

From:

Language Complexity as an Evolving Variable

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An interview with Dan Everett

One of the most startling recent events in linguistics was the publication in 2005 of an article by Dan Everett (then of the University of Manchester, and since 2006 chairing the Department of Languages, Literatures, and Cultures of Illinois State University) on the language and culture of the Pirahãs, a tribe of a hundred or so people in a remote area of Amazonia. Pirahã, as Everett describes it in his *Current Anthropology* article, lacks many basic features often seen as essential to any human language. For instance, it is totally devoid of grammatical recursion, and it has no means of making precise statements about quantities – Frank, Everett, et al. (forthcoming) describe experiments which appear to establish that Pirahãs cannot distinguish even between “one” and “more than one”.

Everett’s account has naturally sparked intense controversy among academics; and his portrait of the remarkable Pirahã world-view has attracted great interest among the public at large. A new book, *Don’t Sleep, There are Jaguars: lessons on life, language, and thought from the Amazon*, is to be published simultaneously by Pantheon in the USA, Profile Books in Britain, DVA in Germany, and Flammarion in France. Film rights are under discussion.

In view of the fascinating and extremely controversial nature of Everett’s work, the Editors felt that rather than including a written version of his Leipzig talk here, it might advance our understanding of the overall workshop topic better to publish an interview challenging Everett on some of the points where linguists have felt sceptical. Happily, Everett was agreeable to this, and the “interview” was conducted via e-mail exchanges {between Sampson and Everett [or all of us?]} after the close of the workshop.

GRS: Most of your critics appear to reject your portrayal of the Pirahã and their language on *a priori* grounds – because the Pirahã language as you describe it

contradicts generative theories about language universals to which they are wedded, and/or because of political objections to the idea that one human culture might be simpler, cruder, or more primitive than another. I have no quarrel with your picture on either of those grounds. I share your belief that there is no evidence for innate language universals (Sampson 2005). And it strikes me as just daft to suggest that all human cultures are equally sophisticated. It seems obvious, for instance, that my own ancestors of fifteen centuries ago, the Anglo-Saxons of post-Roman, pre-Christian England, had a cruder culture than that of, say, their descendants and my recent ancestors in the 19th–20th centuries. If an enclave of Anglo-Saxons survived somewhere with a way of life unchanged from the sixth century, we would doubtless see them as children in adult bodies. Indeed it is really paradoxical for professional academics to deny the existence of such differences: if we ask what purpose the academic profession serves, a standard answer is that its function is to maintain and further refine the cultural heritage of society – which implies that societies can differ in levels of culture.

So I have no “ideological” differences with you. Still, I do wonder whether we are justified as seeing the Pirahã as so extremely distant from ourselves as you claim.

DLE: On the *a priori* objections, I find these troubling at various levels and it seems important that they be confronted head-on. First, there is the idea that if it is claimed that a culture is primitive in any sense, the claimant is thereby asserting that the people of this culture are inferior to other people. I deal with this at length in *Don't Sleep, There are Jaguars*. Here is what I say in one of the later chapters:

Is it possible to live a life without these crutches of religion and Truth? The Pirahãs do so live. They share some of our concerns, of course, since many of our societal preoccupations derive from our biology, independent of our culture (our cultures attribute meanings to otherwise ineffable, but no less real, concerns). But they live most of their lives outside these concerns because they have independently discovered the usefulness of living one day at a time. The Pirahãs simply make the immediate their focus of concentration and thereby, at a single stroke, eliminate huge sources of worry, fear, and despair that plague so many of us in Western societies.

They have no fictional stories. They have no craving for “Truth” – indeed the concept has no place in their values. Does this make them primitive? Many anthropologists have suggested so, which is why they are so concerned about finding Pirahã fiction and creation stories.

But there is an interesting alternative way to think about things. That would be that it is the *presence* of these concerns that makes a culture primitive, if we want to think in terms of primitive vs. non-primitive. It would then be their absence that renders a culture more sophisticated. If that were true, the Pirahãs would be a very sophisticated people. Does this sound far-fetched? As yourself whether you think it is more sophisticated to look at the universe with worry, concern, and a belief that we can understand it all, or to enjoy life as it comes, recognizing the likely futility in looking for Truth or God.

The Pirahãs’ culturally constrained epistemology can only be evaluated in terms of the results that it gives the Pirahãs relative to their own values. Since it serves them very well, there is no sense in the idea that it is inferior. In terms of overall complexity, however, one can make a case that the lack of recursion, the lack of number, numerals, and counting and so on, is more primitive in the technical sense of revealing that there are living languages that correspond to what some theorists, e.g. Ken Hale (1976) and Tom Givón (2007), have argued to be earlier stages of language evolution, when they claim that, phylogenetically and historically, parataxis and adjunction precede embedding. And there will almost certainly turn out to be other languages like Pirahã in many respects as fieldwork continues.

GRS: One of the besetting problems of social anthropology is that the most exotic, and therefore most educative, phenomena relate to inaccessible societies. As a result there has been a history of tall tales being taken seriously. One of the founders of the discipline, Bronisław Malinowski, is best known for his claim, backed up with circumstantial evidence, that natives of the Trobriand (now called Kiriwina) Islands were unaware of the male role in conception of babies – they did not believe that a person’s father was a blood relative. (In early writing Malinowski said the same was true for most Australian aboriginal tribes.) When a local District Officer of the colonial administration objected that the Trobrianders did know about fatherhood, Malinowski seems to have begun by blustering about non-scientists being unqualified

to debate with professional anthropologists, and finally more or less conceded that he was wrong (Pulman 2004) – though I wonder if one person is aware of that, for every hundred that have read about and accepted Malinowski's claim. Then there was Margaret Mead, who on the basis of a nine-month stay on Samoa with an expatriate white family in her early twenties published a romantic account of the Samoans as a carefree society entirely devoid of the stresses familiar in European life (Mead 1928); this has turned out to be an absurd travesty of the truth, seemingly based in part on her informants amusing themselves by winding her up (Freeman 1983), yet for decades it was taken extremely seriously and I believe it was even influential in American policymaking.

I certainly do not mean to accuse you of sharing Malinowski's pomposity, or the innocence which was no doubt appropriate to a well-brought-up American girl in the 1920s. Nevertheless, you will appreciate that people are bound to wonder whether Everett's Pirahã may be another case of the same syndrome. What considerations convince you that the more remarkable aspects of Pirahã culture as you describe it are actually correct, rather than a misinterpretation of some more humdrum reality? And if the answer is largely gut feeling from immersion in the culture, then what considerations ought to convince the rest of us, who have no realistic opportunity to share that immersion?

DLE: The Pirahãs are not inaccessible. I have taken over twenty researchers with me to the Pirahãs over the years, from Peter Ladefoged (UCLA) to Ted Gibson (MIT). Jeanette Sakel (University of the West of England) and Eugénie Stapert (University of Manchester) are learning the Pirahãs' language. Gibson and I have submitted a proposal for funding to eventually publish all the data I have collected in thirty years of field research on Pirahã on the internet – to provide anyone access to a searchable data base of Pirahã sounds, stories, grammar, and videos of experiments. And there are three other Pirahã speakers that I have used to check my ideas for quite some time: Steve Sheldon, Keren Madora, and Arlo Heinrichs, all of the Summer Institute of Linguistics. Sheldon lived among the Pirahãs from 1967–76 and speaks the language well. Heinrichs lived among the Pirahãs from 1959–67 and still remembers a great deal of the language – he was the first one to work seriously on the language (there is a brief description of the conditions under which Heinrichs worked in William Samarin's book, *Field Linguistics*). Keren Madora has lived among the

Pirahãs since 1977, for long periods of time. None of them disagrees with the core facts of my claims about the Pirahãs' culture and language.

But of course, the examples of Malinowski and Mead do come to mind. I worried quite a bit in the early years that I could be “committing a Malinowski”. And one of my favourite quotes on field research comes from Mead in a letter of 16 January 1926 to her thesis adviser, Franz Boas:

I have no idea whether I am doing the right thing or not, or how valuable my results will be. It all weighs rather heavily on my mind.

This is the angst of every honest field researcher, in my opinion. As I describe field research in Everett (2004):¹

The history of research in general and field research in particular, is the history of fallible humans, evolved creatures, struggling to understand nearly infinite complexity in an alien environment. No one person is up to the demands of fieldwork, requiring as it does an idealized character from Arthur Conan Doyle. The outputs of our fieldwork will necessarily be incomplete records of our progress in understanding parts of wholes that exceed our abilities. Thus, our research reports, whether grammars or articles or talks or webpages are never more nor less than our efforts to communicate with interested interlocutors about the beliefs we have come to form and hold, based on our experiences and how these beliefs affect our actions in science and in life. This is our canopy of epistemic humility.

Could I be wrong about Pirahã? Yes, of course. I probably am in many ways. But is unlikely that I am wrong simply because the Pirahãs are inaccessible or because I am being hoaxed like Mead (if she indeed was – I am not all that sure that she was wrong, in spite of Derek Freeman's regular assertions to the contrary). Field researchers like Mead, Malinowski, and, in more recent times, Napoleon Chagnon, are often criticized after the fact for various reasons, many of them petty and personal.

¹ I am guessing that this refers to your piece in the van Sterkenberg volume, is that right?

I think that Chagnon, for example, is the best anthropologist to ever work in the Amazon. But many criticize him, accuse him of staging the things he described, ascribe evil motives to him, and so on, because he made the politically incorrect assertion that the Yanomami are a “fierce people”. He has been accused of racism, of failing to help the Yanomami in times of need, of being disrespectful of the Yanomami’s political aspirations and so on. So far as I have been able to tell, these assertions are all false and Chagnon has given us the best account of the Yanomami that we have to date. I have come to sympathize with him more and more as people have begun levelling many of the same accusations against me.

When I first realized that I was not finding numbers, recursion, quantifiers, and so on in the language, all I thought about it was that if I looked harder and could devise better methodologies, I would discover these things. I believed then that all languages had to have these things. I know that “absence of evidence is not evidence of absence”. I thought that maybe I just missed these phenomena. That was my opinion for a couple of decades. But I am now a more experienced field researcher. I have worked on more than two dozen languages in Mexico and Brazil. I am convinced that we are not going to find these things in the Pirahã language.

On the other hand, that kind of authoritative assertion is not going to, nor should it, convince other researchers. That is why there is now a team of researchers conducting experiments on all of these claims. On numerals and counting, for example, Mike Frank, Evelina Fedorenko, and Ted Gibson (of MIT’s Brain and Cognitive Sciences Department) and I have just had a paper accepted in *Cognition* (pending revisions), where we provide strong empirical support for the absence of any numerals or counting in Pirahã. With regard to recursion in Pirahã, Jeanette Sakel and Eugénie Stapert have a paper to appear in a special issue of *The Linguistic Review* that I am guest-editing in which they argue that there is indeed no evidence for recursion in Pirahã, based on their own fieldwork and a series of experiments and surveys of all the data I have collected on the language. Other experiments have been conducted and results are still being analysed on a wide range of cognitive and linguistic behaviours of the Pirahãs. So far, there is not only no counterevidence to my claims, but the data are all consistent with and/or supportive of my claims. The videos of our experiments are archived in web-accessible format in Ted Gibson’s MIT laboratory.

Whatever errors I may have committed, they are not of the Mead type, nor because the Pirahãs are as inaccessible as the Trobriand Islanders were, so that my claims cannot be independently investigated.²

In recent correspondence with William Poser, he makes the following points that seem relevant to this issue (e-mail dated 30 September 2007 to Everett):

... granting that there is a history of anthropological fieldworkers making serious errors that were not recognized for a long time because there was no one to contradict them, we need to recognize that a lot of linguistic and ethnographic claims about much better known and accessible languages and societies have been similarly flawed. Cultural blinders, personal biases, theoretical fads, and flawed methodology, especially, in the linguistic case, elicitation techniques, operate even when the language or culture is well known and accessible. Furthermore, many questions are studied only by a small number of scholars, even in well known languages, so that, unless and until something triggers careful investigation, a factual claim may remain unchallenged for a surprisingly long time. (A minor example is the claim found in almost all descriptions of Japanese phonology that all syllables contain either one or two moras – in fact, all speakers of Japanese have some tri-moraic syllables.)

This is not to say that claims about obscure, inaccessible languages and cultures should not be carefully scrutinized, but rather that we need to recognize that received views based on better known languages and cultures do not contrast with them as discretely as we might think.

GRS: In 1986 you published an extensive description of the Pirahã language which made it sound interesting but not extraordinary. In your recent publications, you have sometimes described some of the very same language forms in new ways which do now put them outside the spectrum of language phenomena familiar to most linguists. Your leading critics, Nevins et al. (2007), proceed largely by saying that they find 1980s-Everett more believable than 21st-century-Everett. You have explained (e.g. in Colapinto 2007) that in the 1980s you were a convinced generative

² Dan, I have modified the English slightly here to make it more readable – OK?

linguist, who forced any observation into the Procrustean generative analytic framework (as members of that school commonly do), while deeper experience of Pirahã, and more willingness to allow observation to control theorizing rather than *vice versa*, have led you recently to a better interpretation of the workings of the language.

That is fair enough. But Nevins et al. do have a point, surely, when they complain that you do not justify your changes of mind? Your *Current Anthropology* piece contains only few references to Everett (1986), and so far as I have seen those references do not focus on differences between the two language-descriptions. If someone publishes a full-dress document saying *X*, and twenty years later publishes others that say not-*X*, we usually look for material within the later writings that draws attention to the inconsistency and explains why the earlier account has proved to be in error. If readers do not find much of that, can they be blamed for saying “They can’t both be right, we are given a free choice so I choose *X*?”

DLE: When I published the *Current Anthropology* article, I knew full well that I would need to do a lot more work and publish a great deal more to convince the linguistic public. But that paper was never intended to be the final word. It was the “opening salvo” in what I hope will be a revolution in linguistics, a revolution in which linguists come to see field research as a fundamental part of their identities as linguists and in which culture’s effects on language are seriously investigated. There is a widespread view that it was long ago shown that culture has no significant influence on grammar. Steve Pinker has made this claim to me in recent e-mails, and says pretty much the same thing in his book *The Stuff of Thought* (2007). I think that is just wrong. I knew that a linguistic journal would be unlikely to publish my culture–grammar paper for these very reasons. So I submitted the paper to the premier journal of cultural anthropology instead. I felt confident in my analysis, but I wanted the proposal out there, with rigorous testing of all the claims to follow. I don’t see that this order of things is any different than goes on in most linguistic theorizing or scientific research in general. And now the testing is underway.

But the contradictions between Everett (1986) and Everett (2005) are just not all that big in terms of the data discussed. The differences have been grossly exaggerated. I told Chomsky in 1984, when I was a Visiting Scholar at MIT, that I could find no evidence for embedding in Pirahã other than the *-sai* nominalizer (which was a focus of Nevins et al.’s discussion). It turns out that *-sai* has functions

that overlap with nominalization but that this is not the best analysis of it.

Experiments by Mike Frank and others, and the new paper by Sakel and Stapert, show this clearly. With that gone, there just is no evidence for recursion in Pirahã. As often happens in field research, a minor difference in the way this or that morpheme or construction is analysed can have profound effects on the grammar as a whole. One doesn't see all of this at first.

So Everett (1986) is a good study, I think. I am proud to have written it. And my opinions about the facts have changed really very little since then. I have found that some of the data were incomplete and so on, as I state in Everett (2007). But these are minor factual adjustments that turn out to have major theoretical consequences. What *has* really changed over the years is my analysis of how all the pieces fit in the grammar as a whole. That is natural. Some would call it progress, not contradiction.

And we know that if we look at, say, Chomsky's writings over the years, we get changes of a similar nature and even changes in the judgements on the grammaticality of English utterances. The difference is that the methodology of Chomskyan grammar is much poorer, much less likely to test claims by experimentation or to move beyond solipsistic grammars towards testable grammars subject to, say, the methods being developed by Josh Tenenbaum and his laboratory colleagues at MIT's Brain and Cognitive Sciences department. The methodology of descriptive linguistics or structuralist linguistics, as outlined in, say, Longacre (1964), Harris (1947), Samarin (1967), or even my own field guide (Everett forthcoming b), is arguably superior to the introspective work characteristic of what I call "armchair linguistics". Couple field methodology with standard psychological experimentation and you begin to approach a much more scientific basis for a linguistics which is cautiously deductive, enthusiastically inductive, and mainly abductive – with the different types of reasoning appearing, of course, at different stages in the analytical process.

Another point worth reminding readers of is that although there is an increased burden of proof on claims like mine that go so against the grain, just about every point that is raised against the lack of replicability of my claims and differences in earlier and later claims can be raised, as I mention with regard to Chomsky, to just about any linguist's body of work. But we generally are not as demanding on the factual claims that support our pre-existing ideas as we are on the claims that contradict them. So there are likely many "false positives" in published grammars for

ideas popular in certain theories, but those go unchallenged because they are “comfortable” facts. The possibility of “false negatives” is what worries most of us. This is human nature.

GRS: Nevins et al. quote Anna Wierzbicka (2005) as saying that your recent writings gratuitously “exoticize” Pirahã, by describing morphologically complex words with simple meanings in terms of the etymological sense of the roots – as if a Frenchman were to argue that English-speakers have a weird view of intellectual activity, because instead of the simple concept *comprendre* they say that people *se tiennent debout sous* (under-stand) an idea. In 1986 the Pirahã phrase *hi xogi* was treated as unproblematically meaning “all” or “everyone”; the two morphemes literally translate as “3rd person” and “big”, and in 2005 you insist on translating *hi xogi* as “he big” as opposed to “all”. I have not seen Anna Wierzbicka’s commentary, but she has a real point, hasn’t she? It has often struck me that if the French were a remote jungle tribe, English-speakers who learned about the gender system of their language might conclude that they are animists who believe that every object embodies a male or female spirit. We know that the French are no such thing, but that is because many of them are eloquent intellectuals who tell us what they do and do not believe; we could not infer their rational world-view from the structure of their language.

In Everett (2007) you respond to Wierzbicka’s objection by saying that translating *hi xogi* as “all” would be inaccurate “because ... there is *no* word in Pirahã with the meaning of the English word ‘all’”. That is circular: “Pirahã has no quantifiers, and if you doubt this and think you have found one you must have mistranslated it, because Pirahã has no quantifiers”. What guarantees that it is your new treatment of *hi xogi* which is correct and your earlier treatment erroneous?

DLE: What I say about “all” (*hixogii*) is not circular.³ It is a simple point. When I asserted that it meant “all” in 1986 I didn’t check the truth conditions. The truth conditions for universal quantification are not met by any Pirahã word, at least not by the word that I used to think meant “all”. There is no situation in which the use of the expression *xogii/xogiaagao*/etc. would be rejected if there were an exception. “He ate meat *xogio*”, for example, is acceptable to any Pirahã, anytime, even if part of the meat is still uneaten, so long as a lot of the meat is eaten (not even “most” of it – just

³ Does the orthographic contrast between *xogi*, *xogii*, etc. matter?

a lot). I answer Wierzbicka's objections, and those of Nevins et al., at length with similar reasoning in both my 2005 *Current Anthropology* article and in Everett (2007). I don't think that there is anything to add.

Of course it is easy to exoticize other languages and peoples. We do it all the time. And this can be a mistake. In *The Language Instinct* Pinker criticizes Whorf for doing exactly this with the Hopi language in order to make a point about language's effect on thought.

But the reverse mistake is just as bad – homogenizing languages and cultures so that they all fit the same assumptions about what languages can do and how they should sound. If one language has a word that doesn't have a counterpart in another language, then it can be and usually will be wrong and misleading to translate them as the same. True, if we don't do this, one language can look “exotic” to the speakers of another language (the Pirahãs could say that my translation of English “all” exoticizes English, if they cared about such things). My translations of Pirahã utterances in my writings on the language reflect my best judgement on what is meant. If it sounds like natural English, fine. If it sounds exotic that is because it doesn't mean exactly what any English word or utterance means.

I do not believe in Universal Translation. I don't believe that what can be said in one language can be said naturally (or even at all) in all other languages. I do not believe that all languages have the same expressive power. In fact, if I am right, some languages are non-finite and others are finite.

This is one of the reasons why I am sceptical of any grammar that lacks a certain exotic flavour. I would bet that for any grammar in which all the sentences can be translated into idiomatic, natural English, the field researcher is probably missing something. But that is my view.

GRS: A point that is not a criticism but a request for clarification: I am not sure what you are saying about the history of Pirahã language and culture. We are told that the Pirahãs are closely related ethnically to a group called Mura, who used to speak various languages (or dialects) related to Pirahã but who have now assimilated to Portuguese-speaking Amazon culture, so that by 1986 (p. 200) you described the Mura language(s) as “(probably) extinct”. Is enough known about those languages, and the pre-assimilation Mura culture, to say whether they shared the special features and “gaps” of Pirahã language and culture? (Comments you make in Everett (2005)

referring to the work of Curt Nimuendaju suggest that they did not share some of them.)

In his critique of you, Richard Boast (2006) saw it as a potential refutation if one could show that Pirahã language/culture had decayed in the historical period from a more sophisticated earlier state. We know that material cultures do sometimes decay – the well-known example (Diamond 1998: 312–13) is the indigenous Tasmanians, who lost much of the culture they inherited from mainland Australia; so perhaps the same might happen with a language. Unlike Boast, I cannot see how that would go against anything you say. Surely it would be just as interesting to find that an “ordinary” language can fall so far down the scale of complexity, while still being the sole language of a community, as it would be to find languages which have been as strikingly simple as Pirahã throughout known history. But it would be good to know which it is in this case; or is the answer that we just do not know?

DLE: I also address this in the *Current Anthropology* article. First, there is no evidence for “decay” of Pirahã culture. Second, even if there were, this isn’t relevant. We know that for more than 100 years, likely for 300 years, the Pirahã culture has been pretty much the way it is now. That is a stable system and needs to be described for what it is, not for what it was. If this description concludes that Pirahã lacks recursion or has an Immediacy of Experience Principle, etc., then it is just irrelevant that the language might have had recursion or no Immediacy of Experience Principle 500 years ago.

I wouldn’t be mistaken describing current English culture without reference to Arthur and the Knights of the Round Table, whether or not the culture of that fable was ever the dominant culture. One might claim that English culture has “degenerated” since the days of chivalry, since degeneration in this sense is largely in the eye of the beholder and is not a scientific notion, but that would not entail that description of the current state of the culture is wrong because it fails to refer to earlier stages. This is the cultural equivalent of Saussure’s diachronic v. synchronic distinction in the study of language.

As to the Mura, we know relatively little about them before their assimilation of Brazilian culture, other than that they had large villages, controlled the Amazon from Peru to modern-day Manaus, and were eventually nearly wiped out by the

Mundurucús⁴ who were armed by Brazilians for exactly that purpose. We do know that the Muras alive today, near Manaus, look very much like the Pirahãs, though they live as Brazilians. So one probable lesson we can draw from this is that – contrary to what many have claimed (never me!) – there is no genetic difference that “limits” the Pirahãs’ grammar or culture – the Muras are doing fine speaking Portuguese, with recursion, numerals, and so on. Along this same line, Pirahãs that were kidnapped and then raised from an early age among Brazilians (there are some) speak Portuguese just fine, natively, and show no obvious cognitive or linguistic differences from the Brazilians that have raised them.

GRS: Much of your theorizing about Pirahã language and culture focuses on what you call the Immediacy of Experience Principle, which is central enough for you to reduce it to an acronym, IEP. You claim that this principle, that Pirahã assertions always relate directly to the moment of speech, predicts a range of quite diverse “gaps” in the language, from recursion to numerals to colour terms and more – it is often far from obvious how the IEP as you state it does entail all these consequences. But one thing it surely must entail is that the Pirahãs would not talk about hypothetical future events. Yet Anthea Fraser Gupta points out to me that Kate Douglas’s (2006) account of your work quotes a group of Pirahãs on tape saying things like “When will we see you?”, “When you come, bring us some matches”, and so forth. Presumably these are translations of utterances in Pirahã, but how are they compatible with your “IEP”? Is this just a case of popular journalism getting the wrong end of the stick, or what?

DLE: Asking me when I am returning to the village is not *speculation* about future events. Counterfactuals, discussions of what to do in three years from now, hypothetical future events, do not occur so far as I can tell. These are speculations about future events. But the Pirahã⁵ experience people coming and going from villages all the time. Comings and goings are part of their immediate experience. So asking me when I am coming is asking for an assertion relative to the moment of

⁴ Accent on last U correct?

⁵ Most of the time you use “Pirahã” for the language, and when you talk about the people you give them an English plural, Pirahãs. Consequently I have added -s similarly in my questions. But here you use the form without -s for the people. Probably it would be best to be consistent one way or the other – have you a preference? (Or is there a distinction I’m missing?) – In your previous response similarly you write Mura where I might have expected Muras.

speech, well within their everyday experience, and is allowed by the Immediacy of Experience Principle as I state it.

GRS: I also wonder why it is important to you to derive diverse properties of Pirahã from a single, simple abstract principle such as Immediacy of Experience. That feels like the kind of intellectual move that is attractive to the true believers in innate knowledge of language. They hold that there is something in human DNA, we know not what, which leads to our already knowing most of what there is to know about the grammar of our mother tongue before we hear examples of it. If that is credible at all, it is more likely to be true provided what look superficially like many separate structural features all follow from a few basic principles; so the generativists postulate mathematical abstractions with names like “A-over-A”, or “Move Alpha”, and argue that large ranges of specific structural facts about particular languages follow as logical consequences of those abstractions. If one sees languages as cultural institutions, evolved step by step like other products of human culture, that picture becomes implausible. Products of evolution typically do not have that all-of-a-piece quality; they successively incorporate separate, unrelated structures, as the long-drawn-out process of evolution seizes on whatever solutions it happens to encounter to particular fitness issues. Evolutionary products are more like the Imperial system of weights and measures in its full glory, with, I don’t know, two units of length of the same order of magnitude for measuring horizontally (in yards) and vertically (in feet), or one complicated system for weighing most things and a loosely-related complicated system for weighing gems, or weighing medicines, than they are like the metric system in which units for every possible physical quantity are derived logically from a few fundamentals. How does your rejection of innate Universal Grammar square with your wish to derive many separate facts about Pirahã from the IEP?

DLE: My rejection of Universal Grammar is based on my observations that culture can exert architectonic effects on grammar. And I also reject the methodology of Universal Grammar – the deductive reasoning from an assumption to facts. By and large I don’t think that this is useful in linguistics.

Does the IEP sound like this? I suppose that it does in some narrow way. Maybe it is wrong or less than useful for the same reasons. That needs to be tested.

But the IEP is fundamentally unlike UG on closer examination. It is the result of trying to understand or make sense of the number of “gaps” in *Pirahã* culture and

grammar. Are these coincidences in the same language or do they all follow from a single principle? I think that the evidence suggests that they follow from a single principle.

The IEP is unlike UG because I doubt that it is found in any other language. I suspect that what we will find when we begin to research the interactions of culture and language more carefully is that we will have at least as many principles governing grammar–culture interactions as there are grammar–culture pairings. Probably many more, because it is less likely that each grammar–culture relation can be summarized by a single principle.

GRS: Again, Everett (2005) includes a passage about “parameterization” of a universal feature [R] (for “reference”), with a default value that can be reset by a child’s language experience. This sounds as though you are assuming the kind of innate Language Acquisition Device, imposing specific properties on any humanly-learnable language, which the bulk of your recent writing rejects. I am puzzled.

DLE: That is a citation from an earlier work of mine (Everett 1993) during the time that I believed in parameters. Now I would simply say that if we applied a neo-Reichenbachian model of tense to Pirahã, Pirahã’s tense system lacks Reichenbach’s “R-point”.

GRS: At one point in your reply to Nevins et al. you push your rejection of the Universal Grammar idea further than your evidence seems to warrant, even if we accept your interpretation of Pirahã: you write “I am claiming [that] every language is non-trivially different from other languages in ways incompatible with UG”. It is a bit difficult to unpick the logic of the quantification here! You might be saying “for any language L , there is at least one member of the set of all other languages that is sufficiently different from L to disprove UG” – in which case all you need to do is point to one really strange language, such as Pirahã. But you *sound* as though you are saying something about all languages, rather than about one strange language. It sounds as though you are saying that for any plausible theory of Universal Grammar, English will refute it in some way(s), French in other way(s), Chinese in other way(s), and so on and on. Is this what you are saying? In that case, you might believe it but you would surely agree that no amount of discussion of Pirahã alone could substantiate it.

(To me it seems implausible. Because of intensive cultural interactions, the main present-day European languages, at least, seem to me pretty well wholly intertranslatable. What I find really offensive about generative linguists is that they observe this confluence of cultures and argue that it is not a contingent fact about the present-day world but a biological necessity, and hence they refuse to recognize the separateness of third-world cultures or those of our own ancestors; cf. Sampson (forthcoming). That is robbing people of their patrimony, to my mind.)⁶

DLE: I am claiming merely that I do not believe that Pirahã is unique. I hypothesize that all grammars are likely to show cultural effects. Role and Reference Grammar is unique among linguistic theories, it seems to me, for explicitly connecting pragmatics, syntax, semantics, and morphology in such a way that cultural constraints can in principle be easily fitted into the grammar. For example, one could say that cultural constraints restrict the types of syntactic templates that an RRG grammar provides for a particular language. To a lesser degree, Construction Grammar also gives us a way of thinking about this connexion (see especially Nick Enfield's *Ethnosyntax*, Enfield 2002). I do believe that every language will turn out to refute UG in some way. And theories are already around that give us a potentially useful tool for testing this as researchers turn to examine if or how constructions emerge from the cultures of their speakers. It is likely to me that we will discover evidence for this in every language. The chapters in Enfield's book point in this direction, it seems to me.

I have no evidence for this assertion other than Pirahã and studies such as those in the book just mentioned. But I need no evidence. That claim is a proposal for future research, not a statement of completed research. It simply asserts that this would be a useful way to reexamine languages and to conduct future field research.

On the other hand, I think that UG is too vague to be subject to testing or verification of predictions in any useful sense of the word. Even Chomsky, in personal correspondence with me, admits this, saying that UG is not a hypothesis. It makes no predictions. It is a field of study, like Biology, according to Chomsky. That is unlikely. I can't imagine scientists studying living creatures apart from Biology.

⁶ Dan: In your responses, you have set your points out in a way that leaves this point of mine "orphaned"; but I am really reluctant to omit it, and this is the best place for it. In this draft I have enclosed it in brackets so that the reader can see you are really responding to the preceding para; hope that's all right?

But many psycholinguists and linguists study language and its acquisition just fine without the idea that there is a specific biological endowment for grammar.

GRS: In Everett (2007) you describe yourself as working in the Boas tradition. But what I think of as Boas's master idea is something which you are undermining as actively as you undermine the Chomsky tradition. Boas was pre-eminently the scholar who argued that there are no "primitive languages". If an indigenous third-world language lacked parallels to the structural features which give typical European languages their logical sophistication (Boas said), it did not follow that the former languages were unsophisticated; if you looked, you would often find that they had highly subtle structural features of their own for which European languages have no parallel. (Cf. Boas 1911: 39.)

In the first half of the 20th century, most Europeans and North Americans did take for granted that third-world languages were inferior as vehicles for thought and communication. I still have the 1956 edition of the Guinness Book of Records, which I was given as a boyhood birthday present: one of the records it includes is "most primitive language" – the answer was the Australian language Arunta (I believe the name is nowadays written Aranda) in which "Words are indeterminate in meaning and form". That was the received wisdom of the time, but linguists were people who thought they knew better; and the person who had taught them better was Boas. I appreciate that you make comments about certain features of Pirahã being unusually complex, but the overall balance of your account, as you would surely agree, is to portray Pirahã as a remarkably intellectually-crude language. So why do you place yourself in the Boas tradition?

DLE: I am following Boas's research tradition by taking seriously the idea that grammars and cultures interact in significant ways and that each language should be described in its own terms. I believe that linguists too often fail to recognize that there were two parallel research traditions in American linguistics following Boas's initial crop of students, including Sapir. These were Bloomfieldian structuralism and Sapiran descriptivism.

The structuralists were concerned with finding the tagmemes and syntagmemes of each language's grammar and assumed that these elements would come from a universal set of structures. This is found clearly in Bloomfield's

Language of 1933. I have long thought that Chomsky's work fits in this tradition and that in this sense Chomsky is the most sophisticated proponent of structuralism.

Descriptivists followed the Boasian tradition of describing languages “in their own terms”.⁷ Kenneth Pike, Edward Sapir, and Boas were in this tradition, at least in my reading of their works. In my first linguistics class Kenneth Pike, my first linguistics teacher, began the lecture by saying that “Languages are extremely different from one another. Of course they're not *utterly* different or there would be no linguistics”. The Boasian tradition to me is the inductive approach to the study of language and its interaction with culture, allowing that languages and their grammars may be more or less shaped by their cultures, at the same time that the cultures and the speakers' ways of thinking can also be affected by their language. The Boasian, according to this conception, sees language and culture as a complex symbiosis.

There is nothing in what I have written that should be interpreted as making the Pirahã or their language seem intellectually crude. Rather, what should be concluded is that their language fits their culture and their culture fits their needs and their environment. This is another reason that it is so crucial to study endangered languages and cultures – not because they are all alike in some fundamental way, but because they are so different in interesting ways – each culture–language pairing is unique in this sense – at least I am placing my bets on this expectation – and thus each pairing has something to teach us that no other does. In this sense, it becomes more urgent and imperative that linguists develop again a culture of field research so that field research is the norm, not the exception, for all linguists' careers, and that we document, describe, and theorize about all the language–culture pairs that are known. That is an ambitious goal, but a good one to have, I believe.

GRS: A specific point about the Boas tradition has to do with speech sounds. Most of what you say about Pirahã is about its grammar and semantics; but you also write about its phonology, commenting that it has an unusually tiny phoneme inventory and that the phoneme distinctions it does include are not always maintained – what you call the “sloppy phoneme effect”. Boas wrote about phonology in connexion with his arguments against the “primitive language” idea; he showed (1911: 12–14) that what Europeans took to be vagueness of sounds in exotic languages were really cases where allophone/phoneme relationships in those languages conflicted with

⁷ I have moved your opening inverted commas to include the word “in”, it seems to make better sense.

corresponding relationships in European languages, so that the impression of vagueness was symmetrical – a Kwakiutl speaker would perceive English as having vague sounds.

I am curious whether you see the limited phonology of Pirahã as related to its simplicity of structure in other respects. To me, the two domains seem unrelated. The exotic language I know best is Chinese, which relative to any European language has a strikingly limited phonological system (it is tonal, but the tones do not make up for the fewness of distinct consonants and vowels). Yet, until our Industrial Revolution, China was probably the most sophisticated culture on earth. Conversely, so far as I know there are primitive societies whose languages are phonologically highly complex. Does your discussion of sloppy phonemes mean that you see a connexion with the main points you were making about Pirahã language and culture?

DLE: Yes, I do see a connexion between culture and grammar even in Pirahã speech sounds. But not in the way you put it – I see no connexion between the simplicity of a culture and its phonemic inventory.

I have written on this at length, and I published an account of this compatible with my current work back in 1985. The idea is that the segmental inventory and the process of “free variation” that I refer to as the “sloppy phoneme effect” are conditioned by Pirahã’s “channels of discourse”.

Pirahã can communicate with consonants and vowels, but it can also use whistle speech, hum speech, yell speech, and musical speech, as I describe in the 1985 paper and in the on-line appendix to my *Current Anthropology* paper. All channels of Pirahã discourse use prosody (tone, syllable length and boundaries, and stress), and each has its particular cultural function.

The culture, not the grammar, determines how and with what frequency the different channels are used. Because the consonant-vowel channel is used so rarely relative to other languages, I proposed the implications below as a way of accounting for the free variation and the small inventory:

Functional Load Principle:

- a. greater dependence on the channel → greater contrast required
- b. lesser dependence on the channel → less contrast required

This just means that you need contrast to use a channel, a way of telling units apart. The greater your dependence on a channel, the more it is used across a variety of communicational contexts, the more contrast you need to be able to distinguish units in a variety of contexts. The less you use a channel, the less you need contrast. Notice that the lesser need of contrast doesn't imply that a language will lack contrast. Lalana Chinantec of Mexico, to take one example, has whistle speech every bit as functional as Pirahã's from what I can tell, but it also has a very complex segmental inventory. Pirahã's inventory is consistent with the principle above, but so is Chinanteco's.⁸

So in this case, what connects Pirahã's phonology and culture is different from what connects its syntax and culture. The morphology could turn out to be different yet. I am not claiming any single effect that accounts for all. But I am claiming that the effects of culture can be seen potentially in any component of the grammar.

GRS: Thank you, Dan; and good luck for *Don't Sleep, There are Jaguars!*

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Everett, D.L. forthcoming a. *Don't Sleep, There are Jaguars: lessons on life, language, and thought from the Amazon*. To be published by Pantheon and others.

Everett, D.L. 1993. "Sapir, Reichenbach, and the syntax of tense in Pirahã". *The Journal of Pragmatics and Cognition* 1.89–124.

Everett, D.L. 2004. "Coherent fieldwork".⁹ In P. van Sterkenberg, ed., *Linguistics Today*, John Benjamins, pp. 141–62.

Everett, D.L. 2005. "Cultural constraints on grammar and cognition in Pirahã". *Current Anthropology* 46.621–46.

Everett, D.L. 1986. "Pirahã". D.C. Derbyshire and G.K. Pullum, eds., *Handbook of Amazonian Languages*, vol. 1, Mouton de Gruyter (Berlin), 200–325.

⁸ Assuming there is no difference between Chinantec and Chinanteco, should we standardize on one of them?

⁹ Assuming my guess under note 1 was correct.

- Everett, D.L. 2007. “Cultural constraints on grammar in Pirahã: a reply to Nevins, Pesetsky, and Rodrigues (2007)”. <ling.auf.net/lingBuzz/000427>.
- Everett, D.L. forthcoming b. *Linguistic Fieldwork: a student guide*. To be published by C.U.P. <ling.auf.net/lingBuzz/000442>.
- Everett, D.L. 1985. “Syllable weight, sloppy phonemes, and channels in Pirahã discourse”. In Mary Niepokuj et al., eds., *Proceedings of the Berkeley Linguistic Society*, 11, pp. 408–16.¹⁰
- Colapinto, J. 2007. “The interpreter: has a remote Amazonian tribe upended our understanding of language?” *New Yorker* 16 Apr 2007.
- Frank, M.C., D.L. Everett, Evelina Fedorenko, and E. Gibson. forthcoming. “Number as a cognitive technology: evidence from Pirahã language and cognition”. To be in *Cognition*.
- Sakel, Jeanette, and Eugénie Stapert. forthcoming. “Pirahã in need of recursive syntax?” To be in *The Linguistic Review*.
- Sampson, G.R. forthcoming. “Minds in uniform: how generative linguistics regiments culture, and why it shouldn’t”. To be in Proceedings of IADA 2006, Mainz. <www.grsampson.net/AMiu.html>.
- Diamond, J. 1998. *Guns, Germs and Steel: a short history of everybody for the last 13,000 years*. Vintage.
- Nevins, A.I., D. Pesetsky, and C. Rodrigues. 2007. “Pirahã exceptionality: a reassessment”. <ling.auf.net/lingBuzz/000411>.
- Boas, F. 1911. Introduction to *Handbook of American Indian Languages*. Bureau of American Ethnology, Bulletin 40, Part I, pp. 1–83; reprinted in one volume with J.W. Powell, *Indian Linguistic Families of America North of Mexico*, University of Nebraska Press, 1966.
- Bloomfield, L. 1933. *Language*. Holt (New York).
- Enfield, N., ed. 2002. *Ethnosyntax: explorations in grammar and culture*. OUP.
- Boast, R. 2006. “Tragic Piraha past?” *New Scientist* 15 Apr 2006.
- Douglas, Kate. 2006. “Lost for words”. *New Scientist* 18 Mar 2006, pp. 44–7.
- Pinker, S. 2007. *The Stuff of Thought: language as a window into human nature*. Viking.
- Wierzbicka, Anna. 2005. Commentary on Everett (2005). *Current Anthropology* 46.641.

¹⁰ Is this the correct item?

- Sampson, G.R. 2005. *The “Language Instinct” Debate*, revised edn. Continuum.
- Hale, K. 1976. “The adjoined relative clause in Australia”. In R.M.W. Dixon, ed., *Grammatical Categories in Australian Languages*, Australian Institute of Aboriginal Languages (Canberra), pp. 78–105.¹¹
- Givón, T. (2007) "Toward a diachronic typology of relative clauses", Symposium on the Genesis of Syntactic Complexity/, Rice University, Houston, March 2008
- Pulman, B. 2004. “Malinowski and ignorance of physiological paternity”. *Revue française de sociologie*, 45, Supplement, pp. 125–46.
- Mead, Margaret. 1928. *Coming of Age in Samoa: a psychological study of primitive youth for Western civilization*. Morrow (New York).
- Freeman, D. 1983. *Margaret Mead and Samoa: the making and unmaking of an anthropological myth*. Harvard University Press.
- Samarin, W.J. 1967. *Field Linguistics: a guide to linguistic field work*. Holt, Rinehart, and Winston (New York).
- Longacre, R.E. 1964. *Grammar Discovery Procedures: a field manual*. *Janua linguarum*, series minor, 33. Mouton (the Hague).
- Harris, Z.S. 1947. *Methods in Structural Linguistics*. University of Chicago Press. (Later republished as *Structural Linguistics*.)
- Pinker, S. 1994. *The Language Instinct: the new science of language and mind*. Allen Lane.

¹¹ Is this the item you meant?