of embedding. We will here look at two very different constructions in which **-sai** is used.

The first part of our discussion is based on findings by Stapert (2007) and Stapert et al. (in preparation) [I was not able to find this paper, DLE]. They tested the functions of **-sai** experimentally in a sentence repetition task. In this experiment, two clauses representing semantically connected propositions, such as it is raining and I don't go to the forest were combined.

The suffix **-sai** was added to either the verb of the first or that of the second clause, cf. (1a) and (1b) and the informants were asked to merely repeat the sentence.

(1) a. Piiboi-bai-sai ti kahápi-hiaba.
 rain-INTENS-SAI 1 go-NEG
 'If it is raining I won't go.'
 b. Piiboi-bai ti kahápi-hiabi-sai.
 rain-INTERS 1 go-NEG-SAI
 'If it is raining I won't go.'

The work cited in the link earlier is important, precise, experimental and contradicts both my earlier analysis and Salles's current analysis. Her failure to interact with this is another mystery. But there is no doubt any longer that **-sai** is *not* a nominalizer, nor a subordinator, contra her glosses. And therefore, as pointed out by Sakel and Stapert in the quote just given it cannot clearly support the subordination analysis that Salles presupposes without argumentation. Everett and Oliveira, Jr. (2010) further show that **-sai** does not fit the analysis suggested by Sauerland (2018, pp1-24). It provides no evidence for subordination. So why Salles cites Sauerland in this section without mentioning the works just cited, which - each independently - refute Sauerland's claims is yet another enigma.

The examples from her work just discussed again fail to discuss "pro-drop" of both objects and subjects in Pirahã why, based on discourse analysis, it is common to omit overt subjects in contiguous sentences when there is no change of subject. This is a discourse phenomenon and - I claim - has nothing to do with embedding, though I did analyze it this way before I could understand their texts, an analysis Salles seems to have stuck with, ignoring the intervening literature. Again, it would be fine for her to disagree with any and all conclusions of the previous literature. But she must say why. She doesn't do that here. Instead, as she usually does, she considers a novel analysis as though there were no previous analysis and fails to say what her chosen analysis is preferable to the previous analysis.

In her (125) and (126), p987, she continues this line of exposition:

"Purposive clauses may also use either a non-finite complement, as in (125), or a nominalized complement, illustrated in (126). In (125) the non-finite complements may also be postposed to the matrix verb.

(125) hi bagia -áo ?io hi ?ao ?agaoa kobai
 3.pro come -decl -ingr 3.pro poss canoe see
 'He is coming to show his canoe.'

(adapted from D. Everett 1986: 265)

(126) ti ?i ?iga áti ?agaoa kaiti -sai
 1.pro 3inan.pro take -imp canoe bore -nmlz
 'I must take it to bore the canoes.'

(adapted from D. Everett 1986: 265)

However, contra Salles, I analyze these as two-sentence clusters and, according to my work, they should have been represented as:

(125') hi bagia -áo ?io. Hi ?ao ?agaoa kobai
 3.pro come -decl -ingr. 3.pro poss canoe see

A total of nine speakers of Pirahã – 7 women and 2 men – participated in this language task. In their response, informants attached -sai to the first clause, the second clause, both clauses, or neither of the clauses (cf. 2) independent of the input and with no reported change in meaning or judgement of (un)grammaticality.

(2) Piiboi-bai ti kahápi-hiaba.
 Rain-INTENS 1 go-NEG
 'If it is raining I won't go.'

Out of a total of 39 relevant responses -sai was attached to both clauses in 9 cases, to none in 6, and to one clause – either the rain part or the forest part – in 24 instances. The alternative in which -sai does not occur in either clause (2) was not part of the input of the experiment. Hence a simple repetition of an ungrammatical sentence from the input is ruled out. This means that the concept can be expressed without the presence of -sai; thus this marker can not be an obligatory marker of embedding. Still unclear, however, is the exact function of -sai in these constructions, but it does not appear to be a marker of subordination, as originally claimed by Everett (1986)."

'He is coming. He will see the canoe.'
 (126') ti ?i ?iga áti. ?agaoa kaiti -sai
 1.pro 3inan.pro take -certainty canoe bore -old information
 "I it will take. (I will) bore it."

Since **-sai** is not a nominalizer, there are *no* non-finite clauses in Pirahã. My analysis in (125') and (126') might, again, be incorrect. But it better fits the analyses of Everett and Oliveira (2010) and Sakel and Stapert (2010). At the very least, this alternative analysis of the structures, crucial given the controversy surrounding embedding in the language should have - both ethically and for better scholarship - have been cited and the authors' divergences addressed by Salles (as I am in fact doing here with regard to Salles's analysis).

Moreover, Salles ignores all of Everett (1986)'s extensive discussion of verb structure, though she cites that source. I suspect that this is because, again, she does not speak the language fluently/well and verb structure requires a more than beginning command of semantics, as well as an ability to discuss the examples in detail with native speakers in their language. Anyone working on Pirahã must acknowledge that no Pirahãs (none of them, not even the so-called "gatekeepers") can carry on a conversation in Portuguese, just as she cannot carry on a conversation in Pirahã.¹⁷

Following her example (128), p988, Salles discusses Sauerland (2018) without mentioning the severe shortcomings and incoherence of crucial parts of his experimental design, as pointed out in Everett and Gibson (2019): So she claims (p988): "Reported speech also uses nominalization, but of the matrix verb; the verb **gái** 'say' appears in possessive constructions to introduce a direct quote, shown in (128).

(128) h. gái-sai t. pio.hai ?ogái
 1.pro say-nmlz 1.pro soda want
 'His saying is: "I want soda".'

She continues (ibid): "Sauerland (2019 [sic, 2018]) presents evidence that the direct quote acts as a complement clause in such constructions. He conducted a comprehension study where consultants heard a speaker producing a false statement followed by another speaker reporting this false statement in a structure such as (128). When asked for felicity judgments, consultants often correctly classified the first speaker's statement as false but the report as true. A coordinated or paratactic construction would not render the reported statement as true, so Sauerland concludes that there is embedding; that is, what is true is that the first speaker said the sentence, not the sentence itself. Further evidence that this is an embedded construction comes from the fact that they can stack, as in (129), where the speech of a third speaker is embedded under the speech of a second one.

(129) Maaga hi gái-sai Hiahoai hi gái-sai t. pio.hai ?ogái
 Maaga 3.pro say-nmlz Hiahoai 3.pro say-nmlz 1.pro soda want
 'Maaga says that Hiahoai says "I want soda". (Lit. 'Maaga's saying is:
 "Hiahoai's saying is: 'I want soda'".)'

Ignoring the phonological, morphosyntactic, and phonetic errors in Sauerland's data, these are not examples of embedding. At least they provide no experimental evidence at all for syntactic embedding (though, again, semantic subordination does occur - see the works linked earlier). This was shown by Everett and Gibson (2019, 783) in their review of Amaral, et. al.

¹⁷ It is possible that since my last visit to the Pirahãs in 2009 (see Pullum (2024) on why that was my last visit), that this has changed, although Keren Madora says it has not (and she has lived with the Pirahãs almost continuously since 1980).

(2018) which evaluates Sauerland's experimental claims very negatively (ignored by Salles). I cite at length here because Salles has ignored this discussion entirely: "There are several problems with the research reported in this chapter. Most importantly, Sauerland confuses a potential embedded interpretation with a need for syntactic embedding to obtain that interpretation. In particular, there is no reason to assume that interpreting 2 as 'Toe said "I have been to the stars"' requires any syntactic recursion. As many others have noted in the discussion of recursion (including many authors in this very volume), sets of nonembedded syntactic materials can easily give rise to an embedded semantic interpretation, especially if such an interpretation is contextually supported. For example, in Ch. 2, Bart Hollebrandse makes exactly this point about English examples like 5 (ex. 7a, p. 37). (5) Malcolm is guilty. The jury thinks that. The judge knows that. An available interpretation of 5 is that the judge knows that the jury knows that Malcolm is guilty, in spite of the fact that there is no syntactic embedding in this example.

Similarly for 2, given a context in which someone has just said 'I have been to the stars', if a second speaker says 'Speaker 1 said something. I have been to the stars', most listeners will agree that the meaning of this in the context is that speaker 1 said that he has been to the stars, even though there was no syntactic embedding in the original statement.

Indeed, this alternative possibility to Sauerland's assumed reading is testable, so we tested it. We ran the relevant control experiment in English, with twenty participants from Amazon's Mechanical Turk. In creating our experimental items (given in the appendix to this review), we started with the written versions of all ten of Sauerland's items (as presented in the appendix in his paper), and we used the instructions that Sauerland provided ('Did speaker B hear well?'). Example target and control items are given in 6 and 7.

(6) Example target item

John: 'I have been to the stars.'

Bill: John said something. I have been to the stars.

(7) Example control item

John: 'I have been to the moon.'

Bill: John said something. I have been to the sun.

Note that there is no syntactic embedding in the written form of what Bill says in each discourse: there is no quotation or embedded sentence. The embedded meaning would have to be inferred, because it is not present in the syntax. Our English participants agreed with the target sentence on 99% of the trials, demonstrating that they obtained the embedded interpretation in spite of the lack of embedded syntax. Furthermore, they disagreed with the control (as desired) on 98% of the trials. All materials and results are available at <https://osf.io/z86k2/>.

It seems difficult to reach any other conclusion but that Salles's chapter on Pirahã, is the result of faulty, hasty research.¹⁸ There is much more that could be said about Salles's analysis,

¹⁸ There are those who have done more than merely criticize my analyses of Pirahã based on descriptions like Salles's. They have made many personal accusations against my relationship with the Pirahãs. So it was refreshing recently to see a Brazilian master's thesis (Felizes (2023), which stated the following:

"A relação de Daniel e Karen Everett com os Pirahã é algo que perdura até aos dias atuais. Durante mais de quarenta anos de convívio – permanente ou esporádico – conquistaram a reputação de grandes amigos, de saberem bem a língua, de serem exímios contadores de histórias e de se tornarem importantes aliados, a quem os Pirahã geralmente recorrem para resolver potenciais conflitos ou aprender coisas sobre o mundo dos brancos."

["Daniel and Keren Everett's relationship with the Pirahã is something that has endured to the present day. During more than forty years of coexistence — permanent or sporadic — they gained the reputation of being

but the above should suffice to show how, frankly, weak the work is. Her efforts to show that Pirahã has recursive structures relies on poor (or no) argumentation, failure to cite *any* of the voluminous research that disagrees with her conclusions, and making semantic judgements without semantics knowledge of the language. Therefore, I see no reason to believe that Salles's data have any bearing on previous work on Pirahã. They are misleading parentheses in the study of the language.

Before concluding, I will consider here one other potential counterproposal on Pirahã recursion, this one having to do with quantification (also see my remarks on quantification in Everett (in progress) and in my earlier LingBuzz paper: <https://ling.auf.net/lingbuzz/007670>). Imagine that a person committed to quantification as a syntactic category, rather than as a semiotic concept as discussed above, might take a different tack, arguing that, contra all of my recent work, Pirahã does in fact have quantification. For example, one might take evidence from the very title of my book (if one assumes it is a literal translation from Pirahã), *Don't Sleep. There Are Snakes*.¹⁹ Doesn't the phrase "there are snakes" presuppose existential quantification? The answer is "not linguistically." This is just a fact about human inferential ability (see Everett (2023b)). Claiming this would be to confuse quantification with genericity. As discussed above, nowhere do I deny the existence of generalizations or generics in Pirahã. But generics are not quantification, as we have seen. A more syntactic suggestion might run along the following lines. Suppose, for example, that we construct the following Pirahã sentence (a linguistic experiment):

hi xogiágaó gáí-sai [pro hi káixihí xoab-á-há

he bigness say - old information. He paca kill -remote - complete certainty

This would translate literally into Pirahã as "The bulk (of the people) spoke. He killed the paca."

In English a freer, more natural (but inaccurate literally) translation would be

"They all said they killed a paca."

No complex or more abstract (e.g. with phonologically null categories) is needed. But suppose one wanted to defend a more abstract analysis, e.g.:

[pro hi xogiágaó gáí-sai [pro hi káixihí xoab-á-há]]

3.AGR all say-old.inf **3.AGR** paca kill-remote-compl.cert.

"They all said (or so I hear) they definitely killed a paca."

According to this analysis if the **pro**, like a quantifier, binds the pronominal subject of "kill," then **pro** must c-command a pronominal variable. Therefore, if this is the case, the phrase's sister clause must contain the pronominal variable, a term in an hierarchically structured embedded phrase. Such an analysis might lead us to conclude that Pirahã indeed *does* contain

great friends, of knowing the language well, of being excellent storytellers and of becoming important allies, to whom the Pirahã often turn to resolve potential conflicts or learn things about the white world."]

¹⁹ This in fact is an idiomatic construction in Pirahã for which nouns other than "snakes" may be substituted:

?áitabísaha?ái baóhoipaií/tigaiti

sleep-remain-prohibitive jaguar/snake

"Sleep not! Jaguar/Snake."

The meaning is not guided so much by the syntax as by inference. Just about anything could follow "sleep not." In fact, the original title of my book was not to be *Don't sleep, there are snakes*, but *Don't sleep, there are jaguars*. That was nixed by my publishers who thought people would think of Jaguar automobiles.

recursive structures and quantifiers, generated by a recursive procedure. A perfectly natural result if this turns out to be correct.

But this is incorrect. This analysis assumes that there is only one analysis, a Minimalist analysis, possible for analyzing such sentences. Yet, as I have shown in many papers (once again a partial list is given here - <https://daneverettbooks.com/collection-of-most-of-dans-papers-on-recursion/> for a list of my papers on recursion), there are two sentences here. "The bulk of them spoke. (They) killed a paca." The understood subject of the second sentence can refer to "the bulk of them" or not. It is not a bound variable. The first task, therefore, of the hypothetical analysis of syntactic quantification in Pirahã would be to show that what I have represented as bisententiality is unisententiality. I won't rehash in detail all the arguments here for the conclusion that the examples in this footnote represent two sentences (given in the papers linked to just above), but consider the fact that if the order changes or if there is an intervening clause nothing changes:

Hi káixihí xoab-á-há. Hi ?itii?isi xoab-á-hiaba-há. Hi xogiágaó gáí-sai.

He paca kill -remote - complete certainty. He fish kill not remote complete certainty. He bigness say - old information.

"He (singular or plural) killed a paca. He did not kill a fish. The bulk of the men say this."

No intrasentential binding is here. These are three separate sentences (at the very least, the final clause is not part of the first clause). So this counterfactual line of reasoning is irrelevant to the analysis of Piraha quantification and recursion.

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