Chapter 2

Daniel Everett on Pirahã syntax

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Daniel Everett's generalizations about the syntax of the Brazilian indigenous language Pirahã in 2005 provoked not just a linguistic dispute but also an international campaign of vilification and abuse against him. Yet many other languages have been claimed to have the properties he attributes to Pirahã (basically, absence of devices like hypotaxis and clausal coordination). Nevins, Pesetsky, and Rodrigues attempt to represent Everett as having dishonestly concealed earlier evidence of hypotaxis in the language. They are not successful. Later attempts by others to exhibit self-embedding in Pirahã syntax fare even worse. The issue has little general importance for linguistics, since nothing important about language or humanity hangs on whether an upper bound on sentence length exists. In pursuing the matter, Everett's accusers have done him a gross injustice.

1 Everett's dangerous idea

The war on Daniel Everett's reputation and research began soon after the fall of 2005, when he he gave a two-part language tutorial session on the Brazilian indigenous language Pirahã at the annual meeting of the Linguistics Association of Great Britain in Cambridge, England (September 1–2), and published an article entitled "Cultural constraints on grammar and cognition in Pirahã" in the August-October issue of *Current Anthropology* (*CA*). The publisher of *CA*, the University of Chicago Press, put out a news release about the article which led to some newspaper stories. The surprising result was that in the following years Everett was subjected to bitter attacks impugning not just his work but his integrity and character. The attacks emerged first within the linguistics community, but have come to the attention of a much wider public, particularly among admirers of Noam Chomsky.

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The Pirahã are an uncompromisingly independent tribe of indigenous Amazonian people living a subsistence-level low-technology lifestyle on the banks of the Maici river in Amazonas state. They hardly interact with mainstream Brazilian society at all, and show no interest in reading, writing, counting, history, politics, or religion.

Their language appears unrelated to any other now spoken, and they have remained resolutely monolingual in it for at least 200 years, despite occasional contacts with other indigenous people, and acquaintance with three generations of American missionaries, and sporadic and superficial contacts with mainstream Brazilian river traders. A very small number of Pirahã men have a smattering of Portuguese and can act as interlocutors for Pirahã villages that come into occasional contact with Portuguese-speaking Brazilian river traders. Sakel (2012) calls them "gatekeepers", and provides some interesting data on their very rudimentary Portuguese (she also notes some use of a local pidgin based on the Tupian language Nheengatu). But the women speak only Pirahã, and the gatekeepers basically shelter the vast majority of the Pirahã community (including most of the men) from needing even a minimal competence in Portuguese.

The Pirahã language is linguistically unusual in several ways, from its tiny phonemic system and unusual phonology to its complete absence of numerals and pure color terms. But although Everett's statements on these points raised some linguists' eyebrows,[1] they did not provoke anger. What did, and what motivated the surprising events described in Section 2 below, was sentence structure. This might seem an unlikely trigger for angry diatribes and libelous allegations (at least for anyone who did not know the history of generative syntax chronicled in Harris 2021).

It is highly relevant that all production of Pirahã is oral: though an orthography has been devised, no member of the community has shown any interest in learning to read or write. And oral discourse in the language shows no signs of such familiar syntactic phenomena or devices that writers use in constructing long sentences. Everett reports that there are no signs of no multiple coordination (*It takes* [*skill, nerve, initiative, and courage*]), complex determiners ([[[*my*] *son's*] *wife's*] *family*), stacked modifiers (*a* [*nice,* [*cosy,* [*inexpensive* [*little cottage*]]]]), or – most significant of all – reiterable clause embedding (*I thought* [*you already knew* [*that she was here*]]). These are the primary constructions that in English permit sentences of any arbitrary finite length to be constructed, yielding the

[1]See Dobrin & Schwartz 2021 for an interesting discussion of the ways in which knowledge is based in fieldwork, and how differing assumptions about things like how to devise glosses contributed to the conflict between Everett and his critics on the quantifier issue.

familiar argument that the set of all definable grammatical sentences in English is infinite.[2]

Linguists versed in syntactic typology were not the ones who expressed shock at the syntactic facts: similar claims had long been made about other languages, sparking no particular controversy. The anthropologist Brent Berlin, commenting on the *CA* paper (p. 635, one of eight invited responses published with the article) expresses no surprise about the absence of subordination, and quotes a remark by Foley (1986: 177) about the Papuan language Iatmul, where "Linking of clauses is at the same structural level rather than as part within whole.".

The late Kenneth Hale (1934–2001), a long-time MIT faculty member, argued as early as the mid 1970s that the Australian language Warlpiri could not even be said to have phrase structure, which would necessarily entail it did not have syntactically subordinate clauses. Hale's work, together with that of R. M. W. Dixon, founded a rich subdiscipline of work on Australian languages, particularly the Pama-Nyungan family. The literature is too large for a proper survey here, but suffice it to say that examination of the example sentences presented in works on Pama-Nyungan languages such as Hale (1976), Nash (1980), Dixon (1981), Austin & Bresnan (1996), and Pensalfini (2004), one finds no sign of any embedded complement clauses. Sentences seem to consist solely of word-level constituents, word order often being astonishingly free. There are signs of what might be non-finite secondary predications at main clause margins which could perhaps be called "functionally dependent" but "structurally unembedded" as Austin & Bresnan suggest (1996: 228, esp. n. 13), but there is none of the clause subordination familiar from English and other languages of the sort Benjamin Lee Whorf called "Standard Average European".

The relevant literature goes far beyond the work on Pama-Nyungan. More than four decades ago the syntactic typology specialist Talmy Givón (1979: 298) wrote in very general terms about languages of "preindustrial, illiterate societies with relatively small, homogeneous social units" in which "subordination does not really exist". Kalmár (1985: esp. pp. 157–159), citing Givón, elaborates further, giving several earlier references and raising the interesting possibility that Canadian Inuktitut is in the process of developing subordinate clauses for the first time in writing on serious subjects.

Mithun (1984) studies the noticeable avoidance of subordination in highly agglutinative languages employing polysynthesis in their verb structures. She fo-

[2] The soundness of the argument even for English can be questioned: Pullum & Scholz (2010: 115–124) argue that the claim of an actually infinite number of sentences cannot be sustained. But we can set that theoretical point aside here, concentrating on more concrete matters like whether the language permits embedding of clauses within clauses.

cuses on Gunwinggu (= Kunwinjku, citing 1951 and 1964 sources), Kathlamet (from a 1911 source), and Mohawk (from her own contemporary informant work), and observes that they all resist resorting to subordination, some almost completely. Evans & Levinson (2009: Section 6) take the view that quite generally in Bininj Kun-wok (of which Kunwinjku can be regarded as a dialect variant) there is no clausal embedding, and morphological embedding is possible only to one degree. They also note (p. 442) that Kayardild (another Pama-Nyungan language) allow subordination, "but caps it at one level of nesting": the subordination cannot be employed to put clauses inside clauses inside clauses and thus make sentences arbitrarily long.

Mithun offers an interesting conjecture about why even one-level subordination is avoided in such languages: in oral-only languages it should perhaps not be seen as implying any shortcoming or lack on their part, but rather an indication that once languages are written, the necessarily slower composition and reception of the written form leads to the development of new syntactic tools "to compensate for the loss of mechanisms inherent in skillful oratory" such as intonational phrasing (p. 509).

Many other instances could be cited of linguists commenting long before 2005 on languages in which arbitrary sentence extensibility seems not to be possible. And not just languages of hunter-gatherer cultures but also languages of early antiquity in Europe and Asia: comments about the lack of true hypotaxis can be found in literature on early Akkadian, Old Chinese, Homeric Greek, and Proto-Uralic.

The late Wayne O'Neil (1931–2020), an MIT faculty member like Hale, published a paper in 1977 arguing that early Old English also showed no signs of clause embedding. Writers would just tack an additional clauses on the end of a main clause, very loosely attached (very much as in Pama-Nyungan). Once Old English speakers were able "to take advantage of the leisure for the composition and decomposition of sentences that being able to read and write afforded them", O'Neil says, "they took advantage of it in the simplest possible way … by simply adjoining sentences to sentences, sometimes without even deleting the shared nominal" (O'Neil 1977: 210). The implication is that before Old English was written, subordination was basically absent from the language.

The claims referenced in the last half-dozen paragraphs may or may not be correct in their detailed analytical claims; I am not trying to evaluate them here. My point is merely that they provide descriptions of languages in which it looks as if it would not be possible to construct sentences of arbitrary length, and they have been sitting uncontroversially on library shelves for decades. It is peculiar

that things changed so dramatically in 2005, and that the reaction was so extreme, given that Everett was merely making a point about Pirahã that had been repeatedly made before about other languages.

What had changed? The answer is that a paper co-authored by Marc Hauser, Noam Chomsky, and W. Tecumseh Fitch had been published in the prestigious general scientific journal *Science*: Hauser et al. (2002), henceforth HCF. The paper contains a lot of evolutionary biology and zoology, and it is reasonable to assume that the first-named author did most of the writing. Fitch was an associate in Hauser's lab at Harvard, and Chomsky may have been added more as a co-signatory, without having a role in detailed work on the paper's content (this attributional matter is not irrelevant in the light of the findings of scientific misconduct against Hauser five years later; see footnote 11 below).

HCF included an informally phrased conjecture about what Chomsky calls "Universal Grammar" (UG). The conjecture was that the SOLE aspect of linguistic structure attributable to a biologically rooted "faculty of language in the narrow sense", unique to *Homo sapiens*, is a special cognitive capacity for unbounded combining of mental syntactic representations through repeated applications of a posited binary set-formation operation called "Merge".[3] To motivate this idea for a general scientific readership, HCF pointed to a putatively self-evident fact about human language (p. 1571):

> The core property of discrete infinity is intuitively familiar to every language user… There is no longest sentence (any candidate sentence can be trumped by, for example, embedding it in "Mary thinks that …"), and there is no non-arbitrary upper bound to sentence length. In these respects, language is directly analogous to the natural numbers …

Notice the phrase "every language user", which suggests we are talking about every language of biologically normal human beings anywhere on earth. Note also HCF's claim that the human "faculty of language in the narrow sense" must

[3] In HCF and a voluminous subsequent literature these matters are discussed in terms of "recursion". I will avoid the use of this term (which HCF nowhere defines) because linguists' use of it is a morass of confusion, as Lobina (2014) correctly points out. In mathematical logic, "recursion" refers to either definition by induction or computational routines that invoke themselves (Soare 1996: esp. 286–289), and "recursive" is used of sets to mean "having a decidable membership problem". Linguists use "recursion" to refer either to self-embedding in phrase structure, or to iterated application of the "Merge" operation, or to HCF's conjectured mental syntactic combinatory capacity, and they use "recursive" as a predicate of rules or grammars. I focus instead on the relatively clear issue of WHAT KINDS OF EXPRESSIONS THE GRAMMAR PERMITS.

"construct an infinite array of internal expressions from the finite resources of the conceptual-intentional system" (p. 1578).

The content of the quotations above is entirely in line with Chomskyan ideas, though it is plausible to assume that Hauser drafted much of the article's text. The claims in HCF simply restate more emphatically a view that stemmed from Chomsky's earliest work and had been standard fare in linguistics textbooks for decades. Nearly half a century before, Chomsky (1956: 113) had claimed that the key purpose of a grammar was to project a finite corpus "to an infinite set of grammatical sentences", and over the next decade this became a part of the usual motivation for generative grammar. Ronald Langacker (1968: 31), for example, was merely elaborating on it when he wrote that "The set of well-formed sentences in English is infinite, and the same is true of every other language", adding the standard argument that given a sentence of any length you can construct a longer one by embedding it as a *that*-clause. HCF was merely echoing such statements.

Two years before HCF, Lasnik (2000: 3) had put things even more assertively in a syntax textbook, calling the availability of infinitely many sentences a "central" universal of language:

> Infinity is one of the most fundamental properties of human languages, maybe the most fundamental one. People debate what the true universals of language are, but indisputably, infinity is central.

And six months before Everett's *CA* article was published, Sam Epstein and Norbert Hornstein (2005) cited HCF in a letter (intended for publication in *Science* but published in *Language* instead) defending the Chomskyan program and asserting that "human language is a highly structured formal combinatorial system and, in addition, the number of discrete well-formed sentences generated by the system is infinite". They continued (p. 4):

> This property of discrete infinity characterizes EVERY human language; none consists of a finite set of sentences. The unchanged central goal of linguistic theory over the last fifty years has been and remains to give a precise, formal characterization of this property and then to explain how humans develop (or grow) and use discretely infinite linguistic systems. [Emphasis in original – GKP.]

This differs from earlier claims only in being even more strident and explicit.

The trouble for Everett was that by the mid 2000s, endorsing HCF's view of the biological basis of language had become something of a test of loyalty to the

Chomskyan mainstream conception of syntax. Everett's simple descriptive observation (with its many precedents in unnoticed earlier literature) had become an ideologically dangerous idea.

Some attempts were made to answer it by reinterpreting HCF in a way that could allow Everett's claims to be true without being relevant. The tactic is to neutralize the dangerous idea by asserting that only a vastly weaker hypothesis was ever really at issue. The main attack on Everett in the refereed literature, Nevins et al. (2009b), briefly mentions such a reinterpretation, claiming that under theories of the sort HCF assumed, "what is at stake is in fact the GENERAL ability to build phrases that contain phrases as subparts" and nothing more (pp. 366–67, fn. 11). This retrospectively interprets HCF as saying merely that phrases may contain other phrases. That must involve Merge applying to objects formed by Merge, and that can be called "recursion", vindicating HCF.

There are two problems, though. First, HCF's actual claim about languages was never simply that some phrases can contain certain other phrases (which could be entirely compatible with an upper bound on sentence length). The reference to a literal infinity of sentences quoted above ("There is no longest sentence") is crystal clear. Second, the notion that phrases may contain other phrases is absurdly weak: no one ever doubted it, and no one could think it merited publication in *Science*.

Chomsky has nonetheless essayed a retreat to an even weaker thesis (or at least a less empirically accessible one), which does not say anything about languages at all. He has maintained in various interviews that HCF was merely suggesting that there was a genetically inherited mental capacity of our species that WOULD permit humans to learn languages with arbitrary sentence length, IF they chose to use it. Whether or not speakers of attested languages show signs of using it is, Chomsky now claims, a total irrelevance. Speaking to a 2016 interviewer, Chomsky stated that we can dismiss the evidence of Pirahã syntax because "if some tribe were found in which people wear a patch over one eye and hence do not use binocular vision, it would tell us nothing at all about the human faculty of vision".[4]

Hornstein (2019: 792–794) expounds this view at greater length idea for anyone who didn't get the memo the first time. He distinguishes "Greenberg universals", to which evidence about languages can be relevant, from "Chomsky universals", which apparently await future advances in neurophysiology for support or refutation. Unfortunately, putting it this way reduces to nothing more than saying

[4] "Chomsky: We are not apes, our language faculty is innate." Interview with Filomena Fuduli Sorrentino, *La Voce di New York*, 4 October 2016, online at https://lavocedinewyork.com/en/2016/10/04/chomsky-we-are-not-apes-our-language-faculty-is-innate/

that there must be some special combinatorial ability (HCF's "faculty of language in the narrow sense") built into our brains somehow. The view makes no testable predictions except that some sort of linguistic ability will exist in normal humans; but we knew that when we arrived at the lab.

In the interview with Filomena Sorrentino mentioned above, Chomsky makes an additional revealing remark. Sorrentino asked him, "Is there something especially interesting about the Pirahã language?" and he said:

> The interesting properties of Pirahã have been studied in depth for many years in a wide range of languages, most prominently by Everett's mentor, MIT linguist Kenneth Hale, one of the leading figures in the study of indigenous languages, who has produced many important studies of these topics from the 1960s.

There are some straightforward untruths here – Chomsky's MIT colleague Kenneth Hale, though admired by Everett and everyone else who knew him, never served as "Everett's mentor," since Everett's MA and PhD theses on Pirahã had been completed before the two men met, and Hale never worked on Pirahã at all – but notice that Chomsky seems to be acknowledging the existence of a language with no apparent syntactic embedding. As mentioned above, Hale did point out in the 1970s that Warlpiri lent no support to any theory of hierarchical constituent structure, which would imply the absence of subordinate clause constituents, and at that time Chomsky saw no reason to attack him for it. It was only his pique at seeing HCF contradicted that motivated his going on the offensive against Everett.

Everett (2005) was really just drawing the attention of syntactic theorists to a pre-existing conflict. For decades linguists had been drawing motivation for generative grammars from the proposition that all human languages had infinite numbers of grammatical sentences. Pirahã provides a particularly clear and much publicized case of a language lacking the key syntactic constructions that could support the truth of such claims. For those aggressively committed to the totality of Chomsky's program, especially those knowing little of the syntactic literature from two or three decades earlier, this message had to be addressed by attacking the messenger.

The public part of the war on Everett began with a long paper about his work first circulated in 2007 and ultimately published by *Language* in 2009. It was written by David Pesetsky of MIT, Andrew Nevins, then at Harvard (now University College London), and Cilene Rodrigues, then at Emmanuel College, Boston (now the Pontifical Catholic University of Rio de Janeiro). I will refer to this trio as NP&R.

NP&R's paper (Nevins et al. 2009b) contains lengthy discussion of a topic about which I will say hardly anything: the extent to which, and the ways in which, culture can influence grammar. Everett holds that a single feature of Pirahã cultural life – their focus on immediate experience rather than remote considerations like the distant past, the far future, or the abstractions of mathematics or philosophy – predicts a whole slew of properties of their language. I doubt it, as do NP&R. But it is not their disagreeing with Everett that I will be concerned with here. In Section 3 I will turn to the rather meager results of their search for false syntactic claims in Everett (2005), but first I review some of the ancillary actions they and others took, and the way they instigated and promoted a remarkably vicious attack on Everett's character and integrity in the years that followed. I will survey the events only briefly in the next section, without attempting to be exhaustive.

2 Character assassination and career disruption

The obvious course of action for linguists who felt Everett's *CA* paper must be mistaken would have been to engage with him collaboratively to find out more about relevant properties of the Pirahã language. This was not the path chosen by NP&R. Their paper was written without contact with either Everett or anyone else who knew the Pirahã language. This made it wholly an exercise in textual exegesis. And it did not stop at addressing factual claims; from the start it employed thinly veiled inferences and accusations of prejudice, dishonesty, and even research misconduct.

The suggestion NP&R made was in essence that Everett's early descriptive writings on Pirahã did offer evidence of subordinate clauses (along with various other things like numerals, quantifiers, and color names), so his 2005 position was a suspiciously unsupported and possibly mendacious retraction of earlier views.

Despite mentioning the idea that HCF had only ever intended a weak claim about phrases containing other phrases (pp. 366–67, fn. 11), NP&R only made that point in passing; their central aim was to argue that in 2005 Everett was telling lies about CLAUSAL embedding, and that one could learn this by simply looking at his work of a quarter-century before, where he did tell the truth. In the refereed paper they published in *Language* (2009b) they could only adumbrate the claim of dishonesty, but in less constrained channels they and others were less guarded: emails, tweets, blogs, remarks to journalists, and posts on Facebook can slip the surly bonds of scholarly decency.

The attack mounted by NP&R, and taken up by other anti-Everett linguists, was not the worst that a social scientist ever suffered; the libeling of anthropologist Napoleon Chagnon and geneticist James Neel by Patrick Tierney (2000) was surely worse.[5] But the trashing of Daniel Everett runs a fair second for nastiness.

Tom Bartlett of *The Chronicle of Higher Education* heard about it from linguists that he interviewed in 2012. His account of linguists' behavior (Bartlett 2012) is not edifying, but fully accords with my knowledge and experience of the events. He speaks of a linguistics discipline "populated by a deeply factionalized group of scholars who can't agree on what they're arguing about and who tend to dismiss their opponents as morons or frauds or both". Other disciplines have disputes too, he admits, but even so, "linguists seem uncommonly hostile". If anything, Bartlett somewhat understated things; the following subsections refer to documentable incidents that he did not even mention.

2.1 The BCS lecture

In the fall of 2006 Professor Edward Gibson arranged for Daniel Everett to give a lecture on Pirahã syntax in the Brain and Cognitive Sciences department (BCS) at MIT. David Pesetsky, of MIT's Department of Linguistics and Philosophy, contacted Gibson by email. Details of the interaction are disputed,[6] but Gibson reports Pesetsky as apparently thinking that Everett held reprehensible views about the Pirahã people, mentioning a claim that the Pirahã talk like chickens and act like monkeys. Gibson knew the latter remark. It was from a page headed "Pirahã: The People" on the University of Pittsburgh website,[7] and reported a contemptuous remark by Brazilian merchants who traveled the Maici river and occasionally traded with men from Pirahã villages. Everett wrote: "The local traders say they 'talk like chickens and act like monkeys"'. He was quoting, not endorsing the characterization; he despised the ignorance of the people who repeated

[5] Tierney falsely alleged that Chagnon and Neel had deliberately exacerbated a fatal measles epidemic among the Yanomamö people in pursuit of some kind of eugenics experiment. For a time anthropologists Leslie Sponsel and Terence Turner persuaded the American Anthropological Association to support these charges and condemn Chagnon and Neel. See Dreger (2011) for detailed research on the whole sordid story of this affair, and a vindication of Chagnon and Neel. Tierney is now regarded as totally discredited.

[6] Pesetsky asked Gibson to assure him that he was not forwarding the email exchange to anyone else, and Gibson gave that assurance. Gibson has since honored Pesetsky's wish to keep his emails private. When I asked Pesetsky to show me the emails, he refused, so I have only Gibson's broad paraphrase of them as my source.

[7] In 2007 it was still accessible at http://amazonling.linguist.pitt.edu/people.html but it did not survive Everett's subsequent moves to other universities and seems not to have been preserved by the Wayback Machine archiving site.

the saying. Gibson pointed out that an unendorsed direct quotation entailed nothing about Everett's views, but when the first draft of NP&R's paper was circulated about three months later,[8] it contained a statement that the authors felt a "general discomfort with the overall presentation of Pirahã language and culture" that Everett gave, and in a footnote (p. 51, fn. 74) it repeated the quote from the river traders.

The extent of NP&R's hostility to Everett's views and suspicion about his relations with indigenous Brazilians became much more explicit on Tuesday 28 November 2006, when Gibson sent out a formal announcement of Everett's lecture to the mailing lists for linguists and BCS people at MIT and Harvard. Immediately Andrew Nevins (who had never met Everett, and refused when Gibson later suggested a meeting) sent out a scathing email from his Harvard account to the same lists about the expected content of the talk.[9] The subject line was "enough is enough" and it opened by saying:

> although david, cilene and i are working on a paper about the linguistic features of piraha, i thought some of you should see some of the more obvious counterexamples to everett's cultural claims before his talk at mit on friday, especially since we may not be allowed to ask questions without being cut off.

He then gave a link to Everett's "Pirahã: the people" and said: "have a look at this archived web page from just over 6 years ago. Did the Piraha change since then, or did Everett?" – an indication that NP&R were going to try to show that Everett was not just wrong, he was lying about facts he had previously acknowledged. After giving a few links to Brazilian anthropological literature, Nevins ended with a sarcastic parody of advertising copy:

> You, too, can enjoy the spotlight of mass media and closet exoticists! Just find a remote tribe and exploit them for your own fame by making claims nobody will bother to check!

This struck me as like an intrusion into linguistic science of the sort of attack ads typically seen in political election campaigns. I commented on it in a dis-

[8] LingBuzz, 8 March 2007, https://ling.auf.net/lingbuzz/000411/v1.pdf?_s=AES_1bvQN0ZRFPhy

[9] At the time I had a Radcliffe Institute email address that David Pesetsky had kindly added to the MIT visitors' email list to keep me informed about colloquia during a sabbatical at Harvard, so I was an accidental recipient of Nevins's email. He had tried to reach the MIT Brain and Cognitive Sciences list as well as the lists for the two linguistics departments, but found it closed to external senders.

cussion of the issue on Language Log the next day,[10] speculating on whether the attack might be motivated by a combination of Chomskyan orthodoxy, liberal hypersensitivity regarding ethnic minorities, and academic prejudice against missionaries.

The talk attracted a large audience. Nevins, Pesetsky, and Rodrigues all attended, and so did Marc Hauser, the lead author of HCF. Hauser was well acquainted with Nevins, who regularly attended Hauser's lab meetings at the time. Ironically, seven months after Nevins's email about "claims nobody will bother to check", Harvard investigators began to check some of Hauser's claims about primate behavior, and within four years he had been found responsible for serious research misconduct and had lost his professorship and quit academia.[11]

2.2 Refusal of research permits

In 2007, Everett received an unexpected phone call from the distinguished journalist Larry Rohter, who had been South American bureau chief for *The New York Times* since 1999. Rohter was in the office of the director (*presidente*) of FUNAI (Fundação Nacional do Índio, later renamed Fundação Nacional dos Povos Indígenas), the Brazilian government agency charged with overseeing the welfare and protection of the country's indigenous people. He had in his hands a letter written to FUNAI by Cilene Rodrigues. Rohter read the Portuguese text to Everett over the phone.

The letter expressed objections to Everett's linguistic research and his representation of Pirahã culture. It may also have expressed the view that he was not a suitable person to be permitted to work with Brazilian Indians. I have not seen the letter, and Rodrigues did not respond when I asked her for a copy of it, but Rodrigues's role in the interaction with FUNAI is confirmed in an article in *The*

[10] 'Fear and loathing on Massachusetts Avenue,' on Language Log, 29 November 2006, online at http://itre.cis.upenn.edu/~myl/languagelog/archives/003837.html

[11] In July 2007 investigators entered Hauser's lab while he was away, seizing computers, video records, and documents. By August 2010 they had found him solely responsible for "eight instances of scientific misconduct", including "problems involving data acquisition, data analysis, data retention, and the reporting of research methodologies and results". After a year's leave of absence, Hauser learned that he would not be allowed to return to teaching at Harvard, or maintain a laboratory, or apply for grants. He resigned effective 1 August 2011. Later a separate investigation by the federal government's Office of Research Integrity found in September 2012 that he had fabricated data, manipulated results, and wrongly described experiments supported by several federal grants (see DHSS notice 77 FR 54917, 09/06/2012). Gross (2011) provides a detailed discussion of the Harvard investigation and its aftermath.

New York Times,[12] which reports that "She declined to elaborate on the contents of the letter, which she said was written at Funai's request and did not recommend any particular course of action", and that "asked about her overall opinion of Dr. Everett's research, she said, 'It does not meet the standards of scientific evidence in our field.'".

A few years earlier, Napoleon Chagnon's enemies had managed to persuade FUNAI to deny him permission to visit the Yanomamö people in Brazil (see Dreger 2011). Something similar now appeared to happen to Everett. The next time he applied for permission to bring some researchers to the Pirahã territory (which, ironically, he had originally assisted FUNAI in demarcating in order to protect the Pirahãs' right to their land), he found that he was denied. He was later able to get permission from the local FUNAI office to visit the area merely as an aide and interpreter to a film team during the making of the 2012 documentary film *The Grammar of Happiness*,[13] but his applications to do grant-supported field research on the language met with negative decisions.

Everett flew to Brasília to discuss the situation, accompanied by the doyen of Amazonian research, the late Aryon Rodrigues (1925–2014), who had been a mentor to him during his doctoral studies. They had set up a meeting with the national director of FUNAI, Márcio Meira, but Meira did not show up. Instead he sent a deputy had no power to make executive decisions. Everett was thus cut off from visiting the people he had known intimately for more than thirty years.[14] Among other things, this was a material loss for the Pirahã, because every time Everett arrived in their village he would bring medicines and other valued items.

2.3 Chomsky's "charlatan" insult

In early 2009 Noam Chomsky was interviewed about the dispute by *Folha de S. Paulo*, the the largest-circulation newspaper in Brazil, and with evident irritation he told the interviewer (see the issue of 1 February 2009):

> Ele virou um charlatão puro, embora costumava ser um bom linguista descritivo. É por isso que, até onde eu sei, todos os linguistos sérios que trabalham com linguas brasilieiras ignoram-no.

[12] "How Do You Say 'Disagreement' in Pirahã?" by Jennifer Schuessler, *The New York Times*, 21 March 2012.

[13] On YouTube at https://www.youtube.com/watch?v=5NyB4fIZHeU and also via SLICE at https://www.youtube.com/watch?v=_LAR6eeiVtY

[14] Everett lived in Pirahã villages for 10 days in 1977; 3 weeks in 1978; 6 weeks in 1979; 8 months in 1980; 4 months each year from 1981 to 1985; a total of 12 months during 1986–1988; a total of 36 months during 1989–1999; 20 months during 1999-2001; and three months during 2001–2009, a total of just over 100 months.

> ["He became a pure charlatan, although he used to be a good descriptive linguist. That is why, as far as I know, all the serious linguists who work on Brazilian languages ignore him."]

The petty abuse of the first sentence is followed by a piece of dishonesty: since Chomsky has never worked on Brazilian indigenous languages and has never discussed any detailed work by those who have, he has no knowledge of the wider community of Amazonianists (many of them missionaries, others secular linguists or anthropologists in a variety of universities in Europe, Australia, and the Americas), and therefore has no grounds for assessing Everett's standing among Amazonianists. The truth is that Everett's expertise has never been questioned by the linguists with whom he has worked, or by any of the roughly twenty researchers who have spent time with him among the Pirahã to do research, or by any of the few outsiders who (like Steven Sheldon) have actually made progress on learning the Pirahã language.[15]

Chomsky continued with a clearly unverifiable claim about Everett's private thoughts and hopes:

> Everett espera que os leitores não entendam a deferença entre a GU no sentido técnico (a teoria do componente genético da linguagem humana) e no sentido informal, que dis respeito às propriedades comuns a todas as línguas.
>
> ["Everett hopes that the readers do not understand the difference between UG in the technical sense (the theory of the genetic component of human language) and the informal sense, which concerns properties common to all languages."]

Chomsky is alluding to his reinterpretation of HCL's "recursion" claims as having never been about languages, but only about the genetically transmitted human ability to acquire language. He is claiming that Everett wanted to fool *CA* readers into paying attention to sentence structure when really he knew the focus should have been on genetics and neurophysiology.

But HCF never provided any genetic or neurophysiological facts about the human language capacity that Everett could have focused on. As Everett noted in a response to NP&R, if the "genetic component" is the issue on the table, then Chomsky's claim seems virtually empty: humans simply have whatever special

[15] Chomsky had perhaps forgotten that Everett had mentioned the lack of syntactic embedding in Pirahã during a personal conversation with him at MIT 25 years earlier; see Everett (2007: 12, fn. 7). I return to this briefly in Section 3 below.

thing it is that permits them to acquire and use language (see Everett 2009: 439). Since he was motivated by what HCL actually said ("There is no longest sentence", etc.), he concentrated on "properties common to all languages". That isn't charlatanry.

2.4 Rodrigues's overt accusation of racism

Later in 2009, Rodrigues increased the rhetorical temperature some more. She explicitly alleged in a magazine interview with the German journalist Malte Henk that Everett held racist beliefs: "Everett ist ein Rassist. Er stellt die Pirahã auf eine Stufe mit Primaten" ["Everett is a racist. He puts the Pirahã on a level with primates"].[16] By "primates" she clearly means apes and monkeys, unless she has forgotten that all humans are primates.[17]

As Bartlett (2012) remarks, "When you read Everett's two books about the Pirahã, it is nearly impossible to think that he believes they are inferior. In fact, he goes to great lengths not to condescend.". He does indeed. He stresses their sharp intelligence, ingenuity, strong group identity, rich social life, and ability to grasp complex discourse. He lived with them, hunted with them, raised his three children among them, talked with them endlessly, and learned from them during periods of residence totaling well over eight years. His many accounts of interaction with them (most engagingly in Everett 2008) often evince admiration, and never for a moment suggest he sees them as racially inferior beings.

But accusations of racism are potent weapons in contemporary intellectual and political debate, whether grounded or not – more powerful than any points about syntactic analysis could be.

2.5 The fraud libels

While working on his 2012 article, Tom Bartlett asked Nevins for some comments on the war on Everett. Nevins refused to be interviewed, but emailed back: "it seems you've already analyzed this kind of case!" – appending a link to an earlier Bartlett story about Diederik Stapel.

The implied defamatory claim here is extreme. Stapel is famously an admitted fraudster. He voluntarily returned his PhD certificate to the University of Amsterdam because he acknowledged that his scientific misconduct had been "inconsistent with the duties associated with the doctorate". So far 58 of his papers in

[16] *GEO* magazine (Gruner + Jahr, Hamburg, Germany), January 2010, p. 59.

[17] In an email to Everett, Rodrigues denied ever making the statement, but Malte Henk stands by his claim about what she said to him on the record; see Everett (2013: 13).

social psychology have been retracted on grounds that the data were either manipulated or – in at least 30 cases – simply invented out of thin air. Stapel would invent whole tables of data with no empirical basis at all, and published many reports of experimental studies that were never conducted. Nevins is equating Everett's eight years of immersive fieldwork and data analysis with the proven scientific misconduct of a man described in *The New York Times* (26 April 2013) as "the biggest con man in academic science".

At the time Nevins sent his message to Bartlett, Everett was a dean at Bentley University and happened to be chairing an investigation into allegations against a professor of accounting: Professor James E. Hunton, who ultimately resigned in December 2012. By 2016 at least 37 of Hunton's papers had been retracted under suspicions of wholesale invention of data and publishing reports of studies that had never been conducted.[18] Bentley, therefore, had a well-functioning procedure for dealing with research misconduct, which could have been used against Everett if anyone had come up with a scintilla of evidence about fraud or or other research misconduct.

Tom Roeper of the University of Massachusetts, Amherst, also directly and publicly accused Everett of fraud. Speaking about Everett on camera to the makers of *The Grammar of Happiness*, he said: "I think he knows he's wrong, that's what I really think.". With a knowing smile, he added: "I think it's a move that many, many intellectuals make to get a little bit of attention.".[19] Roeper's claim is not just that Everett is wrong, but that he KNOWS he's wrong, and is telling lies "to get a little bit of attention".

2.6 Illegality accusations

In Brazil, the allegations started to reach further than simply positing dishonesty. Rumors were spread that for decades Everett had been working illegally, never obtaining the required permits for working in Indian areas. Denny Moore, an American linguist resident in Brazil, made forceful allegations along these lines to me in personal conversation and subsequent email (May 2019) and made further remarks on the topic in a Facebook comment in January 2024.

The suggestion that Everett had never complied with the full legal requirements is implausible on its face, because if it were true then his failure to obtain

[18] See *Retraction Watch*, https://retractionwatch.com/2016/05/12/former-accounting-prof-adds-4-more-retractions-total-exceeds-37/

[19] For a bookmarked location of Roeper's remark in the SLICE release of the film, retitled as "Decoding Amazon: life of the Pirahã", go to https://youtu.be/_LAR6eeiVtY?t=1323

a FUNAI permit after Rodrigues's letter of 2007 would have been of no importance. Everett arrived in Brazil in 1977 and was granted permanent resident status under an agreement between the Brazilian government and the Summer Institute of Linguistics (SIL), so he can visit the country without a visa whenever he wishes. But doing research on the Pirahã reservation without a FUNAI permit would be illegal. The only reason Everett has not been able to do any field research among the Pirahã since 2009 is that he strictly respects the law – as one would expect, given the crucial necessity for him to have access to indigenous Amazonian areas.

In 1977 all SIL missionaries were allowed to live among indigenous populations (Desmond Derbyshire had been with the Hixkaryana under such terms since 1955 when I met him). In 1978 the government canceled the contract with SIL and all missionaries had to leave indigenous lands. At that point Everett became a graduate student at the State University of Campinas (UNICAMP), and in that capacity, with the help of Aryon Rodrigues, he received written authorization from the director of FUNAI to return to the area, and spent a year living in a Pirahã village with his American wife Keren (now Keren Madora) and three children – not a visit that could have been accomplished furtively.

Eventually FUNAI reached an understanding with SIL that allowed all of its members to continue working in indigenous villages, not as missionaries but in order to do linguistic research and translate morally uplifting works into indigenous languages. That blanket permission for SIL members covered Everett after he completed the PhD at UNICAMP, until 2001. During that period he never needed to fill out the permit application forms used by university academics, whether Brazilian or foreign, which is why (as suspicious Brazilian researchers have found) searches in the public record for his applications via that channel come up with no results.

In 2001 Everett left SIL. Since then, when doing grant-supported research as a faculty member at the University of Pittsburgh (1988–1999) or the University of Manchester (2001–2006), he has entered the country on the basis of his permanent resident status (contrary to some allegations, he has never entered Brazil on a tourist visa), and he obtained permission for visits to indigenous areas through close contacts with FUNAI.

There are different ways for permanent residents to work: they can apply to the national office of FUNAI, or go through a local FUNAI office in the appropriate region provided Brasília does not object. They can also visit at the request of an indigenous group, which FUNAI is required to accept. One way or another, Everett has always had the needed permits, and two national-level directors of FUNAI (including the much-respected Apoena Meirelles) visited Everett while

he lived with the Pirahã, which would hardly have happened if he was an illegal foreign interloper. He has a letter from FUNAI thanking him for his work, and a short article praising his work appeared in a magazine in 2012[20] and was archived on the FUNAI website.

There was an occasion in 2007 when Everett was with the Pirahã along with several students and a local FUNAI official with a grudge against him reported that they were there illegally. A heavily armed team of military police made the long river journey through a rainstorm to get to the relevant Pirahã village and arrest him. Everett greeted them in fluent Portuguese, showed them his permanent residence document and his letter from the local FUNAI office. The policemen relaxed, and posed smiling for a photo with members of Everett's team. A few days later in Porto Velho, he was called in by the FUNAI office there over the same incident, and again satisfied the organization that he had done everything legally.

Everett is not and never has been the subject of any civil suit or criminal indictment for illegal presence in an indigenous area. Yet allegations that he is a notorious lawbreaker continue to be spread by linguists in Brazil. The strong antipathy felt by many Brazilian academics to North American missionaries may be partly to blame, since Everett is still thought of as associated with that role, more than two decades after he left SIL.

2.7 The Nevins/Carvalho/Rössler video

A conference was held at the Federal University of Rio de Janeiro in 2013 that was devoted entirely to work arguing that Everett was wrong. Everett heard about the planning for it, and offered to attend the conference at his own expense, but he was told he would not be welcome. During the same period (August 2013) Nevins took the opportunity to work with Emerson Carvalho and Eva-Maria Rössler to produce a video[21] which seems to have the primary purpose of further damaging Everett's reputation. It is represented as an interview with two representatives of "the leadership" of the Pirahã (in truth they live an anarchist socio-political life with no political leaders). The main speaker throughout the video is Jose Augusto Diarroi, nicknamed "Verão" by Portuguese speakers because of his SIL contacts (*verão* means "summer"), who falsely represents himself as member of the Pirahã community. His father was Pirahã, but his mother was not, and he was raised elsewhere, never acquiring more than a smattering of the Pirahã language. Sitting beside him is a native Pirahã speaker whose name is given as Yapohen

[20] Marcelo Moraes Caetano, "Indagado pelos Pirahã", *Revista da Cultura* 61, August 2012, p. 33.

[21] Online since 2013 at https://www.youtube.com/watch?v=J3jWI4cPRMg

(not a possible Pirahã name) but is actually Hiahoái. Very few Pirahã utterances are heard in the entire interview, and none are glossed in the subtitles.

Augusto tells tales about Everett engaging in activities seemingly drawn from the worst stereotypical charges against bad missionaries, claiming that Everett had terrorized the people he lived among, threatening them that God would kill them all if they did not come to Jesus and convert to being "true believers", and so on. Nevins's voice can be heard saying things like "Wow!" from time to time. If any of what he says were true, Augusto would not be one to tell about it, because he never lived in a Pirahã village during any time when Everett was there.

At certain points Augusto attempts to elicit some contributions from Hiahoái, who is visibly reluctant to speak, and says nothing for a long time. When he is eventually prompted to say a few things in Pirahã, Augusto pretends quite unconvincingly to translate them, turning a few seconds of Pirahã into several minutes of Portuguese. What he represents as translations are total fabrications. A version of the video with transcription supertitles of the Pirahã utterances was uploaded by Miguel Salinas in 2019.[22] See Everett & Gibson (2019: 781, fn. 3) for brief discussion of some of this video, with examples of the mistranslations.

2.8 Cancelation at Oxford

The work that NP&R have put into representing Everett as a disreputable person and untrustworthy scholar has not had significant material effects on his career: he has served successfully as a department head, dean of arts and sciences, and acting provost, and unlike Hauser or Hunton he remains a tenured full professor to this day. Nevertheless, NP&R have created a kind of folklore, a vague shadow of disrepute, which continues to have effects. Mud sticks, if you throw enough of it. One of Everett's daughters reports having met people in Brazil who say, "Oh, you're the daughter of that racist guy.".[23] And substantive professional consequences do result from this atmosphere of negativity.

For example, on 12 March 2017 Everett offered to give a talk to the linguists at the University of Oxford the following September – at no cost to Oxford because he was planning to visit the UK anyway. The planned lecture was not to have been about Pirahã syntax, incidentally, but about paleoanthropology and the emergence of language in early humans. His offer was greeted with enthusiasm by the head of the linguistics faculty, Professor Aditi Lahiri, who promptly let her colleagues know the good news. But within hours her acceptance was withdrawn in a rather awkward email message.

[22] Online at https://www.youtube.com/watch?v=xeEAufXg8fc

[23] Interview with Liz Else and Lucy Middleton, *New Scientist*, 19 January 2008, p. 44.

The next day Everett learned the reason: two junior faculty had objected by email as soon as they learned of the tentative plan, citing potential "reputational damage" to Oxford if Everett were to speak there.[24] It is hard to believe someone would think a visiting speaker could be so toxic that his mere appearance would inflict reputational damage on Britain's oldest university, often ranked number one in the world. But this is the sort of strange fruit the long campaign against Everett has borne.

2.9 The double review of *Recursion Across Domains*

The conference in Rio de Janeiro in 2013 resulted in a book entitled *Recursion Across Domains* (Amaral et al. 2018). The central aim of the conference and the book was to publish studies saying Everett was wrong, and he was never invited to submit a reply to its criticisms. But the editors of the Linguistic Society of America's journal *Language* invited Everett together with his collaborator Edward Gibson to write a review of the book (it appeared as Everett & Gibson 2019). When this became known to Everett's opponents, the editors promptly came under pressure to alter their decision. After some consultation they made the unprecedented decision to give the book two review articles in the same issue. Several potential reviewers who were thought likely to take a more anti-Everett and pro-Chomsky line were sounded out but declined. Finally Norbert Hornstein agreed to take on the task.

Hornstein (2019) admitted with admirable frankness (p. 791) that he knows nothing at all about the empirical content of the book – topics like the syntax of South American languages and experimental developmental psycholinguistics. In fact he says: "Facts usually make me itchy… My allergies will lead me to pass lightly over many of the specific empirical findings in what follows.". His main qualification was clearly that he could be relied upon to support the Chomskyan line, and that he did. (See Section 5 below for a discussion of one chapter from the book that Hornstein naively accepted as sound.)

Further pressure on the editors of *Language* induced them to do one additional thing regarding the same book that as far as I can see was unprecedented: *Language* (like most scholarly journals) does not publish aggrieved responses to book reviews submitted by authors whose work is criticized. But Cilene Rodrigues sent in a letter of protest about the Everett and Gibson review, which had said that her work did not exhibit "high scientific standards". The editor (Andries Coetzee) initially resisted the idea of publishing it (and told Everett and Gibson that it would

[24] This was reported to Everett by the late Yorick Wilks (1939–2023) in an email, 13 March 2017, which I have seen. Wilks stated that he had seen the objectors' emails but did not name them.

not be published without their having right of reply), but he was eventually persuaded to print it, and it appeared in *Language* 96.2 (2020), 221–223, without a reply (Rodrigues 2020). A short editorial clarification concerning one sentence in the Everett and Gibson review was also printed. Thus *Recursion Across Domains* ended up being the subject of four different items in the pages of *Language* when the usual maximum for any book is one.

2.10 Recent literature overviews

The work NP&R have done to damage Everett's reputation has been ample to color the general impression a newcomer to the dispute will pick up. The superbly detailed survey of Amazonian languages by Aikhenvald (2012) takes the line of treating the issues as unfit for discussion, declaring that "there is neither consistency nor plausibility to the quasi-analytical statements which have been made concerning this language [Pirahã], or its culture, during the past fifteen years. I refrain from quoting these sources" (p. 411, n. 91). She thus avoids any discussion of the polemics of the post-2005 literature. In fact she cites nothing on Pirahã dated later than 1986.

Janet Chernela, an anthropologist specializing in Amazonia, recently tried to survey the whole dispute in an article for *Annual Review of Anthropology* (Chernela 2023). She seems to think she has provided a balanced summary, but her treatment of the relevant literature is hopelessly skewed against Everett. She never even mentions the existence of *Handbook of Amazonian Languages*, and hence never refers to Everett (1986b), unquestionably the most important descriptive document in the whole dispute. She cites Nevins et al. (2009b) without ever mentioning that it was followed by a detailed response (Everett 2009) in the same issue of *Language*, nor the rebuttal to that by Nevins et al. (2009a), nor the final rejoinder to that by Everett (2013). She very briefly mentions the incompetently uncritical review article by Hornstein (2019), but seems unaware of the vastly more expert critical one by Everett & Gibson (2019).

Admittedly, reading all of the post-2005 work just cited would be an exhausting business – anyone who doesn't come out of reading it feeling dazed and confused just hasn't been paying attention. But the skewing of Chernela's coverage is quite extraordinary. It is possible that she fell victim of a major downside to accessing literature online: anyone who had *Language* 85 no. 2 in their hands could not fail to see that Nevins et al. (2009b) is immediately followed by Everett's 37-page response, but if Chernela simply heard about the former and downloaded a PDF of it she might well have had no idea the latter existed.

However, she has less excuse in the matter of the two reviews. She cites Hornstein (2019) in connection with Chomsky's claim that "variation between languages – while possibly interesting for other purposes – is irrelevant to the nature of the FLN" (p. 140). But its first page carried an editor's footnote explaining that "This issue of *Language* contains two review articles focusing on the volume *Recursion Across Domains*", and adding: "Since the topic of this volume (recursion) is one of central interest (and some controversy) in current linguistic theory, we thought it important to publish reviews from scholars who will bring differing perspectives to the topic", and so on. Those differing perspectives do not come through in Chernela's account.

She makes some patently erroneous and unfounded claims, like that NP&R "reanalyzed data collected among the Pirahã by Everett's predecessors"(p. 140). NP&R did nothing of the sort, and do not try to represent themselves as having done it. Steven Sheldon, whose residence among the Pirahã antedated Everett's, did produce some transcribed texts, which are utilized by Futrell et al. (2016), but NP&R appear not to have known about them. NP&R (2009b: 391) do cite a table of six pronoun forms from a paper by Sheldon, but the paper (Sheldon 1988) appeared two years after Everett's main descriptive work on the language was in print.

In another inexplicable piece of invention, Chernela asserts that "Much of Everett's field methodologies involved structured interviews using a recorder" (p. 143), and she asserts that his work "flies in the face of Boasian anthropology" because it fails to "interpret cultures and languages on the basis of each society's own logic and values rather than through a universal yardstick" and "understand language as a social phenomenon in which meanings cannot be understood apart from context". But Everett's work involved interacting more closely with the community than any other outsider has ever done or was ever competent to do. He lived in the community and participated in its life for eight years. His children became fluent in the language and often played with Pirahã children all day. He constantly strived in his work to "interpret cultures and languages on the basis of each society's own logic and values". Throughout Everett (2012) it is clear that language is being seen as intimately linked to culture, and Boas is copiously discussed in Everett (2016).[25] Like NP&R, Chernela never met Everett or even emailed him. She seems to have decided up front that he was to be her representative of the typical desk linguist asking elicitation questions, not the sensitive anthropological investigator attuned to culture, values, and meaning.

[25]Chernela mentions the existence of both these books (p. 144), but only in passing, and she misstates the title of the first.

The general pall of negativity that has been cast over Everett's work may be responsible for some of Chernela's bias. Like NP&R, she worked without any contact with Everett or anyone else who had ever lived with the Pirahã and learned their language. It was an anthropologist, Bambi Schieffelin, who suggested to Chernela that she might write the article, and neither of the two people thanked in her acknowledgment note for reading the paper in draft (p. 146) is a linguist. She does no linguistic analysis; she simply browsed some of the recent literature and came away with the broadly negative view of Everett's work that NP&R were intent on establishing as the default.

The end result is not too surprising given the intellectual climate that the campaign of hostilities created. Linguists should be ashamed of this ghastly parody of science, with its rumors of racism substituting for scientific discussion, and career sabotage replacing rational criticism. But what makes things worse is that it was under-informed from the start. To see why Everett in the early 1980s was trying to provide evidence of subordination in Pirahã, we need to look at certain events predating all of his descriptive work, but the digression is a relevant one.

3 Overlooked prehistory

In 1975, Daniel Everett was 24 and had just completed a Diploma in Foreign Missions from the Moody Bible Institute in Chicago. He and his wife were making plans to enter service as missionaries and bible translators for the Summer Institute of Linguistics (SIL) in South America.

Four thousand miles away, I was a 30-year-old lecturer in linguistics, completing my first year at University College London. I had spent 1973–74 at King's College, Cambridge, learning typology from Ed Keenan and Bernard Comrie, and spent the summer of 1974 at the LSA Linguistic Institute at U Mass Amherst learning from Chomsky, Halle, Keyser, Perlmutter, and Postal.

In 1976, barely done with writing my PhD dissertation on rule interaction in classical transformational grammar, I was asked if I would take on the supervision of a prospective PhD student: a 54-year-old SIL missionary named Desmond Cyril Derbyshire. He had had no college degree; before he became a missionary he had been a chartered accountant in Durham, England. I'm not sure whether my senior colleagues believed the work of a middle-aged missionary would amount to much, but fortunately for me they allowed him to enroll, and I agreed to be his de facto advisor (de facto because the university did not allow someone of my lowly rank to be a doctoral supervisor). He turned out to be perhaps the finest scholar I ever worked with.

3.1 Discovering Amazonian languages

By the time I met Derbyshire he had done nearly 20 years of work on a Cariban language I had never heard of, spoken on a northern tributary of the Amazon. In a lecture on constituent-order typology I presented arguments (set out a in then-forthcoming article, Pullum 1977) that there was no convincing evidence for any language in the world having an object-initial basic constituent order (OVS or OSV). The only surface orders for the major constituents of the clause permitted by universal grammar seemed to be SOV as in Hindi, SVO as in English, VSO as in Irish, and VOS as in Malagasy. Derbyshire raised a hand from the back row and reported that he had been working on a language that he believed strongly preferred OVS as the order in transitive clauses.

The language was Hixkaryana. We arranged to meet after class so that I could learn something about its clausal syntax. Derbyshire had actually published a preliminary study of it back in 1961, when I was in high school (Derbyshire 1961), and it included a remark (using the terminology of Kenneth Pike's largely forgotten tagmemics framework) that "the goal always precedes, and the actor usually follows, the predicate tagmeme". In post-Greenberg terms, that meant OVS. But there had been no discussion of this language in the subsequent literature.

I gave Derbyshire some ideas on how he might confirm that he really was dealing with an OVS language: there was the possibility that (for example) Hixkaryana was just an SOV language in which the subject was occasionally shifted to clause-final position in special discourse contexts. There were substantial stocks of data to consult: a collection of texts transcribed from native speakers and published in Brazil ten years before; a Hixkaryana version of the entire New Testament, checked throughout by native speaker consultants, in press in Brasilia; and plentiful supplies of other data collected during Derbyshire's twenty years of fieldwork, including a remarkable diary privately composed by a native speaker who had learned to write the language.

Text from all sources supported Derbyshire to the hilt. My belief that universal grammar precluded object-initial basic constituent orders was inescapably wrong. Hixkaryana was a rather rigid OVS language: always OV, with auxiliary after the lexical verb, and the subject clause-initial only infrequently, when specifically focused or contrasted with something else (see Derbyshire 1985: 74).

Derbyshire and I began work on publicizing what appeared to be the then new and surprising fact that there was definitely at least one clear case of an OVS language. I worked with Derbyshire on preparing a squib for publication in *Linguistic Inquiry* (Derbyshire 1977). And I suggested to him that his doctoral

work might permit him to also undertake a monograph for the Lingua Descriptive Series (LDS) that was being planned by Bernard Comrie and Norval Smith.

The LDS monographs were required to adhere to a format carefully designed to facilitate comparative research. The instructions for contributors were published as a special issue of *Lingua* (vol. 42, no. 1) as the *Lingua Descriptive Series Questionnaire* (Comrie & Smith 1977, henceforth *LDSQ*). It set out a systematic section-numbering scheme for organizing descriptions in the series.

I showed Derbyshire my copy of *LDSQ* as soon as I received it, and he not only took up the task of writing an LDS monograph, but worked efficiently enough to produce the inaugural one (Derbyshire 1979), a superb description which would have amply justified the award of a PhD – but in fact he also produced a distinct work to offer as his PhD dissertation under the title *Hixkaryana Syntax*, which presented the description somewhat differently and added a second part on typology and discourse syntax plus eleven appendices on phonology and morphology (it was published later as Derbyshire 1985).

The significance of *LDSQ* to this story becomes clear in the light of what its detailed instructions said about subordinate clauses. It specified that Section 1.1.2 of the description was to be headed "Subordination". Subsection 1.1.2.1 was to state whether there are "any general markers of subordination, e.g. word order, particles (in what position?), verb modification, etc.", and 1.1.2.2 was to cover "Noun clauses" – the full finite subordinate clauses that Jespersen calls content clauses. Section 1.1.2.2.3 was to deal with declarative content clauses ("indirect statements"), 1.1.2.2.4 was to treat interrogative ones ("indirect questions"), and so on. This had more significance than we then realized.

Derbyshire made some further visits to Brazil and began learning more about what other SIL linguists had found. We began to pick up reports of other OVS languages, plus one or two that seemed to be OSV. I obtained a grant from the UK Social Science Research Council to support Derbyshire's work, not only on the syntax of Hixkaryana but also on these other reported languages. I learned a lot about the history, geography, ecology, and demography of Amazonia, and the appalling treatment of its indigenous inhabitants, and together Derbyshire and I prepared a paper entitled "Object initial languages" giving brief accounts of a dozen object-initial languages (it was later published in *IJAL* as Derbyshire & Pullum 1981). This led to our planning what became the four-volume *Handbook of Amazonian Languages* (*HAL*).

The relevance of *HAL* is, of course, that around 1983 or 1984 Derbyshire commissioned a chapter for it from the young Daniel Everett. His grammatical overview of Pirahã became the longest chapter in the first volume (Derbyshire & Pullum 1986, henceforth *HAL 1*).

Everett was by this time a PhD graduate of the Universidade Estadual de Campinas in Brazil (the first linguistics PhD in the country), with a dissertation on Pirahã grammar and syntactic theory. Derbyshire was aware that Pirahã was a genetically isolated and notoriously difficult language on which SIL had tried to make headway for a quarter of a century. Two previous missionary linguists had worked on it: Arlo Heinrichs, who did the difficult work of establishing initial contact with the Pirahã and worked with them from 1959 to 1967, publishing a preliminary view of the phonemes of the language (Heinrichs 1964), and Steven Neil Sheldon, who worked on the language from 1967 to 1976 and knows it fairly well. But Everett and his then wife Keren were the first SIL members who learned to speak and understand the language fluently. Everett's translation of the *Gospel of Mark* (Everett 1986a) was the first piece of bible translation SIL had ever achieved for the language.

To guide Everett and the other contributors of the grammatical sketches in *HAL*, Derbyshire and I had produced an analytical table of contents, much briefer than the questionnaire for the LDS but inspired by it. We reproduced it in *HAL 1*, pp. 31–32. And (the crucial point) Section 14 was to be headed "Subordinate clauses". Everett had in fact already seen *LDSQ* as soon as it appeared, and was already assuming that he had to say things about subordinate clauses.

It should not be too surprising, then, if Everett diligently strove to find and exemplify subordinate clauses, looking for all the usual grammatical furniture that speakers of European languages and syntacticians at MIT would expect sentences to exhibit. NP&R represent it as suspicious that he would say in 1983 and 1986b that there were subordinate clauses and then say in 2005 that there weren't. But he was effectively being directed to say something about subordinate clauses by both of the two sets of instructions he was using as guidance.

Looking back now, what surprises me is that Derbyshire and I did not rethink our guidance, and change the question to "Are subordinate clauses found in the language?"; by the early 1980s we knew what Hixkaryana had taught us about the topic of subordinate clauses. Derbyshire followed *LDSQ*'s directions closely, so linguists do not have to wonder about what the subordinate clauses are like in any language with an LDS monograph; they can just turn to Section 1.1.2 and find out. Here is what Derbyshire says about Hixkaryana (p.21):

> 1.1.2. Subordination
>
> Subordination is restricted to nonfinite verbal forms, specifically derived nominals (or, pseudo-nominals that function as adverbials — see 1.1.2.2.6).

Hixkaryana, then, had no content clauses at all. And turning to Section 1.1.2.3, "Adjective clauses (relative clauses)" – I'll use the latter, more modern term – we find that in Section 1.1.2.3.1 the marking of relative clauses was to be described; in 1.1.2.3.2 the description should say whether there is a distinction between restrictive and non-restrictive relative clauses; and other subsections ask about their word order, etc. Here is the relevant passage:

> 1.1.2.3. Adjective clauses (relative clauses)
>
> There is no construction of the adjective clause (relative clause) type. There are various means used to obtain the same effect as such a clause: simple nominalization; placing NPs together in a paratactic relationship, with intonational break; descriptive sentence, usually involving an equative clause (see 1.2.1.1.4); or some combination of these means.

So relative clauses did not exist in Hixkaryana either.

LDSQ also requires that 1.1.2.4 should cover "adverb clauses", i.e. clauses functioning as modifiers of location, manner, purpose, cause, condition, result, or degree (1.1.2.4.2.1 – 1.1.2.4.2.7). On these, Derbyshire says:

> 1.1.2.4. Adverb clauses
>
> The nearest equivalent to adverb clauses is what I have called adverb pseudo-clauses, for the same reason that I use the term ``pseudo-clause'' in connection with nominal constructions (see 1.1.2.2.6). These adverb pseudo-clauses are either (i) postpositional phrases with a derived nominal as head of the phrase, or (ii) constructions whose nuclear element is a pseudo-nominal, without a postposition...

Thus Hixkaryana also lacks finite clauses serving adjunct function; it uses noun phrases (NPs) or phrases headed by adpositions (postpositional ones, henceforth PPs).

One other relevant thing Derbyshire reports (Section 1.3, p. 45) is that "There are no formal means in the language for expressing coordination at either the sentence or phrase level". The English coordinators *and*, *but*, and *or* have no direct equivalents.

To summarize, everything one can immediately think of that might be used as the basis of an argument that sentences could be of arbitrary length in Hixkaryana is ruled out. Hixkaryana could have been mentioned among the languages I discussed in Section 1 for which the possibility of an infinite sentence inventory had been questioned in the literature long before 2005.

3.2 Everett's 1986 grammatical sketch

Everett's description of Pirahã (1986b), a revised English version of the descriptive part of his PhD dissertation occupying 125 pages of *HAL 1*, is considerably more than a sketch. It gives Section 14 (p. 262) a longer introduction than other descriptions in *HAL*, postponing exemplification for the more detailed subsections that followed. He mentioned topics like nominalization, parataxis, and the expression of temporal and conditional adjuncts, and but mostly commented on the complex verb morphology of the language, which allows for new verbs to be formed by including more than one verb root in a single word. Everett calls this "verb incorporation", mentioning the phenomenon known in relational grammar as clause union, but what he calls verb incorporation lacks two defining features of clause union: the amalgamated verb roots are invariably understood with the same predicand, and (significantly) he mentions that evidence of "underlying bisententiality" is absent

Everett states unequivocally that "There is no preclausal complementizer such as English *that* in Pirahã" (p. 262). In the early 1980s it was of course very natural to look for a "complementizer": Everett was strongly interested in government-binding theory (his dissertation title includes the words "and the theory of syntax"), and he wanted to show how transformational grammar would apply to Pirahã. But there was no COMP node to be found, because there were no finite complement clauses for them to introduce. This means the familiar right-branching nested English constructions that we invariably exhibit to undergraduates in our syntax classes (*A knows that B said that C thinks that P*) cannot be paralleled in a single Pirahã sentence.

Having noticed this, Everett voiced his suspicions to Noam Chomsky in conversation. Directly after receiving his PhD, before *HAL 1* was published, he received a fellowship enabling him to spend a year (1984–85) as a visiting scholar at MIT, where he had a conversation that he describes as follows (Everett 2007: 12, fn. 7):

> I talked to Chomsky about my idea that there seemed to be very little evidence for embedding of any kind in Pirahã, apart from these *-sai* examples which I was beginning to question. We discussed it briefly and Noam gave me some ideas for further testing the idea. Mark Baker, writing his PhD under Noam at the time, mentioned to me one day as we were having lunch that Noam was really intrigued by the idea that a language might not have embedd[ing] (Mark said something like "You really got Noam's attention with what you told him about Pirahã" …).

Chomsky, then, had heard about the apparent lack of embedding in Pirahã from Everett himself, twenty years before the *CA* paper, and was quite interested.

Everett adds: "I had a growing suspicion that my 1982 analysis was wrong, based … on artificially and exclusively elicited data" (I return later to the highly significant issue of data elicitation), but he says he "did not take the time to work out an analysis with no hypotaxis at all until 2004, when working at the Max Planck Institute in Leipzig".

4 Subordination and nominalization

NP&R were well aware that there were Amazonian languages that seemed to use nominalizations to do the work that English would do with subordinate clauses. They make this relevant point:

> As is well known, it is quite common for embedded clauses to look more "nominal" than their main-clause counterparts, due to a partial or complete suppression of tense, aspect, or agreement distinctions found in the verbs of main clauses. Koptjevskaja Tamm (1993) adopts from Stassen (1985) the term DERANKED (vs. BALANCED) for reduced embedded clauses of this sort. Koptjevskaja-Tamm offers many examples of languages that (either exclusively or quite generally) use deranked constructions with nominal properties for complement-clause embedding. (Nevins et al. 2009b: 370)

They cite languages like Adyghe, Ancient Greek, Classical Latin, Inuktitut, Quechua, and Turkish as illustrating such "deranking", and add that "deranked embedded clauses appear to be common among Amazonian languages", citing Derbyshire (1987) and several descriptions from *HAL*, among them Wai Wai, Macushi, and the *HAL 1* chapter on Apalai (Koehn & Koehn 1986).

What they don't mention is that they are just repeating this point from Everett (2005: 629). It is Everett who cited Koptjevskaja Tamm's book. And that book is about nominalizations, not subordinate clauses. If we "rank" constituents by reference to main clause features such as tense, nominalizations could be regarded intuitively as "deranked" compared to content clauses. But nominalizations are NPs, not clauses. *We were unaware that the enemy had destroyed the city* has a subordinate clause in it, but *We were unaware of the enemy's destruction of the city* does not. After the publication of Chomsky (1970), generative grammarians ceased even trying to derive nominalizations transformationally from clauses.

What's more, linguists still do not know how to draw a clear line between embedded clauses and nominalizations. It is clear even for English. There are several constructions that can (at least approximately) express the semantic content

of a clause in a less assertion-like or prominent way. Some express the downgraded material in a clause-like constituent that lacks certain main clause properties such as tense or agreement; but others, like Hixkaryana, have only very rough semantic parallels to clause structures, exhibiting both the structure and the distribution of NPs. Consider the following English expressions related to the declarative main clause *I ate it*:

(1) a. that I ate it
[finite content clause]
b. for me to eat it
[infinitival clause]
c. me eating it
["ACC-*ing*" construction]
d. my eating it
["POSS-*ing*" construction]
e. my eating of it
[event nominalization, genitive determiner NP as agent]
f. the eating of it
[event nominalization with definite article]

Uncontroversially, (1a) is a transitive content clause, and most modern linguists would call (1b) a transitive clause too. And (1f) is certainly a simple definite NP. But in between there are other constructions. The trouble starts with (1c). Linguists differ radically on where clauses stop and NPs begin. The morphology of the head in (1c) and (1d) is no help: the *-ing* verb form is called the "gerund-participle" in *The Cambridge Grammar of the English Language* (Huddleston & Pullum 2002) because no verb in English distinguishes the form used in the progressive aspect (*I am eating it*) from the form used in (1c) and (1d). Morphology therefore does not help draw the line between clauses and NPs (after all, many words ending in *-ing*, though derived from verb roots, do not belong to verb lexemes at all).[26]

The generative literature on these constructions has considered arguments based on a wide range of phenomena; Pullum (1991) gives a systematic survey of the data. Calling (1d) an NP accounts nicely for the way it can be the object

[26] An unhelpful irrelevance, which I will ignore, is that many prescriptive usage authorities insist that (1c) is a deprecated form that should be corrected to (1d). I take this view to be untenable; the more scholarly usage manuals reject it, noting the free variation between them found throughout English literature.

of a preposition, as in *She disapproved of my eating it.* Jackendoff (1977: 222–223) accordingly takes that view; so does Pullum (1991); and so does Blevins (2005), despite having criticisms of Pullum (1991) and citing others who disagree with it.

Kiparsky (2017), however, carefully argues for treating (1d) as a clause – but a clause with the unusual property of needing (in Chomskyan terms) to be assigned case, as NPs are. That is essentially what Stowell (1981) also advocated. It agrees with Jackendoff, Pullum, and Blevins that (1d) has the external syntax of an NP, but differs by assigning it the root node label that clauses have.

Neither Pullum nor Kiparsky is very clear on the status of (1c), the so-called "ACC-*ing*" construction. Blevins argues firmly that it too is an NP. However, Rodney Huddleston convinced me, a decade after I wrote Pullum (1991), that it is a clause, and also that the "ACC-*ing*" and "POSS-*ing*" constructions are too similar in both external and internal syntax to make it plausible that one is a clause and the other is not. So *The Cambridge Grammar* treats both (1c) and (1d) as non-finite subordinate clauses differing only in the superficial case-marking of the subject. My earlier view disagrees with my later view, and I am still not entirely sure which is right. (I was lucky enough not to face an inquisition by NP&R accusing me of trying to dishonestly cover up my earlier view.)

There is much more generative literature on "ACC-*ing*" and "POSS-*ing*" constructions than I can discuss here, but the bottom line is that six decades after the earliest generative studies of English nominalization and subordination, there is still no sign of broad agreement on where to draw the line between NP and clause constituents. And if linguists are not clear where we should draw the line between clauses and nominalizations in English, we can hardly be confident about answering similar questions in vastly less-studied languages. For Nevins and colleagues to claim they know exactly where to draw the line between clauses and NPs for Pirahã is absurd hubris.

4.1 A few Pirahã examples

NP&R spend 50 pages of *Language* trawling through Everett's work looking for dishonesty. They blow plenty of smoke but come up with essentially nothing definitive. I'll discuss just three examples that might appear to be of interest because their English translations contain non-finite subordinate clauses. They can be found in Everett (1986b) Section 14.2.1, headed "Infinitives, participials and gerundives", pp. 262–263 (just the terms that might be used if the section were describing English).[27]

[27] In citing Pirahã I'll follow Everett's transcription, except that his orthographic '*x*' for the glottal stop consonant is singularly hard for a linguist to get used to, so I replace it with the IPA glottal stop symbol 'ʔ' in transcribed examples.

(2) a. Kóʔoi soʔóá ʔibíibihaí tiobáhai bíio kai-sai
Kóʔoi already order.PROX.RELCERT child grass do [+*sai*]

b. hi obáaʔáí kahaí kai-sai
3rd see/know.INTENS arrow make [+*sai*]

For (2a) and (2b) Everett gives English translations containing infinitival subordinate clauses. His free translation of (2a) is 'Kóʔoi already ordered the child to cut the grass' (where 'RELCERT' is an epistemic mood suffix signaling a report of something relatively certain). His translation of (2b) is 'He really knows how to make arrows.' NP&R seize upon these as examples of the subordinate clauses that Everett is supposedly now trying to conceal. But Everett actually took both to be nominalizations correspond to the subordinate clauses in English (an echo of the way Derbyshire had found nominalizations doing the work that English does with subordinate clauses). Both have the verb stem *kai*, which is like French *faire* in covering the meanings of both 'do' and 'make'. The constituents at issue are *bíio kai-sai* (grass-doing) and *kahaí kai-sai* (arrow-doing).

In the 1980s Everett thought *-sai* was a nominalizer, glossing it 'NOMLZR', and he continues to gloss it as 'nominalizer' in the *CA* article (where it is misprinted several times as 'NOMINATIVE' owing to careless proofreading). This could mean that the examples might have been better translated as 'Kóʔoi already assigned the child the grass-cutting' and 'He really knows arrow manufacture.' NP&R, of course, have no idea whether the NP analysis is correct, or whether we are looking at subjectless non-finite VPs.

The results of their poring over Everett's work cannot be construed as adequate support for the claim they want to make – that Pirahã has clause embedding of the sort familiar from the Indo-European languages.

A few pages later Everett gives (in his (290) on p. 278) example (3), which might look more promising as a case of a subordinate clause.

(3) hi ti ʔapi-sai ʔogi-hiab-a
3rd 1st go [+*sai*] want.not.REMOTE

It consists of a 3rd-person pronoun, a 1st-person pronoun, a verb meaning 'go' with *sai* suffixed, a verb stem that means 'want', the negative suffix *hiab*, and the remote aspect suffix *a* (on which see Everett 1986b: 293–294). In his early work, up to 1986, Everett thought it might best be translated as 'He doesn't want me to go.' NP&R seize upon it as a highly significant case of his having cited a sentence with a subordinate clause in object position preceding a matrix verb of desiring (see their (23) on p. 375). It surely could not be plausibly treated as two successive main clauses in paratactic relationship.

But (3) is problematic in a way that Nevins et al. were unaware of – and here they fell victims to their policy of avoiding all contact with Everett. Looking back at the origin of sentence (3), Everett recalls that he constructed it himself, and asked speakers whether it was acceptable – a use of the problematic "can-you-say" question.

Everett was never able to make much use of questions put to speakers in his language learning. "How-do-you-say" questions (Samarin 1967: 114, Ch. 6, Sakel & Everett 2012: §6.4) were ruled out because he had no contact language in which to ask them.[28] Hardly any Pirahã men (and none of the women) have even the crudest smattering of Portuguese (again, see Sakel 2012); no one raised as a native speaker in the Pirahã community seems ever to have subsequently become fluent in Portuguese. Everett does mention that early on he would sometimes be able to point to something and ask "How do you say that?" (Everett 2008: 20) – presumably to elicit a noun; but that won't do for most concepts.

Later on, when he had attained a basic grasp of the language, he relied a lot on "perambulatory elicitation" (Everett 1986b: 200), which means walking around the village chatting to people. But that still cannot be called upon to elicit some key form that will help resolve some puzzle about syntactic possibilities. When his conversational abilities had improved enough, therefore, Everett sometimes used "can-you-say" questions. These have the advantage of being usable in a fully monolingual situation, given only enough command of the target language to express the question "Can you say *S*?" and pronounce the conjectured candidate utterance *S*. So it becomes possible, at least potentially, to check hypotheses about what is grammatical. But of course you don't know what you're going to get.

This mode of proceeding calls for great caution, especially when working with linguistically unsophisticated speakers (which will be most speakers of most languages in the world, of course). "Can you say" questions presuppose that the consultant will understand that the *S* is being mentioned, not used, and that the linguist is not asking for permission to say something, or asking about physical possibility, but rather wants a judgment of concerning grammatically correctness in isolation from context. What Everett discovered in later years was that the Pirahã had regularly been saying "Yes" to his occasional "Can-you-say-*S*?" questions, just to humor him, even if the *S* was decidedly unidiomatic.

Everett was caught out by exactly this behavior in another case. Early in his study of Pirahã he assumed it obviously should be possible to have more than one attributive modifier in the structure of a Pirahã NP, just as in English. In

[28] Fastidious field linguists shun them anyway, even when a contact language is available. Bloomfield never used them at all, according to Voegelin (1960: 204).

example (268) of Everett (1986b: 273) he cited (4) as the largest NP he had in his corpus (and I give his 1986 glosses):

(4) kabogáohoi biísi hoíhio ʔitaíʔi
barrel red two heavy
'two heavy red barrels'

The two modifiers might suggest modifiers can be stacked in NP. But he had made several errors with (4). The example wasn't really in his corpus in any strict sense. He expressed unease even when citing it, acknowledging that the example "is rather artificial" and "was not taken from textual material but rather was separately elicited". He later became convinced that the example is ungrammatical. Just as he discovered that *biísi* (based on *bií* 'blood') means 'bloodlike' rather than 'red', and *hoíhio* doesn't mean exactly 2 but rather 'a couple' or 'a bit' (in a vague sense that implies roughly 2 or 3 with count nouns), he also learned that it was another case of informants who said things were fine as a way of being tolerant of his imperfect grasp of their language: they would nearly always assent to his "can you say" questions. When he finally persuaded a speaker to give him the straight truth on whether (4) was acceptable, he was told: "Pirahã don't say that. You can say that. You are not Pirahã" (Everett 2009: 422).

The same sort of thing seems to have happened with (3). Since Everett never recorded anything like it in spontaneous use, he recently decided to seek a second opinion on it from Keren Madora (the only outsider who has lived with the Pirahã longer than Everett, and the only other outsider who is truly fluent in Pirahã). Formerly married to Everett, today she still lives very near the Pirahã area and is in regular contact with speakers. Her opinion (email, Madora to Everett, 10 January 2023) was that he is correct, (3) is ungrammatical. Pirahã speakers never spontaneously say anything like (3).

Highly relevant information concerning the suffix *-sai* was published in 2010 but was not available in 1986 or 2009. New empirical evidence indicates that *-sai* is not a nominalizer at all. Two of the only linguists outside of SIL who have worked directly with Pirahã speakers in a context where they could get reliable translations, Jeanette Sakel and Eugenie Stapert, constructed some test sentences by concatenating two Pirahã clauses translatable as 'it's raining' (*piiboibai*) and 'I don't go' (*ti kahápihiaba*), intended to suggest the meaning 'If it's raining, I don't go', and suffixing *-sai* to either the first clause or the second. They then asked nine speakers (seven women, two men) to simply repeat back what they'd said. They found that the informants' responses might have *-sai* on the first clause, or the second, or both, or neither, regardless of which input sentence they were given.

Their conclusion (see Sakel & Stapert 2010: 5–6) is that *-sai* "does not appear to be a marker of subordination, as originally claimed by Everett (1986)" (and they mean that it is not a marker of nominalization either). Everett agrees, and now believes it may be an optional marker for sentences conveying discourse-old information. Its random placement in sentence repetitions would be as expected if its old-information signaling role only made sense in a discourse context: speakers charged with repeating two sentences with no context apparently recalled vaguely that there was a *-sai* in there somewhere, but didn't necessarily remember where.

What does it mean for sentences like (2a) and (2b), if *-sai* might not be either a nominalizer or a subordination marker after all? I'm not sure. And I don't think anyone really is. But when looking at attested Pirahã examples, with their short clauses and unclear syntactic linkages, it is definitely useful to recall the perceptive remarks of Liberman (2006) on Language Log, published before either the Nevins boycott move or the first draft of NP&R's paper, about sentences in conversational English as recorded by novelists with a good ear for colloquial speech. Liberman gives examples from Elmore Leonard. One character is quoted as saying things like 'We get to a phone, we're out of the country before morning.' In the context it is clear that the intended meaning is conditional. One can imagine such a speaker saying, 'It's raining, I don't go.' Everett cites very similar examples of what he then thought were conditional clauses. For example (Everett 1986b: 265, ex. (241)):

(5) Paió hi abópaisaí ti ʔií oáboíhaí.
Paió hi ab-óp-ai-sai ti ʔií oá-boí-haí
(name) 3sg turn-go-ATELIC-COND I thing buy-come-NEAR-CERTAIN
'Paió comes back, I'm gonna buy something.'

I am not in any way suggesting that everything is now resolved and the picture is clear. Far from it. We have no truly reliable principles to use in order to decide whether some Pirahã construction is more analogous to *if he returns* or *him returning* or *his returning* or *his return*. All sorts of unclarities remain. Everett acknowledges having made errors in both elicitation and analysis; in 1986 he thought *-sai* was a morpheme forming subordinated constituents of some kind, probably nominalizations that played the role subordinate clauses would play in European languages, but after the convincing work of Sakel and Stapert he no longer thinks that. It has been definitely confirmed that *-sai* sometimes appears on what in English might be a subordinate clause but also sometimes appears on what in English would be a main clause.

In 1986 Everett also thought there were two *-sai* morphemes, differing in tone, but subsequent F0 measurements by Miguel Oliveira have revealed no statistically significant tonal difference (a rough set of slides presenting the results was made available as Oliveira & Everett 2010). Everett now thinks there is just one *-sai*.

Given the present state of our knowledge, we certainly cannot say that NP&R have refuted Everett's thesis about Pirahã sentences never exhibiting clause embedding. One might perhaps argue that the case is still open, but not that NP&R examined the matter and settled it – which is what far too many linguists (Chomsky included) have been lazy enough to assume. Simply citing Nevins et al. (2009b) without getting into any of the details is not sufficient. Those who are truly intent on trying to support the ungracious claim that Everett lied are going to have to start learning Pirahã.

4.2 The crucial issue of embedding depth

There is a vital point about nominalizations that NP&R either failed to notice or chose not to mention. What we really need to know, if we are to address the only issue that makes this discussion sensible, is whether a Pirahã nominalization (or non-finite clause or whatever) can be embedded inside another, and the result inside another, and so on, to arbitrary depths. NP&R struggle to find even a single case of a fully clear subordinate clause in Everett's early work (and they never venture to propose a structure for even a single sentence), but they certainly never even touch on the matter of showing embedding that can be reiterated to arbitrary depth. Nothing they say suggests that subordination in Pirahã (if it has any) can give rise to sentences of arbitrary length. And that is what any serious notion of "recursion" has to be about.

In Standard English, after more than a thousand years of literacy (which O'Neil 1977, Givón 1979, Mithun 1984, and Kalmár 1985 suggest might be a crucial consideration) now has fairly rich nominalization resources: even a clause like *A knows* [*that B said* [*that C thinks* [*that D predicts* [*it will rain*]]]] can be paired with a cumbersome NP analog like *A's knowledge of B's statement about C's opinion concerning D's prediction of impending rain* with roughly the same content.[29] But are such multiple embeddings of NPs constructible in every language? I have never been able to see a way in which the nominalization resources of languages like Hixkaryana, Apalai, or Pirahã could be used to replicate any such internally

[29]The reader might like to consider whether one could construct a nominalization that exactly captures the content of the husband's thought in Bruce Eric Kaplan's well-known *New Yorker* cartoon (26 October 1998), where a man earnestly assures his wife: "Of course I care about how you imagined I thought you perceived I wanted you to feel.".

ramified NP constructions. The most that NP&R have to suggest is that in one or two Pirahã examples there may be depth-1 subordination of a non-finite secondary predication, but they really only have what look like adsentential modifying phrases appended to a clause. They cannot exhibit Pirahã evidence supporting the claims of so many linguists that iterated embedding in human languages is always allowed to unbounded depth. That is the claim Everett was challenging.

5 Hallucinated PP self-embedding

The work presented in *Recursion Across Domains* (Amaral et al. 2018) is of astonishingly low quality, replete with glaring mistakes. The review by Everett & Gibson (2019) provides a selection of the evidence, concentrating most on Pirahã, on which the authors had worked together in the field. For the second review that *Language* commissioned, the editors certainly found the right man for the job: Hornstein (2019) faithfully repeated Chomsky's theoretical position on "recursion", elaborating the rhetorical escape-hatch arguments (see Section 1 above), and then proceeded to uncritically endorse all data-oriented contributions in the book regardless of their merits. Thus he reported that by using a truth-value judgment experiment Uli Sauerland (2018) had managed to "provide pretty dispositive evidence that Pirahã allows sentential embedding under 'say' " (p. 796). In truth Sauerland's statistical analysis has vitiating flaws, and when his experiment is run on English speakers it does not produce the results that would be needed to support his claims anyway (see the analysis by Everett & Gibson 2019: 781–784, who took the trouble to review the use of statistics and test his experimental design on English speakers, and the more detailed critique by Gibson (2024), Chapter 1 of this volume).

I will not attempt a general survey of the material in *Recursion Across Domains* here, but I will just address a particularly incompetent chapter about Pirahã PPs. Neither of the *Language* reviews mentioned the stunning error, and presumably none of the referees for the book noticed it either.

The chapter by Filomena Sandalo, Cilene Rodrigues, Tom Roeper, Luiz Amaral, Marcus Maia, and Glauber Romling da Silva (2018) claims that Pirahã syntax allows PPs to be embedded inside other PPs, and reports experiments purportedly showing that native speakers have no difficulty in processing and interpreting such phrases. The authors assume (as is clear from their (15) on p. 285) that the English phrase *the coin on the paper on the chair on the board* has a right-branching structure with a single NP constituent containing all the PPs as modifiers of successively embedded NPs: *chair on the board*, *paper on the chair on the board*, and so on.

They claim that Pirahã has precisely analogous phrases, with two differences. First, Pirahã lacks determinatives such as the English definite and indefinite articles; accordingly, it makes sense to ignore articles in the English structure shown below – it simplifies the tree structure considerably. And second, Pirahã PPs are postpositional. The right-branching structure for English diagrammed (without articles) in Figure 1 is claimed to have an analogous left-branching structure in Pirahã with the terminal string '*tabo apo tiapapati apo kapiiga apo gigohoi*'. (Sandalo et al. mistranscribe all of these words, but I set that aside that for now.) Taking into account "the fact that Pirahã is a head-final language", they assume that an English structure in Figure 1 – where I omit determiners to save space – has an exact analog in Pirahã, which they depict (in their (19) on p. 287) as shown in Figure 2.

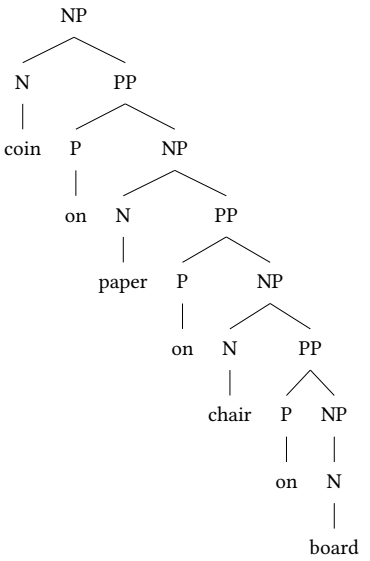

Figure 1: PP modifiers of NP in English.

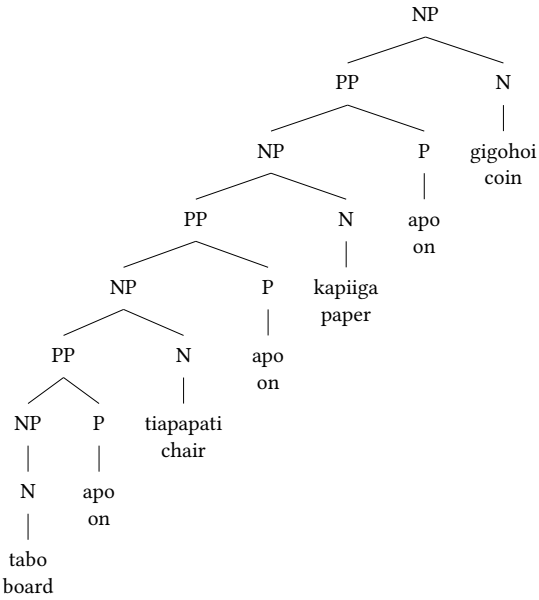

Figure 2: Sandalo et al.'s (2018) Pirahã PP structure.

Sandalo et al. have overlooked a crucial syntactic fact. Pirahã is NOT a uniformly head-final language. As Everett noted forty years ago, in the noun phrase "modifiers follow, while possessors normally precede, the phrase head" (Everett 1986b: 272). He lays out the sequence of elements in the NP as follows (p. 273):[30]

[30]See also Everett (1983: 132ff). Pirahã has no true numerals in the sense of names for the natural numbers, but presumably its vague quantity-related items like *báagiso* or *ʔaíbá* 'many', *ʔogií* 'a lot', and *ʔoíhi* 'few' take that slot in the NP.

(6) (POSSESSOR) + (PRO.CLITIC) + N + (MODIFIER) + (NUMERAL) + (DETERMINER)

The vital point is that modifiers follow the head in NPs. So if there were noun-modifying postpositional PPs embedded in NPs within other such PPs, the result would be nothing like the fictive left-branching tree in Figure 2. In fact there's a good reason that languages with nouns postmodified by PPs don't allow iteration of the construction: it yields center-embedding of the sort that poses major difficulties for human sentence processing – the kind seen in English center-embedded sentences like ?? *The children the women the soldiers left saved protested.*

The purported phrase Sandalo et al. are trying to diagram would actually come out as in Figure 4, where I correct the transcriptions and word identification as well as the structure.

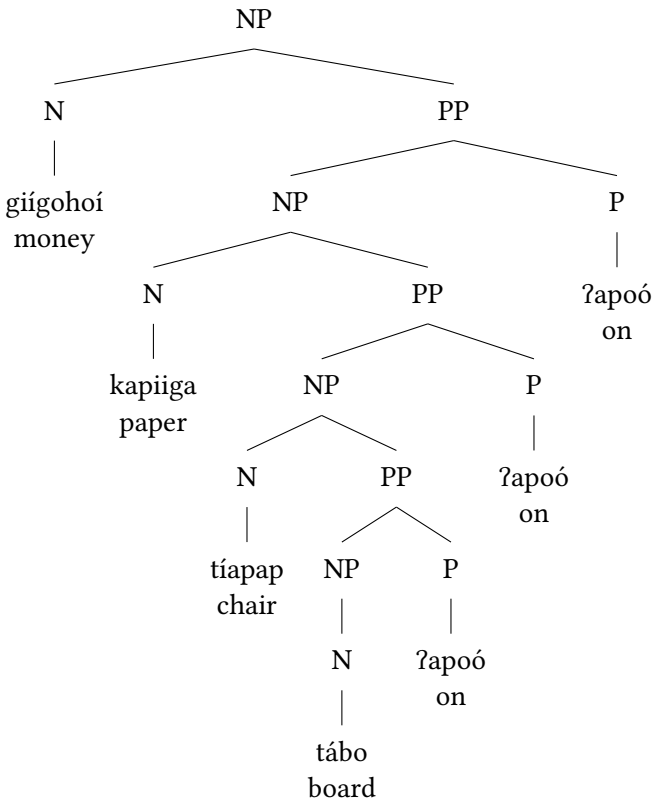

Figure 3: Expected structure if Pirahã had nesting of PP modifiers in NPs

No one has ever suggested that PPs like in Figure 4 are encountered in Pirahã discourse, and no such structures were presented to Sandalo et al.'s hapless informant.[31]

It is difficult to guess what must have gone on in their experimentation (they stress that it is to be regarded only as a pilot study). They claim to have found that a native speaker named Iaoá understood their pronunciation of the purely fictional phrase (Figure 2). Given that the word they write as *tiapapati* seems to be the imperative verb *tíapapaáti*, meaning 'sit down' (Everett & Gibson 2019: 786–787), Iaoá would have heard them as saying something that meant roughly 'Sit on the board. On top. On the paper. Money.' The corrected string is given in (7):

(7) tábo ʔapoó tíapap ʔapoó kapiiga ʔapoó giígo-hoí
board on chair on paper on money

The most likely guess at how Iaoá or any native speaker would have parsed this would be as a list of successive PPs and a final NP, as in Figure 4.

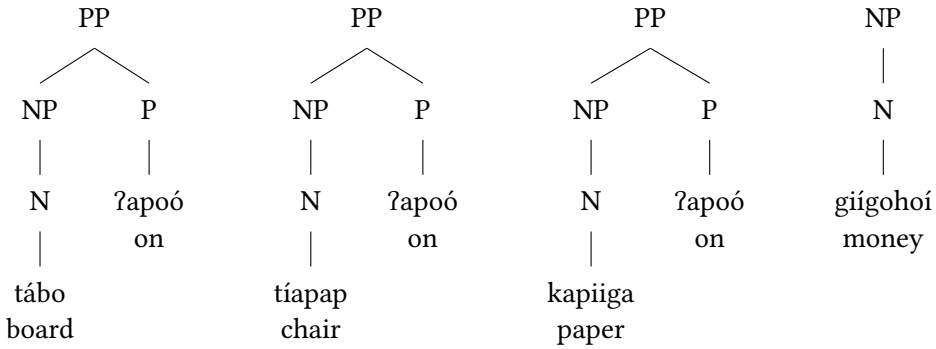

Figure 4: Most likely native-speaker parse of (7)

Convinced that they had identified nested PPs in Pirahã, Sandalo et al. (2018: 289–292) proceeded to construct some test sentences paired with pictures of alligators on mats on rocks on beaches, and claim to have used them to produce

[31]Later they give a second similar structure for NPs containing PP modifiers which is best ignored. Their (21) on p. 287 has nodes labeled "PP*" dominating other nodes with that label. On p. 284 they say they are using "notation adapted from traditional Kleene* system" [sic], but Stephen Kleene's star notation symbolizes a unary operation mapping a set of strings to its reflexive and transitive closure under concatenation. It makes absolutely no sense in a node label.

evidence for interpretation of nested PPs. They claim a picture of an alligator on a mat on a beach was reliably distinguished from a picture of an alligator on a mat beside another alligator on a beach. Further discussion of this experiment is not really feasible; their account is too ill-informed and confused, replete with botched transcriptions, mistaken glosses, misidentified words (*tahoasi* is glossed as 'mat' when it actually means 'beach' – the word for 'mat' is *paahóísí*), and so on.

In another experiment they tried to get Iaoá to play a "game" involving coins being put on a paper that was on a chair on a board, or on a paper on a chair, or on a paper on a board. They note (p. 294) that where they supplied a string like "gigohoi kapiiga apo tiapapati apo tabo apo" (intended to be *giígo-hoí kapiiga ʔapoó tíapap ʔapoó tábo ʔapoó*, glossed 'coin paper-on chair-on board-on'), when Iaoá repeated the string "he switched the order of the PPs in the sentence", yielding what they wrongly transcribe as 'tabo apo tiapapati apo kapiiga apo gigohoi' ('board-on chair-on paper-on coin'). This was a sign of something gone terribly wrong: Iaoá was unable to come anywhere near repeating what they thought was a single NP in his language. But in an almost unbelievable fit of wishful thinking (hope springs eternal in the human breast), they interpret this as "spontaneous evidence" in favor of their hypothesis! It seems more likely that Iaoá scarcely knew what was going on, but took their attempted PPs to be independent phrases, not successively embedded modifiers in an NP, and repeated them back in LIFO (last in, first out) order.

There is also a very simple semantic observation that may play a role in interpreting the events that they take as vindication of their hallucinated PP embedding claims. We normally take the 'on' relation between medium-sized physical objects to be transitive. Any coin on a piece of paper on a chair is also a coin on a chair. Any alligator on a mat on a beach is an alligator on a beach.

The most plausible conclusion from Sandalo et al.'s bungled experiments is that Iaoá parsed the fictive PPs individually, and then (with the sharp general intelligence Everett has always noted among the Pirahã) simply guessed what the linguists wanted him to do.

6 Sentence-length extensibility more generally

As promised earlier, I have avoided the impenetrable thickets of confusion found where linguists use the words "recursive" and "recursion"; I have focused instead on the clearer issue of syntactic devices that can in principle allow the construction of sentences of arbitrary length.

The issue does not have the fundamental importance that some have seen in it. Linguistic creativity is not tied to any claim about an infinitude of sentences, since human linguistic creativity (as Everett has often stressed) resides mainly at the discourse level. Nor is it tied to the ability to grasp concepts. Absence of propositional attitude verbs in a language, for example, does not entail speakers' inability to engage in metacognition. Everett deftly illustrates how a complex proposition with a logical form like [*if* [*P and Q*]] *then R* does not need to be expressed in one sentence when he titled a conference paper: "You drink. You drive. You go to jail. Where's recursion?" (Everett 2010).

Everett's opponents seem to assume that linguistic life with only simple main clauses would hardly be worth living. But there is no reason to regard a language lacking unbounded sentence extensibility devices as less useful or expressive than a language. Kornai (2014) argues that the information-carrying complexity of a finite language can actually be greater than that of an infinite one.

One way of stressing the difference between finite and infinite languages, often touched on in undergraduate textbooks, depends on pointing out that for a finite language the grammar could be given in the form of a simple list of sentences. But that was never a very sensible point to harp on. From the complexity of verbs alone (Everett 1986b: 288–301) it is apparent that the set of Pirahã sentences would be way too vast even to be compiled, stored, or accessed by either a brain or a currently imaginable computer, let alone to be of real online use either cognitively or computationally. The grammatical complexity of Pirahã would still pose the usual problems for the theory of language acquisition: inducing generalizations from exposure to data would have to be involved, not just memorizing complete utterances. As Gibson (2024), Chapter 1 of this volume argues, what's important is compression of information (Kolmogorov complexity), not infinitude.

Whether the set of all sentences in a language is finite or not is in any case inherently difficult to settle, for a number of reasons, and would remain so even if all of Everett's specific claims about Pirahã syntax are accepted.

First, the lexicon has to be stipulatively fixed at some finite number N of words, though we have no clue about what N might be because new words (e.g. personal names) are being coined all the time, and the interaction of agglutinative word formation and lexicalization in languages like Turkish or Inuktitut makes it implausible that there is any such N at all.

Second, the notion "sentence" needs a clear definition; syntacticians casually assume it is a well-understood primitive term, but it is not easily defined at all. Separating a passage of spoken language into sentences in a way that a different linguist would replicate is very difficult, and beset with problems raised by

false starts, parenthetical interruptions, direct quotations, appositional expansions, rhetorical repetitions, whatever semicolons represent in writing, and asyndeton (coordination without coordinator words, as in Dickens's *It was the best of times, it was the worst of times, it was the age of wisdom, it was the age of foolishness …*').

Third, with regard to hypotaxis (subordination), Pawley & Syder (2000) argue that it hardly occurs at all in spontaneous speech, even in English, once we set aside a limited number of high-frequency partially customizable schemata like *I think* ___ or *It depends whether* ___, and similar formulas. This would presumably be all the more true for languages spoken in cultures where no one writes or reads. A few folk tales or epic poems might have a broadly fixed (or even faithfully memorized) traditional form, but most language use will be informal chatting, and Pawley and Syder claim that spur-of-the-moment construction of hypotactic sentences will be rare to nonexistent.

There are other phenomena that could introduce difficulties: NP apposition, roughly definable as adjacent iterated NPs with the same reference and syntactic function (Karlsson 2010 cites an attested five-NP example in Swedish); intensificatory or iconic repetition of attributive adjectives (*a big, big, big problem*) or adverbs (*I really, really mean it*) or VPs *They hit me and hit me and hit me …*) or NPs (*cows, cows, … cows, as far as you could see*). Such possibilities are seldom noted in reference grammars. Only study of large corpora of texts will tell us whether such iterable sentence-lengthening constructions are found in the syntax of an exclusively oral language like Pirahã.

How might we even estimate the likelihood that Pirahã truly has no unbounded syntactic resources for sentence lengthening? A beautiful and oddly neglected paper by Widmer et al. (2017) addresses this question. Widmer and colleagues suggest some additional methods that could be employed to figure out the probability of a language lacking such resources. They identify five ways in which NPs in Indo-European languages can be lengthened by embedding other NPs inside them: stacked genitive determiners, adjectivization-derived modifiers, modifiers with head marking, adpositional modifiers, and simple noun juxtaposition (I assume apposition is to be included under the latter heading). They show that Indo-European languages have repeatedly developed such devices and also lost them through syntactic change over the past few thousand years.

Through a clever calculation they then assess how likely an Indo-European language is to end up at a given time with at least one such device in its NP syntax, concluding that it is very high indeed: they estimate that with probability ~0.98, any Indo-European language, at any given point in its history, will have at least one grammatical device for arbitrarily expanding NPs. As an explanatory

conjecture, they suggest that for some reason the human processing capacity finds it helpful for there to be some such mechanism provided by the grammar.

However, they add (p. 822): "With regard to sentence-level syntax, it remains an open question whether syntactic recursion or simple conjunction is preferred.". To settle it, "a larger sample of data would be needed". We cannot know what the answer is, or how likely it is that any arbitrary language in the world (not just in the Indo-European family) would have some kind of iterable sentence-lengthening syntactic device available at all times in its history. But suppose the probability of languages having such features were as high as ~0.99. It would still be expected, given the 7,000 languages attested in the world today, that there might be 70 languages or more in which such devices are absent. The literature on ancient languages and languages of preliterate cultures has thrown up quite a few candidates, as discussed in Section 1. Pirahã just happens to be the clearest case – and the one that kicked the hornets' nest politically.

7 Conclusions

No one should claim, in the present state of our knowledge, that we have a good understanding of the syntax of Pirahã (or for that matter any other language, even Standard English). The corpus study of Pirahã syntax by Futrell et al. (2016) is a sterling effort at utilizing what materials we have (specifically, parsing texts collected by Steven Sheldon in an effort to find evidence of subordination), but in many ways it just underlines how woefully unclear things are. Much more work has to be done.

That work will not be accomplished without collaborations that involve people who (i) have no advance commitment to particular results or empirical claims and (ii) are prepared to spend time paying close attention to everyday usage in the Pirahã speech community. That will mean extended residence in Pirahã villages, and consultation with people who have substantial experience with the language.

Such people exist. Steven Sheldon worked on the language from 1967 to 1976, and knows it well. Caleb Everett, Kristene Diggins, and Shannon Russell all learned to speak and understand the language when living in Pirahã villages as children, and their parents Daniel Everett and Keren Madora are outsiders with unprecedented fluency. Madora has studied the language in depth since 1977 and still lives close to the Pirahã villages; Everett spent a total of about eight and a half years with the Pirahã between 1977 and 2006, and made various visits thereafter, becoming fully fluent in the language. He translated the *Gospel of Mark* into it (Everett 1986a). Yet NP&R decided to work without having a single conversation with any of these people.

This represents a sadly missed opportunity. If linguists like NP&R had applied their analytical theoretical abilities to the available data in a collaborative spirit, drawing on the knowledge of active speakers of the language (particularly Everett himself), new linguistic insights might have been gained. That chance has been lost, probably forever. They have wrecked their credibility by making it so obvious that from the start they aimed simply to bring Everett into disrepute. All that linguistics ended up getting out of their work was an uninformed retrospective document review. They have divided linguists into two irreconcilable warring camps, and made the entire discipline of linguistics look, as it did to Tom Bartlett, like a snakepit of hostility.

Like any scientists, linguists have a duty to maintain ethical standards and intellectual open-mindedness – even when someone is claiming Chomsky was wrong about something, or when the popular press tries to fluff up a science story into something earth-shaking or theory-trouncing and publishes absurd overstatements.

Certainly it was ridiculous hyperbole for *New Scientist* (18 March 2006) to call Everett's account of Pirahã "the final nail in the coffin for Noam Chomsky's hugely influential theory of universal grammar". If we're honest we'll admit that Chomsky does not have enough of a detailed theory of universal grammar to constitute a full coffinload. Nor do his opponents have solid enough empirical accounts of language acquisition to nail down the lid of such a casket anyway.

It was similarly absurd for the *Chicago Tribune* (10 June 2007) to suggest that Everett's work is analogous to a high-school physics teacher finding "a hole in the theory of relativity"; but we all know that sort of thing often happens when popular news media try to cover science. Providing better and clearer hype-free accounts of our work to science journalists will be an enduring burden, but one that we all have to shoulder. Calmly, and with some understanding of the fragile and difficult business of popular journalism.

I can well imagine how irksome it has been for Chomsky to see overblown hype about a putatively theory-shaking discovery in the jungle repeated in scores of news sources. But that doesn't justify the petty spite of his "charlatan" remark to *Folha de S. Paulo* in February 2009, or his assertion that "Daniel Everett's contributions are basically nothing" in a 2021 video interview.[32]

Over the past four decades, Everett can be fairly said to have done more for Amazonian linguistics than any other linguist now living. His detailed descriptions of Pirahã and Wari' are lasting contributions, as is his energetic promotion and encouragement of descriptive work on other Brazilian languages. His basic

[32]https://www.youtube.com/watch?v=UBla-h36ywA

claim about Pirahã syntax not permitting unbounded sentence length is very probably true. He did not deserve the years of hot-tempered public allegations and insults (or the worse incidents of insult, hate mail, and shouting in his face that he does not publicly report). A sector of our field seems to have lost its moral compass over this issue.

It speaks well of Everett that never in all the years since 2005 has he responded to his tormentors with insults or abuse: he argues points of fact, but he refrains from accusing his enemies of scientific misconduct, devious motives, or self-interested mendacity. For that, and much more, we should salute him.

And as regards the validity of the accusations hurled at him by his many opponents, none of them familiar with the lives and spoken language of the Pirahã, I quote in conclusion the opinion of a young Brazilian anthropologist writing recently about Pirahã culture (Felizes 2023: 59):

> 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 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.]

That is the view formed by an independent third party with a personal commitment to studying the life of the Pirahã, some who has spent time in Pirahã villages, made the acquaintance of Keren Madora [formerly Everett], and witnessed the consequences of the Everetts' 46 years of friendship with the Pirahã at first hand.

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and statements from suitable sources or personal reminiscences as far as that was possible. I thank many friends who have spent time reading drafts, answering my questions, helping me to verify facts, saving me from errors, and making useful suggestions. Among them are Judith Aissen, Ash Asudeh, Peter Austin, Jim Blevins, Bernard Comrie, Peter Culicover, Hope Dawson, Lise Dobrin, Ted Gibson, John Goldsmith, Randy Allen Harris, Lloyd Humberstone, Brian Joseph, John Joseph, Ed Keenan, Bob Ladd, Pim Levelt, Bob Levine, Noah Ley, Joan Maling, John McWhorter, Philip Miller, Stefan Müller, Georgia Morgan, David Nash, Johanna Nichols, Steven Piantadosi, Steven Pinker, Jerry Sadock, Jeanette Sakel, Rich Thomason, Sally Thomason, Tom Wasow, Rebecca Wheeler, Melinda Wood, and Annie Zaenen. Correspondence with David Pesetsky and Cilene Rodrigues helped me to improve the accuracy of certain claims in Section 2. Errors may remain, and they are solely mine. Presentations of some of the content of this paper were made during 2023 at MIT, George Mason University, the Max-Planck Institute for Psycholinguistics in Nijmegen, and the 2024 meeting of the North American Association for the History of the Language Sciences.

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