The shrinking Chomskyan corner:
A Final reply to Nevins, Pesetsky, and Rodrigues[1]

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0. Introduction

This paper is a reply to the most recent criticism of my work by Andrew Nevins (University College London), David Pesetsky (MIT), and Cilene Rodrigues (University of Brasilia), in Language (2009, pp671-681; henceforth NP&R). Apparently, their main motivation for replying to my work was to counter the publicity my work has been receiving.[2]

It seems to me that this installment of NP&R's criticisms, like others, fails to offer sound criticism or plausible alternatives to my conclusion that Pirahã syntax lacks recursion. As in their previous work, they focus on minor points without addressing the predictions made by my analysis about the syntax of Pirahã. In fact, they don't even offer any predictions for their own analysis. They propose no experiments to test their ideas or mine. Perhaps most interestingly, they fail to notice the significant fact that even if it turned out that their criticisms of Everett (2005) and Everett (2009) were correct on every point, *their own analysis concludes only that Pirahã manifests at most a single level of embedding - not recursion.* Pirahã would only have recursion under the idea that any language that places more than two words in a row *must* have recursion (NP&R (2009b, 679)). More on this ditsy idea below.

I am posting this paper on the web in order to make my reply widely available in a timely manner. The policy of the journal Language prohibits me from publishing a reply there because Lg. wants to avoid recursive discussions. This seems like a good rule. After all, the paper you are reading here is a reply to a reply (NP&R 2009b) to a reply

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[1] I believe that a lot of this debate, as other debates in linguistics, has at its root the lack of fieldwork experience of many linguists. Although I do not believe that field research is either a necessary nor a sufficient condition for doing good linguistics, it can help get a better 'feel' for the holistic enterprise of language analysis. Bob Dixon expresses a more radical view, one that I do not accept, but one that still still has a grain of good sense to it: *"Once a linguist has served their apprenticeship, as it were, by producing full documentation of a language, they may move on ... to typological comparison...There are some linguists... who avoid the travails of field work and do not themselves produce a grammar, lexicon, and text collection for a previously undocumented language, but attempt straightaway to work on linguistic theory. This is rather like a biologist who has only observed animals in picture books (or perhaps a zoo) and then proceeds to statements about the nature and habits of a particular animal... one should learn the art of analysing a language, constructing a grammar, before embarking on theoretical generalizations based on examination of a selection of good grammars."* R.M.W Dixon (2009: 2-3)

[2] Of course, I have nothing to do with whether or not there is media coverage of the Pirahã or my research. My technical publications are in major journals of anthropology and linguistics and my popular-level scientific book is published by Pantheon in the US. I should say that many journalists have done a better job of understanding my claims and their significance than NP&R.

(Everett 2009) to a reply (NP&R 2009a) to Everett (2005).[3] In any case, I think that with this final reply, that these exchanges between me and Nevins, et. al. have reached the end of their usefulness, given both their failure to suggest predictions or experiments and their name calling (Rodrigues has labeled me a 'racist' in a major international publication (see 1.3. below). Claims like Rodrigues's make it clear that they have a nonscientific agenda.

The nature of recursion in human cognition and its applications in language and other domains is something I address in Everett (2010a).

I have organized the discussion here as follows. First, I discuss Merge and NP&R's portrayal of it as an important background to the remaining discussion. I argue that what NP&R say about Merge globally diminishes both the usefulness and credibility of the Minimalist Program (MP)'s view of recursion and the notion of the FLN (Narrow Faculty of Language), of Hauser, Chomsky, and Fitch (2002, henceforth HC&F).

Next I discuss NP&R's assertion that I have intentionally misled readers of Everett (2009) by backdating or covertly retracting analyses. After this I move on to discuss in succession nominalization vs. old information marking, the verb 'to say', the focus marker, **go-ó**, numbers, Pirahã cosmology, and Pirahã intonation. We should be careful not to lose sight of these predictions in the details of the discussion. Section two discusses the predictions of Everett's (2005a and 2005b) analysis and emphasizes that although these predictions are far from proven, they are consistent with my analysis and supported by standard linguistic evidence. This stands in contrast to the discussion by NP&R, which make no predictions at all. This is followed by an outline of future research priorities for Pirahã. The final section considers the corner that NP&R and Chomskyan theory more generally have painted themselves into: on the one hand, they claim that they do *not* care about my analysis of Pirahã because the claims, even if correct, are irrelevant to the notions of Universal Grammar and Minimalism. But on the other hand they obviously *do* care - objecting to just about every point I have made in my analysis. If the criticisms that Chomsky in particular has raised against me recently are taken seriously, then his work has become non-empirical, as I show.

A caveat: I do not discuss here in general terms the nature of recursion in human language and cognition, because this is addressed in Everett (2010a) and Everett (2010b).

1. Objections & Answers

1.1. Merge

NP&R claim that I have misunderstood what HC&F intended about recursion:

> *"Hauser, Chomsky, and Fitch (2002, HC&F) presupposed, rightly or wrongly, an approach to syntactic structure in which all phrase structure—not just clausal embedding or possessor recursion—serves as a demonstration of recursion. We had this in mind when we noted in NP&R that if Pirahã really were a language whose fundamental rule is a nonrecursive variant of Merge, no sentence in Pirahã could contain more than two words."* Nevins, et. al. (2009: 679)

The first part of the quoted paragraph seems unsupported by an examination of HC&F. The second sentence of the paragraph is, as I have said, non-empirical. In this

[3] But let me emphasize that my main statement on Pirahã grammar and culture is Everett (2008).

quote, NP&R say only this: 'Our theory cannot work unless three words in a row is recursion, so three words in a row must be recursion.' This is a nonempirical, untestable claim. Moreover, I don't believe that anyone would have found the HC&F proposals worth publishing or worth responding to if they had said merely that 'The narrow faculty of language consists in the fact that humans can put more than two words in a row.' Any number of researchers on language abilities in non-humans could point out that under this version of recursion, humans are not the only ones with a 'narrow faculty of language'. NP&R's claim underscores a serious problem for Chomskyan theory. This problem, was pointed out in Everett (2009,439ff), when I said that Universal Grammar has two versions, a nonempirical, tautological version that is little more than a *façon de parler*, and one with an empirical hypothesis, the FLN, which is falsified by Pirahã. Thus the path from the general claim about recursion in HC&F to the content-bleached notion of Merge urged in the above quote leads Minimalism to an intellectual cul de sac. If 'at least three words in a phrase' is all that HC&F intended, then they haven't made much of a claim at all.

Also, if phrase structure or Merge were all that HC&F intended, then they were non-perspicuous. One might even say that they labored to hide any reference to Merge. To see this, let's consider a few of the relevant quotes from HC&F, which I number for convenience (all emphasis, boldface, is mine). Along the way I offer comments on HC&F's text. Though my comments are occasionally orthogonal to the discussion of NP&R's paper, they are relevant to the larger issue of FLN/UG.

(1) *"We assume, putting aside the precise mechanisms, that a key component of FLN is a computational system (narrow syntax)* ***that generates internal representations*** *and maps them into the sensory-motor interface by the phonological system, and into the conceptual-intentional interface by the (formal) semantic system...* ***All approaches*** *agree that a core property of FLN is recursion, attributed to narrow syntax in the conception just outlined. FLN takes a finite set of elements and* ***yields a potentially infinite array of discrete expressions****. This capacity of FLN yields discrete infinity (a property that also characterizes the natural numbers). Each of these discrete expressions is then passed to the sensory-motor and conceptual-intentional systems, which process and elaborate this information in the use of language." (HC&F 2002:1571)*

There is nothing in this quote that mentions phrase structure. It may be that the 'internal representations' HC&F have in mind are phrase structures. But they do not say this explicitly, so they could be most anything, from neuron firings, to pictures that stand for meanings. Moreover, to take a random example, Richard Hudson's (2007) *Word Grammar*, shows us that it is false to say that 'all approaches agree' if by that we mean that all approaches posit phrase structure, since Word Grammar, among others, does not. In fact, in Everett (2010a) I suggest that Pirahã might indeed lack phrase structure altogether, though nothing crucial hangs on this for now.

Let's turn to another quote on the topic of recursion in HC&F:

(2) *"The core property of discrete infinity is intuitively familiar to every language user. Sentences are built up of discrete units: There are 6-word sentences and 7-word sentences, but* ***no 6.5-word sentences****.* ***There is no longest sentence*** *(any candidate sentence can be trumped by, for example, embedding it in "Mary thinks*

that . . .”), and there is no nonarbitrary upper bound to sentence length. In these respects, language is directly analogous to the natural numbers..." (HC&F 2002:1571)

The 'discrete' part of 'Discrete infinity' is largely a red herring. The fact that there are no half words seems to follow from what it means to be a word, phonologically and grammatically, and what it means to express a concept (are there any half concepts?). So I don't see any need to make this either an explanandum or explanans of the computational system. As for infinity, this is indeed a relevant part of languages that the computational system should tackle. It's just not clear where it should be computed. Should we focus on sentences, as Chomskyan theory has done since its inception (using 'S' as the grammar's start symbol, for example) or should we focus on discourse - coherent and cohesive (Halliday and Hasan (1976)) combinations of sentences? Should linguists expect to locate the creativity and infinity of language in discourse construction or phrase structure? And how could they answer such a question in the first place? These are not questions that are raised in the Minimalist Program (or indeed any version of generative theory) because they lie outside its 'solution space'. As I say in Everett (2008) Pirahã sentences do appear to have a finite boundary beyond which no words can be added (though a finite sentence can still be a very long sentence!). Now consider in this light:

(3) *"This is made clear by the observation that, although many aspects of FLB are shared with other vertebrates,* ***the core recursive aspect of FLN currently appears to lack any analog in animal communication*** *and possibly other domains as well."* (HC&F 2002:1571)

Once again, this depends on one's definition of recursion.[4] As I mention in Everett (2008), it is well-known that iteration is a form of recursion (tail recursion). Many animal communication systems involve iteration (just remember the last time you said "I wish that dog would shut up!" - you were referring to the iteration of its barking). If HC&F meant to refer to applications of recursion that produce long-distance dependencies or phrase structure, etc., then they might be correct. But, again, they were unclear. If that's what they meant they should have said so.

(4) *"In fact, we propose in this hypothesis that* ***FLN comprises only the core computational mechanisms of recursion*** *as* ***they*** *appear in narrow syntax and the mappings to the interfaces. If FLN is indeed this restricted, this hypothesis has the interesting effect of nullifying the argument from design, and thus rendering the status of FLN as an adaptation open to question. Proponents of the idea that FLN is an adaptation would thus need to supply additional data or arguments to support this viewpoint."* (HC&F 2002:1573)

HC&F have just taken us from *recursion* a singular noun, to 'core computational mechanism***s*** of recursion', a plurality. These 'mechanisms' are more than any single,

[4] HC&F fail to define recursion anywhere in their article.

general notion of recursion, apparently. So HC&F here admit that FLN is more than simply recursion. But they don't explain what this 'more' consists of. Perhaps they meant MP-type syntactic structures? Or the 'interfaces'? Who knows. This is typical of their lack of precision. But because they are unclear, there is nothing in this quote to support NP&R's claims that HC&F meant 'phrase structure' or Merge when they wrote 'recursion'. It is further important to emphasize that even the assumption that recursion refers to structures that are found in all human languages (in what the MP calls 'narrow syntax') is an empirical hypothesis, certainly not something that 'all approaches' agree to or an a priori truth. See Bybee (2006), Evans & Levinson (2009), Hudson (2007), Goldberg (2006), Croft (2001), and a number of others.

What about HC&F's reference to the interfaces - semantics and phonetics? Do we know a priori that these involve recursion, in spite of HC&F's declaration? Of course not - unless we are back to the unenlightening notion that Merge establishes recursion by fiat. Perhaps this is OK within MP, but not to other linguistic or cognitive scientists. Even if a language has a complex semantics or phonology, there will only be recursion at the 'interfaces' if we can find empirical evidence for things like nested structures, hierarchical organization, and so on.[5] I can only say that to this point in my research, there is no evidence for recursion in the syntax, semantics or phonology of Pirahã. Pirahã certainly has a semantics that builds what are in effect compositional sentence meanings. But it is by no means clear that we need recursion to yield such meanings (see especially Hobbs (2008) and Language Log - http://itre.cis.upenn.edu/~myl/languagelog/archives/005380.html for a paratactic account of semantics). The same considerations hold for the phonology. I return to this in Everett (2010a). The following quote is important because it seems to come closest to motivating NP&R's understanding of HC&F and HC&F's proposal of FLN:

(5) *"... **long-distance, hierarchical relationships are found in all natural languages** for which, at a minimum, a "phrase-structure grammar" is necessary. It is a foundational observation of modern generative linguistics that, to capture a natural language, a grammar must include such capabilities."* HC&F (2002:1577)[6]

Whether humans choose a finite vs. phrase structure grammar is precisely the empirical point that Pirahã raises. The 'infinity' of the Pirahã language, for example, might lie *outside* the grammar in the Chomskyan sense - in discourse - via the ability to fashion stories out of sentences rather than sentences out of phrases. There could, in other

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[5] Since Merge assumes recursion by definition, it would be circular to use it in the investigation. If evidence is found, *then* Merge may be used, if it turns out to be the best way to implement recursion formally. But Merge is not a tool for discovering recursion - nor is the number of words in sequence.

[6] Although HC&F go on to review experiments in which Fitch and Hauser claim to have gotten cotton-topped tamarins to distinguish finite-state vs. phrase-structure grammars, most researchers believe that they showed nothing of the sort (see, for example, http://itre.cis.upenn.edu/~myl/languagelog/archives/002822.html). Assume however that they did show this. Nothing follows for whether or not human grammars can be constrained by cultural values.

words, be a longest sentence in Pirahã, yet not a longest story. If that were the case, then NP&R would be wrong, since Merge applies only to form sentences and phrases from lexical items. And HC&F would be misguided by failing to relate the general property of recursion to stories in lieu of or in addition to recursion in sentences. Theories that do not have anything to say about facts external to sentences (e.g. all versions of Chomskyan theory) cannot appeal to discourse, thought, etc. for support for their theory of grammar, e.g. the role that recursion plays in the FLN. To beat this horse another way, recursion could be responsible for the infinitude of natural languages in a way unanticipated by Chomskyan theory, by allowing infinity to be a property of discourses, rather than sentences.

HC&F claim that what is enables such dependencies is the type of grammar employed:

(6) *"At the lowest level of the hierarchy are rule systems that are limited to local dependencies, a subcategory of so-called "finite-state grammars." Despite their attractive simplicity, such rule systems are inadequate to capture any human language. Natural languages go beyond purely local structure by including* ***a capacity for recursive embedding of phrases within phrases*** [emphasis mine, DLE]*, which can lead to statistical regularities that are separated by an arbitrary number of words or phrases..."* HC&F (2002:1577)

Here we do get a mention of phrases as an output of recursion. Notice, though, the phrase 'recursive embedding'. As I have mentioned, not all embedding is recursive. Yet there are no long-distance dependencies in Pirahã sentences, outside of discourse. So that argument for the universality of recursion is wrong. It seems that HC&F, by this type of quote, have in mind more than mere phrase structure, but especially recursive embedding. Not once do HC&F discuss Merge in their article. They do discuss the importance of phrase structure in the quotes above, but only in the sense of the difference between finite state vs. phrase structure grammars, embedding of phrases within phrases, and long-distance dependencies. They say that the fact that no natural language can be described by a finite-state grammar is 'foundational' to the generative research program.

But Pirahã falsifies these 'foundational observations' if Everett (2005) is correct (more facts are presented, along with these, in section two):

(7) a. There are no "long-distance hierarchical relationships" in Pirahã (though this is somewhat fuzzy because HC&F never define long-distance).

b. Pirahã sentences do have upper bounds (i.e. there are sentences, which I define in Pirahã as potential modification on each word + lexical frame of the verb and the categories mentioned in the verb's lexical frame + absence of recursion to which no further words may be added. I discuss this and give an example in *Don't sleep there are snakes: life and language in the Amazonian jungle).*

c. Pirahã lacks recursion either in its nondiscursive syntax, or at the interfaces so far as I have been able to tell (though more tests are planned for my next field trip, in early 2010). But it most clearly lacks any evidence of syntactic recursion. certainly no phrase within phrases, which was the focus of the HCF.

d. There is no strong evidence even for phrase structure in Pirahã. Though I am not prepared to argue in detail for this hypothesis at present, I offer a non-phrase

structure analysis for Pirahã that seems to account for Pirahã syntax in Everett (2010a).

To return to the question "Did HC&F *really* mean Merge and phrase structure whenever they talked about recursion, long-distance dependencies, and phrase-structure grammars?", the answer is, again, maybe. It is entirely possible that NP&R are correct and HC&F said 'recursion' but meant 'Merge'. Such 'code talk' for followers of MP, or whatever theory Chomsky is defending at the time, isn't uncommon in Chomsky's writings. But because HC&F do not state this, they can only be judged by what they do say, which is recursion, not Merge. On the other hand, if HC&F do mean Merge and if their interpretation of Merge matches NP&R's, in the quote above, then MP and the generative enterprise more generally have reached the end of their Hegelian dialectic.

So I think that NP&R are mistaken about what HC&F actually say. But another possibility exists. Maybe NP&R are doing what they accuse me of: hoping that no one will check the original source, and *backdating* what HC&F say, from 'recursion' to 'Merge' and covertly retracting and changing analyses from 'long-distance dependencies' to any phrase structure at all. I don't know. So I won't accuse them of it.

To better understand the issues, I'd like to turn now to examine the relationship between recursion and Merge and ways to test for it. Merge takes one item from a set of items and joins it to another, either a phrase or a word. If it is lacking in a language how would we tell? Well one way would be to have a language that never allowed more than *one word* in a clause, because more than one would require Merge. I doubt that such examples will be found in natural languages, though, for any number of reasons (see Everett (2010a)). Another way would be to find phrases with ternary branching (because this is not allowed by Merge). Such phrases are argued to exist in recent works, e.g. Culicover and Jackendoff (2005)). Yet another way to show that a language lacked Merge would be to establish that the language in question had no phrase structure at all, e.g. units composed only of words linked by semantics and linear precedence rules. Notice, however, that even if we showed that a language lacked Merge, we would still not have shown that it lacks recursion. Since recursion is more general and Merge is a specific form of recursion, the entailments are the following:

(8) a. Merge □ Recursion
b. ¬(Recursion □ Merge)
c. ¬Recursion □ ¬Merge
d. ¬(¬Merge □ ¬Recursion)

Any discussion of the FLN, UG, or recursion on its own must be careful to adhere to empirical questions. As we look at the entailments between recursion and Merge, the empirical must be kept at the forefront. By (8a), if the data suggest that there is Merge, then there will necessarily be recursion in a language, unless Merge is blocked in some arbitrary way, as NP&R would have it. (8b) shows though that even if we find recursion in a language, this does not mean that the language has Merge. Example (8c) means that if we show that a language lacks recursion, then it cannot have Merge. And (8d) means that even if a language lacks Merge, it can still have recursion. Again we see that if HC&F meant Merge rather than recursion, they were misleading because these are different operations. What is at stake cannot be settled by simply defining Merge or saying that it *must* occur in all languages.

These issues can become even more complicated. So imagine that a language lacks any recursion other than Merge. In this case, let's say that Pirahã might be one, Merge would have to be limited to prevent embedded clauses, nominal phrase recursion, long-distance dependencies and the like. There are various ways one can imagine to restrict the operation of Merge. First it could be restricted to 0 applications in a language (producing only one word per utterance), e.g. this is a $Merge^0$ language, or a $Merge^1$ language or a $Merge^2$ language, etc. Or it could be restricted to 1 application (producing utterances of only two). Or 2 applications (three word utterances) and so on. The possibilities are literally endless if Merge can be limited as NP&R allow in their quote about evidence for Merge (because if it can be limited/bounded, then Merge can be limited for any value from 0 to n iterations). And they are empirical. This is not the simple situation that NP&R, Chomsky, HC&F, or any others describe.

To sum up, the proposal that the FLN consists of recursion must be testable to have any interest. It doesn't matter what theory we are assuming. But while there are in principle ways to test for the presence of the general notion of recursion in a language, there is no way to test Merge, as presented in NP&R. There it is a nonempirical, a priori commitment to a theory. It is not a hypothesis about the data of natural language. Since, as I have said, even if NP&R are correct, Pirahã has no more than one level of embedding, it still lacks recursion or it imposes an arbitrary bounding of Merge. This would require the NP&R account to limit Merge in one of the ways mentioned (since they cannot otherwise derive the fact that embedding is limited to one level, under their analysis). As soon as Merge is formally limited, however, it ceases to be recursion (in any theory except MP). This sets the majority of the evidence for recursion aside. And it means that if that is what HC&F had in mind, they should have said so, because this is not what the majority of readers would have taken away from their discussion.

HC&F do not discuss Merge, directly or indirectly. And if Merge is what HC&F had in mind when they wrote 'recursion', then *John eats sausage* is recursive. I think other species (that HC&F hypothesize to lack the FLN) could produce this sentence just fine in one way or another.

1.2. There was neither backdating nor covert retraction of my analyses

Just as it is counterproductive and irrelevant for me to try to guess NP&R's motives for retracting the claims of HC&F with regard to recursion, i.e. claiming that they meant the specific operation of Merge and not the general property of recursion, it is unworthy of the debate for NP&R to accuse me of backdating and covertly retracting analyses. Even if it looks like I did, unethical behavior of a colleague shouldn't be the first hypothesis entertained, certainly not publicly I should think. A mistake - rather than deception - might be responsible for the matter. Of course, mistakes shouldn't be made in premier journals like Language. But they don't constitute deception. I made a mistake in this case.

I missed a couple of things when I proofread my ms. for Language.[7] I cited the wrong Everett twice in my discussion of the nominalizer **-sai**. Here are the offending passages from Everett (2009), cited by NP&R (2009b):

> *"In Everett 2005 [CA] I analyze them [-sai clauses] as juxtaposed old information. NP&R spend considerable effort to show that my original analysis was better and that -sai is indeed a nominalizer. (E09:408)*
> *"As I observed in Everett 2005 [CA], the distribution of -sai in quotatives is strange if it is a nominalizer/subordinator, but it is expected if it is a marker of old information. (E09:418)"*

NP&R quote me correctly. I mistakenly cited Everett (2005), when I should have cited Everett (2007). On the other hand, on the very next page in Everett (2009), 409, I refer again to the reanalysis of **-sai** as a marker of old information and correctly cite Everett (2007). I won't offer excuses for the mistaken citation. But I it was not an attempt to deceive anyone. There was nothing for to gain from any 'backdating', anyway! What difference could it possibly make to me or to the general debate about Pirahã whether I reanalyzed **-sai** in 2005 or 2007? Certainly, I did introduce this new analysis in 2006. In fact, I discussed it at a talk at MIT attended by Nevins, Pesetsky, and Rodrigues. (See http://tedlab.mit.edu/tedlab_website/news/everett-recording-2006.wma for the complete audio of the talk, including Pesetsky's questions on exactly this point.)

I pointed out indirectly in Everett (2005) how the nominalizer analysis of **-sai** predicts that **-sai** should appear on the content of saying in quotatives, rather than where it does occur, on the main verb. In that sense **-sai** was problematic even then (Everett 2005 - Current Anthropology), in spite of the fact that it seemed to support my nonrecursion analysis, since if **-sai** marks embedding, it is on the wrong clause in quotatives. There is a lot of evidence to support my reanalysis of **-sai** as a marker of old information and that it is not a nominalizer. So let's turn to that now.

1.3. Evidence that **-sai** marks old information not nominalization in Pirahã

The examples below show **-sai** appearing on both nouns, group A, and verbs, Groups B and C. The data come from a text posted on my website (http://www.llc.ilstu.edu/dlevere/Pirahã/vids/index.shtml), '*Kato's baby falls in the fire*'. After the free translation in each example, I give the line number in the text where the example comes from.

*Group A **-sai** on nouns*

(9) **Hi aigía gái hi ig ái big -ó**
3person therefore here 3person -carry sound ground-direction
apaó *-sai* i bas -ápa -ó -xio
head *-old information* 3person bed -head -direction -location
'Yes, and over there on the ground is his head, over by the head of the bed.'
(found in 'Kato's baby' text, line (47))

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[7] Let me say from the outset, however, that this debate has been followed by enough people for anyone to know that it would be naive to try to fool readers by claiming that I said something in Everett 2005 that I in fact did not say.

(10) **Hi t -ab -i -si. Xigiábi -so**
3perso ı sleep -remain -proximate -state seems -conditional
hi -o s ib -áo -b -ábagaí
3person -inside animal hit -completive -down -frustrated initiation
xagaoaaag -i *-sai*
canoe be -proximate *-old:info*
'Even when it seems that he is sleeping, he almost shoots an animal. From inside the canoe (the canoe we know about).'(found in 'Kato's baby' text, line (65))

*Group B **-sai** on main verbs other than 'to say'*
(11) **Xi higíhi aab -ó -p -í *-sai* -xo -ái**
3person flesh bite go -up -proximate *-old:info* -doubt -be
'As has been said, the ants could be biting his flesh.' (found in 'Kato's baby' text, line (55))

(12) **Hi aigía Xoii hi aigía kagí otí *-sai***
3person thus Xoii 3person thus expected angry *-old:info*
associate
-ha -xa -xaí
-comp:cert -emph -be
'Xoii thus, Xoii thus was very angry with his wife.' (found in 'Kato's baby' text, line (67))

-be
-frustrated:initiation
baósa ó -xiai -hí
cloth direct -potential -interrogative
'It almost fell down (as we have been saying) into (the fire)? Did the cloth fall into (the fire)?'
(found in 'Kato's baby' text, line (90))

*Group C **-sai** on 'to say'*
(14) **Xaí tíi g -á *-sai* gai gói.**
thus 1person comit. -sound *-old information* here right there
hi o -b -á -ab -ábai hoaí.
3person direct -down -vertical -remain -frustrated fire.
completion
Hi sá -i -xí.
3person cry -proximate -comp:cert
'Thus I spoke right there. He almost sat in the fire. He's crying.'
(found in 'Kato's baby' text, line (93))

(15) **Xoii hi aigía g -á -sai -híai -aí.**
Xoii 3person thus com. -sound -old:info -hearsay -declar
Xí oho -ab -a -haí pixái -xíga
3person eat -remain -vertical -rel:cert now -emphatic
xío -si ib -i -haí his -ái
inside:body -name hit -proximate -rel.cert sun -be
'Xoii thus spoke (as we have been discussing) it was heard. "(I) will eat you right now ('eat' = have sex). (I) will heat inside you in the daylight.' (implying that she is too sleepy to know better) (found in 'Kato's baby' text, line (61))

(16) **Tíi xaigía g -á *-sai.***
1person thus comit. -sound *-old:info*
Xihi -ábío
put -remain:with
hí ogí -ó -xio *-sai*
3person big -direction -with *-old:info*
'I thus spoke. I will put (it) with its mother (that we were talking about).'
(found in 'Kato's baby' text, line (99))

To sum up, because **-sai** may appear on nouns, as well as a variety of *main* verbs (verbs which appear in a solitary clause with no candidate for a complement clause), it does not appear to be a nominalizer, contra Everett (1981/1986). I believe that it marks old information. Whatever it marks, however, since **-sai** is not a nominalizer (and because there is not no other candidate for nominalizer in Pirahã), there seems to be no formal marker of complements or embedding of any kind in Pirahã. This is discussed further in section two below.

NP&R argue that **-sai** could be one polysemous or two homophonous suffixes:

> *"Even if the claim were true, it would not argue against an analysis of -sai as a marker of clausal embedding or nominalization, since -sai might be ambiguous between a determiner marking old information and a clause-embedder or nominalizer. Much the same dual function, after all, is played by English that and German das(s). (We thank Brian Joseph for raising the possibility that the multiple uses of -sai are homophonous and reflect diachronic convergence.) Furthermore, -sai could also mark a clause as old information while simultaneously functioning as its nominalizer; the two functions are not incompatible."* NP&R 372

This reasonable hypothesis has no bearing on the issue of recursion, however. For example, the data in above show a typical range of functions for **-sai**. It doesn't appear to be a nominalizer, certainly not in groups A and B, in which it appears on sounds and fully inflected verbs. But even if someone wanted to hold on to the idea that **-sai** is a nominalizer *some* of the time, the cases taken together in which it might be offer no support for recursion.

To see why, consider the examples (12), (14), (15) and (16), repeated here:

(12) **Hi aigía Xoii hi aigía kagí otí *-sai***
3person thus Xoii 3person thus expected angry *-old:info*
associate
-ha -xa -xaí
-comp:cert -emph -be
'Xoii thus, Xoii thus was very angry with his wife.' (found in 'Kato's baby' text, line (67))

In this example, inflection follows **-sai** and there is no possible subordinate reading for the verb, since it is the only one and clearly part of a single, independent sentence. It is unlikely that **-sai** is a nominalizer in such examples.

(14) **Xaí tíi g -á *-sai* gai góí.**
thus 1person comit. -sound *-old information* here right there
hi o -b -á -ab -ábai hoaí.
3person direct -down -vertical -remain -frustrated fire.
completion
Hi sá -i -xí.
3person cry -proximate -comp:cert
'Thus I spoke right there. He almost sat in the fire. He's crying.'
(found in 'Kato's baby' text, line (93))

(15) **Xoii hi aigía g -á -sai -híai -aí.**
Xoii 3person thus com. -sound -old:info -hearsay -declar
Xí oho -ab -a -haí pixái -xíga
3person eat -remain -vertical -rel:cert now -emphatic
xío -si ib -i -haí his -ái
inside:body -name hit -proximate -rel.cert sun -be
'Xoii thus spoke (as we have been discussing) it was heard. "(I) will eat you right now ('eat' = have sex). (I) will heat inside you in the daylight.' (implying that she is too sleepy to know better) (found in 'Kato's baby' text, line (61))

In these examples, if **-sai** marks nominalization, the wrong clause appears to be nominalized, as I have pointed out before. The verb 'to say' should not be nominalized, especially if nominalization is associated with subordination, since it is semantically the main verb. So if we analyze **-sai** as a nominalizer here, that wouldn't argue for or against recursion.

(16) **Tíi xaigía g -á *-sai.***
1person thus comit. -sound *-old:info*
Xihi -ábío
put -remain:with
hí ogí -ó -xio *-sai*
3person big -direction -with *-old:info*
'I thus spoke. I will put (it) with its mother (that we were talking about).'
(found in 'Kato's baby' text, line (99))

In this example, a very common type, both clauses are marked by **-sai**. This would make no sense if **-sai** were marking nominalization. But such double **-sai**-marking is predicted by the 'old information' translation of **-sai**. Outside of conditionals, these examples largely exhaust the range of uses for **-sai**.

Notice too that these examples all show that **-sai** is fully compatible with verbal inflection.

NP&R make another statement about **-sai** that seems at odds with the data:

"Furthermore, as far as we can tell from the available data, a -sai clause fulfills the selectional requirements of some nearby predicate—just as we expect from a nominalization or embedding marker." NP&R 372

This remark is incorrect. Look at the examples in Group C above. NP&R's proposal is unable to account for direct speech, in which the 'matrix clause', marked by **-sai** in their hypothesis, would never be supposed to be selected by the quotative complement in any theory.

To sum up, then, if **-sai** marks old information, then this is consistent with the proposal that Pirahã lacks recursion, since there would then be no marker at all of subordination in the language.

Summary

NP&R's remarks just seem off the mark in about every respect. More troubling, there is growing evidence that scientific argumentation is merely a guise for them to oppose me. In a new article in the German magazine, GEO, Cilene Rodrigues, p59, says baldly that:

> *"Everett ist ein Rassist. Er stellt die Pirahã auf eine Stufe mit Primaten", sagt die Linguistin Cilene Rodrigues, eine Anhängerin Chomskys... ("Everett is a racist. He puts the Pirahã on a par with primates," says the linguist Cilene Rodrigues, a follower of Chomsky's...)*[8][9]

I have no reply to this. It is like asking 'When did you stop beating your wife?' Or trying to prove to your spouse that you have been faithful. But if the evidence that I am a racist, after giving thirty years of my life in friendship to the Pirahãs, is that I have claimed that they lack numbers or syntactic recursion, then this is not a concept of racism I am familiar with. Still, let's consider how my claims could be misconstrued as racist. The consideration is much more revealing about the assumptions of some linguists than it is about the issues involved with Piraha grammar.

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[8] Let me make it clear that I *do* place the Pirahãs on a par with primates. I put myself with the primates, you, and all other humans. We are primates.

[9] On January 18, 2010 Rodrigues sent me the following email:

"Dear Dan,

I would like to let you know that I have ever stated "Everett is a racist" nor that "he puts the Piraha on a level with primates". This does not express my opinion. Not only do I not consider you a racist, I also know full well that humans are primates and would never have expressed myself in the manner the magazine GEO attributed to me.

All the best,

Cilene Rodrigues"

I forwarded this email to the GEO Magazine reporter, Malte Henk. He replied that GEO sticks by its story.

My own theory, Ethnogrammar (Everett 2010b) makes the case that language is a tool for communication and thought. Different components of language, e.g. recursion, binding, phrase structure, and so on, are themselves subtools. Numbers, color words, and the like are themselves cognitive tools. To say that Piraha culture doesn't need or desire certain cognitive tools is no more to disparage them than it is to criticize someone who doesn't play golf for lacking a set of golf clubs.

On the other hand, if you believe that UG or the FLN is part of the human endowment and that languages are cut from a standard human cloth, then if I deny something that is today all the rage as a core property of UG, then I am claiming, by this distorted logic, not that UG is misguided, but that the people whose language falsifies it (ex hypothesi, I am not claiming that I have falsified it) are somehow inferior. This is wrong. But some linguists buy into it. In other words, if my theory predicts that all people must have 'x' and you claim that some people lacks 'x', then they must not be people after all. That is crazy, it seems to me. But it seems to be the only reasoning that could explain the shrill shouts to the effect that if I claim Piraha lacks recursion or numbers that it is somehow subhuman in my view. As John Wayne used to say, 'Not hardly'.

Now let's turn to consider the verb 'to say' in Pirahã.

1.4 'to say' in Pirahã

With each criticism and comment of fellow linguists, I return to my data to rethink portions of my analysis. This has led me to a reanalysis of the verb 'to say', **gai**, which is in fact the verb for comitative action **ig** (as in **ig-aha** 'to carry' or 'to cross the river in a canoe/on a board/etc.', and so on,) + the verb to make sound, **ai** (as in **xai -ai-baai** 'to make a lot of noise'). Instead of meaning simply 'to say', it means 'to carry/be associated with sound'. I hadn't thought about this much before. The reason is that since sequences of like vowels are often shortened in Pirahã, **Tíi → Ti**; **Tii + ig → Tig**, etc., it is often impossible to hear the /i/ at the beginning of **igai** as a separate sound, because all the short forms of Pirahã pronouns end in /i/ (tii, hi, gi - see Everett & Thomason (1990)). I have long been aware that this was a possibility, but Hjelmslev's (1943) comments on glossemes was sufficient to make me worry about chopping things too finely.

The form of the verb I discussed in Everett (2005), **gái** (which should in fact be **ig-ái**) almost always occurs with **-sai**. As NP&R say, I was clear on this:

(17) T**i** **gái** **-sai** **Kóxoí** **hi** **kaháp** **-ií**
I say -nominative name he leave -intention
'I said that Kóxoí intends to leave.' (lit. "My saying Kóxoí intend-leaves.")

*'The verb 'to say' (**gái**) in Pirahã is always nominalized. It takes no inflection at all. The simplest translation of it is as a possessive noun phrase "my saying," with the following clause interpreted as a type of comment. The "complement clause" is thus a juxtaposed clause interpreted as the content of what was said but not obviously involving embedding.'*

This was very unclearly stated and NP&R are right to call me on it. When I wrote it, I was thinking only of the most common tonal form of the verb 'to say'. This sequence

of the verb, **g, á, i** seems to always be followed by **-sai**. However, there are other forms that are not, as data in (18)-(22) below show.

I should have said 'The form of the verb 'to say'... NP&R are correct to point out what appeared to be an inconsistency since, as the following examples show, the verb to say in other forms may be fully inflected (again, the line number in the Kato's baby text is given following each free translation, since this text is where all the examples below come from):

(18) **Ti xaigía ig -á -xai -ai. Xaí ti aigía hi xoi**

1 thus com. sound -do -INTEN. Then 1 thus 3 jungle

(biosphere)

-si k -a -o -p -aí

-old info it (obj) -move vertical -direct up -do

'I spoke. Well I will go pick him up.' (From Kato's baby text, line 8).

(19) **Ti xahoa -gí ti ig a -ó -p -í -xi**

I day -real I com. -sound -MOT -up -proximate -DECL.

hi ig -á -xai baígipóhoasi. Kató hi ob -a -p

3 com. -sound (verb/noun) -do Baígipóhoasi. Kato 3 eye -move vertical up

-í -aag -oxoihí Kató

-proximate -be -INTER Kato

*'Early in the day I spoke (lit: carried sound)[10]. That **Baígipóhoasi** spoke (carried sound) "is Kato sleepy?"'* (From Kato's baby text, line 1).

(20) **Ti ig á -xai -ai. Hi big a -í**

1 com. sound do -INTEN 3 ground move vertical -proximate **-is -áaga -haí Tixohóí**

-cry -be -RELCERT Tixohói

'I spoke (carried sound). Tixohói is crying on the ground.' (From Kato's baby text, line 3).

(21) **Ti xaigía -ig -á -xai -ai. Xaí Opísi**

I thus -com. -sound -do INTEN Then Opisi

hi o -b a -hohí pixái-xíga Tixohói

3 vertical move -down remote INTER now-EMPH Tixohói

'I spoke to Opisi. 'Did Tixohói burn himself just now?' (From Kato's baby text, line 5).

(22) **Ti xaigía ig -á -xai -aí. Xopísi hi o -b -a**

I thus com. -sound -do INTEN. Xopisi 3 eye down vertical-

[10] The verb 'to say' (as I originally glossed it) has been puzzling to me for quite some time, due to the fact that it always acts as a single clause.

-p **-í** **-aag** **-ab** **-i** **-sahaxaí**
up -proximate -be DUR -transitional vowel -PROHIBITIVE
'Thus I spoke. Xopisi, don't be so sleepy (lazy)!' (From Kato's baby text, line 10).

Let's turn now to my reanalysis of the particle **go** as a focus marker, another analysis that NP&R criticize as unsupported by the data. This particle found in interrogatives, relative clauses, and elsewhere.

1.5. **Go**

In Everett (1986) I described various uses of **go**. I did not explicitly consider at that time that it might have a more general function, as a deictic and focus marker. Here are some examples of its various uses:

Idiomatic use of **go:**

In a common idiom:

(23) **go gí** 'What('s up with) *you*?' (Idiomatic - you are in focus)

To indicate 'where' and 'there' (focus + locative suffix)

Locational use:

(24) **Hi go -ó -xio tiaos -ai. i go -ó**
3person focus -direction -location hip -be
3person focus direction
-xio
location
'His hips are over there. Over there.'

Interrogative use:

(25) **Hi go -ó. ti óo -s -aaga.**
3person focus -locative/directional. 1person ignorant -transition
-be -
báas á -p -ai. Go -ó -há
sleeping:platform vertical-up -do/be. focus -locative compl:cert
'Where is he? I do not know. (one speaker to the other) His hips are near the head of the bed.
(answer to first speaker)'

(26) **Ti xaigía hi xo -aó -p -ao.**
1perso ɪ thus 3perso ɪ jungle -direction -up -completive
g -á -xai -ai. Toíao hi go g -i -aí
com. -sound -do -declarative Toíao 3person focus com. -prox.
-do
sito -aó -p -i -hí
rise -completive -up -proximate -interrogative
'Well I went to him. I spoke. Toíao why are you getting up? (lit: what is associated with your rising?)'

(27) **Kaxaxái hi go - ó xaabáítá xo -ó.**

name 3 FOC -loc lost jungle -locative
'Kaxaxái [topic] got lost in the jungle.'

1.6. Numbers

The conclusions of Frank, et. al. (2008) that Pirahã lacks numbers seems as sound as the day we published it, though alternative interpretations might be possible with more data, as always. But as further evidence from a very distant source, I have posted a casual conversation involving numbers with Kaioa Pirahã (http://www.youtube.com/watch?v=SHv3-U9VPAs).

1.7. Pirahã texts

As I have mentioned in many papers and in Everett (2008) at length, in my observations, the topics of Pirahã discourses, conversations, and stories never include cosmology, except when pressed by outsiders. And as I have also discussed at length, when they do discuss this topic with outsiders, their stories are repetitions of common stories that they have heard from *caboclos*. Not one single researcher or missionary in recorded contact with Pirahã has ever collected a single Pirahã text about creation or cosmology in general. Hence the claim that they have no creation myths or stories.

1.8. Intonation

NP&R take me to task for saying that intonation is not a 'smoking gun' for recursion. Let me make it clear that it can be very important evidence when it is carefully analyzed and when the researcher realizes that the phonetics of intonation interprets phonological constituents, not syntactic constituents. The relationship between phonology and syntax is close but, as we have known since Pike's work in the 50s and 60s, the relationship between phonology and syntax is nonlinear.

However, Everett and Oliveira (in progress) does address Pirahã intonation in detail and offers suggestions on its relevance to Pirahã syntax.

2. Predictions of non-recursive syntax[11]

The following facts are all predicted by if Pirahã lacks recursion. However, they are nothing more than a set of mysterious coincidences in the approach of NP&R. Let me emphasize this. If just one of these facts obtained in a particular language it would be a matter of curiosity and something in need of explanation. That they all occur in a single language is even more striking (but see footnote __ above. I am not claiming that these have been proven. But their absence from texts and conversations is striking). My hypothesis, that recursion i s lacking in Pirahã, predicts all of them. The NP&R account predicts none of them at all. Their cumulative effect argues strongly against the NP&R analysis. Interestingly, in none of their papers do NP&R ever address these facts as a set. They never say how their analysis would account for them nor what the implications are for my analysis since it does predict all of them.

I hasten to add that even if I am right about all of these things, this doesn't mean

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[11] As my former Manchester University colleague, John Payne points out, we'd need to rule out the possibility that the lack of these characteristics are not due to sampling error or unrelated historical developments. True enough, but my account predicts them and so, for now at least, I take their absence in Piraha to support my position.

that Pirahã lacks recursion. All linguists know that there are functional alternatives to these formal devices. My claim must also entail that there are no functional alternatives employed in Pirahã and I need to test for the presence of such alternatives more thoroughly. On the other hand, the absence of all the formal devices is predicted by and consistent with my proposal that Pirahã lacks recursion.

The predictions are:

1. Pirahã lacks factive verbs and epistemic verbs, except for a use of the verb 'to see' for 'to know'

If Pirahã lacks recursion, then we predict that there is no way to express factive verbs as independent verbs, since they would then require a complement clause, requiring embedding and thus, likely, recursion. Pirahã expresses such notions via verbal suffixes, consistent with the 'no recursion' hypothesis.

2. It has no marker of subordination.

This is also predicted by my hypothesis, because if P lacks recursion, there is no subordination to mark.

3. It has no coordinating disjunctive particles.

The absence of explicit markers of disjunction is predicted by my hypothesis, since disjunction entails recursion.

4. It has no coordinating conjunctive particle. There is only a more general particle, piai, which may appear preverbal or sentence final and which means 'is thus' (vague meaning), which never works like proper conjunction, but only supplies the information that these two things were simultaneous (it is related to pixai, now).

Again, this is predicted by my analysis, since coordination also entails recursion.

5. No unambiguous complement clauses.

If Pirahã actually had recursion, where is the unambiguous data? I have claimed that it lacks them. NP&R claim that it has them, but only after stretching and straining to show that this or that example *could* be embedding. But they never hint from any earlier data of mine that there could be multiple levels of embedding, which certainly would be expected under their analysis, if Pirahã has recursion like any other language. What would stop multiply embedded clauses under their analysis? Nothing. And yet even they don't find any evidence for this.

6. No multiple possession.

The point of Pirahã possessives that I have made is not simply that it lacks prenominal possessor recursion, but that it lacks recursion of possessors *anywhere* in the noun phrase. NP&R might be correct to suggest that German, like Pirahã, lacks prenominal possessor recursion. But German does have postnominal possessor recursion. Pirahã has *no* possessor recursion. This is predicted by my analysis, but not by theirs.

7. No multiple modification.

As I have discussed in Everett (2008) and (2009), there can at most be one modifier per word. You cannot say in Pirahã 'many big dirty Brazil-nuts'. You'd need to say 'There are big Brazil-nuts. There are many. They are dirty.' This paratactic strategy is predicted by my analysis since multiple adjectives, as in English, entails recursion, but the paratactic strategy does not.

8. No scope from one clause into another:

'John does not believe you left' (where 'not' can negate 'believe' or 'left', as in 'It is not the case that John believes that you left' vs. 'It is the case that John believes that you

did not leave')

In this example 'not' can take scope over 'believe' or 'left'. That is not possible without recursion, so my analysis predicts the absence of such scope relations. In (), 'who' is at the beginning of one clause but holds a semantic relation to another clause. This is also predicted, correctly, to be impossible in Pirahã under my account, since it would entail recursion. But it is not predicted by NP&R's analysis.

9. No long-distance dependencies:

'Who do you think John believes __ (that Bill saw__)?'

'Ann, I think he told me he tried to like ___'

3. The shrinking chomskyan corner

3.1. The ever diminishing claims

In the 'golden age' of generative grammar (a few months in 1965), it seemed like the formalisms and proposals of Chomskyan theory were on the verge of providing the first truly explanatory theory of language, its nature, use, acquisition, and origins, in the history of the study of language. It was original, brilliant, and promising. Scores of linguists, psychologists, philosophers, computer scientists, and others came to believe that it might be the most significant breakthrough ever in the study of language. Language was innate and it was a set of rules available to all languages.

But it didn't take long for cracks to appear. The biggest crack, the proper theory of meaning, led to what Randy Harris (1995) has called 'The Linguistic Wars'. From that point on, a division grew in the field between so-called functionalism and formalism. Chomsky's position changed dramatically during and after these linguistic wars. But each successive new proposal (all with their own names, e.g. Extended Standard Theory, Revised Extended Standard Theory, Government and Binding Theory, Principles and Parameters Theory, and now the Minimalist Program) met with so many counterexamples and difficulties that, in the opinion of many who have followed the debates and developments for decades, Chomsky was pushed into the corner that led him to claim, along with Hauser and Fitch, that what was really essential to language/grammar is recursion (of some unspecified variety).

In more than fifty years, Chomskyan theory has made surprisingly few empirical discoveries about language - the 'island constraints' of Ross (1968), first noticed by Chomsky (1964) and 'parasitic gaps' (also discovered by Ross in the 60s).

I think that part of the violent reaction to me and my proposals, apart from the publicity, which has exacerbated the ill will, is the fact that if a language lacks recursion, then it is difficult to make the case that it is the underlying cognitive capacity specialized for and enabling human grammars/languages. Although many people, including Chomsky, Bickerton (see below), Hauser, and others, claim that even if I am right, it is irrelevant for theories of language, I think that the nastiness of the attacks and their various forms shows that they do not really believe this (this in spite of that fact that Chomsky has recently said, in GEO Magazine, January 2010, that my only motive is to be famous but that famous people must have ideas and I have none). I discuss Chomsky's own bizarre response to me in my paper in Language 85:2. But let me discuss now another very common reaction, using a recent quote from Derek Bickerton.

3.2. Recursion in cognition and the 'toolbox' argument

3.2.1. Is Pirahã irrelevant?

In a recent book, Derek Bickerton (2009,238ff) discusses the case of Pirahã and claims that Pirahã has no bearing on Universal Grammar one way or the other. This is a common enough claim, one that I believe is based on a superficial understanding of the issues, but it is worth considering in some detail. Bickerton says that:

"What hardly anyone noticed [in the debates about Pirahã, DLE] was that it didn't make the slightest difference whether Everett was right or wrong... Suppose he was right. Then the only question was, could a Pirahã baby learn a language that did have recursion? If it could ... [Everett 2008 in fact gives examples of exactly this and Pirahã babies certainly can learn languages with recursion] then the absence of recursion from Pirahã grammar might be rarer, but was no more remarkable than the absence of ... clicks... from English."

This is about as deep a misunderstanding of the issues as I can imagine. The claim of HC&F was that recursion *is* the FLN (narrow faculty of language). They never claimed this about clicks. And there is good reason for that. The point of their discussion was to establish that the computational system of humans enables human languages to be nonfinite, to have infinite communication systems with only the finite means of the human brain. Recursion is at the heart of all of this. Moreover, HC&F (whether under my exegesis or the interpretation of NP&R) were concerned with building linguistic structures. And in Chomskyan linguistic theory, discourse has always been outside the computational system proper. This means that HC&F's proposal was about the sentential syntax using recursion and being nonfinite. So, whether a Pirahã child is able to learn recursive structures (again, they most certainly can) is irrelevant. This only shows that the recursion we already know to be a general cognitive ability, based on Pirahã discourse, can also be exploited in the grammar. That is not the proposal of HC&F, however, who want recursion to be responsible for infinitude of sentential syntax. If Pirahã sentential syntax is neither nonfinite nor uses recursion, then for HC&F and the Chomskyan notion of Universal Grammar as the biological endowment underlying *sentential syntax*, the jig is up.

It matters not at all if human reasoning is recursive via human discourse/story structure, etc. The FLN is falsified if a language can be shown that expresses its nonfiniteness in discourse but not in units of sentence size or smaller.

3.2.2. Why the toolbox argument fails

In recent years, a number of researchers have begun to argue that Universal Grammar is in fact a set of features that languages can choose from. Some languages choose some features, other languages choose others.

The problem with this view is that it entails a lack of clarity as to the source of these linguistic features. The question of interest has always been whether or not there is a specific component of the brain dedicated to language, Universal Grammar. All researchers agree that humans are uniquely capable of learning language. But some believe that there is no need for a Universal Grammar but that general cognitive abilities, e.g. intelligence, learning capacity, and so on, as well as the general way that the brain is wired, are responsible for language and many other skills. Saying that that there is a large grab bag of features to draw upon for language could mean that these features come from general problem-solving capabilities of Homo sapiens and not from a Universal

Grammar. In fact, the 'toolbox' hypothesis favors the former interpretation over the latter it seems to me. Therefore, if what I have shown is that recursion is part of a toolbox, then this still does not tell us where the toolbox is located nor how specialized it is. Why suppose that it is completely dedicated to sentential grammar (the Chomskyan limitation), when the evidence shows that recursion can be found in cognition generally, as in Pirahã discourse? The answer is, no reason to suppose this at all given our current knowledge.