Human Development

Letters to the Editor

Context and Comments on Dan Everett's Claims

Dan Everett seeks to quickly make several different kinds of points here, based on extensive publication and debate on the language and culture of the Pirahã. There is no space to elaborate any of the points; at best, his brief essay serves to lead the reader to want to know more. His 2005 article in *Current Anthropology* has stimulated dozens of responses, in print and on email lists, and John Colapinto's 2007 *New Yorker* article has brought the Pirahã into the popular imagination. *Wikipedia* (http://en.wikipedia.org/wiki/Pirahãah%C3%A3_language) provides detailed information and links; at the moment, a Google search for Pirahã yields about 38,000 hits in a number of languages. So what can one say, in a few paragraphs, to readers of *Human Development*?

1. Look first at the main claims, which partially overlap, and are not presented very clearly. There seem to be three of them: Although Chomsky has made important contributions, data from Pirahã are "incompatible with Chomskyan theory and thereby falsified it."

2. The critical factor is the purported lack of recursion in the grammar of Pirahã – that is, the absence of "a feature of form … that underlie[s] all the various principles of UG."

3. Furthermore, Pirahã lacks recursion as part of a package of linguistic peculiarities, all determined by a cultural convention that limits speakers to assertions based on what they have witnessed, deduced, remember, or have heard from others.

Let me say at the outset that Everett has made many valuable observations about the Pirahã, about their culture, about their language, and about linguistic theory. And I can only applaud the many years of hard work – ethnographic and linguistic – lying behind these claims. Regardless of the implications one may draw, data of this sort are immensely valuable. However, I do not find these three claims convincing.

As for the first claim, Chomskyan theory as a whole does not stand or fall on the basis of the absence of recursion in a particular language. The theory, as set forth by Hauser, Chomsky, and Fitch [2002], merely proposes that recursion may be "the only uniquely human component of the faculty of

language." Their conception of the "narrow" faculty of language includes only that bit of computational machinery that may not be shared with any other animals and that is biologically unique to humans. All of the rest of language as we know it (the "broad" faculty) lies outside of this narrow definition, and so all of Chomskyan theory that deals with any other aspect of language is untouched. Indeed, the theory as a whole cannot be "falsified." It is a combination of beliefs and practices, dealing with language in particular ways. A method or ideology cannot be falsified, but can only be modified, or abandoned, or replaced in the light of data and alternative models (more on this with regard to Everett's third claim).

One might have thought that the second claim would stand. As the popular example goes, finding one black swan falsifies a claim that all swans are white. But one must convince oneself that the swan is really black. Chomsky is not phased. With regard to the Pirahã data, Fitch, Hauser, and Chomsky [2005, p. 242] take the position that a lack of recursion "surely does not affect the argument that recursion is part of the human language faculty: … our language faculty provides us with a toolkit for building languages, but not all languages use all the tools." Although I don't agree with the overall thesis of these three scholars [see Pinker & Jackendoff, 2005, for what I consider a convincing response], here they are certainly consistent – and unfalsifiable. Everett seems to have misunderstood their position with regard to the "narrow language faculty" and the purported unique biological basis of recursion.

This having been said, I am not yet convinced that Pirahã lacks syntactic recursion. And this is because of the basis for establishing recursion in much of syntactic theory, Chomskyan and non-Chomskyan alike. There is an unfortunate limitation in most linguistic description to the forms of language that are used in relatively planned discourse, typically representable in writing. Our inherited writing systems, and our devised systems of linguistic notation, almost entirely exclude *prosody* (intonation, stress, rate, pause, and so forth). Yet it is prosody that knits together utterances in face-to-face use – the context in which language has been used since it first evolved, and as it is still primarily used worldwide. The sentence is a written artifact: in European languages, a sentence is what begins with a capital letter and ends with a period. In linguistic terms, it is the highest node in a hierarchical structure that dominates a collection of formally defined elements. It is an overarching formal unit – but that unit is rarely defined intonationally and the component elements are only defined by conventional linguistic criteria. If I say, "The man I told you about is my neighbor," this is an example of recursion because one sentence-like form, "I told you about (something)" is contained in another sentence, "The man is my neighbor." By definition, this is recursion because of the decision that the two forms are of the same type, one is "contained" in the other, and the two forms together are also an example of the same type – that is, a sentence with two sentence-like elements within it. But suppose I say, for example, "Y'know that man I told you about? He's my neighbor." This doesn't count as *syntactic* recursion, because here, on paper, we have two free-standing sentences. The linguistic analysis ignores the intonational contour that ties these two together – and it is an arbitrary fact of history that such larger units are considered different from the syntactic units called "sentences." Furthermore, in much of natural interaction, one of the forms need not be a "sentence" at all, and the overall utterance

may not be a sentence either: “Y’know that guy – well, he’s my neighbor.” That is, any definition of recursion relies on some criterion for establishing the relevant pieces of language that can be combined recursively. I suggest that the criterion that both Everett and Chomsky are relying on hides other kinds of linguistic recursivity that might just as well be considered “syntactic.”

It is reported that Pirahã is rich in intonation, but since Everett is battling conventional linguistics, he doesn’t seem to have told us whether his non-recursive, “paratactic” constructions are actually identifiable prosodic units, rather than strings of utterances loosely related to one another. So we may have yet another instance of “pragmatic recursion,” such as is probably found in all spoken languages used in non-formal oral settings.

In any event, I am not yet convinced that the language lacks *formal* linguistic machinery for more closely tying two utterances that could be described as an embedded construction. This is not a linguistics journal, and the reader may have had too much linguistics already, but take a simple example from Everett’s *Current Anthropology* article [2005, p. 629, example 24]. The Pirahã sentence means “I said that Kó’oí intends to leave.” In English this is an embedded construction: the sentence “Kó’oí intends to leave” is embedded in the sentence “I said” and the embedding is marked by a linguistic element, “that.” The Pirahã version is something like, “I said-SAI Kó’oí he intend leave”. At issue is the linguistic status of the ending -SAI on the first verb. There are various arguments in the literature as to whether it is a subordinator, but, in any event, it appears that “I say-SAI” is not a form that stands on its own. It establishes a linguistic context in which a comment should follow. Why is this not an example of syntactic recursion? To make a very contentious debate overly brief: it is not yet resolved whether Pirahã lacks syntactic embedding, even on formal linguistic grounds.

Finally, let us turn to the third claim. Everett proposes that Pirahã lacks recursion, number and quantification, color words, and other expected components of language because speakers are limited to assertions based on what they have witnessed, deduced, remember, or have heard from others. Thereby he concludes that grammar is shaped by culture. It is certainly well attested that the languages of small, preliterate, face-to-face communities tend to have various grammatical features and pragmatic practices that are probably due to demographic factors. However, no one has claimed that their grammars are different *in kind* from grammars of human languages used by other groups. Everett seems to be claiming that Pirahã is an example of a language with a distinctly different sort of grammar, determined by the preference of the Pirahã not to talk about what they don’t know on the basis of culturally accepted types of evidence. The link is not obvious to me. Even if you are completely committed to never talking about something that you don’t know directly, have deduced, or have heard about, you could still draw on a larger range of grammatical complexity. Certainly you could still embed one proposition within another, count and keep track of quantities, and so forth. It is not the issue of evidence that might limit the Pirahã to a particular type of grammar, but rather conventionalized interaction practices lead people to build certain types of grammatical and discourse structures. It remains to Everett to fill in this picture with more convincing argumentation and example,

with particular attention to the flow of back-and-forth information in natural conversation. Such cultural facts cannot be determined by analyses of isolated constructions.

In brief, the data and claims about the Pirahã people and their language are tantalizing, but much more needs to be known before one can evaluate the position set forth by Everett. More is promised, and the debates will continue. I would suggest, however, that Chomskyan claims about the language faculty are quite irrelevant to these endeavors. Much of value can be learned by exploring and developing alternate grammatical models for relatively unknown languages like Pirahã, rather than relying on traditional conceptions of academic linguistics. Everett's missionary spirit – successfully transferred from religious conversion to deep linguistic and cognitive investigation – is now admirably applied to the conversion of linguistic theory. But these efforts can only succeed on the basis of systematic work on appropriate models. The gospels are not out there and they will not be revealed; they remain to be written.

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On Pirahã and Everrett's Claims About Recursion

Pirahã is a language like many others – it serves the purpose of communication and it has a form similar to other spoken languages: sounds, lexical and grammatical elements, discourse structure, different registers of speech and so on. Nonetheless it is at the centre of a great controversy in linguistics for the alleged lack of several features considered universals of human language. Most discussions have centered around the claim that Pirahã has no recursive structures [Everett, 2005].

I conducted fieldwork on Pirahã in early 2007 to test some of the hypotheses set out by Everett [2005].[1] I was part of a group of four psychologists and linguists who accompanied Dan Everett to the field, while conducting the research as independently as possible. Before going to the field, I had worked with Pirahã language data collected by Dan Everett and his predecessor Steve Sheldon. I had made a list of elements that could possibly be markers of recursive structures, such as the suffix *–sai*, which figures prominently in the current discussion [e.g., Nevins, Pesetsky, & Rodrigues, 2007]. A number of other markers were interesting as well, such as *–só* with the allomorph *-ao* that seemed to express a temporal relation similar to that of temporal adverbial clauses, such as "when." My working definition of recursion was very broad as sentences within sentences and phrases within phrases. I constructed interview settings and other experimental settings in which recursive structures could appear and collected data from various different speakers; I also collected a substantial amount of spontaneous language examples. Some of the work was collaborative with the other researchers in the group.

My conclusions so far are that the markers I identified do not combine clauses and they are not obligatory markers for such clause combinations. Rather, in the case of *-só/-ao* in particular, they seem to mark the detachment from the speech situation, expressing something like "this does not happen right now." For example, an English sentence like "when it rains, I do not go to the forest" would be expressed as two independent clauses in Pirahã: "It rains. I don't go to the forest." One part of the sentence, such as "it rains" would be marked by one of these suffixes to express the detachment from the speech situation, such as "it does not rain right now. " Pirahã makes a lot of use of parataxis, which means that sentences appear side by side. The way clauses relate to each other is usually understood in the context, rather than expressed syntactically. Eugenie Stapert and I have submitted a paper [Sakel & Stapert, in press] discussing various functional motivations for why Pirahã lacks recursive structures.

I tested another of Everett's [2005] claims, namely that the Pirahãs are monolingual even after hundreds of years of contact with the outside world. My findings are that the Pirahã society as a whole is monolingual, but a number of middle-aged men take over the role of communicating with outsiders in a form of Portuguese that is highly influenced by Pirahã. In this variety the grammar is mainly Pirahã with lexical elements from Portuguese. Clauses are combined without syntactic embedding and

connections between clauses are understood in the context. As opposed to the ways in which these elements would be expressed in Portuguese, much attention is paid to marking clauses as “not the case right now” [cf. Sakel, in preparation; Sakel & Stapert, in press].

In conclusion, my findings so far endorse Everett’s claims about the absence of recursion in Pirahã.

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Footnote

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Essentialism, Power and the Representation of Social Categories: A Folk Sociology Perspective

Response to Commentaries

It is fortuitous to have commentaries from scholars whose work represents two major traditions in psychological study of identities that are central to my paper: (1) the cultural-psychological approach emphasizes the situated nature of social identities which are constructed, mediated, and transformed by cultural practices; (2) the social-cognitive developmental tradition focuses on how the interaction between various constituents of a context and the cognitive developmental changes shape representation of various social identities. Both traditions take the social constructivist nature of identities seriously despite their differing theoretical and methodological commitments. They study whether social categories are represented mainly as a category of mind, or whether they only become a salient category of mind by virtue of the social arrangement and the context in which these categories are embedded. Bhatia's response embodies the cultural-psychological tradition and Bigler & Patterson's response exemplifies the social-cognitive developmental tradition. Their stellar empirical and theoretical contributions to their respective fields of inquiry enabled them to situate my paper to make a case for the need to take power, social marginality, and narratives seriously in their respective intellectual traditions.

Bhatia's commentary highlights how postcolonial psychology needs to take social location and marginality seriously to develop a global psychology that gives due emphasis to issues of power and social inequalities. Although cultural psychology has been a dominant voice in the study of cultural practices, it has rarely focused on issues of power and the asymmetries in the marginalized group members' access and ability to fully participate in cultural practices. In his study of Indian immigrants in the United States, Bhatia [2007] has argued for the need to understand "Whiteness" and issues of privilege to theorize the asymmetries in Indian immigrants' ability to "fully" participate in the "American" culture as people of color. His research has documented how Indian immigrants' face the dilemma of how to configure and negotiate their relationship to "Whiteness." Often they essentialized their ethnic identity to construct an alternate positive image of their ethnic identity which is claimed to be rooted in a deep and ancient cultural genealogy. Postcolonial cultural psychology needs to theorize more carefully how members of marginalized communities strategically use narratives of identity to empower, resist, or transcend stigmatizing identities [Mahalingam, 2007]. A folk sociology framework that looks at power and social marginality will be useful to study the cultural psychology of transnational and global identities of growing immigrant populations.

Bigler and Patterson's commentary makes a case for the usefulness of a folk sociology framework to the cognitive developmental research on stereotype and prejudice. For example, Bigler

and Liben's [2006] model explains how social categories become salient and identifies the specific conditions and dimensions in which children develop stereotypes and prejudicial attitudes toward these categories. Their model integrates a vast body of social psychological research on stereotyping and social developmental research on prejudice. I view the process model as a critical link to understand how various contextual factors contribute to dominant group members' use of essentialism to justify their privileged social status. Their process model is a valuable theoretical link to understand how the specific contextual constituents of a cultural practice contribute to the transformation of a social category from a *cognitive to social essentialist* representation. Perhaps individual difference factors, such as social dominance orientation, may also mediate the relationship between participation in the cultural practice and the social essentialist representation of social categories.

It is exciting to see two responses from the two traditions that have strongly shaped the theoretical impetus and motivations of my paper. These commentaries have pointed out several productive possibilities and new directions for research on essentialism using a folk sociology perspective. Future research on cultural psychology that integrates folk sociology and postcolonial theory could look at how power and social marginality transform essentialist accounts of the construction of "self" and the "other." Social-cognitive developmental research could also look at the relationship between cultural practices and narratives in the construction of social essentialist accounts of identity among marginalized social groups to understand how essentialist notions of identity could also be used to resist, disidentify, and transcend stigmatizing representations of their ethnic identity by the dominant groups [Mahalingam, 2007]. These commentaries open up several exciting new possibilities for studying the relationship among strategic essentialism, cultural narratives and resilience in children who grow up in marginalized communities in different cultural contexts.

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