PRAGMATICS & COGNITION

Board of Consulting Editors

Culture – Language – Cognition

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Opening remarks

Most scientists would be thrilled, as I am, to have their ideas taken seriously enough to be made the focus of an entire journal issue. Before beginning, therefore, let me offer my profound thanks to Marcelo Dascal and each of the authors of this special edition, for taking the time to shape and contribute to this volume about Language: The Cultural Tool. Their cogent and incisive criticisms have helped clear my thinking on many issues.

This is not to minimize disagreements between me and my commentators. There are many and some are profound, touching on foundational assumptions about linguistics, as well as the philosophy and psychology of language and the mind.

Overall, however, this discussion as a whole ought to convince readers of two things. First, that many researchers agree that the idea of an innate Universal Grammar, UG, (of any variety) has passed its sell-by date. Second, that the role of culture is greater than they may have thought prior to reading this special issue of Pragmatics and Cognition.

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Introduction

Marcelo Dascal

Dear readers, authors, reviewers, referees, members of the Board of Consulting Editors, Associate Editors and all those who have collaborated throughout the years with *Pragmatics & Cognition*.

Welcome to this *very* Special Issue, which commemorates the 20th year of existence of our journal.

Were it not for your interest, loyalty, enthusiasm and steady cooperation, the journal would not have achieved the aims set up at its foundation and the prestige it enjoys. To refresh our memory, here is how the Introduction to Volume I spelled out our vision:

> A new journal, specially an interdisciplinary one, helps to shape a new research niche, carved out by a critical mass of work already in the making, but which has not so far found an adequate vehicle of diffusion and crystallization. The niche *Pragmatics & Cognition* has identified, and purports to develop, lies at the intersection of two rapidly expanding areas of research: <u>Pragmatics</u> and <u>Cognition</u>. Each of these areas is concerned with one of the two most important kinds of (human) activity — the use of symbols in the performance of mental operations. Though the interdependence between these activities has been often asserted and debated, it has not so far received the kind of systematic attention and specific research it well deserves. It is to the investigation of the interrelations between these two domains that *Pragmatics & Cognition* is primarily devoted.

The Introduction goes on stressing the broad sense in which both 'Pragmatics' and 'Cognition' should be taken in our newly created journal, the variety of disciplines for which it would be relevant, and the necessary presence of discussions and critical reviews as a sine qua non for the growth of understanding and knowledge, especially for shaping an interdisciplinary niche that purports to become an open space rather than a closed disciplinary sphere.

The research niche *P&C* developed turned out to be far from static. It had to satisfy a growing interest, manifested in many submissions and in proposals of expanding its scope towards new aspects of its announced purposes. As a result, in the course of its dynamic evolution, *P&C* not only fulfilled its initial promises

and goals. It was virtually forced to expand its original vision, slowly encompassing many more domains and disciplines, where the interaction between pragmatics and cognition is acknowledged as a fundamental ingredient and is usefully explored both in the journal's regular and Special Issues. Part of this expansion consisted in acknowledging the relevance of recent technological developments to our concerns. Accordingly, we launched a series of Special Issues focused on 'Cognition and Technology'. The first of these Special Issues, published in 2005 as Volume 13, Number 3, explored the Cog-Tech connection, bearing the title "Cognitive Technologies and the Pragmatics of Cognition". The significance of the pragmatics-cognition-technology interface was soon recognized by authors and readers and was thereafter naturally embedded in the ensuing regular issues of *P&C* as articles, discussions and book reviews. From our modest two issues per volume the journal's size thus jumped to three yearly issues, a total of 600 pages per volume.

It is in the light of this brief historical summary that the present Special Commemorative Issue has been conceived. Its chosen theme and focal author illustrate the journal's original goals, achievements, and issues debated and show why *P&C* is proud to have become a dynamic forum for innovative thinking and dialogue, and looks forward to continuing to pave the way for ever more to come.

In Volume I, Number 1, 1993, an American linguist who graduated at the University of Campinas, Brazil, specializing in Amazonian languages, published a paper titled "Sapir, Reichenbach, and the syntax of tense in Pirahã". Daniel L. Everett, who had been working in the Amazon forest with the Pirahã tribe for years, and became Associate Professor and chair of the Department of Linguistics at the University of Pittsburgh, argued in this paper that the adoption of a parametrized neo- Reichenbachian model of tense syntax was able to account for tense-related facts in Pirahã, e.g., for its speakers' lack of concern for precision in time statements. His conclusion was that this model offered new support for Sapir's linguistic relativity hypothesis. He was thus suggesting a syntactical solution for what rather a context dependent, viz. pragmatic phenomenon. Everett's hypothesis thus fit as a glove *P&C*'s objectives: the investigation of the interdependence between the use of language and cognition, the growing development of new trends in linguistics and in the recently born Cognitive Sciences, the acknowledgment of the necessarily interdisciplinary character of these disciplines, and the elaboration and use of so far unknown empirical data and still unemployed theoretical logical-philosophical models in linguistics.

One year later, *P&C* published, in Volume II, Number 1, Daniel Everett's large review article "The sentential divide in language and cognition", where he criticizes the collective volume *Pragmatics of Word Order Flexibility* (Payne (ed) 1992). Unlike his former paper, with data referring to the Pirahã language, this is a

theoretical article that "presents no new data" and seeks to reexamine "important issues that are far from settled" (Everett 1994: 132). The issues in question bear upon basic assumptions of generative grammar, such as the fundamental role of syntax. Therefore the article couldn't but be explicit, if compared to its predecessor, about the relationship between syntax and pragmatics. Against those linguists who argue that sentences shouldn't be studied in isolation because "the structure of sentences is largely the result of constraints imposed upon them by the discourses they are embedded in", Everett contends that this is a misguided approach, for "sentence-level syntax and discourse structure constitute distinct domains" (Ibid.: 131), which require different explanatory frameworks: "discourse and sentence structures illustrate two types of cognition, *dynamic* vs. *static*" (ibid.).

The divide in question is not only cognitive but also linguistic and it is quite radical, for it is described as involving both epistemological and ontological factors (Ibid.: 131–132). The gap between discourse and sentence is so deep because, while the former "may exert a diachronic influence on the production of some linguistic forms, its essential relationship to form is opportunistic — discourse *exploits* form as it finds it" (Ibid.: 132). Everett therefore presents the central conceptual contributions of the paper as a double dichotomy, namely "the notions of *static* vs. *dynamic* cognition and *form-structuring* vs. *form-selecting* operations" (ibid.).Consequently, he claims that "a full understanding of the relation between language and cognition must recognize the sentence as the principal dividing line between two types of cognition, … *dynamic* vs. *static* cognition" (ibid.), and warns that "failure to recognize [these distinctions] leads to serious confusion about the relationship between grammar, cognition, and pragmatics" (ibid.).

He concludes that, "contrary to the claims of the book under review, syntax must be studied independently of pragmatics and that such study is, by definition also a study of cognition, static cognition" (Ibid.: 133). Further conclusions are that "principles of sentence grammar cannot be discovered by studies of discourse, but only by careful, theory-guided examination of individual sentences" (Ibid.: 131) and that, due to the fact that "it draws on multiple cognitive domains, discourse itself is inherently resistant to analysis by any single theoretical framework or discipline",[1] whereas sentence grammar … is amenable to study by a single discipline, i.e., linguistics" (Ibid.: 131).

Everett's argument for the strict dichotomous distinction between discourse and syntax, or pragmatics and grammar, entails a radical gap in the study of language, up to the point of granting to one of the two components of this study the privilege of being *the* single discipline entitled to be called 'linguistics' or 'the scientific study of language'. He was thereby taking a stance in an ongoing polemics, as his 1994 article reveals. Indeed, the dichotomy adopted by Everett had already been used by Chomsky in the 70's, albeit in terms of another, deeper counterpart, which

he believed to be capable to determine what was either a proper object of scientific research or not — his famous distinction between 'problems' and 'mysteries':

> It may be that the operative principles are not only unknown but even humanly unknowable because of limitations on our own intellectual capacities, a possibility that cannot be ruled out a priori; our minds are fixed biological systems with their intrinsic scope and limits. We can distinguish in principle between "problems", which lie within these limits and can be approached by human science with some hope of success, and what we might call "mysteries", questions that simply lie beyond the reach of our minds, structured and organized as they are, either absolutely beyond those limits or at so far a remove from anything that we can comprehend with requisite facility that they will never be incorporated within explanatory theories intelligible to humans. We may hope that the questions we pursue fall into the domain of "problems" in this sense, but there is no guarantee that this is so (Chomsky 1980: 6).

Nevertheless, what for sure belongs to the domain of "problems" according to Chomsky is, in his terminology, "the language faculty" thanks to which humans acquire their "grammatical competence", defined as "the cognitive state that encompasses all those aspects of form and meaning and their relations, including underlying structures that enter into that relation, which are properly assigned to the specific subsystem of the human mind that relates representations of form and meaning" (Ibid.: 59). For him, "a grammar, then, purports to be a true theory of one of the cognitive structures attained by a particular language-user, and linguistic theory may be regarded as a theory of one aspect of the "initial state of the organism", a species-specific property common to all normal humans" (Chomsky 1977: 33).

Yet, knowing a language requires more than grammatical competence. Additional competence must be acquired in order for a person to be able to use the language for his human needs: "We may say that he attains a system of "pragmatic competence" interacting with his grammatical competence, characterized by the grammar. Thus we distinguish grammatical and pragmatic competence as two components of the attained cognitive state" (Ibid.: 3).

In spite of their close interaction, however, the two competences remain polarized, for the 'problem'/'mystery' dichotomy keeps them rigorously apart:[2]

On the one hand,

> Roughly, where we deal with cognitive structures, either in a mature state of knowledge and belief or in the initial state, we face problems, but not mysteries (Chomsky 1975: 138).

> There is nothing essentially mysterious about the concept of an abstract cognitive structure, created by an innate faculty of mind, represented in some still-unknown way in the brain, and entering into the system of capacities and dispositions to act and interpret (Ibid.: 23).

On the other,

> When we turn to such matters as causation of behavior, it seems to me that no progress has been made, that we are as much in the dark as to how to proceed as in the past, and that some fundamental insights are lacking (Chomsky 1975: 138).

> When we ask how humans make use of these cognitive structures, how and why they make choices and behave as they do, although there is much that we can say as human beings with intuition and insight, there is little, I believe, that we can say as scientists (ibid.).

It is now time to move ahead to the book that is the focus of this Commemorative Special Issue, *Language: The Cultural Tool*. Be it by coincidence, design, or luck, this new book of Daniel Everett appears precisely when *Pragmatics & Cognition* is about to commemorate its twentieth birthday. In some respects, it pursues to a certain extent the line of inquiry hinted at in his first *P&C* article, while in others — as every reader will certainly detect — it deviates radically from the major trend of his second *P&C* article, which was discussed above. Whatever *Language: The Cultural Tool* shares with the author's earlier work, however, it is a book that, after a process of maturation and intense debate, is already an interdisciplinary landmark in a research-cum-respect pattern of cultural, linguistic, cognitive, anthropological, social, and ethical forms of life. This courageous book challenges entrenched models of linguistic research and theory and stimulates debate about undisputed assumptions, methods, and data. *Language: The Cultural Tool* is thus an innovative and revealing book, which demonstrates the dynamic evolution of contemporary research in overcoming disciplinary barriers and deserves detailed consideration and discussion — which is what the contributors of this Commemorative Special Issue, including Daniel Everett himself, have voluntarily provided.

The concluding words of Chapter 10, "Language, Culture, and Thinking", can be read as an eventual response to Chomsky's and to Everett's own dichotomizations we discussed:

> We have seen how words, culture, and cognition can be related in different ways by the examination of kinship terms, color words, and math in Pirahã. There are many linguists, however, who will continue to believe that culture's effect on language is insignificant unless we can show it affecting the syntax — the core of a language's grammar — works. So that is where we are headed next (Everett 2012: 272).

Such a conclusion couldn't be more appropriate. It reminds the reader of the still widespread belief in the predominance of both language and its alleged core — syntax — over culture, cognition, other semiotic systems, semantics, and above all pragmatics. It calls attention to the next chapter, "You Drink. You Drive. You

Go to Jail. Cultural Effects on Grammar", which does exemplify how syntax can be shaped by culture and even dispensed vis-à-vis the relevant cultural background.

But we shouldn't forget that the Chomskyan paradigm of 'sentence fundamentalism' was not only accepted as a dogma of modern linguistics: there were also attempts to apply it to other areas of linguistic theory. One example that attracted much interest was the so-called "Text Grammar" movement, which sought to develop grammatical rules beyond the syntax-level, to texts or discourses of various sizes. Loyalty to the paradigm led some opponents of "Text Grammar" to reject the possibility of treating texts as if they were extended sentences, due to interpretive as well as syntactic difficulties this raised.[3]

Another example of the attraction of the (apparent) simplicity of syntactic structures and their respective rules can be observed in Searle's denial that conversation is a rule-governed kind of language use. His argument was that, whereas speech acts (which correspond to sentences) are governed by a set of precise conditions or rules, there is no equivalent to this kind of rules for conversation. Obviously, this argument is based on the consideration of syntactic rules as the paradigmatic model of linguistic rules for virtually any linguistic phenomena — a presumption that rules out, for instance, Grice's 'logic of conversation', among other proposals for a pragmatic theory (e.g., an account of Chomsky's 'pragmatic competence'). My reply to Searle consisted in pointing out that, as shown by Grice, there are rules or 'maxims' for pragmatic inferences, but they aren't of the syntactic-obligatory type, being rather defeasible rules, abductive or heuristic in nature.[4]

This Introduction has so far focused on details that do not highlight with the merited halo what are perhaps the most significant contributions of *Language: The Cultural Tool*, which can be even enhanced. Here is an example.

Daniel Everett had the kindness of using as the motto of Chapter 3, "Crossing the Communication Threshold", a sentence from my article "Language as a Cognitive Technology", namely "[I]t seems to me appropriate to view the … features of natural languages and their use as 'cognitive technologies'" (Dascal 2002:37). If readers of this quotation infer that I consider language not a *cultural* tool but a *cognitive* tool, or that I emphasize *tool* because my main concern is with the designed (or occasional) function(s) of language, it is of course my fault, though the article leaves room for further exploration. For instance, I distinguish three different ways in which features of natural languages can be useful for some purpose: as environment, as a resource, and as a tool. These three categories correspond to three forms of presence of natural languages in our lives, regardless of the specific purpose or function. They can therefore be applied to any purpose or end, not only to cognition, to culture, to communication, to transportation, etc. Furthermore, *tool* is only one of such categories, while all of them are worth using to capture and apply as much as possible features of language. Likewise, technology

is a term applicable to many tasks or functions. My interest is not to criticize, but rather to help the thrust of Everett's revolutionary innovation by increasing the scope of the 'services' language can provide to culture and life. If we manage to allow it to contribute beyond the limitations of a designed 'tool' (e.g., a formal language), and let culture enjoy language also as environment and resource; and if language crosses the thresholds of 'communication', of 'conflict', of 'singing', and so on, we are indeed expanding the benefits culture, cognition, and so on derive from language. That's all. But it is quite a lot, for culture itself develops thereby — a development that does not consist in combatting competitors waiting for their parcel of Language Services, but by establishing with them non-dichotomous cooperative links or relations, as stressed by Daniel; a development analogous to that of the mutual assistance the branches of pragmatics — sociopragmatics, psychopragmatics, and ontopragmatics — provide to each other.

By the way, a similar development of culture, now via the dialectic technology of Language Services, emerges from the multi-perspective feedback obtained from the articles about *Language: The Tool of Culture*. The future editions of the book and their future readers, Daniel, and *P&C* readers are sincerely grateful to the authors of these valuable culture-developing contributions. And, lest but not least, our Pirahã friends deserve unquantifiable gratitude for their marvelous cultural gentleness and cooperation, from which other cultures will continue to learn so much.

Notes

1. Cf.: "Use of language involves cognitive systems beyond grammatical and pragmatic competence" (Chomsky 1977: 3).

2. Dichotomies, against the current belief, are rarely logically compelling. Consequently, their polarization does not imply that there is no alternative besides opting for one or the other pole. Rather than semantically, therefore, many dichotomies can be treated pragmatically, for their use serves discursive as well argumentative purposes. In this respect, one can either dichotomize or de-dichotomize a polarity in accordance with one's ends. It seems to me that the fact that Chomsky's own illustrations of 'problems' and 'mysteries' are presented as flexible suggests that he is dichotomizing rather than employing a logical dichotomy. For an analysis of the use of dichotomies and their effects on different types of polemics, see Dascal 2008.

3. I confess that I was an active member of the opposition to the "Text Grammar" movement and defended the 'standard sentence' as a condition for syntactic acceptability. It is worth noticing that in so doing, we the opponents preserved of course Chomsky's as well as Everett's dichotomization strategy. Cf. Dascal and Margalit 1974.

4. For more details on this example, see Searle 1992a and 1992b; Dascal 1992; Amel 1994. It is worth noticing that the exclusion of rules other than the syntactic or the speech acts ones for accounting to other linguistic phenomena amounts to establishing a criterion for claiming

that these phenomena belong to the category of 'mysteries', rather than to that of 'problems', i.e., scientifically explainable.

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No need for instinct

Coordinated communication as an emergent self organized process

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Language serves many purposes in our individual lives and our varied interpersonal interactions. Daniel Everett's claim that language primarily emerges from an "interactional instinct" and not a classic "language instinct" gives proper weight to the importance of coordinated communication in meeting our adaptive needs. Yet the argument that language is a "cultural tool", motivated by an underlying "instinct", does not adequately explain the complex, yet complementary nature of both linguistic regularities and variations in everyday speech. Our alternative suggestion is that language use, and coordinated communication more generally, is an emergent product of human self-organization processes. Both broad regularities and specific variations in linguistic structure and behavior can be accounted for by self-organizational processes that operate without explicit internal rules, blueprints, or mental representations. A major implication of this view is that both linguistic patterns and behaviors, within and across speakers, emerge from the dynamical interactions of brain, body, and world, which gives rise to highly context-sensitive and varied linguistic performances.

Keywords: Cognitive science, coordinated communication, instincts, instinctual behavior, modularity self-organization

1. Introduction

Why do people speak in the various ways they do? Conducting research on people's linguistic abilities, and discerning the underlying causes for this facility, can be tricky business. Daniel Everett is offering a compelling example of this challenge in his book *Language: The Cultural Tool*, when he describes his efforts to test native Pirahã speakers' knowledge of grammar by asking informants to state whether a given string was "pretty" — i.e., the best available word to use given that these informants had no words for 'grammar' or 'grammatical' (pp. 93–94).

When one of his informants, a language teacher, replied that one utterance was indeed "pretty" Everett asked him to repeat the statement, to which the informant answered, "I can't". Puzzled by this response, Everett asked why he could not utter the statement, to which the informant replied, "Pirahã do not talk like that". When Everett then said, "But you said I could say that", the informant pointed out, "Yes, you can say anything you like. You are paying me"!

Everett's story illustrates one of the difficulties of doing empirical research on what people really know about language. The simple fact is that there is incredible variation among people (e.g., their personalities, cultural backgrounds, and expectations about the situation), languages, tasks (e.g., what people do, or are asked to do, with language), and methods for assessing language abilities that greatly complicate any effort to study linguistic structures and behaviors. Much of Everett's book describes some of the complexities associated with doing linguistic research and takes direct aim at the traditional belief that linguistic behavior exhibits strong regularities that are best described by "nativism", particularly in terms of Chomsky's concept of "universal grammar". Everett's most convincing arguments against this nativist view are (1) that languages are highly variable, such that a parameter-setting approach advocated by proponents of the nativist view, runs into the problem of having to posit a large or even unconstrained set of innate parameters, and (2) that recursion, long thought to be a defining quality of language, is better considered a property of human thought. He also points out that a biological nativist perspective is essentially tasked with identifying all of the social, cognitive and neural requirements for language, then tossing out all of the elements that implicate more general psychological and embodied mechanisms, and declaring the remainder to be the "innate language module".

We fundamentally agree with Everett's criticisms of the nativist position. Everett is also generally correct, in our view, when he claims that language is a cultural tool. However, Everett's embrace of an "interactional instinct" does not adequately explain the complex, yet complementary nature of linguistic regularities and variations. Part of the difficulty here is Everett's assumption that the "interactional instinct" is an internal property of individual speakers. For example, Everett frequently discusses the goals and intentions of individuals in a community as driving the cultural and linguistic changes, which suggests that individuals choose what to say and how to say it in order to adapt to their particular niche. Our alternative suggestion is that language use, and communicative behavior more generally, is an emergent product of human self-organization processes. This theoretical perspective claims that both broad regularities and specific variations in human behavior, like all natural systems, can be accounted for by self-organizational processes that operate without explicit internal rules, blueprints, or mental representations. A major implication of this view is that both linguistic patterns and

behaviors, within and across speakers, emerge from the dynamical interactions of brain, body, and world, which gives rise to highly context-sensitive, and varied linguistic performances.

2. Searching for regularities in linguistic behavior

The major task for advocates of a "language instinct" has been to discover strong regularities in linguistic structure and behavior and then posit specific mechanisms, including instincts, presumably responsible for these regularities. But how can we determine what regularities exist in language given the diversity of languages and linguistic behaviors? One way, of course, is to simply ask people to introspect about their linguistic knowledge and abilities. Everett's attempt to elicit grammatical knowledge from a Pirahã speaker, described above, is a good example of this research strategy. But many cognitive scientists will react to Everett's cautionary tale about interviewing a Pirahã speaker by placing blame on classic introspective linguistic methods. Asking questions of only a small number of informants is typically viewed as insufficient to gauge important statistical regularities in linguistic performance across members of a language community. One reason why experimental studies, employing large numbers of naïve participants, are popular in psychology and cognitive neuroscience is that they enable scientists to discern people's linguistic and cognitive abilities using "indirect methods". These methods require that people engage in different experimental tasks, under controlled conditions, where their performances on these tasks may be scrutinized for evidence of the main underlying psychological or linguistic causes for the differences in their behaviors between conditions or tasks.

But even controlled experimental studies investigating the behaviors of many participants can be problematic. For example, many years ago, one of us was studying American university students' understanding of common idiomatic phrases, such as 'John kicked the bucket', 'Sally spilled the beans about Beth's divorce', and 'Peter blew his stack when he got fired for no apparent reason'. Idiomatic phrases, like 'spill the beans', are interesting because they deviate from linguistic expressions that appear to have compositional meaning, such as 'John spilled the milk onto the floor'. One way to investigate if people understand the figurative meanings of idioms is simply to ask native, and sometimes non-native, speakers, to define or paraphrase the figurative meanings of different idiomatic phrases. People often, in this situation, simply state another idiom that has a similar figurative meaning (e.g., 'blow your stack' means "get pissed off"). As a result, psycholinguists employ different experimental methods to explore whether people have tacit knowledge of the conceptual and experiential reasons for why different idioms mean what they do.

As part of one research project, California university students were instructed to form mental images for various idioms and then answer detailed questions about their images (Gibbs and O'Brien 1990). The most interesting findings from this set of experiments was that people reported very regular imagery for different idioms referring to similar figurative ideas (e.g., "blow your stack", "flip your lid", "hit the ceiling"), and gave consistent responses when asked about the causation, intentionality, and manner of the events depicted in their imagery (e.g., people frequently reported that internal pressure was the cause of the stack blowing, the lid flipping, and the ceiling being hit). This regularity was not due to these phrases having similar figurative meanings (e.g., "to get very angry"). Instead, the regularity in people's mental images was due to different idioms being motivated by similar underlying conceptual metaphors, such as THE MIND IS A CONTAINER and ANGER IS HEATED FLUID IN A CONTAINER. Thus, people's images for phrases like 'flip your lid' suggested a container, about the size of a human head, being under great internal pressure, often because there was fluid in the container that was heated, leading to an explosion that was violent, yet unintentional, where each of these inferences arose from the mapping of HEATED FLUID IN A CONTAINER onto the idea of MIND.

It is a very big leap to draw conclusions about the inner workings of people's minds from an analysis of their mental images for metaphoric idioms. Still, many cognitive linguists, psychologists, and some philosophers have cited this work on idioms as providing scientific evidence for the psychological reality of conceptual metaphors, a conclusion that previously was grounded in linguistic analyses alone (cf. Gibbs 2008). This experimental research, therefore, appeared to uncover something that was maybe ubiquitous in the minds of American students, namely the existence of entrenched metaphoric concepts in the human conceptual system.

Yet a closer look at this psycholinguistic research on idioms reveals a far more complex story, one that harkens back to Everett's cautionary tale from his own work with the Pirahã. One small window into this complexity was given by a student who participated in one of the idiom studies cited above. After completing the set of mental imagery tasks, this student was being debriefed about the study, before being given "course credit" that was his "reward" for taking part in the experiment. Before the student left the laboratory, he said something along the following lines: "You know, this sounds like an interesting project, but you should realize that the phrases you gave me to imagine were really not anything I would ever use in my own speech". When asked to further explain this comment, he said, "Well, these are phrases I sort of know, or maybe know of some of these, but nobody speaks like this anymore, except maybe my grandmother, and I am not sure whether she would understand all of these phrases". When the student was then asked whether he still believed that the data he gave may be useful, he replied,

"Maybe. But I was here really to get the course credit, and the answers I gave in the experiment were mostly just me trying to give you what you may have wanted. I am not sure any of this has much to do with my ordinary speech. A lot depends on the questions you ask people".

There is a striking correspondence between this tale from the narrow confines of a California psycholinguistics laboratory and Everett's description of his native informants in the Amazonia. Both Everett and psycholinguists, like us, seek to explain the regularities in the behavior of not just the groups of people participating in our surveys or experiments, but to generalize the findings from these individuals to larger populations of people (e.g., all speakers of idiomatic American English or of Pirahã, and maybe even all speakers of language). We may sometimes acknowledge that people differ in their responses to questions about the grammaticality of word strings in Pirahã, or their mental images for clichéd metaphors. In fact, in the idiom studies described above, people varied considerably in the extent to which they gave consistent responses to questions about their mental images, and each individual participant did not give the same type of answers to questions about different experimental stimuli. Yet one can still maintain that the idiom studies capture something important about typical, or average, linguistic performance which, again, may be caused by some specific causal processes of mind or language.

The big question to consider is whether it is appropriate to equate average performance on a psycholinguistic task with some specific internal mental structure. Proponents of the "language instinct" in various ways argue that making this equation is not only reasonable, but necessary. Psychologists' embrace of the "average experimental effects = common inner mental structures" equation has led them to posit different cognitive and linguistic "instinct" or "modules" that presumably operate as the causal basis for linguistic structure and behaviors. For example, starting with Fodor's (1983) thesis of the "modularity of mind", psychologists have tried to link specific regularities in linguistic performance as evidence for a specific "language module" that is distinct from other modular systems such as perception and motor skills, and which are also independent of more central, and non-modular, cognitive processes. Under Fodor's view, a module is a perceptual input system that has a number of characteristics, including being informationally encapsulated, unconscious, fast acting, mandatory in its operation, providing shallow output, localized in specific brain areas, innate, domain-specific, and exhibiting both ontogenetic and pathological universals.

One major empirical implication of Fodor's view of linguistic modularity is its assumption that the dedicated language module automatically completes a purely linguistic analysis of any speech input before other cognitive and contextual information is brought in to infer context-sensitive meaning. Over the past 30 years,

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experimental psycholinguists and cognitive neuroscientists have considered the implications of this modularity thesis, particularly in regard to whether the language module is really informationally encapsulated and localized in specific brain sites. In each case, the claim is that the "language module" operates in an automatic, instinctual manner, as a reflex, and is impervious to the influence of, once more, higher-order cognitive operations, including people's experiential concepts and their understanding of context. But the results of many psycholinguistics studies, focusing on topics such as lexical ambiguity resolution, sentence parsing, and pragmatic language interpretation, indicate that linguistic processing does not operate in a serial manner in which linguistic analyses are performed before contextual information is accessed (Gibbs and Colston 2012; Gibbs and Van Orden 2010). Many studies within cognitive neuroscience demonstrate that damage to particular brain sites does not provide selective functional deficits, contrary to the predictions of classic modularity theory.

The difficulty in this debate over modularity centers on the question of "average" linguistic performance. It is surely possible to find some evidence that people, under some tasks and contexts, appear to initially ignore context when interpreting language. But the problem for the modularity view is that the variation in behavioral data, both within and across individuals, strongly indicates that people have much greatly flexibility in their online use and understanding of language than would ever be predicted by the strict, serial model favored by modularity theorists. Many psycholinguists now prefer interactive models, also known as constraint-satisfaction models, in which numerous sources of information, including contextual and real-world knowledge, simultaneously act to create immediate understandings of speech and writing (Katz 2005; McDonald and Seidenberg 2007). These interactive models are far better at explaining the wide variation seen in behavioral and neuroscientific studies of language use, because they more readily acknowledge how different sources of information, ranging from culture to embodied experience, probabilistically shape the immediate production and processing of speakers' communicative intentions.

Some of the empirical demonstrations against classic modularity have led to a dramatic reconceptualization known as "massive modularity theory". The massively modular mind comprises a large number of knowledge-and-action stories, designed in a piecemeal fashion (over evolutionary time) to serve specific, adaptively important ends. As the term "massive" implies, modules have been proposed specific to wide ranging domains, including face recognition, perception of emotion, friendship, rigid objects' mechanics, spatial orientation, tool-use, folk biology, semantic inference, specific food aversions, music, number and mathematical concepts, to name just a few. These modules are "functionally specified" to promote fast-acting instinctual behaviors that solve adaptive problems. Some

scholars argue that virtually every human concept constitutes its own distinct module (Sperber 2002), such that modularity is viewed as "a fundamental property of living things at every level of organization" (Barrett and Kurzban 2006: 628).

The empirical literature on massive modularity adopts the "average experimental effect = single module" fallacy. Thus, the average finding of some experimental study is simply attributed to a particular module of mind (i.e., an evolutionary given instinct) with little consideration of broader principles that may drive different experimental effects across many studies. This has led to a veritable explosion of different possible modules being proposed in the evolutionary psychology literature. At the same time, some theorists argue that many modules are embedded within one another.

Individual studies show the effects of different, though closely related, experimental manipulations in human performance and theorists then postulate that different, yet closely related modules must also exist. For example, one proposal suggests that language understanding is accomplished, partly, through the activation of several modules, most notably "default semantics", "relevance comprehension", and "theory of mind" modules, with each of these having close connections to the more central language module (Capone 2009).

But how do we define the very specific conditions that necessarily trigger the operation of one modular system, as opposed to another, to direct linguistic processing? We lack empirical justification to motivate modular distinctions, other than effect = module association. Drawing a causal link between average behavior and some psychological structure or mechanisms fails to explain the incredible variability in linguistic structure and behavior. As much as we desire to find broad scientific principles to describe and explain linguistic behavior experience, scholars should not fall prey to constructing theories based on "average" behaviors of the particular individuals participating in our studies and surveys. More specifically, the theoretical reflex to assume that some general regularity in behavior truly exists, and is specifically motivated by some particular instinct/module or system of instincts/modules ignores the variations seen in linguistic structure and behavior in many diverse human contexts.

Further, it is not clear that the definitional criteria of a module/instinct as a localized and encapsulated can be properly satisfied by *any* functional networks in the brain. The most common method of locating neurons active during a specific task (thus providing a candidate localization for the module responsible for the processing implicated in the task) rely on a subtractive method of comparing fMRI blood oxygen levels during the target task to activations in the same region of interest (ROI) during a "baseline" task. But as Sporns (2011: 156) notes, even at rest, the brain is spontaneously active, with neural firing exhibiting "complex dynamics and rich patterning of spontaneous network activity". This observation begs the

question of which "baseline" tasks are most relevant to determine a threshold of activation in a particular ROI.

Sporns (2011) also notes that the functional connectivity of human brains is substantially denser than the structural connectivity. Thus, the same purportedly-modular sets of neurons participate in multiple task-dependent functional networks, even across very similar tasks. For example, He et al. (2003) found that the functional connectivity between Broca's and Wernicke's areas varied based on whether participants were engaging in a non-linguistic tongue movement task, reading silently, or reading aloud. Further, this functional connectivity also varied based on whether the written stimuli were presented with an alphabetic pinyin script, or a logographic orthography. The organization of functional brain networks is not permanent, but rather emerges from the dynamics of structural brain networks under pressure to resolve particular tasks in specific cultural and physical environments. Changes in processing demands are associated with the dynamic reconfiguring of the brain's functional architecture (Sporns 2011: 182). The functionally-promiscuous and temporally-instable nature of patterns of neural activity under particular task demands cannot be resolved with the massive modularity hypothesis, which attempts to link specific processing functions with discrete, informationally-encapsulated, innate, species-universal, spatially-localized brain regions.

Given the task-dependent variability of results of behavioral and neural investigations of human cognition, people's linguistic behaviors may better be characterized as the in-the-moment outcomes of dynamical processes where all sources of constraint interact to give rise to an emergent product, one that is always specific to the particular person, language, task and social situation. The psycholinguistic and neuroscience literatures clearly demonstrate the importance of people, language, task, and measurement tool as critical variables in explaining people's use and interpretation of language. Our preferred alternative theory seeks to explain both the regularity and context-sensitive variation in human performance by acknowledging that adaptive behaviors are self-organizing and require no functionally pre-specified mental mechanism, such as modules or instincts. Dynamical theories of human cognition possess the flexibility to account for the complex interactions among these different variables.

3. An embodied view of instinctual behavior

We agree with Everett's claim that language is a cultural tool which emerges from interaction among people facing adaptive, communicative challenges in a changing, complex world. But the fact that people use language habitually, and as children

learn it so readily, does not imply that there is a single, specific mechanism, such as an interactional instinct, generating this behavior. Everett (pp. 183ff) acknowledges Lee et al.'s (2009) reintroduction of Aristotle's "social instinct" as providing the primary motivation for the phylogenetic roots of language. These authors define the interactional instinct as "an innate drive among human infants to interact with conspecific caregivers" (Lee et al. 2009, as cited in Everett, ibid.) Everett then describes several other examples of purported "instincts", for more specific human behaviors like language, religious faith, or moral behavior. His principal argument against these claimed instincts is that the behaviors which are labeled as "instinctive" show development over time, while a truly instinctual behavior should have "zero learning curve" (p. 184). According to Everett, the "interactional instinct" does not run afoul of the traditional view of a language instinct, because it "requires no learning" and is more like a "simple reflex" (p. 185). Furthermore, "the role of this instinct comes out of the need to communicate which forces all humans to learn a language, and to invest the effort into learning a language." (ibid.).

The difficulty with Everett's definition of the interactional instinct is that it is teleological — the interactional instinct is the tendency to interact with others, which is invoked to explain why we interact at an early age. This explanation seemingly contradicts Everett's own criteria given that people differ in their attainment of sociality. The mere positing of an "instinct" mechanism does not come close to explaining the diverse experimental results in psycholinguistics, including the temporal dynamics of how people come to closely coordinate in real life communicative interactions.

Our alternative suggestion is that so-called "instincts" of whatever sort, posited to explain regularities in behavior, are emergent properties of self-organization processes and not single, causal forces that shape specific behaviors. Many stable human actions may be viewed as "instinctual" without necessarily being driven by underlying genetic or evolutionarily-given instinctual mechanisms. This possibility has its roots in philosophy dating back to Heidegger's (1996) idea of "attunement" and more recently Merleau-Ponty's (1963) argument that instincts are embodied meanings, and not internal mechanisms, concerning "the primary sense of things mattering to the organism" (Keeping 2006: 183). When viewed in this way, an animal's actions or behavioral repertoires are not directed toward particular goals that are caused by underlying instinctual mental representations (Keeping 2006). Instead, adaptive behaviors are embodied meanings that emerge in specific contexts. This view of instincts as embodied meanings, by the way, suggests no opposition between culture and instincts, given that cultural artifacts and behaviors are realizations of context-sensitive embodied actions (Keeping 2006).

The behaviors scientists typically refer to as "instinctual" do not require single "instinct" mechanisms to bring about these behaviors. We now consider several

lines of empirical and computational evidence that is consistent with the idea that coordinated communication emerges as a whole body system without having to posit anything about underlying instinct.

4.   Coordinated communication as embodied action

Much debate in psycholinguistics has focused on which empirical task best reflects people's ordinary communicative abilities. But a different way of looking at this question suggests that no single task captures the complex underlying psychological reality when speakers and listeners talk with one another (Gibbs and Colston 2012; Gibbs and Van Orden 2010). Any instance of speech communication fundamentally constitutes a different task for the participants given their idiosyncratic histories, dispositions, and situation, resulting in a differently self-organized mind and body. For example, people's choices when speaking in conversation derive from perpetually iterated dynamical processes, whereby multiple simultaneous conflicting constraints self-organize people's in-the-moment potentials for utterances and meaningful experiences. The coordinations between speakers and listeners, ranging from postural and gestural coordination (Shockley, Richardson, and Dale 2009) to coordination of brain dynamics (Stephens, Silbert, and Hasson 2010), which are seen in speech communication are themselves aspects of an emergent coordinative structure encompassing the participants in the conversation in their entirety.

For example, psychologists and sociologists have long argued that speakers design each utterance so that their addressees can figure out what they mean by considering the utterance against their current common ground (Clark 1996). Some common ground information is cultural (i.e., information broadly shared by members of a community) and some information is personal (i.e., information uniquely shared by two or more speakers). Consider, for example, an experiment in which two persons talk to each other, but cannot see each other. Both sit before schematic drawings of cartoon figures, new to both parties. One conversant describes a specific figure from her set of figures, and the other identifies the correct picture from his set using the heard description alone (Clark 1996). Unsurprisingly, participants get better at this task over time. Speakers initially provide detailed descriptions of the figures to make initial identifications possible, but over time each pair of dialog partners eventually evolves a shared idiosyncratic lingo specific to the given task environment allowing them to pick out figures more quickly. Thus, on a first trial, one speaker referred to a figure by saying, "All right, the next one looks like a person who is ice skating, except that they're sticking two arms out in front". But on the sixth trial in this study, the same speaker simply said, "The ice skater".

Many psycholinguistic studies have demonstrated, similarly to this, how people build up different common ground information through conversational interaction that provides important constraints on speakers' pragmatic choices (Clark 1996; Gibbs 1999). One possibility is that this kind of coordinated behavior is guided by the interactional instinct which forces people to align their different mental representations, perhaps through priming of lexical, syntactic and semantic information (e.g., Pickering and Garrod 2004). Yet this explanation does not specify how different levels of linguistic and physiological activity become coordinated to produce coherent, meaningful behavior. How might lower-level aspects of speech rhythm and body sway, for instance, shape higher-level aspects of word choice, syntax, and conceptual intentions? How might higher-level aspects of social and cultural norms shape lower level properties on prosody and body posture? And how do the lower and higher levels constraints mutually affect each other as a global system both within individuals and across people in conversational interactions?

In our view, the bases of any speaker's utterance or the reasons for people's interpretations of language are contingencies, which are to an underappreciated extent the products of idiosyncrasy in history, disposition, and situation. Relevant historical constraints include anatomical and physiological constraints that allow us to move and speak in some ways and not others, the details of the speakers' previous history, including a speaker's native tongue and unique history of wellbeing and idiosyncratic relations with other living beings, as well as what has been said previously in some conversational exchange. These contingencies shape people's in-the-moment, context-sensitive linguistic and nonlinguistic actions through self-organization processes. Biological and physical scientists now recognize that nature is composed of many interacting subsystems that exhibit a strong tendency to self-assemble or self-organize, which are now studied within complexity science (Bak 1996; Kaufman 1993). Any system whose structure is not imposed from outside forces or from internal blueprints (e.g., internal mental representations) alone can be said to self-organize. Self-organizing systems are capable of creating new structures because their dynamics are dominated by these interactions instead of by the activity of isolated components. Emergent mechanisms are temporary, or "soft-assembled," because they do not endure as passively stored representations within the system's dynamics. Soft-assembly processes operate in highly context-sensitive ways within particular environmental niches to create the very specific physical patterns and behaviors within each system.

A self organizational approach to linguistic behavior maintains that different regularities and instabilities unfold over time according to particular types of dynamics (Gibbs 2006; MacWhinney 2005; Raczazek-Leonardi 2010). No single set of components, or individual instincts, dominate language use. But many forces

dynamically organize in-the-moment linguistic experience. Consider just a few of these forces:

> Evolutionary forces (bodily, cultural, cognitive, linguistic)
> Present cultural conditions
> Present social context
> Knowledge of language (lexical, grammatical, pragmatic)
> Present bodily states
> Present motivations and cognitions
> Immediate linguistic processing
> Neural processes

Each of these factors operate at different time-scales, with some crawling along at very slow speeds, such as evolutionary and historical forces, with others zipping along at very fast speeds, such as immediate linguistic processing and the firing of neurons in human brains. The various time scales are not independent, but are hierarchically organized, and nested within one another such that different forces affecting language experience are coupled in complex, nonlinear ways. For instance, some factors that shape spoken interaction involve slow moving processes such as cultural ideas about how to conceive of, and talk about, certain topics. But the slower moving cultural and historical processes are not independent of what occurs in faster acting immediate experiences, such as people's ongoing bodily actions, and even faster neural processes associated with language production and processing. Overall, characterizing the ways people use language in interpersonal communication must take into account all of these interacting forces, and not simply focus on only what are traditionally conceived as more immediate, or proximate factors (i.e., interactional instinct).

Previous cognitive science research on self-organization in language use has examined cognitive performance in laboratory environments, as a whole context-sensitive measurement system. These studies track temporal patterns that emerge across a participant's sequence of response times or judgments (e.g., quickly judging whether a letter string is a word or not), spanning all the trials of the experiment; Spivey 2007). Close examination of changes, trial by trial, across the repeated measurements, reveals characteristic dynamical signatures that actually gauge the coupling between a person, the language stimuli encountered, and the experimental task at hand. Self-organizational approaches see linguistic behavior as a whole system activity (i.e., interaction of brain, body, and world) giving rise to both stability and instability in a wide variety of laboratory and real-world behaviors.

Consider, for instance, context-sensitive self-organization in language use involving speech perception. If speech perception is a classic modular system, or an instinct, then it should be possible to specify input criteria for the atomic units

of speech perception in the transduced speech signals that become meaningful speech. But the salient units of speech perception may emerge from the specific contexts in which speech is used, and not as invariant input units to a fixed speech perception module. For example, one set of experiments had naïve participants enact scripted performances about producing speech or examining speech (Goldinger and Azuma 2003). When participants enacted speech as in a play, they produced speech that changed the relative salience of phonemes versus syllables, simply because the script dictated them to do so. But when participants had to act as scientists and analyze recordings of the previous speech as stimulus inputs, they confirmed that syllables were the primary unit of speech.

Speech perception appears to fluidly accommodate task demands even at the causal interface of speech input, which allows context to be constitutive of perceived speech. The critical ratios of constraints that define boundaries in speech perception "adjust flexibly with factors such as phonetic context, the acoustic information available, speaking rate, speaker, and linguistic experience" (Tuller 2005:355). These demonstrations of self-organized speech perception are not rarified experimental phenomena, but laboratory analogs of the flexible perception required to recognize the same word produced by males, females, speakers of different ages, and with different dialects and accents and by the same speaker in markedly different linguistic and intentional contexts (Gibbs and Van Orden 2010).

A self-organization perspective claims that interpersonal coordination, including that taking place in conversation, is all about emergent coordinative structure in which all aspects of dynamics of each body and brain may come together to more closely mimic the other, as the two systems come to change and behave as one (Fowler, Richardson, Marsh, and Shockley 2008). For example, one study looked closely at this, using two people sitting next to one another in rocking chairs. Intrinsic rocking frequencies of the chairs were manipulated by positioning weights at the base of the chairs (Richardson, Marsh, Isenhower, Goodman, and Schmidt 2007). Participants observed each other's chairs or looked away from one another. Most interestingly, when participants looked at each other, they soon settled into a dynamic of rock synchronously, even when the natural frequencies of their chairs differed. Thus, people unknowingly rocked against natural frequencies in order to reach synchrony, an example of temporal self-organization, producing an emergent temporal structure, instead of some internal executive representation. This kind of dynamic coordination stems from the biomechanical and physical constraints that naturally, and nonlinearly, couple together different limbs of the body, or of two bodies in close proximity. The stability in behavior that emerges cannot be reduced to properties of the components, which counts out internal motor programs as being solely responsible for coordinated behavior.

Instead, self-organization in behavior arises from the interplay of brain, body, and environment as a single "context-sensitive" system.

Bodily movement and linguistic action, at all levels, are dynamically coupled in coordinated communication. For example, one study examined people's postural sway in cooperative conversation (Shockley, Santana, and Fowler 2003). Pairs of people were asked to discuss the differences between two cartoon pictures. Some of the pairs completed this task while facing one another, with others standing back to back. Moreover, some of the participants directed their talk to their partners, while others spoke to a confederate (again either facing them or facing away from them). An analysis of the participants' postural sway revealed that there was greater synchrony when the two people were performing the main task with each other compared to when they spoke with another (i.e., the confederate) person. But participants' postural sway did not differ even when they were faced away from one another. This suggests that language alone serves as a physical coordination device.

Further studies investigated whether people's speaking patterns had a direct impact on their postural sway (Shockley et al. 2007). Two people stood facing one another and had to utter similar words, words that differed but were similar in their stress emphasis patterns (e.g., "ethnic" vs. "ancient"), or words that differed and also had different stress emphasis patterns (e.g., "ethnic" vs. "deserve"). Participants produced their words either simultaneously or in an alternating manner. Not surprisingly, people's postural sway was most similar when they uttered words with similar stress, suggesting that synchrony in posture was facilitated by similarity in speech patterns. However, a different experiment asked people to do the same task when they were not physically co-present. Thus, the two participants uttered the different words in the presence of a different (i.e., confederate) partner. An analysis of the postural sway for these virtual pair of participants did not show increased postural coordination when speaking words with similar stress emphasis. This finding demonstrates, then, that speaking similarly alone is not sufficient for postural coordination given that another person must be physically present for people to sway in a similar manner. Again, coordination in linguistic interaction is tightly linked to synchrony in bodily actions.

Similar to the postural sway findings, Loehr (2007) also documented bodily synchrony across interlocutors, including bodily movements like eye blinks, head movements, and foot taps, as well as beat, deictic, and representational manual gestures. Loehr found that, not only did a speaker's bodily movements coordinate in time with the prosody of their own speech and with the movements of other bodily effectors, but in fact the whole group interlocutors exhibited similar, phase-locked patterns of bodily movements. For example, not only was the prosodic pattern of stress generally constant from speaker to speaker across turns,

both the speaker and the listeners tended to perform beat-like bodily movements (beat gestures, but also head nods, foot taps, etc) in phase with the stress pattern in the current utterance's prosody. These bodily movements are generally not part of our conscious awareness, suggesting that this synchrony does not emerge from a deliberate negotiation by the interlocutors with respect to the rhythm of the conversation.

Studies on interpersonal interaction have been extended to show that people's eye gaze is another important source of coordinated communication. One task measured two people's eye-movements as they looked at and discussed six cartoon characters from a TV sitcom (e.g., "The Simpsons") (Richardson and Dale 2005). Analysis of participants' speech and eye-movements showed significant coordination. For example, when one speaker looked at a specific character, the listener was likely to look at the same character within a period of two seconds. This overlap corresponded to the time it typically takes to fixate on an object, name it, followed by a listener then fixating on the same object. More interestingly, when participants were later asked comprehension questions about what they saw during the experiment, they performed better in cases where their eye-movements were tightly coupled.

Coordination in people's eye-movements also depends on their common ground knowledge (Richardson, Dale, and Kirkham 2007). When two people were first given information about the history and content of a specific painting by Salvador Dali, they later exhibited significantly more eye-movement coordination than did people who saw and discussed the Dali painting without previous information. Thus, knowing something about a visual scene beforehand facilitates coordination of two people's looking behavior at that scene, showing how historical contingencies affect the dynamics of interpersonal interaction.

This body of research from experimental psychology provides strong evidence of behavioral cooperation in conversational interactions, including dynamic relationships between people's speech, their visual attention, and bodily control. The specifics of how these different behavioral feats are accomplished have been shown to follow specific dynamical laws that are the signatures of self-organizational processes in action. Thus, people's abilities to coordinate their interactions when communicating emerges from self-organizational processes operating along multiple, linked time-scales. These experimental findings cannot be easily described in terms of average behaviors being driven or caused by simple instinctual mechanisms or behaviors. Further, these findings of behavioral coordination mirror (at different time scales) experimental results from studies of alignment (e.g., Pickering and Garrod 2004) and lexical entrainment/grounding (e.g., Clark 1996), which suggest that similar processes of self-organization may be responsible for the whole suite of interactional coordination effects.

5. Computational models of self-organization

We have described many empirical findings which support a dynamical account of language in action at a synchronic time scale. But we have not yet offered an alternative to the interactional instinct as an explanation of language origins, language change, or language acquisition over phylogenetic, diachronic, and ontogenetic time scales. From a dynamical systems perspective, it is necessary to consider the phenomenon of language across these different scales. To address this issue, we now discuss some computational models in which self-organized language-like structures emerge in artificial agents. These models offer two key lessons: (1) that domain-specific, biologically-endowed mental mechanisms are not necessary to explain the appearance of language-like structure; and (2) that language emergence and language change are not driven by individuals' intentions, but rather by dynamical couplings of interactants with each other and with their ecological context.

One crucial question for investigating the evolutionary history of language is that of learnability: To what extent are linguistic structures, or mechanisms for learning linguistic structures, innately specified? Everett convincingly argues that linguistic structures are learnable, and he points to the interactional instinct as the general learning mechanism that sets language acquisition in motion. Recent work in developmental robotics suggests that the interactional instinct may itself be explained by an even more general process of active exploration. Oudeyer and Kaplan (2006) report on embodied artificial agents' development of affordance understanding through interactions with a real environment. In this study, robots were endowed with bodily effectors (including a mouth that could bite, a vocal tract that could articulate sounds, limbs that could move around), human-like perceptual abilities (e.g. visual feedback from cameras, proprioceptive feedback from effectors), and a very simple cognitive system. The cognitive system has two components, a mechanism that predicts subsequent sensory inputs, and a meta-predictive mechanism that estimates the accuracy of the predictive mechanism. These robots were then situated in an ecological context (including pre-programmed "adult" robots), and allowed to autonomously and spontaneously act. Importantly, the robots had no innate knowledge whatsoever.

These robots generally progress through several phases of action, first exhibiting generalized body babbling, then focused babbling with specific individual effectors (e.g., repeatedly bashing a limb, repeatedly producing similar vocalizations), then manipulation of specific effectors with respect to affordances in the environment (e.g., bashing a hanging object, and later vocalizing at an adult robot). The behavioral repertoire that these robots develop should not properly be considered language, as it lacks semantic content, but importantly, there does seem

to be an intent to interact with other robots, or at least a recognition that other robots afford interaction. The robots tend to vocalize towards adult conspecifics, rather than just off into space, without any proclivity to do so being expressly designed. Rather, the tendency to interact derives from a more general endowment that encourages exploration — intrinsic motivation. These robots exhibit intrinsic motivation in that they appear to maximize the utility of their predictions. They progressively engage in more difficult behaviors, which allow them to gain more information from comparing predicted and observed outcomes, while still avoiding behaviors so difficult that they are unable to make useful predictions about them. Reaching for and bashing at an object has a very limited set of possible outcomes when compared to vocalizing towards a conspecific, so it would require much more time spent exploring the social context for an intrinsically-motivated individual to learn about sociocultural affordances like language. The crucial observation here is that vocal interaction emerges from domain-general cognitive abilities and an ecological context that affords exploration of the body and the environment. The social instinct may thus be a facet of a deeper curiosity.

But while a general cognitive mechanism offers a promising explanation for humans' tendency to interact, can it explain how language came to be, or the synchronic and diachronic variation among languages? Artificial agent models have been successfully applied to both of these questions for many aspects of language. We will focus on two particular models, which offer evidence for self-organizational processes in the emergence of phonological inventories (De Boer 2001) and in historical changes to morphological inventories (Dale and Lupyan, 2012).

One simulation modeled the emergence of shared vowel inventories from iterated interactions among a population of simulated agents (de Boer 2001). This simulation revolves around an imitation game between agents in a virtual environment, in which individuals with similar perceptual and articulatory capacities take one of two roles. The first role, the "Initiator," randomly chooses a sound from its inventory and produces it (stochastically matching the idealized motor routine for that sound). The "Imitator" maps this percept onto its own articulatory inventory, and then (stochastically) produces the closest match back to the Initiator. The initiator then maps this imitation back onto its own inventory, and signals whether the imitation matched the original production. The only elements "innately" specified for the agents are the routine for this imitation game, a humanlike auditory perceptual ability, and a vocal tract modeled after human articulators. The agents do not know the mapping between articulation and acoustic percept, nor are there any innately specified phonological categories. These specifications are constant across the population, but the demographics of the populations are allowed to change, such that some agents randomly died with new agents having the same specifications being born.

De Boer iterated these simulations, and over time the individuals in the population developed shared mappings between articulatory and acoustic space. Comparing the inventories sampled from converged populations to those observed in extant human languages, de Boer (2001:104) notes striking similarities in terms of the distributions of number of vowels in each inventory, and the configuration of the vowels in articulatory space. For example, in the simulations inventories all have at least three cardinal vowels, and these vowels tend to emerge in orders paralleling typologies of human languages. For example, all 3-vowel systems comprised a high-front vowel (/i/), a high back vowel (/u/) and a low vowel (/a/), which is the only known 3-vowel configuration in human languages, and the most common six-vowel inventory comprised a high-front vowel, a high-back vowel, a mid-front vowel (/e/), a central vowel (/ə/), a mid-back vowel (/o/), and a low vowel (/a/), which is the also most common six-vowel system among human languages. Although there is no actual communication occurring here (the interactions follow an obligatory script, and the sounds carry no meaning), the important point is that language-like structure can emerge in a population without a preexisting language, and without an innate specification for such structure.

The previous computational models offer formal support for the role of self-organization in language emergence and acquisition. They show how human-like structures dynamically emerge when agents interact with their socio-ecological context. This emergence reduces the explanatory need for biologically-endowed mechanisms for language structure or language learning. We must also recognize that language, as a socially-constituted artifact for communication, changes along with the individuals who speak it.

These historical changes can also be accounted for within the self-organizational perspective. For example, Dale and Lupyan (2012) have used artificial agent models to examine the influence of the proportion of adult learners in the speaker population on changes in morphological inflection. This work, based on their earlier "linguistic niche hypothesis" (Lupyan and Dale 2010), examined how the structure of a language reflects the environment in which it is learned and used. Their initial study established a broad trend across more than 2000 world languages that demographic variables like population size and proportion of adult learners are related to the presence of various linguistic features of inflectional morphology, like tense (English past tense "-ed"), number (e.g., English plural "-s"), gender, case, and others. They found that, in general, the larger a language's population of speakers, and the higher proportion of adult learners, the less likely a language will use inflectional morphology to encode these features. Large language populations with many adult learners tend to use lexical items instead of inflection (e.g., English negation "not"). Thus, the structure of particular languages

reflects the ecological context in which those languages are used, echoing Everett's point that languages are cultural tools

The artificial agent model in Dale and Lupyan (2012) offers formal support for this proposed "linguistic niche hypothesis". In their simulation, they systemically varied the proportion of "infant" and "adult" learners in a population, and examined the influence of this demographic variable on the morphological inventory upon which the population converged. For this simulation, a morphological inventory was represented as a series of binary values, indicating whether or not a particular grammatical contrast (for example, aspect) was encoded with an affix. During the simulation, agents were randomly paired to interact. After each interaction, the agents modified their morphological inventories to make them more similar to their partner. Each "infant" agent chose at random one contrast that could be morphologically encoded, and if its partner had a richer morphological inventory, the "infant" agent began encoding that contrast morphologically. But if the infant agent's partner had a small morphological inventory, the infant agent no longer encoded that contrast morphologically. "Adult" learners followed the same general rules as the "infant" agents, but they were modified so that during randomly chosen interactions, they failed to produce morphological encoding for a randomly chosen contrast. Across many runs of the simulation, the populations with higher proportions of adult learners tended to have less morphological affixation. This result offers formal support for the linguistic niche hypothesis, underscoring the appropriateness of Everett's description of language as a cultural tool. But equally important, this model illustrates how the socio-cultural influences Everett describes can arise in a less mentalistic framework.

Computational models from a dynamical systems perspective demonstrate that self-organization processes may explain many language phenomena, without resorting to domain-specific biological endowments or individual intentions. These explanations are based on the accumulated interactions of individuals within a socio-ecological context, across evolutionary, historical, and developmental time scales. The language-like structures that emerge reflect those contexts. Thus, such models offer strong support for the dynamical systems perspective on communication as arising from self-organizational processes.

6. Conclusion

There is now a vast body of experimental and computational evidence from cognitive science that provides a motivated explanation for linguistic structure and behavior in terms of dynamical, self-organizational processes. A critical part of this theoretical perspective is its ability to account for context-specific language as part

of an overall adaptive system of coordinated actions as people interact in physical and cultural environments. Emergent coordinated communication is a whole body activity that cannot be reduced to separate components (i.e., modules or instincts) of mind. Understanding the exact ways that language serves as a cultural tool, as Everett rightly proposes, demands that we recognize the interplay between stability and variability in human language use, and that we do not blindly assume that language regularities are caused by simple underlying instincts.

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Like the breathability of air

Embodied embedded communication

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I present experimental and computational research, inspired by the perspective of Embodied Embedded Cognition, concerning various aspects of language as supporting Everett's interactionist view of language. Based on earlier and ongoing work, I briefly illustrate the contribution of the environment to the systematicity displayed in linguistic performance, the importance of joint attention for the development of a shared vocabulary, the role of (limited) traveling for language diversification, the function of perspective taking in social communication, and the bodily nature of understanding of meaning.

Keywords: dialect formation, embodied embedded cognition, embodied meaning, joint attention, language, recipient design, systematicity

1. Introduction

In Everett's wide-ranging and profoundly stimulating book, language is studied at the intersection of culture, cognition and communication, and some of its properties likened to "the breathability of air" (p. 151). In this paper I would like to present some of my work, inspired by the general perspective of Embodied Embedded Cognition (EEC), as being very much in line with, and in support of, Everett's interactionist approach, although the material presented will be of a computational and experimental kind, instead of anthropological. Hopefully, these different forms of studying language can strengthen the interactionist perspective.

I welcome Everett's emphasis on focusing at where 'everything comes together'. To my mind, in the past cognitive science has suffered from allowing its reliance on the standard, and itself methodologically sound explanatory strategy of 'divide and conquer' to color its portrayals of the phenomena in question. Obviously, to study a phenomenon experimentally, as in cognitive neuroscience and psychology, one has to take the phenomenon out of its natural context and investigate it under ideally completely controlled conditions, probing it in various ways while

observing its behavior as closely as possible.[1] This is sound scientific practice and the diminished ecological validity of the observations is often a reasonable price to pay for the detailed and replicable observations obtained. But a sound scientific practice has unfortunate consequences, if one starts forgetting that the phenomenon thus studied is not 'the real thing', but an artificial construction. One can learn a lot about humans by systematically manipulating and observing their shadows, but such an approach will never do justice to the full richness of human behavior and cognition. In my view, this is one of the most general and important messages of the Embodied Embedded Cognition approach (see, e.g., Thelen and Smith 1994; Clark 1997; Haselager et al. 2008; van Dijk et al. 2008) that gained momentum in the 90's. The title of Hutchinson's (1995) classic, *Cognition in the Wild*, best captures this idea.

2.   Systematicity and the breathability of air

A case in point, and of great influence regarding the study of language, is Chomsky's competence — performance distinction. As is well known, Chomsky (1965:4) proposed to make "a fundamental distinction between the competence (the speaker-hearer's knowledge of his language) and performance (the actual use of language in concrete situations)". The competence outlines a space of possible behaviors of which the actual behavior of an agent provides only a sample (Fodor 1968:130–131; Haselager 1997:107–109). Thus, competence becomes an idealized version of the phenomenon in question, "unadulterated" by the influence of extraneous factors (Katz 1974:232). As in the case of experimentation, specifying an idealized version of a phenomenon can be good scientific practice, as long as one does not get blinded by its unadulterated beauty. As Simon (1981:23) noted: "There is a continuing danger that focus upon an ideal competence that resides in some kind of Platonic heaven (or a Cartesian one) will impose normative constraints on the study of actual language behavior".

The debate about productivity and systematicity provides an interesting illustration of how an idealized competence may bias the perception of what needs to be explained, as well as how. One may attempt to explain the productivity and systematicity displayed by linguistic behavior (and thought) by postulating a representational system with constituent structure and compositional syntax and semantics (Fodor and Pylyshyn 1988; Fodor and McLaughlin 1990), such as Fodor's (1975) language of thought. But, as Everett notes (p. 94), at least one of the questions to ask is "Is there a simpler explanation?" This has led to many attempts to model the systematicity of language on the basis of a representational system without constituent structure (e.g., distributed representations utilized by

connectionist networks; van Gelder 1990), that have, however, encountered significant difficulties (e.g., Hadley 1994; Haselager and van Rappard 1998). One recent attempt, that I think is congenial to Everett's perspective (e.g., pp. 131–135, 138–139), is to see systematicity of language as capitalizing on the structure inherent in an organism's environment rather than its internal representational system (Frank, van Rooij, and Haselager 2009).

This is not the place for going into the details of the computational model presented in that paper, so I here merely wish to point out its underlying idea. Systematicity as displayed in the *actual practice* of human cognition and language may depend on only 'weak' representational resources *combined* with a largely systematic world under an appropriately wide variety of circumstances (Frank et al. 2009: 374). Likewise, the displayed systematicity of the connectionist model developed by Frank results from the interaction between the architecture and its environment. That is, the representational system need not have full-blown constituent structure, nor does the solution need to lie in strong architectural constraints and quite specific training or learning processes, typical for many early connectionist modeling attempts. From this perspective, a representational system capable of *reflecting* the systematicity in the environment might suffice for displaying a psychologically plausible degree of systematicity. "Under this view, similarities among languages, such as they are, would be like resemblances between bows and arrows across the world", Everett says (p. 87). Likewise, we argue that it is the organism's embeddedness in the world, rather than its internal structure, from which the systematicity of its linguistic behavior derives. I think that Everett's remark (p. 151), made in relation to the learnability of language, about the breathability of air not necessarily being a feature of air but a feature of life as it evolved on this planet, also nicely captures the intent of our work. Systematicity need not be a feature of a representational system (e.g., in virtue of it having a constituent structure), but a feature of the world in which cognitive systems evolved.

3.   Social interaction and sharing a vocabulary

Similarly, an organism's embeddedness, specifically related to the interaction with other agents, might help to explain the development of shared vocabularies. In Kwisthout et al. (2008) we took up Tomasello's (1999) suggestion that the ability to engage in various forms of joint attention provides an essential mechanism for, and may have co-evolved with, cultural learning and language. In checking attention, participants (e.g., a child and parent) consider what the other is focusing his/her attention on (establishing what the other is looking at, for instance). In following attention, one is being led by the other to shift one's attention to focus on a new

object or event. In directing, one does the opposite, namely actively guiding another's attention to a specific object or event. A computational approach to the study of how agents can develop a common lexicon (shared word-meaning mappings), is to have artificial agents (robots, software agents) play language games, i.e., agents exchanging utterances that stand for features (e.g., shape, color) of objects present in a shared environment (Steels 2001; Steels and Kaplan 2002). The task for these agents is to end up with a common lexicon, a shared set of symbols, so they have to solve the *social symbol grounding problem* (Cangelosi 2006; Vogt and Divina 2007). Everett provides many wonderful illustrations of this problem when describing his attempts to communicate with the Pirahã and other native tribes.

The three forms of joint attention allow individuals to reduce the number of possible meanings when learning a new word. As is well known, Quine (1960) pointed out that each unfamiliar word that we learn could in principle mean an infinite number of things (*gavagai* might mean 'rabbit', 'undetached rabbit parts', 'running furry animal', 'dinner', 'it will rain', and so on). So, in order to learn the meaning of a word in actual practice, one must be able to substantially reduce the number of possible hypotheses. Our focus was on the role of the three joint attention skills for reducing the uncertainty with which the meanings of words can be inferred, by examining their effects on achieved accuracy and on how much they speed up the language game.

Checking attention turned out to be most effective. In our computer simulations we found that in the absence of the capacity to check attention, communicative accuracy simply failed to reach 100%, even if the two other attention skills were present. However, when the capacity for checking attention was present, all simulations converged to 100% communicative accuracy. Still, following and directing attention played a major role in increasing the speed with which a 100% communicative accuracy was achieved, going from around 66,000 language games for checking attention alone to about 18,000 games for directing attention and 2500 games for following attention. Our results (only briefly and incompletely summarized here) indicate that the ability to *check* attention is more crucial than the ability to *follow* attention, which in turn appeared to be more crucial than the ability to *direct* attention. The large differences in the effectivity of these skills for the language game may further be used to suggest hypotheses about their evolutionary ordering.

Assuming that the most effective mechanism evolved first, if our simulations are correct in indicating that checking attention is of primary importance for creating a common lexicon during language games, then it would have been beneficial for early hominids to have these capabilities before more advanced language usage could emerge. So checking attention may have evolved first, following attention second, and directing attention last in human evolution. Of course such general

hypotheses would need more investigation, but they might serve as a rough indication of how computational models can lead to evolutionary hypotheses that are in line with Everett's general interactive perspective on communication. Moreover, including the child's growing participation in more complex social interactions in the dynamics of the computational model provides a concrete illustration of the embeddedness of cognition.

4.   Technologies don't remain the same: Vowel change

Everett reminds us that language is a cognitive technology (p. 46; cf. Dascal 2002), and that as a tool it is both shaping us and being shaped by us (p. 218). Language changes all the time, and, as far as we can establish, has become more varied and diverse over time. "Why did English stop being German?" Everett asks (p. 325), and answers: "For the exact reasons that you talk more like the friends you grew up with than your own parents; for the reason that you talk more like members of your economic class than another. The basic rule of language change, again, is 'You talk like who you talk with'". As another example of how computational modeling might assist in our understanding of language, and of how much it is connected with the embodied embeddedness of our cognition, I would like to briefly introduce a recent computational model of vowel change as developed by du Pau et al. (in prep.).

Du Pau created a language simulation tool (entitled DEViL for 'Dialect Emergence Virtual Lab') that was used to implement a computational model of vowel change, based on a theoretical framework consisting of three hypotheses coming from Croft's (2000) general theory of language change. The three hypotheses concerning language change are: (1) the *formation hypothesis*, according to which reinforcement of communicative success and punishment of disuse drive the formation of a shared lexicon; (2) the *change hypothesis*, stating that alignment to variation in perceived signals (vowels in our model) drives linguistic change; and (3) the *divergence hypothesis*, claiming that social fragmentation leads to linguistic divergence. In our model, it's where the agents are, and who they interact with, that drives language change; so, during runs of the computational model, agents could do little else but walk and talk, moving to (or staying in) specific regions and verbally interacting with the 'locals'. The communicative behaviour of the agents could be influenced in the model through various factors such as entrenchment of signals (due to reinforcement after communicative success), alignment (the adjustment of an individual's pronunciation to their partner's pronunciation), memory decay (the forgetfulness of an agent concerning the meaning or pronunciation of a signal), and noise.

The verbal interactions consisted in the basic language game (Steels 2001) described in the previous section. In one of the simulations we ran, we tested the hypothesis that geographical constraints are a potential cause of vowel divergence. The distance between languages depends on the interactions between members of the groups that speak them. We simulated agents of two neighbouring speech communities that could change their vowel systems simultaneously for 250,000 time steps, while we manipulated the amount of travelling to and fro. We found that physically separated communities that start with the same language gradually develop their own language systems when simulated geographical or other constraints reduce travelling between them. The speed and degree of divergence depended roughly proportionally on the severity of the constraints imposed on the travel opportunities. Under conditions with a lot of travelling, the languages were found to remain shared. Little travelling led to dialects and eventually non mutually comprehensible languages.

5.   Social communication and recipient design

How do humans tune their communicative behaviors to different types of agents, e.g., people that differ (to widely varying extents) in their language and culture? In a recent paper van Kesteren et al. (submitted) analyze recipient design in human communication. In Amsterdam, where I live, I get asked the way to the Rijksmuseum, van Gogh museum, red light district, concert hall, or just a particular street repeatedly, sometimes by locals, more often by Dutch people from out of town, and very often by people from other countries. Obviously in such cases, we share a lot of common ground, probably much more than found during Everett's fascinating encounters (even if my questioners are from very different countries and cultures, we share at least some knowledge about what a city, a museum, a bus or a taxi are). Still, I can notice myself addressing all these different (types of) questioners differently, e.g., by speaking more slowly and/or loudly, making clearer gestures, etc., often adapting my behavior in response to their facial and bodily expressions of understanding or bafflement. The tuning of my communicative signals to the person in front of me is known as recipient design (Sacks, Scheglof, and Jefferson 1974), discussed by Hockett (1960) under the label of 'feedback'. Cases can be even more complex when I also (have to) take into account the group of overhearers of which the addressee may be part, monitoring their responses in addition to the one who asked me the question, and adapting my communicative actions to them simultaneously (audience design). How do I (and everyone else) do it?

One way to understand my behavior is that I am forming (subconsciously or consciously) hypotheses about what the questioner might want, believe and know,

and use this type of perspective taking to improve my communicative actions (Clark and Carlson 1982; Grice 1974, 1989; Levelt 1989). However, one might suggest that involving all this cognitive machinery of hypotheses generation, testing, and updating is computationally very expensive, especially considering the generally easy and rapid communicative performance displayed in everyday circumstances. An alternative, recently attracting adherents, would be that recipient design is done through applying simple heuristics or rules-of-thumb triggered by the presence or absence of certain cues (Galati and Brennan 2010; Epley et al. 2004; Shintel and Keysar2009). One may think of the example of a dog increasing his pawing when he is being ignored in his entreaties for a treat as a case of the dog following the inferentially relatively simple strategy of recipient design based on heuristics (e.g., 'when insufficient response, repeat more often').

Van Kesteren et al. (submitted) studied human communication and the two competing suggestions as to how people engage in recipient design, through the use of the so-called 'Tacit Communication Game' (TCG: De Ruiter et al. 2010; De Ruiter et al. 2007; Noordzij et al. 2009). TCG is a communicative task where two players, a sender and a receiver, play a game on a 3 x 3 grid board. The sender knows both her own goal state and that of the receiver, and has to communicate, through game movements only (no verbal or gestural communication is possible during the experiments), the goal state to the receiver, and reach her own goal state as well. Basically, the sender communicates about the game, using game movements only, while playing the game at the same time. This setup allows to study different communication strategies, and their adaptation, in an abstract form. Based on experiments and formal analysis (that, again, do not allow rapid recounting here) we suggest that, even under such relatively impoverished experimental conditions, communication is better explained by the suggestion that the sender engages in a form of perspective taking. Errors on the part of the receiver help to clarify how the receiver is misinterpreting the speakers' communicative intentions, helping the speaker to hypothesize about the 'why' of misinterpretation, which can then form the basis for adjustment of the new communicative signals. In communication, one is really trying to understand the other, in order to be better understood oneself. Even during such brief meetings as with a tourist asking the way in Amsterdam, culture, cognition, and communication come together.

6.   Telling stories: Embodied meaning

Finally, I'd like to focus on an example of experimental work in relation to meaning. Meaning is obviously crucial to Everett's studies. He rightly points out (p. 47) that questions about communication are preceded by the question about *why*

people want to communicate. This question leads him to the investigation of culture and how communication made it possible to share values (p. 48–49). This seems entirely sensible to me. However, although Everett discusses the importance of the body for culture very often, and especially how culture influences or shapes the body (e.g., p. 28) he does not refer to work in cognitive neuroscience that shows how the body, i.e.,our sensorimotor capacities, is crucial to our understanding the meaning of language. Given his perspective, this is understandable, and I hope here to provide some information, based on a paper by Kerkhofs and Haselager (2006), regarding an experiment of Glenberg and Kaschak (2003), that may complement Everett's interactive position.

The issue is how representations acquire meaning for the system that has them. Harnad (1990: 335) formulated the basic question as follows: "How can the semantic interpretation of a formal symbol system be made intrinsic to the system, rather than just parasitic on the meanings in our heads? (…) The problem is analogous to trying to learn Chinese from a Chinese/Chinese dictionary alone". The study of the *embodiment* of cognition can be helpful here. The body is more than a mere transducer of information between the organism and the environment: It actively shapes the form cognitive tasks can take and also presents possibilities for solving them (Clark 1997; Chiel and Beer 1997; Lakoff and Johnson 1999; Haselager 2004). Therefore one might expect to find traces of an organism's sensorimotor interactions with its environment in the way organisms understand and respond to meaning. The basic idea is that meaning depends on an individual's history of bodily interactions with the world. People recreate or simulate those experiences in response to linguistic input, and use them to produce meaningful behavioral (including but not limited to) linguistic output. From this perspective, perceptual and motor processes are not peripheral to but form the *core* of mental content.

In Glenberg and Kaschak's (2003) experiment participants had to decide whether certain sentences were sensible (e.g., Andy handed you the pizza) or nonsense (e.g., Leonard drank the sun). Three kinds of transfer sentence were used: Imperatives like (1), concrete transfer sentences like (2), and abstract transfer sentences like (3). Note that all sentences have a form where the transfer or movement is towards the "you" person, and an 'opposite' form where the transfer or movement is away from the "you" person.

(1)   Open the drawer / Close the drawer

(2)   You handed Andy the pizza / Andy handed you the pizza

(3)   You told Jim the story / Jim told you the story

The participants had to make their yes/no judgments with a button-box with three vertically aligned buttons. After pressing the middle button the sentence appeared

1st proofs

on the screen. The position of the "yes" button was either above or below the middle button. This means that the motor response that the participant has to make is either in line or in conflict with the direction of the motion that is described in the test sentences. Glenberg and Kaschak found that participants are faster to accept a sentence (to respond "yes") when the motor response and the described action *matched*. Importantly, this also applied to the more metaphorical direction involved in telling someone a story versus being told a story by someone. An embodied sense of direction exists even in understanding meaning in that case.

7.   Conclusion

There is much more in this rich and readable book that invites links to ongoing research in experimental psychology, cognitive neuroscience, and artificial intelligence. Traditionally these sciences may have taken a more single-minded, less inclusive, less interactive perspective on language, cognition, and culture. Over the last few decades however, there is a clear tendency to turn back to 'cognition in the wild' and, though still using experimental and computational methods, this tendency at least prevents the methodology from obscuring the phenomena to be studied. A recent plea for a more inclusive approach in cognitive neuroscience is presented by Hasson et al. (2012: 114): "Cognition materializes in an interpersonal space (…) Despite the central role of other individuals in shaping one's mind, most cognitive studies focus on processes that occur within a single individual. We call for a shift from a single-brain to a multi-brain frame of reference". While I applaud the move to a multi-brain frame of reference, I think Everett is doing us a great service in clearly demonstrating that this in itself is not enough. Language "is the cognitive fire of human life" says Everett (p. 327), and to understand the cognitive wildfires of natural organisms, we need to look at much more than just brains, even taken together. The fuel of cognitive fire consists of embodied practices, embedded ways of living and speaking that mesh imperceptibly with one another, then diverge or even split at times, coming back together at other moments. Everett's book shows us how warming it can be to consider this fire in its natural splendor.

Note

1. At this point the reader might bear a reminder about my defense above of taking a phenomenon out of its natural context in order to investigate it under completely controlled conditions. There is nothing wrong with this, but it would be a serious error to assume that this way one could capture and understand all aspects of 'communication in the wild', because in reality one would be changing the topic of investigation rather substantially.

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Linguistic fire and human cognitive powers

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To view language as a cultural tool challenges much of what claims to be linguistic science while opening up a new people-centred linguistics. On this view, how we speak, think and act depends on, not just brains (or minds), but also cultural traditions. Yet, Everett is conservative: like others trained in distributional analysis, he reifies 'words'. Though rejecting inner languages and grammatical universals, he ascribes mental reality to a *lexicon*. Reliant as he is on transcriptions, he takes the cognitivist view that brains represent word-forms. By contrast, in radical cognitive theory, bodily dynamics themselves act as cues to meaning. Linguistic exostructures resemble tools that constrain how people concert acting-perceiving bodies. The result is unending renewal of verbal structures: like artefacts and institutions, they function to sustain a species-specific cultural ecology. As Ross (2007) argues, ecological extensions make human cognition hypersocial. When we link verbal patterns with lived experience, we communicate and cognise by fitting action/perception to cultural practices that anchor human meaning making.

Keywords: Biosemiotics, cognitive linguistics, cognitive niche, distributed cognition, distributed language, linguistic relativism, mental lexicon, Pirahã, skilled linguistic action

> What the hammer? what the chain?
> In what furnace was thy brain?
> William Blake — The Tyger (1956)[1]

1. Following descriptive linguistics

Linguists generally choose to focus their attention on matters such as identifying verbal patterns, how their parts are arranged in sentences, how constructions differ between languages, and what can be learned from studying linguistic change. For those who follow Saussure (1916) in treating phenomenological experience of words as central to language, linguistic 'objects' must be either synchronic or diachronic. In synchronic tradition, Daniel Everett has a well-established reputation

for describing verbal structure in little known languages. In so doing, moreover, he emphasises how people draw on culture to make meaning. Yet, given the politics and sociology of science,[2] his work is better known for findings about recursion in an Amazonian tongue. In his new book, striving to get beyond phenomenology, he raises important questions about the interdependencies between cultural traditions and communities of people.

For many it is surprising that speakers of Pirahã lack colour words, number terms, and grammatical markers of recursion. *Language: The Cultural Tool* (LCT) aims to explain the findings. As the title suggests, language is viewed as a tool that influences, but does not determine, a population's thinking. Speakers of Pirahã see and sort colours without using colour terms and, in story telling, embed thoughts within each other. They also live without counting or math. The observations show, Everett suggests, that cultural cohesion is sustained by word-forms that shape thought and communication. So, "like culture, language is an abstraction" (47). In shaking off appeal to form-based automata, linguistics is taking on new life. On a distributed view of language, utterance-types and parts are "second-order constructs".[3] Unlike physical events or first-order activity, the verbal patterns constrain what we say, what we are told and, generally, how we behave (see Love 2004; Thibault 2011). Tracks laid down by a language stretch individual experience: new territory opens up as we grasp a group's or people's ways of speaking. Like a literary tradition, cultural exostructures extend human intelligence. Without encountering a Tyger, William Blake could picture their fearful symmetry. As a cultural tool, language is "cognitive fire that illuminates the lonely space between us" (3). For Everett it can be traced to an *interaction instinct* (cf. Lee et al. 2009) or, further back, to Aristotle's view that affiliation prompts people, or social beings, to speak like those around them.

LCT also bangs another nail into the coffin of those who use physical symbol systems to model human language and cognition. As a cultural tool, language can be explained by neither genes, nor brains, nor mental grammars. Like literature or internet use, its functions can enrich intelligence; like the gift of Prometheus, it has allowed humans to spread across the face of the earth. While challenging Everett's psychology, I suggest that recognition of cultural exostructure is a major step towards a new people-centred linguistics.

2.   Organism-centred views

Chomsky (1965) aspired to give descriptive linguistics an explanatory role. Linguistics was not only to describe a system (or grammar) that allows people to generate infinite sets of sentences but it was to explain how children develop the

capacity. This allowed Chomsky to posit that rules enable mind/brains to manipulate formal structures: They are, or resemble, physical symbol systems. Though still influential (especially in America), given the social aspect of language, the majority of cognitive scientists reject such "radical fundamentalist views" (Dale 2012: PAGE). In nearly 50 years since the publication of *Aspects*, cognition is increasingly seen as inseparable from situated, embodied activity.[4] If brains manipulate symbols — a view still held by many — the capacity must be based in action (see De Vega et al. 2008; Belpaeme et al. 2009). In all species, cognition enables viable means of engaging with a changing environment. Reliance on folk categories from psychology and linguistics (e.g., perception, grammar, attention) led cognitive science to premature theorization of disembodied mental faculties.

In rejecting Skinner's (1957) view that cognitive powers could all be explained by how operant conditioning shapes learned responses, psychology focused on how, during various tasks, people might use systems that relied on input/output processes. The mind was compared to a mechanism that generates, for example, problem solving, chess moves, and visual representation. In Chomsky's vision, a mind/brain can even specify and process sentences. The organism-centred focus of behaviourism was replaced by models that put cognition 'between' action and perception. Turning from learning, the focus fell on hypothetical heuristics and competences. Until the early 1990s, cognitive function was identified with formalisations of inner process; however, the field was transformed by robotics and the new brain sciences (Anderson 2003). Study of embodied agents that encounter situations challenges any *epistemic conception of mind* (Cowley and Spurrett 2004). First, it leads to scepticism that the brain (or mind/brain) is the central locus of human cognition. Second, it undermines the view that neural networks and/or symbol systems have a special *cognitive* capacity. Third, human communication comes to be contrasted with transmission of probabilistic or Shannon information.[5]

Just as with perception, attention or working memory, behaviourists and cognitivists assume that a single organism controls language. Instead of defending the view, it is simply assumed that individuals rely on linguistic knowledge (i.e., of syntax, phonology, morphology, semantics). Then, the picture is applied to teaching, sociolinguistic variation, and cognitive issues. Psycholinguists re-examine old chestnuts — do learning and/or acquisition give us *knowledge* of language and how can a (hypothetical) language faculty evolve? Such questions echo the psychologism of philosophers like Locke, Descartes, Kant, and Hume. In linguistic versions of internalism, Saussure made *parole* into the product of a single mind. Since utterance-acts can be analysed in terms of unobservable language-systems (*langues*), many came to assume that brains represent verbal patterns.[6] On this view linguists have an alternative to the hocus pocus view (Householder 1952)

that language-systems are descriptive products. However, by focusing on forms and texts, analytically-based models leave aside how people manage sense-making. Mentalism thus survived the 20th century by relying on the fact that, in colloquial settings, silent thinking is described as inner. Failure to recognise this as metaphorical, as Ryle pointed out, leads many to invoke a "corporeal studio" (1949: 36). Rather than examine the complexity of skills, habits and dispositions, brains or minds are taken to represent linguistic knowledge.

When executive control is ascribed to how brains manage a language faculty, "language use" becomes the output of networks or representational systems. Rather than ask how language is integrated with action, analysis of linguistic form is used to hypothesise functional systems. Linguists highlight utterances, sentences and their parts (e.g., words) while overlooking people, nonverbal activity and what remains unsaid. In psycholinguistics, brains are said to identify and process formal patterns — they have (mysterious) capacity to perceive/produce phonological units. Even functional linguists treat language as text-like because is "can be separated from its material expression" (Thibault 2011: 2). By assuming that this can be achieved,[7] language is said to depend on knowledge ("semantics") that is disconnected from human activity. It is assumed that content, stored in brains, issues forth (and is re-identified) through acts of utterance. To explain language use, theorists posit capacities that, for example, calculate relevance, use common ground to identify intentions or, perhaps, rely on aligning representations.[8] Putative processes map semantic or neural representations onto forms and, indeed, control how people think. Human language is seen as a prototype of computational machine code.

Language is often taken to be communicated like (or as) probabilistic information.[9] So, while one person encodes and transmits, the results are decoded by another. Like cables (or wireless signals), utterances allegedly contain content. Conveniently, it is forgotten that, if we — or brains — create/construe Shannnon information, meaning is statistical. Like computers, human-systems depend on probabilistic types that covary with patterns common to vocal and graphic forms. Each person allegedly stores the language-types that make speaking/hearing and writing/reading into reversible operations. On this engineering based view, Shannon information is sufficient for communication. Yet, as visible in plants, animals or human bodies, the symmetries of engineering lack any natural counterpart. True symmetry of form or function is artificial; when living beings link sameness with difference, Bateson suggests, mind and nature are unified by the resulting pattern (1979: 16). Even if mind/brains ran language-systems, this would fail to clarify how people make interpretations, construe circumstances, or become differentiated as individual selves. Living systems use the modulations of experience. To be true to life, therefore, linguistic forms cannot be separated from

bodily dynamics. Before turning to how cultural tools serve embodied and situated activity, I show that LCT is flawed by its residual cognitivism.

3. Internalist thinking

To maintain cultural cohesion language depends on living beings. How do verbal patterns constrain thinking? Although viewing language as a cultural tool, Everett is conservative. Allowing cerebral and cognitive "platforms" to be ultimately the same, they are separate from "the physical" (158). Language is disembodied and, yet, both a cultural resource and a mental tool. Learning bridges the gap: Everett tells us, "there *is* an organ for this. It is called the brain" (96). The brain "recognizes words" (59) because, using Shannon information, an acoustic signal can "carry a meaning that is capable of interpretation" (ibid.). The brain identifies a probabilistic pattern. Even using *think* draws on how the verb is programmed in English. While echoing Rorty on programs (63), Everett allows meaning to drive most of grammar (96). This fits neither information processing nor his evident sympathy for the view that constructing a language uses intention recognition/reading (Tomasello 2003). While challenging generative linguistics and universal grammar, brains house a mental lexicon that depends on self-programming.

> [W]ords are the starting point…because everything else in grammar is built on words. Words are kept inside our mental dictionary, somewhere inside our long-term memory, organized in various ways, such as by their meanings, their frequencies, and by the order in which we encountered them (141–142).

In activating long-term memory, "elecrochemical energy" (59) prompts a brain to unearth lexical items. This might be a harmless case of the mereological fallacy, were it not that Everett ascribes such importance to meaning. Invoking Peirce, "one of the most brilliant and original American thinkers of any century" (118), Everett suggests that he 'extended' Saussure's view. However, he fails to note that Peirce saw meaning-making as dynamic. Had Everett recognised, say, the power of habit-taking, LCT might have challenged the view that brains identify (and issue) meanings.[10] Yet, while rejecting universal grammars, words remain in the head. Vaguely, history is said to give populations discursive strategies that depend on inner words and information. People, not brains, make sense of the said by drawing on lexis, syntax, and coherence. If this is correct, and especially if we vary in appraising 'information', the claim sits ill with Everett's avowed position. Why should each of us possess a mental lexicon? Why, as we listen to speech, should we rely mainly on verbal patterns? Why not rely, at least in part, on physics and direct experience? After all, much sense making plainly depends on, not

lexical meaning, but bodies, coordination and, in literate cultures, how texts can be read.

Everett's residual cognitivism reappears in his view of communication. Brains are not only "equipped to process" (174) phonetic signals but even identify "the correct meanings of the signs" (59). Further, leaving aside spontaneity — and the other-orientation of dialogue — he suggests that, as we speak, we make unconscious decisions (142) about how to pick and order our words. Apparently, a central executive controls speech. Seeing Searle's (1983) work as "profound", he makes intentionality a crucial "feature and sign of our mental life" (161). Perhaps this is why, in a striking passage, he presents morphology as a "set of principles" that regulate how a brain "is able to encode (assemble) and decode words" (142). While ambiguous, the intuition is presumably that morphological categories constrain thought. However, rather than generalise this to verbal patterns, Everett relies on the findings of distributional analysis. By separating sense-making from language products, he reduces utterance-acts to linear patterns. Thus what people say and tell depend on a single mind whose output resembles a transcription. Language enacts messages rather than, say, allowing people to set off predictable effects.

Although an important book, LCT is flawed by its cognitivism. Once excised, I will argue, Everett's work points towards a new view of cultural tools. Accordingly, I turn to how exostructures enable human cognition to link brain and world-side constraints. Stressing contrasts with other primates, I concur that culture makes human cognition ecologically special (Ross 2007). We communicate and cognise by integrating artifacts with verbal patterns as circumstances come to be embedded in cultural ways of life.

3.1   Thinking bodies and beyond

The collective aspect of human cognition, identified by Hegel, and pursued by Marx, Vygotsky, Bakhtin, Leonti'ev (among others) has begun to influence cognitive science (Crisafi and Gallagher 2010). In part, the impetus comes from biology: Living species differ in use of collective, social and solo intelligence. Thus, while humans are social primates, our lives have been transformed by cultural exostructure. Pursuing how thinking uses cultural tools, Edwin Hutchins's (1995a) work on pilotage was seminal in taking the study of cognition beyond the laboratory. A crew brings a ship into harbour by cooperating as they use artifacts, trained bodies (and brains) and the ship itself. Plainly, human thinking uses *aggregates* of resources. Later use of material, cultural and neural resources connects their material and institutional traces with individual memories. This links human skills and modes of action with artifacts, institutions and languages. Thus, while situated and embodied, human cognition is *also* historical, distributed and non-local.

As bodies draw on cultural tools, other people's experiences transform how a person acts and perceives. For example, when navigating, brains do not rely on tokens of what ships' charts represent: people appraise events by imagining the surroundings. Flying is similar (Hutchins 1995b) in that pilots do not memorise possible situations but, rather, develop practical skills within aggregates that include simulators and real aircraft. Expertise comes to be triggered by resources and instruments that allow behavioural flexibility.

Cultural tools are neither represented in brains nor controlled by a single mind. This is the insight that allows cognitive science to abandon folk categories (e.g., attention, grammars, and perception) by turning how human aggregates manage complex activity. It permits investigation of events in, say, hospitals, crime scene investigation and road construction.[11] Science itself becomes a paradigm of how cognitive outcomes draw on cultural tools. Pursuing this argument, Giere (2004) invites readers to consider use of the Hubble telescope. When looking at pixelated outputs, an astronomer (or web-surfer) participates in an assemblage or *distributed cognitive system* (DCS). The image observed depends on other people, light detection, and, of course, software that runs on hardware. To clarify systemic output, Giere draws a comparison with farming. In agriculture too, distributed systems connect natural, cultural, linguistic, and artifactual resources. He thus suggests a functional parallel between crops and, for example, the pixelated image. The outputs of relevant systems are *designed* for varied human use. Neither a distributed system nor its products (e.g., potatoes) are to be explained by individual actions, minds or brains.

Agricultural products and pixelated images presuppose *future* activity. Just as crops can be turned into cash or food (for example), much can be done with a pixelated image. This, however, depends on sense-making. A distributed system is cognitive if, and only if, it contributes to intelligent activity (Giere 2004). An image is part of a cognitive system only in the act of construal. By definition, the aggregate functions to meet human requirements, wants and needs. Thus, the 'same' products contribute to many systems. Telescopes and languages are exostructures that link bodily activity with diverse functions. Linguistic pathways lead beyond experience if, and only if, we *actively* construe the products. Cognition is action-perception or, as Mace (1977:43) says, "Ask not what's in your head, but what your head's inside of". Writing offers an intuitive example: rather than depend on a corporeal studio, inscriptions are made in a physical space. As a character of E.M Forster's wrote: "How can I tell what I think till I see what I say?" (1956:101): Writing enacts thinking (Menary 2007). As we look and monitor, the time scales of hand movements/ typing strokes link phenomenal experience to enduring events (e.g., memories and pieces of text). The cognitive power of cultural tools links Shannon information with the interactivity of human bodies.[12]

Writing generalises to face-to-face language-activity. Like a Hubble image, verbal patterns elude analysis on a single time-scale. As cultural exostructure, language is neither synchronous nor a phenomenological object. Languages such as Pirahã or Arwi result from coordination within cultural assemblages. Verbal patterns connect a history of human intercourse with a cultural ecology. As verbal patterns are *abstracta*, they connect individual activity with patterns used by populations. In Love's (2004) terms, we perceive second-order structures during first-order activity. Unlike physical words (or inscriptions), verbal patterns are non-local: Wordings arise *as* signs are made and construed. Like images, they evoke more than can be seen. Though Everett invokes a mental lexicon, culture may be sufficient to anchor human sense-making. In construing a wording, a reader uses interactivity to link experience with repeated verbal patterns. Acts of looking bring about construal: Nature's trick is that sense-making *also* calls up second-order structures (e.g., wordings). Phenomenology makes action-perception both dynamic and symbolic (see Cowley 2011b; Rączaszek-Leonardi 2011): People coordinate by using structures that originate in quite different time-scales. Cultural life uses symbiosis between wordings and first-order activity. Speaking, writing, hearing, thinking and looking all connect movements (e.g., saccading) with percepts that endure in the specious present. Acts of articulation and listening exploit *discrepant awareness* (Gibson 1979) such that a picture appears as both as pixelated image and, say, a galaxy. Language is spontaneous sense-making influenced by verbal patterns (Cowley 2011b). Just as an observer can see a picture as representational, we perceive language around wordings. Since observation is based in looking, listening and otherwise moving, brains (or minds) need neither representations nor physical symbols.[13] The residual cognitivism of LCT lies in that, leaving aside first-order events, language is traced to a mental lexicon.

4. Towards people-centred linguistics

Where seen as 'representational', words in social intercourse are identified (or conflated) with how they function as cultural tools. This simplifying idealization ignores the fact that that they are, at once, dynamical and abstract or, in other terms, situated and non-local. In appealing to the lexicon, LCT leaves out their physicality and their situatedness. At the same time, in lauding Peirce's view of signs — and verbal symbols — Everett hints at an alternative. Given triadic semiosis, a sign-vehicle achieves effects through a living system. Sign-perception gives rise to interpretants, or results that draw on circumstances. On such a view, the mental lexicon is unwarranted. Indeed, the role of Peirce's habit taking may be especially clear in a society where everyone knows everyone and most knowledge is shared.

Rather, the perceived setting itself induces people to orient by using wordings and other cultural tools. As Everett stresses, cultural interdependencies constrain not only what is said but also experience and action. In thus questioning the mental lexicon, one moves towards a people-centred linguistics.

Although paying lip-service to brains that encode and decode Shannon information, Everett emphasises meanings. What is said depends on the setting: "All meanings, literal, colloquial, figurative and so on, are produced by culture for its own purposes" (130). Like Peirce's signs, meanings are "all around us", part of the lived world. Unlike memes, they do not replicate. Rather, they shape "discursive strategies" (293) or speaking whereby people use ranked values to filter what to say and do. In the lived world, strategies change continuously. Indeed, Jennings and Thompson (in press) argue that, just as populations are subject to natural selection, verbal patterns are "engendered" in historical time-scales. Concurring with Everett that form follows function, their biological view of language emphasises populations. Individuals participate, with varying degrees of skill, in colloquial speech. Populations use *colloquy* or a patterning of patterns that are heard (and transcribed) by attending to vocal coordination in a special way.[14] For Jennings and Thompson, populations select structures like recursion if, and only if, they fit with local practices. In Pirahã society, Everett argues, such value is given to immediate experience that this does not occur (289). Of course, this does not reflect lack of intellect. Since recursion appears in story-telling, only cultural exostructure is missing: The Pirahã show directedness or, in Everett's terms, intentionality.[15] Their capacities to make and construe signs are typically human. Further, as people report experience, they orient to a culture's ranked values. Not only is the Pirahã value of immediacy likely to block the rise of recursive constructions but it is consistent with both Peircean abduction and tracing "human civilisation" (3) to verbal *abstracta*. On a people-centred view, the world is culturally enchanted by meanings (and verbal patterns) whose values and value-rankings influence the shared memory of a community.

For Everett, there is synergetic interaction between cultures and languages (204). This too prefigures a more people-centred linguistics. By regarding language as a part of a complex cultural system, its synergies rely on events in one time-scale that impact on (or "enslave") those of another. In the Banawá language, for example, the feminine gender is unmarked: In making self reference, men (and women) mark themselves as 'she'. While careful about drawing conclusions, Everett suspects that this encourages segregation between the sexes or the, to Western eyes, extraordinary rites associated with a girl's first menstruation. If so, slow-scale cultural facts correlate linguistic practices with how people live. On a people-centred view, the unsaid takes on new importance. The Pirahã's "principle of direct experience" not only keeps recursive patterns out of the cultural exostructure but

stops "unnecessary generalizations beyond the here and now" (256). Further, if principles like politeness, or political correctness, constrain what people say, probabilistic data-structures (lexical items) must detach from perceived invariances. Meaning potentials are cultural or, in Everett's terms, a "roadmap of our existence" (153). This makes it hard to see how information processing could generate a phonologically structured mental lexicon. On a semiotic view, populations coordinate actions as individuals use cultural tools.

4.1   Intimations of biosemiosis

Just as social experience matters, the same applies to individual lives. A people-centred linguistics cannot ignore the synergies that connect people. Indeed, individual sense making is necessary if *abstracta* are to take on causal powers. Without people, there would be neither cultures nor languages: only interpersonal synergies can animate human practices. Making signs unleashes meaning potential as wordings influence first-order events. In Peircean terms, abductions arise as people construe observations. More neutrally, using first-order activity, we go beyond (Shannon) information as, for example, an image is seen as a galaxy. While Everett's cognitivism masks first-order activity, his writing hints at its presence. For example, he exalts "the joy of language" (4) and reports that "musical speech" accompanies Pirahã mutual grooming (238). This is, he suggests, one of four modes of speaking — *kapérne* (normal speech), *sarén* (instructional), *ngére* (musical), and *sangére* (special kinds of musical speech). Further, the pitch, loudness, and length of hum-speech "communicates all the richness of normal human speech." To my mind, it is unimaginable that a brain could use the distinctions to map utterly different sounds onto the same lexical items. Yet, as meaningful coordination, they all enact first order activity, or "coconstructed and dialogically coordinated body dynamics that occur in real-time interactive behaviour between persons" (Thibault 2011: 14). Cultural processes can thus use language to influence or (enslave) abductive thinking. Indeed, though hum speech is not seen as primitive, Everett insists that languages have ancient precursors. Yet, he does not follow Peirceans such as Hoffmeyer (1996, 2008) in tracing the use of cultural tools to biosemiotic principles.

Everett stresses that individuals resist a culture's pressures of conformity. In contrast to Tomasello's (2003) view of language construction, individuals do not rely on a naked brain. While they "filter out" meanings, the mental lexicon prevents these arising *ex novo*. Ironically, recent work in cognitive science shows that dialogue can engender cultural tools. Coordinating generates symbols (Galantucci 2005) and, importantly, procedures (Mills 2011). However, rather than pursue pragmatics, Everett offers broad generalizations: We talk "to be understood and

to convey the emotions we are feeling to our fellow humans" (146). While likely that affect shapes understanding, Everett fails to link this to a mental lexicon or information processing. Rather than explain synergies, people "exploit what our bodies and brains already have" (173). Productivity is human –not linguistic. In Peirce's terms, "habits of intentionality" unite the biosemiotic world. However, Everett cannot make any such move or allow thoughts to arise from abduction. This is because, alongside the mental lexicon, he posits that all learning can be traced to an interaction instinct. Before rejecting this "simple reflex" (185), I turn to how a people-centred view can build on how cognitive functions use cultural exostructure.

4.2 Dark cognitive and cultural matter

Everett invokes "dark cognitive and cultural matter" that appears in what is *not* said in discourse (198). For example, while some cultural groups favour phatic communion, others emphasise direct experience. Verbal expression is thus *one* thread in culture and cognition. In terms of distributed cognition, the unsaid is like the distant galaxy evoked by an image. Output (pixelated pattern) differs both from what appears to the eye and, of course, how we construe what we behold. As Gibson (1979) suggests, we exploit (at least) two kinds of awareness: in first-order language, it seems, we link the said with the unsaid.[16] In Everett's idiom, dark cultural matter imbues language with values. Second, this sets off an abduction-like process where its dark cognitive counterpart links affect and understanding. Population level *colloquy* gets entangled with experience of first-order language. In orienting to verbal patterns, people use population level functions while, in rapid time-scales, abductive processes drive understanding.[17]

By orienting to verbal patterns (and the unsaid), dark cognitive matter may set off activity that depends on brain and body re-use. No mental lexicon is needed because, as is increasingly recognised, human cognition is anticipatory.[18] Second, biological function depends on parameter setting or, as noted above, what Peirce called habit-taking. Phonetic and semantic knowledge exploit far richer than patterns than those of lexical items. Even if sense making uses "simple forms and simple functions" (116), aggregates permit humans to link circumstances with discourse (or colloquy). Rather than "process" input, we use the perceived circumstances in affectively based action. If dark cognitive matter sensitises bodies to nuances in how — and when — things are said (or remain unspoken), empiricist theories are bound to overplay human conformity. Without special organs, as Everett correctly sees, we can make effective use of over-interpretation and, crucially, under-interpretation. Indeed, only organism-centrism prevents Everett from tracing meaning to bodily engagement with a (partly) cultural world.

Where we apply conscious experience to language, we can identify what people repeat. This principle is common to distributional analysis, lay views of language, and synchronic linguistics. Generally, speech is *not* based on conscious experience or word choice: rather, in Maturana's (1978) terms, we *orient* to wordings in a consensual domain. We link lexical meanings with cultural values to make subjective and circumstance relevant construals. In Everett's terms, these "are crucial in determining how we react (sic) and interpret the sounds, sights and creatures around us" (221). As we exploit vagueness and the unsaid we make use of tacit knowledge. Know-how involves both the brain and how cultural tools shape *acts* of coordinating (within aggregates). More poetically, we link dark cognitive matter with affect and wordings: using telescopes can alter brains, individual thinking and even cultural beliefs. The same almost certainly applies to the cultural tools of language and literacy. Indeed, communities can be transformed by sustained use of writing tools, books, printing or computers. Literacy produces institutions that favour changes in brain-use that arise from habits of education, training and entertainment. It would be odd if, in phylogenetic and ontogenetic time, first-order language (and other skilled activity) did not also reorganize brains. As a result, language shapes lives as individuals sensitise to dark cultural matter. This affects what is translated into thought and action. For this to occur, colloquy need only engender outputs that, like pixelated images, invite construal and sense-making.

In relation to people, plants, animals, and technology (for example), individual knowledge can build on embodied experience. We can live with a cat, eat potatoes, and use computers. However, verbal patterns differ from such entities in being anchored by a network of practices. The potential associated with wordings (i.e., form and meaning) is more like that of the "manifolds" of contemporary geometry.[19] Rejecting ontological reductionism, a people-centred linguistics turns to virtual constructs (e.g., Ross 2007; Love 2007; Itkonen 2008; Markoš et al. 2009). Articulatory processes map onto what are transcribed by a skilled analyst: texts capture neither the essence of language nor how brains process Shannon information. Indeed, analysis formalises practices and, by so doing, nurtures faith in *words* (while allowing that their "existence" is elusive, Everett insists that brains store lexical items).[20] However, as Love (2004) argues, practices depend on first-order events (e.g., talking-while-listening, looking-at-text, making written signs). To Roy Harris's (1981) sceptical eyes, faith in words underpins the language myth or the error of theorising language in terms of *knowledge*. In a people-centred linguistics, instead of reifying linguistic forms, one can ask how belief in words and languages affects human coordination.

1st proofs

4.3 Direct experience

Since the Pirahã avoid unnecessary generalizations, they would by unable to grasp the claim that language arises from mental operations. Perhaps, they would regard it as part of human action. In Thibault's (2011) terms, understanding is enacted by first-order language. Ascribing primacy to a mental lexicon is thus no less erroneous than prioritising abstract syntactic structure. If cultural tools "bring coherence to community lives" (236), virtual structures — emotions, words and social norms — depend on human interactivity. Rather than appeal to a mental lexicon, it may be that "language, mind and culture work together to produce unseen but keenly felt limitations on our thinking, behaviour and inter-personal relationships" (246). We depend on linguistic exostructures that result from activity within a cultural aggregate. On the one hand, as representatives of a population, individuals connect first-order language, direct experience and the repeated patterns of colloquy. On the other, as living bodies, we are language makers and construers. Further, since language is part of the exostructure, an infant is born into a linguistic world. Far from needing an interaction instinct, it learns to anticipate rewards. From the first, its activities are constrained by how a culture's repeating patterns play out through caregiver practices. Using how colloquy is enmeshed with rewarding action, as infants anticipate, they increasingly differentiate signs and meaning potentials. In an isiZulu speaking world, they may begin to recognise calls for ukuhlonipha ('respect') by fourteen weeks of age (Cowley et al. 2004). Later, they meet and surpass adult expectations: infants orient to what adults want and allow. Their motivations develop as they are moved by the movements of others (Trevarthen 1998; Neumann and Cowley, in press).

Direct experience of others allows bodily maturation to be complemented by brains that, as shown in cognitive neuroscience (Anderson 2010), exploit neural re-use. The principle ensures that social expectations affect infants from birth. In Everett's terms, dark cultural matter uses caregivers who shape their childrens' needs, babbling and attitudes to objects. Thus, as Everett notes, "what distinguishes one culture from another ultimately are two things: the totality of values *and* the relationship between these values" (300). Long before the end of the first year or producing what adults hear as *words*, infants develop culturally appropriate preferences. Even if based in Shannon information, bodily dynamics and affect, dominate lived experience. Unlike information transmission, human encounters prompt a baby to feel its way into local ways of managing social events. Far from *learning* wordings, cultural tools serve, first, in orienting to others (so-called intention reading) and, later, in pursuing wants. As argued elsewhere, human symbol grounding (Cowley 2007b) insinuates verbal patterns into brains, encultured actions and, later, phenomenal experience (Neumann and Cowley, in press). Once

utterances are heard as utterances *of* something, the child's articulatory skills take on new potential. There are rewards in the saying, hearing the said, and using what remain unsaid (or dark cultural matter).

The Pirahã show that numbers derive from counting practice. Their basis lies in, not lived experience, but human productivity or what Peirce calls the directedness of nature. Like numbers, wordings derive from experience with encultured activity. Though the Pirahã do not count, their children integrate wordings with concerted action. In a new linguistics, people engage with others — and the surroundings — by moving in and out of distributed cognitive systems. These shape output drawing, in part, on dark cognitive and cultural matter. However, as living beings, individuals construe what they perceive: they use evaluations to develop cognitive powers. Temporal synergies arise as culture enslaves languages and, of course, languaging enslaves human agency. Through colloquy, thought can become increasingly refined.

5.   Cultural tools and the future of linguistics

So where does this leave linguistics? If language is rooted in culturally concerted activity, there can be no pre-given linguistic "object". The multi-scalar domain of distributed cognition depends on linguistic heterogeneity. Far from using *a priori* language-systems (or mental lexicons), human functions arise from linking embodied activity with culturally derived patterns. We use what is said, how we perceive, experience, and how perceived speech echoes and resonates with voices (and texts). In short, we call up regularities that contribute to relationships, groups and cultures. Just as population level phenomena shape Pirahã ways of life, they determine the linguistics of languages. Having rejected Everett's mental lexicon, one discovers the power of viewing languages as cultural tools. A people-centred linguistics can posit temporal synergies that constrain sense-making in two time-scales. On the one hand, exostructures engender parameters that, in Everett's terms, shape discursive strategies. Dark cultural matter, or the unsaid, influences what people say, think and, over time, how their ideas will be perceived. Clearly, this will affect the ranking of values that, as Everett shows, correlates with specific communities. On the other hand, as argued above, these ideational events occur because people *act* as language-makers and construers. In drawing on dark cognitive matter, only synergies can generate valued novelties that may be selected within cultural traditions. Just as statistical dynamics make language irreducible to verbal patterns, their bodily counterparts make linguistic fire rather more than conformity.

Blake exemplifies how language-making can give rise to enduring structures. The beast, of course, burns in a mythical world — the Tyger is a virtual being

created as dark cognitive matter inspired the poet. Indeed, as interactivity induced imagination, we are haunted by a non-rhyme that annoyed me as a child:

> What immortal hand or eye
> Dare frame thy fearful symmetry?

Is the eye the reader's, the poet's, God's or, perhaps, that of a culturally conditioned person? For me, the tiger's fearful asymmetry (sic) makes it part of nature. Burning bright, the Tyger exudes Bateson's (1979: 16) "pattern that connects". What goes for art, also goes for language: only asymmetry can link the synergies of concerted activity with those driving ideational change. Language-making is creative because brains do *not* represent lexical items: they allow human experience to be fuelled by action-perception, colloquy and human phenomenology. While Everett says little about linguistic sparks, to my eyes, the brilliance of LCT derives from having lived with indigenous peoples. A culture's world-conceptualization, like a person's, is limited. As action-perception alters the results, careful study of second-order constructs reveals the limits of both kinds of ideational thinking. In principle, new importance falls to the linguistics of languages. As demonstrated in Pirahã life, the findings challenge psychologism and reductions of intelligence to the Shannon information processing. LCT shows that probabilities are a fraction of meaning: linguistic forms anchor value laden activity to cultural exostructures. Cultural and linguistic synergies allow individuals and social actors to make sense of their worlds by filtering meanings. Rather than invoke a corporeal studio that depends on word-like patterns, we rediscover what Ryle (1949) calls skills, habits and dispositions or skilled linguistic action (SLA). A new linguistics can aim to specify how people act to connect cultural tools, colloquy, and first-order sense-making.

Notes

1. I have retained Blake's spelling from 1794. While not used in the Yeats edition (cited here), it distances the imagined creature from exemplars of the biological group *panthera tigris*.

2. For example, Sampson (2009: 15) deems Everett's finding that Pirahã "has no grammatical embedding of any kind" the "most transgressive" recent challenge to unfounded linguistic assumptions. However, given his radical individualism, Sampson deems the language "astonishingly crude as an intellectual medium".

3. The view derives from regarding wordings as interdependent with forms of life (Wittgenstein 1958). It has been taken up by, among others, Itkonen (2008) and those in the distributed language movement (see Cowley 2007a; 2011a).

4. Challenges include Thompson (2009), Chemero (2010), Robbins and Aydede (2009), and Stewart et al. (2010).

5. Many now see culture as *part* of cognition (see note 2). By contrast, Adams and Aizawa (2009) view the brain's "intrinsic content" as the "mark of the cognitive". Use and misuse of the conduit metaphor (Reddy 1979; Harris 1981) has been widely challenged.

6. Though Saussure's view was sophisticated (Thibault 2001), many view *parole* as broadly equivalent to *speech*.

7. Transcription can be viewed as a practice that no more captures the "essence" of talk than does any form of coding (e.g., using Fourier transforms). This sceptical view is supported by evidence showing that brains track phonetic gestures and use rich information (see Fowler 1986, 2010; Port 2010).

8. I allude to Sperber and Wilson's (1986) *relevance theory*, Clark's (1996) model of language use as joint action and Pickering and Garrod's (2004) claim that dialogue depends on how brains use repeated patterns to align representations.

9. Although dedicating several pages to the argument (56ff) Everett fails to grasp that a non-sense page of monkey typing carries more Shannon information than his book; he conflates formal probabilities with meaning: "we can understand information in the technical sense if we imagine all the things there are to talk about. It is perhaps infinite. But culture and circumstance will severely restrict what might be said in a particular exchange between two people. And as most of us living in the twenty-first century know, information in this formal sense, the sense of the computer age, is measured in bytes. This involves logarithms and some other tedious concepts that fortunately do not need to be discussed here". Even if the concepts are tedious, the probabilistic nature of Shannon information depends on its physical properties — it bears no relation to *what* is likely to be said.

10. For example, he says, "A symbol associated an *arbitrary* form with a culturally defined understanding or meaning" (119). As a result "cultural decisions" are associated "in a language". On a Peircean view (e.g., Hoffmeyer 1991) triadic logic sets off a dynamic *process* that exerts an effect that is meaningful to a living being (not to a brain).

11. Dror and Harnad (2008) and Cowley and Vallée-Tourangeau (2010) offer views of distributed cognition. Patel and Greon (1991) take this to hospital settings, Baber et al. (2006) to the crime-scene and Perry (2010) to road construction.

12. For Thibault (2011: 13), increasing evidence shows that "interactivity, not abstract symbol manipulation, content transmission, or information processing centred on the internal mental processes of the individual, is the key to human learning, cognition and intelligence".

13. The issue is widely debated. Steiner (2010) offers arguments supporting the view taken here; as a computer scientist, Weng (2011) argues that, in principle, brains could neither detect (nor produce) computer style input (nor output).

14. Cowley (2011b) argues that humans depend on taking "a language stance" or hearing utterances as if they really were little units (a view further encouraged by literacy).

15. He links this to Searle's individualist view of intentionality — ascribing it to biology. In ecological psychology, by contrast, action-perception cycles are always intentional or directed: for Hodges (2007), values realising is intrinsic to all activity by living organisms.

16. By analogy, we link invariants "in" the vocalization (roughly, how the unsaid relates to circumstances) with invariants "of" the vocalization (wordings). For discussion, see Cowley (2011b).

17. The roots of abduction — and biosemiosis — may lie in organic coding and/or processing Shannon information. Such views are widely debated in biosemiotics and are central to recent work by Deacon (2011).

18. In a weak form the claim is consistent with Barsalou's (2008) emphasis on simulation (by the brain); in stronger versions, it is supported by evidence that, in activities like reading aloud, organisms find their expectations for the textual environment met or, at times, violated (see, Järvilehto et al. 2009)

19. Whereas traditional geometry focused on relations in three dimensional space, its modern counterparts rely on multidimensional spaces and "manifolds": while manifolds resemble Euclidean spaces near any given point their global structure is more complicated.

20. To demonstrate the "existence" of words he turns to the "concept of distribution": "you can recognise something as an X if it is found only where things like it are found. It and the rest that are found there can be called Xs. Everything else will *not* be an X." (144). Invoking transcription, he implies that words "exist" if informants can come up with functional criteria that match an analyst's skills of transcribing/ reading transcription. While words are repeatable, this "existence" is neither that of linguistic atoms nor data-structures in long-term memory.

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Language: Between cognition, communication and culture

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Everett's main claim is that language is a "cultural tool", created by hominids for communication and social cohesion. I examine the meaning of the expression "cultural tool" in terms of the influence of language on culture (i.e. the Sapir-Whorf hypothesis) or of the influence of culture on language (Everett's hypothesis). I show that these hypotheses are not well-supported by evidence and that language and languages, rather than being "cultural tools" as wholes are rather collections of tools used in different language games, some cultural or social, some cognitive. I conclude that the coincidence between language and culture is due to the fact that both originate from human nature.

Keywords: Cognitive tool, cultural tool, culture, Everett's hypothesis, language, ontological dependence, Sapir-Whorf hypothesis, subitization, supervenience

1. Introduction

Dan Everett is best known for a paper in *Current Anthropology* (2005), which made him famous overnight (the paper made the headlines of national newspapers in the USA, one of the very few linguistic papers to achieve this level of interest). In that paper, Everett claimed that the Pirahã language (a language spoken by a very small tribe of Amazonian indians), which he had been studying for the past twenty to thirty years, had no quantifiers, no color terms, no numbers, no embedding (hence no recursion), a very simple inventory of pronouns, and about ten phonemes. He also explained these "gaps" in the Pirahã language by recourse to a general cultural principle, to the effect that communication should be restricted "*to the immediate experience of the interlocutors*" (Everett 2005:622; author's emphasis).

The debate around these "gaps", whether they really existed and whether, if they existed, they could be explained by the *immediacy of experience principle*, was widespread. This is hardly surprising, since if indeed Pirahã lacks embedding,

then the Hauser-Chomsky-Fitch (2002) hypothesis that recursion is *the single factor specific to language* (hence presumably a cross-linguistic universal) would be starkly contradicted. More generally, Everett's main hypothesis both in the 2005 paper and in the following book (Everett 2008) was that culture influences not only the lexicon but the very grammar of language. Though this hypothesis is not entirely original,[1] Everett's work put it back on the forefront of the debate concerning the relations between language and culture (interpreted widely to include cognition). In what I will call "Everett's hypothesis", language is relative to culture (culture determines linguistic form), while in the so-called "Sapir-Whorf hypothesis" (otherwise known as the "linguistic relativity hypothesis"), culture[2] is relative to language (linguistic form determines culture). Note that both are, arguably, in cognitive terms, analyzable in terms of *top-down* processes.

Everett's main hypothesis, well illustrated by the title of his 2012 book (to which I refer here only by page number), is that language is a *cultural tool*, and that, as cultural tools, different languages, spoken by people inhabiting different *cultural niches*, will differ, not only in their lexicon, but in their very grammar, as shown by the gaps in Pirahã. My main aim is to try to spell out what it means to say that language is a cultural tool, and to assess different interpretations that can be given to it.

2. What does it mean to say that language is a cultural tool?

Here are a few possible interpretations:

1. Culture cannot exist without language:
   a. language is a tool to produce culture;
   b. language is a tool to propagate culture;
   c. language gives its form to culture, cognitively understood as *Weltanschauung*.
2. Language cannot exist without culture:
   a. it is created by culture;
   b. its form is determined by culture.

I will argue that the claims in 1 and 2 can be understood in stronger or weaker terms. In their strongest readings, they can be interpreted in terms of the philosophical notion of *ontological dependence*. In weaker readings, they can be interpreted in terms of the philosophical notion of *supervenience*. It should also be noted that 1c and 2b correspond respectively to the Sapir-Whorf hypothesis and to Everett's hypothesis. I will now examine these hypotheses, beginning with the weakest readings of 1a and 1b, continuing with the supervenience readings of 1c and 2b, and finishing with the ontological dependence reading of 1 and 2.

3. Weak readings of 1a and 1b

Let us begin by interpreting 1b, *language is a tool to propagate culture*. Its weak reading is that language is *one (among several)* tools available to propagate culture. This seems by far the most reasonable understanding and I do not think that anyone would produce the stronger reading (i.e., that language is *the only* tool available to propagate culture). It is indeed entirely in keeping with Boyd and Richerson's (2005: 3; authors' emphasis) definition of culture: "*Culture is information that people acquire from others by teaching, imitation and other forms of social learning*". Rather obviously, language is a form of social learning (which is not to deny that it can have other functions or serve other needs). Thus, 1b appears to be a straightforward and uncontroversial proposal.

Let us turn now to the rather stronger proposal, i.e., 1a. As long as it is not interpreted with the implicature that language is the only tool available to produce culture, it seems fairly easy to interpret it along the lines of Searle (1995, 1998, 2010). As Searle points out, language can be used, not only in *pre-existing* cultural institutions, but to *create* cultural institutions, from rituals to money, through the public communication of which specific cultural value (meaning) is to be given to such and such a material thing, from a specific behavior (e.g., rituals, declaration illocutionary acts, etc.) to a means of exchange (e.g., money, in whatever form, from metal to paper), and so on.[3] This leads to what is known as *material culture*,[4] i.e., the embodiment of culture through the conventional attribution of cultural values to otherwise meaningless material objects and events, in other words, a first approximation of the notion of *cultural niche*, used, but not defined, by Everett in the present book.

So much, then, for the weak (but reasonable) readings of 1a and 1b. They seem highly uncontroversial and indeed, Searle's view of the construction of social reality is not only widely accepted: it also is the most innovative and convincing part of his philosophy. So let us now turn to the more controversial claims in 1c (Sapir-Whorf hypothesis) and 2b (Everett's hypothesis).

4. Relativity hypotheses: The supervenience interpretation

As said, there are two relativity hypotheses as to the relations between language and culture: the Sapir-Whorf hypothesis, claiming that culture (including *Weltanschauung*) is dependent on or relative to the language spoken (1c); Everett's hypothesis, claiming that language is dependent on or relative to the culture where it is spoken. I will interpret these two hypotheses in terms of the philosophical relation of *supervenience*, which is roughly defined as follows:

A set of properties A supervenes upon another set B just in case no two things can differ with respect to A-properties without also differing with respect to their B-properties. In slogan form, "there cannot be an A-difference without a B-difference" (McLaughlin and Bennett 2011: 1).

As pointed out by McLaughlin and Bennett, there can be different forms (and strengths) of supervenience depending on how the modal word "cannot" is cashed out and on the nature of the entities to which the slogan *No A-difference without a B-difference* is applied. I take it that the notion needed here is *weak individual supervenience* (concerned with individuals rather than with regions or worlds) in its modal operator formulation:[5]

Weak Individual Supervenience
Modal Operator formulation: *A* weakly supervenes on *B* iff [= if and only if] necessarily, if anything *x* has some property *F* in *A*, then there is at least one property *G* in *B* such that *x* has *G*, and everything that has *G* has *F*.

$$\Box \forall x \forall F{\in}A[Fx \rightarrow \exists G{\in}B(Gx \ \& \ \forall y(Gy \rightarrow Fy))]$$

Let us call the A-properties the *supervening set* and the B-properties the *subvening set* (the terminology is borrowed from McLaughlin and Bennett). Regarding the Sapir-Whorf hypothesis, the supervening properties are the cultural-cognitive properties, while the subvening properties are the linguistic properties. Regarding Everett's hypothesis, the supervening properties are the linguistic properties, while the subvening properties are the cultural-cognitive properties. It should be noted that supervenience is a necessary but not a sufficient condition for both hypotheses. This is because supervenience basically targets (in this case asymmetrical) variation, but is not explanatory. In contrast, both hypotheses additionally, aim at a causal explanation.

4.1 The Sapir-Whorf hypothesis

Let us begin with the Sapir-Whorf hypothesis. What does it mean to say that the cultural-cognitive properties supervene on the linguistic properties? Here, again, several interpretations are possible, depending on how one characterizes the cultural-cognitive properties. On a first interpretation, one could say that a typical cultural-cognitive property will be *facilitation* (i.e., the ease with which an individual speaking such and such a language will perform a given task, e.g., a non-linguistic task of orienting him/herself in space). This is the weak interpretation of the Sapir-Whorf hypothesis. An alternative is to interpret the cultural-cognitive properties in terms of *ability*. Under that interpretation, one would say that a typical cultural-cognitive property will be the presence or the absence in an individual

speaking a given language of the very ability to perform the task. This is the strong interpretation of the Sapir-Whorf hypothesis.

After decades of rejection, the Sapir-Whorf hypothesis has known a recent revival, under the impulse of, among others, Levinson (e.g., 2004) and Lucy (1992). This has triggered a number of empirical works in domains where languages are known to differ, e.g., space, colors, and numerals.

In the domain of space, languages differ in terms of the reference frames they use. Most languages use the *egocentric* reference frame (where linguistic spatial markers are interpreted relative to the position and orientation of the speaker), the *intrinsic* reference frame (where linguistic spatial markers are interpreted relative to the position and orientation of a given object) and the *cardinal* reference frame (using the cardinal points). A few languages, however, only use the cardinal reference frame. This is predicted to impact the way the speakers orient themselves in space. Basically the idea is that speakers of languages with only the cardinal reference frame will use it in non-linguistic tasks when they have to orient themselves in space. This gives rise to different empirically testable predictions depending on how the supervening properties are described:

- on the weak reading, speakers of such languages are predicted to orient themselves *preferentially* through the use of the cardinal reference frame (a majority of them would use it, but some might use other reference frames, such as the egocentric one);
- on the strong reading, speakers of such languages are predicted to orient themselves *only* through the use of the cardinal reference frame (none of them would use any other reference frame).

In the domain of color, languages differ in the number of color terms that they have,[6] as described by Berlin and Kay (1969). The inventory goes from two to eleven color terms. Again, the Sapir-Whorf hypothesis comes in two versions, depending on whether the supervening properties concerned refer to *ease* of discrimination or to *abilities* of discrimination:

- on the weak reading, speakers of languages with fewer color terms are predicted to discriminate *more easily* those colors for which they have a word than those for which they haven't;
- on the strong reading, speakers of languages with fewer color terms are predicted to discriminate *only* those colors for which they have a word.

In the domain of numbers, languages differ on whether they have number words beyond 2 or not.[7] Some languages have only words for *one, two, many*. Here, the Sapir-Whorf hypothesis again makes two predictions:

- on the weak reading, speakers of languages with an impoverished number lexicon are predicted to count *more easily* numbers inside their lexicon than numbers beyond it;
- on the strong reading, speakers of languages with an impoverished number lexicon are predicted *not to be able* to count any number beyond their lexicon.

All three domains have been empirically investigated with fairly homogeneous results. Only in the case of numbers the strong reading of the Sapir-Whorf hypothesis has been vindicated and, as we shall see, this does not change the overall interpretation of the results. To sum up the empirical findings:

- in the case of space, speakers of languages with only the cardinal reference frame do orient themselves preferentially through its use (at a rate of 60%), but a fair number of them (40%) still orient themselves through the egocentric reference frame (Levinson 2004);
- in the case of color, the results showed that discrimination was not affected by the presence or absence of a color word, though memorization (i.e., recognition after an interval of time) was facilitated by the presence of the relevant word (Davidoff et al. 1999);
- in the case of numbers, it was found that people with an impoverished number lexicon could not count beyond the very small numbers (Gordon 2004).

What conclusions should we draw from these results? Leaving aside the case of number, the fact that facilitation supervenes on linguistic properties (the presence or absence of specific linguistic forms) is hardly sufficient to vindicate the Sapir-Whorf hypothesis, which claims that language *shapes our way of thinking*. As Peterson et al. (1996: 571) have argued,

> Spatial cognition is not necessarily constrained by the language that one knows (…). Exposure to different languages can engage some aspects of spatial cognition to a greater extent than others, it does not support the strongest Whorfian hypothesis that one's manner of thinking about space is entirely determined by the language one learns.

 In other words, the more one uses an ability, the easier it becomes to use it: When a language uses only the cardinal reference frame, its speakers will become more proficient at using it. However, this does not mean that their spatial orientation abilities become reduced to the cardinal reference frame. They can still use other reference frames, such as the egocentric reference frame, which is presumably a basic cognitive ability as it is necessary for action (e.g., grasping an object; see Jacob and Jeannerod 2003). Very much the same goes for colors: Humans share with other primates discriminative abilities in the domain of color perception (see, e.g., Shephard 1992), and these are not touched by the color lexicon in their

languages. However, the ability to memorize colors could very well depend on familiarity, hence on the color lexicon of one's language. So, here, the conclusion is very much the same as it is for space: Frequency of use breeds facilitation.

This leaves the case of number, where the strong reading (in terms of *abilities* rather than *facilitation*) of the Sapir-Whorf hypothesis seems to be verified. It turns out, however, that things are not quite that simple. Subitization is the ability, widely shared among vertebrate species, to "perceive" the number of objects in an array as long as it is within the range of small numbers (the limit seems to be five plus or minus two) (see Dehaene 2011). This ability exists in speakers of languages with an impoverished number lexicon. In other words, in tasks where the number of objects is below five, they perform on a par with speakers of languages with an European type number lexicon. It is when they are tested with a greater number of objects that their performance is below par.

Now, as McLaughlin and Bennett (2011:20) point out, "supervenience claims by themselves do nothing more than state that certain patterns of property (or fact) variation hold. They are silent about why these patterns hold, and about the precise nature of the dependence involved". The Sapir-Whorf hypothesis is precisely such an explanation. Does it hold in the number domain? As we have seen above, in the case of the weak interpretation of the hypothesis (in terms of *facilitation* rather than *abilities*), another explanation is available: It is not that language determines thought; rather, linguistic forms may lead to a greater use of some cognitive processes, resulting in their facilitation. If we stay with the strong reading of the hypothesis, however, the prediction would be that the availability of a linguistic counting system would lead to the replacement of subitization processes by counting processes.

Here, a recent paper by Frank et al. (2008) brings new and interesting data. They compared the performances of English speakers and Pirahã speakers in tasks involving counting and in tasks involving subitization. However, the English speakers, while performing these tasks, had to perform an additional linguistic interference task. The results were that English speakers and Pirahã speakers were matched in these tasks, showing that language is indeed necessary for counting, but that the language-based counting ability of English speakers had not "replaced" their subitization abilities, contrary to the strong reading of the Sapir-Whorf hypothesis. The conclusion drawn by Frank et al. (2008:444) is that

> [L]anguages do not appear to alter the original representations underlying complex cognitive tasks. Instead, they enable their speakers to perform complex tasks (often diferentially across languages) by providing abstractions for efficient information processing.

In other words, the supervenience of facilitation or (in a single case) of a cognitive ability on language differences is not enough to support the Sapir-Whorf

hypothesis in any but a trivial reading, though it can have a rather more interesting analysis in terms of cognitive technology, as is clearly the case for counting (see below Section 6).

4.2 Everett's hypothesis

On Everett's hypothesis, of course, the linguistic properties are supposed to supervene on the cultural properties. Here, as in the Sapir-Whorf hypothesis, the cultural properties are, *prima facie*, difficult to pick out precisely. As noted above, Everett does not give a definition of *culture*, though he seems insistent on *values* (again, without giving a definition). What seems, on the face of it, slightly more promising is the notion of *cultural niche*. Again, Everett does not define it, but he claims that languages are "adapted" to the cultural niches in which they are spoken. So, as a first approximation, one could say that there can be no differences between languages without differences between cultural niches (equally, any difference in cultural niches should lead to a difference in linguistic forms). This, obviously, makes the definition of a cultural niche crucial, though Everett just drops the expression in his book without providing such a definition. Both the notions of the language being *adapted* and of a *cultural niche* suggest an analogy with the notion of *ecological niche*, as used, e.g., in the domain of *niche construction*, i.e., "the capacity of organisms to modify natural selection in their environment and thereby act as co-directors of their own, and other species' environment" (Laland and Boogert 2010:731). In other words, by modifying their environment, species influence selection pressures through the construction of their specific ecological niches (which, of course, are also due to the niche construction activities of the other species sharing them). Ecological niches concern the physical environment, and it is clear that cultural niches should be much wider. They should include the ecological niche itself, but also all the technologies available in the culture concerned (including cognitive technologies), as well as all the material culture (the physical entities — events and objects — to which the culture attributes a cultural signification), and all the cultural *norms* (which, I suspect, Everett would call "values"). So, on that interpretation of *cultural niche*, can we say that linguistic properties supervene on cultural niches?

There are two clear examples of Everett's hypothesis: The claim that the Eskimo language has an extended lexicon for snow because the Eskimo cultural niche presents various snow states, each with its own word; and Everett's claim that the gaps in Pirahã are due to the presence in Pirahã cultural niche of a cultural norm, the immediacy of experience principle (2005, 622). The two examples differ along two dimensions. First, as noted by von Fintel and Matthewson (2008:141, note 5), they differ in the positive/negative dimension: the Eskimo example is clearly on

the positive side, environmental abundance of snow states leading to lexical abundance of words for them; the Pirahã example is on the negative side, a restrictive cultural norm, the immediacy of experience principle, leading to gaps in linguistic forms. Second, they differ in that the first appeals directly to the physical environment (i.e., it seems linked to the ecological niche), while the second appeals to what is additional to the ecological niche in the cultural niche, i.e., cultural norms which seem fairly independent from the ecological niche. It should be clear that the Eskimo example is fairly trivial, as noted by Pullum (1989: 278):

> Even if there were a large numer of roots for different snow types in some Arctic language, this would not, objectively, be intellectually interesting; it would be a mundane and unremarkable fact.
>
> Horsebreeders have various names for breeds, sizes, and ages of horses; botanists have names for leaf shapes; interior decorators have names for shades of mauve; printers have many different names for different fonts.

To which he adds:

> [I]f these obvious truths of specialization are supposed to be interesting facts about language, thought, and culture, then I'm sorry, but include me out (Pullum 1989: 279).

It is hard not to sympathize with him, especially in view of the fact that, despite the hue and cry, the abundance of Eskimo words for snow is, regrettably, an urban myth, a hoax, as shown by Martin (1986), who traced it back to its origin and exposed all the academic sloppy second-hand quoting and exaggeration which led to it.[8] The potentially interesting example, then, is Everett's Pirahã gaps. There are, in fact, three ways of discussing whether this example actually verifies Everett's hypothesis:

1. the first, and most obvious one, is to contest the reality of the gaps, i.e., to contest the validity of Everett's linguistic analyses;
2. the second one is through the distinction, made by von Fintel and Matthewson (2008) in their paper about semantic universals, between universality of syntactico-semantic forms, and universality of (weak) effability, based on the fact that despite syntactico-semantic variability, there is universality of translability through convergence on the expression of identical truth-conditional propositions;
3. the third one is to contest that the immediacy of experience principle explains the specific gaps Everett claims to have found in Pirahã.

The first two have mainly to do with contesting the existence of the supervenient properties. In other words, are the differences claimed by Everett to exist between Pirahã and other languages (e.g., English) actually there? The third one

is additional and has to do with the supposed explanatory link between the immediacy of experience principle and the gaps. Even assuming these differences to exist, can they really be explained, or justified, by the immediacy of experience principle? I will discuss them in turn.

Let us begin with the gaps. These were already discussed in the peer comments following Everett's (2005) paper in *Current Anthropology*, notably by Anna Wierzbicka, who made perhaps the most telling comment, pointing out that Everett's reason to posit the lexical gaps are ill-founded, based on a neglect of polysemy (which leads him to reject quantifiers based on the fact that the lexical items used have another meaning, e.g., "big"/"all"), of lexical forms borrowed from other languages (e.g., pronouns borrowed from a Tupi-Guarani language), and of diachrony (etymology), leading to inappropriate English glosses of Pirahã sentences. She comments: "To say that *ti' ogi* means, literally, "my bigness" [rather than "we"] is like saying that in English to understand means, literally, "to stand under"" (Wierzbiecka 2005:641).

Everett's (2005:643) answer to that criticism is in terms of non-identity of truth conditions: "Pirahã has no word with the truth conditions of universal quantification. Unless Wierzbicka can show that I am wrong about the truth conditions, she has no case". Presumably, Everett's rejection of identity of truth-conditions is based on one of his examples (2005:625), where an anaconda has just been killed. One Pirahã produces an utterance translatable as "The foreigner will likely buy the whole [*báasio*] anaconda skin". The foreigner, in fact, buys only a part of the anaconda skin. However, it is still possible to say truthfully, according to Everett, "Yes, he bought the whole [*báasio*] thing", which, of course, could not be truthfully said, in these circumstances, in English. It is not clear, however, that this is an argument against *báasio* being the universal quantifier, for it may be the case that, in the second sentence, the interpretation of *báasio* as *part of* could be a case of pragmatic loosening (see Carston 2002:27). The question is not whether *báasio* is always used in the same way as English *whole*, but whether it can be used in such a way. On Everett's own showing, it is used equivalently to English *whole* in the first sentence (otherwise, his interpretation of *báasio* is just faulty: it does not mean *whole* in either sentence). Thus, there is no reason to think that *ʻaió hi báasio ʻoaob-áhá hi ʻogió ʻoaob-áha* (the sentence used to mean that the foreigner has bought a part of the anaconda skin in Everett's example) could not have the same truth-conditions as *The foreigner has bought the whole thing* in the standard English interpretation. The same kind of reasoning goes for the other lexical gaps that Everett has listed for Pirahã, with the exception of numbers. In other words, use is not a definitive test of truth-conditions, given pragmatic intrusion (the intrusion of pragmatic processes, such as loosening and strengthening, which apply locally, in the determination of the truth-conditional proposition expressed).

By far the most important criticism, however, is that made in papers by Nevins et al. (2009a and 2009b) regarding the syntactic evidence of absence of embedding (and hence of recursion) in Pirahã. I am inclined to consider their criticisms as extremely convincing, but I would like to add a remark of my own on this specific point. In his present book, Everett (2012: 293–294) gives the following examples intended to show the inexistence of syntactic recursion in Pirahã:

1. Kóxoí higáisai. Kohoi hi goó gáísai. Xaogií báaxáí.
2. 'Koxoi said that. Kohoi said that. The foreign woman is pretty'.
3. Kóxoí higáisai. Kohoi hi goó gáísai. Xaogií goó gáísai. Báaxáí, tíi.
4. 'Koxoi said that. Kohoi said that. The foreign woman said that. I am pretty'.

In the absence of any word-for-word translation, it is of course difficult to draw any conclusions, but the analysis proposed here by Everett for Pirahã rings a bell: It strongly reminds one of Davidson's (1968: 141–142) well-known *sententialist* or *paratactic* account of propositional attitude reports. Davidson proposed that the sentence in (3) (like the one in (5)) be analyzed along the lines of (4) (like the one in (6)):

5. Galileo said that the earth moves.
6. a. Galileo said that. b. The earth moves.

The idea is that *that* at the end of 6a (as in 4a) functions as a demonstrative referring to the sentence 6b (as 4b). In other words, just as in the analyses proposed by Everett for 1 and 2, there is not *one* sentence in 3, but *two*. It is indeed apparently commonplace for complementizers to be grammaticalized from demonstratives. However, as was fairly quickly pointed out against Davidson's paratactic account (e.g., by Schiffer 1987), in many languages complementizers are derived from demonstratives. In other words, even in the languages like English (and Pirahã?) that use a homophonic form for the demonstrative and the complementizer, there is no reason not to distinguish the two and to treat the complementizer as a demonstrative. Thus, Everett's analysis is no more convincing from a linguistic point of view than is Davidson's.

This, of course, is hardly a destruction of Everett's claim regarding the absence of recursion in Pirahã; yet, supposing that Everett's glosses of the Pirahã sentences above are faithful to Pirahã forms, it does throw some doubt on its validity. Finally, still on that specific "gap", von Fintel and Matthewson (2008: 173–174), having noted that recursion is linked with both compositionality and generativity, come up with the following argument:

> A language which lacked recursion would potentially not have need of compositionality. What would such a language look like? Its sentences would not

necessarily consist merely of structure-less strings of lexical items; it is possible to construct non-recursive phrase-structure rules (…). However, in the absence of recursion, the number of possible sentences would be finite. This would mean that a child could potentially simply memorize lexical items and structures along with their corresponding meanings(…)

Everett (2007: 7) states that he is not arguing that Pirahã lacks the property of "discrete infinity".[9] His position is that "Pirahã grammar has no phrase structure" (Everett 2007: 4), and that constructions which might appear to involve recursion involve either "parataxis" or "juxtaposition" of items in a string. What do these claims mean for the semantics? If a potentially infinite set of strings can be generated, then the language learner or user cannot rely on memorization to help decipher meaning. But in the absence of any phrase structure, compositionality cannot apply. It is under this scenario somewhat of a mistery how sentences are assigned interpretations. In other words, there is some tension in the claims that Pirahã has no recursion, no phrase structure, but has "discrete infinity" (i.e., that it is generative, just as is English).

Let us now turn to the second and weaker argument against the existence of the supervening properties. It might be that, though the syntactico-semantic forms are not universal (i.e., differ from one language to the next), nevertheless there is universal translatability in the sense that the same truth-conditional propositions can be expressed in all languages, albeit through different syntactico-semantic forms. That this is the case seems to be recognized by Everett (2009), as we will see below.

The possibility of this sort of weak effability (i.e., identity of truth-conditional propositions expressed *without* identity of syntactico-semantic forms) leads us rather naturally to the last question: Does the immediacy of experience principle, as a cultural norm, actually explain the gaps in Pirahã? It has been well argued by Nevins et al. (2009a) that it does not. I will not retrace the arguments here though I must point out that I do not think that Everett has satisfactorily answered them. For instance, he (rightly) claims (see Everett 2009: 406) that the fact that comparable (isolated) gaps exist in other languages does not mean that these gaps must have the same (cultural) origin that they have in Pirahã. The immediacy of experience principle explains the whole set of gaps, according to Everett. But the fact that the existence of comparable gaps in other languages has nothing to say about the origin of those gaps in Pirahã is hardly a vindication of the immediacy of experience principle. Everett's main idea seems to be that the existence of the gaps in Pirahã is not a "coincidence", that, in other words, they form a consistent class in the sense that if they did not exist (if the corresponding linguistic forms were found in the language), they would allow Pirahã speakers to speak of things which cannot be immediately experienced. This is, however, not really convinving:

- colors seem to be immediately perceived;
- small quantities are also immediately perceived through subitization (and, indeed, Pirahã speakers perform quite normally on tasks implying subitization; see Gordon 2004; Frank et al. 2008);
- quantifiers, again, have uses (probably the most frequent ones even in European languages), where they are interpreted relative to a (restricted) domain of quantification, where they can be immediately seen to be true. What is not immediatly perceivable in "Koxoi has eaten all the/the whole fish"?
- even for recursion, it would be weird to claim that it is only used to speak of things outside of immediate experience. When I say "Everett claims that there is no recursion in Pirahã", I speak from my immediate experience of reading Everett's works; and when I say "I see that John drives a new car", I am speaking of my immediate (and present) visual experience of John driving his new car.

Indeed, the very possibility for some sentences in Pirahã to express truth-conditional propositions identical to truth-conditional propositions expressed in English, despite the difference in syntactico-semantic forms, throws doubt on the validity of the immediacy of experience principle. If it is a cultural norm in the Pirahã society not to say anything beyond immediate experience, if universal quantification flouts that norm and if Everett's hypothesis is true, one would expect it to be impossible to say anything like 'The foreigner will likely buy the whole [báasio] anaconda skin', no matter what linguistic form is used. Yet, as Everett himself acknowledges, this is not the case… Everett answered that objection in the case of colors, claiming that

> [A] property name that generalizes over immediate expressions is an abstraction, a variable. (…) Color terms are abstractions, the descriptions of colors are not. Abstractions violate the proposed principle of immediacy; phrasal descriptions do not (Everett 2005: 642).[10]

This seems to shift the immediacy of experience principle from what one can talk about (which would be vulnerable to the weak effability objection), to a prohibition of abstraction principle. But this triggers a strong objection: It is not only color terms that are abstracted from instances, but any common names, from *cat* or *dog* to *anaconda* and *fish* (indeed, given the variety of species of fish, *fish* is rather more abstract than *cat* or *dog*, corresponding to a concept above the species level). But this (apart from the fact that it does not obviously justify the absence of quantifiers, small numbers or recursion) leads to very much the same type of argument as that by von Fintel and Matthewson reproduced above: it makes it entirely mysterious how Pirahã could have any common name, any adjective and (given the greater abstraction usually attributed to verbs) any verb.

There is a fairly obvious reply that Everett might make to the objection that the things supposedly prohibited by the immediacy of experience principle can be expressed in the language, which is that he has often asserted that, despite their language limitations, Pirahãs have no corresponding cognitive limitations (leaving aside counting). In other words, here, language could reflect cognition, rather than cultural norms. This, however, would clearly be a self-defeating strategy, as it would be tantamount to acknowledging that cognition is a stronger constraint on language and effability than are cultural norms. Finally, as noted above, Everett's hypothesis supposes that there can be no linguistic differences without cultural differences (from the very definition of supervenience). However, as Everett himself acknowledges (p. 212), this is not always the case:[11]

> What interests me about the Xingu, though, is that the cultures from the fourteen groups currently residing in the park are very similar, yet their languages are very diverse, representing multiple linguistic families. Despite this, the peoples of the Xingu participate in cultural ceremonies together, work together for land rights and political representation, and engage in a range of other activities together. What this all shows us is that cultures may change, or intermingle, but languages remain stable. Therefore, when a link between culture and language is proposed, it is vital to determine whether the connection is the result of the current state of the language and the culture or whether it is a vestige of a previous period, but evidence for the link has been eliminated. In other words, it may be that some cultural influences cannot be traced and that the facts of language that the culture might have produced at a different point in time look arbitrary from a contemporary perspective.

One interpretation of the second part of this quotation is that Everett is trying to show that these linguistic differences without cultural differences do not contradict the supervenience of language on culture. His defence seems to rest on the supposition that there are two different timescales of evolution for culture and for language, something we clearly have no evidence for. What is more, the only reasonable interpretation of some other claims of Everett (e.g., that language was "invented"; pp. 5–7, 19), strongly suggests that he thinks that the evolution of language was cultural rather than biological. Why should we suppose that there are different timescales of *cultural* evolution, language lagging behind culture? Finally, given the rapidity of the evolution of languages (of which everyone agrees that it is cultural), the idea that languages are "stable", whatever this means, while cultures are not, seems strange…

4.3 Conclusion

As we saw above, there are two hypotheses regarding the relation between language and culture:

– Sapir-Whorf hypothesis claims that language determines culture, understood widely as including *Weltanschauung*;
– Everett's hypothesis claims that culture, including cultural norms, determines what linguistic forms are found.

Both hypotheses can be interpreted as involving supervenience relations: On Sapir-Whorf hypothesis, cultural cognitive properties are supposed to supervene on language; on Everett's hypothesis, linguistic properties are supposed to supervene on cultural niches.

It seems that these supervenience relations are verified for Sapir-Whorf hypothesis in only a trivial and weak way (involving *facilitation* rather than *ability*), depriving the hypothesis of any explanatory value, given the availability of an alternative explanation in terms of learning or training. Examples for Everett's hypothesis are limited: One of them, the supposed abundance of Eskimo lexicon for snow has been exposed as a myth; basically, this leaves us with Everett's claims that there are "gaps" in the Pirahã language, whose origin is to be found in a Pirahã cultural norm, the immediacy of experience principle. The gaps in question are both lexical (no quantifiers, number terms, color terms, etc.) and syntactic (no recursion). The very existence of these gaps has been contested, as has been the claim that the immediacy of experience principle could explain them, should they be proved to exist. I am rather doubtful of them myself (especially of the absence of recursion), but even conceding their existence for the sake of argument, it is hard to see why the immediacy of experience principle, restricting communication to what can be immediately perceived or experienced,[12] should specifically exclude from Pirahã quantifiers, colors, small numbers and recursion.

5. Language and culture: ontological dependence

The notion of *ontological dependence* "is that of one entity *depending for its existence upon* another entity, not in a merely causal sense, but in a deeper, ontological sense" (Lowe 2010: 2),[13] Lowe (ibid.) proposes the following formula:

> *x* depends for its existence upon *y* $=_{df}$ necessarily, *x* exists only if *y* exists

It might be said that the fact that ontological dependence is independent from causal relations makes the discussion in Section 3 irrelevant to this strongest

interpretation. However, it should be remembered that most of that discussion was couched in terms of supervenience rather than explanation as such. Supervenience is (asymmetrical) variation. Ontological dependence is a much stronger relation than supervenience, and hence yields the strongest readings of interpretations 1 and 2 (reproduced below) of the notion of language as a cultural tool:

1. Culture cannot exist without language.
2. Language cannot exist without culture.

But the very fact that supervenience is the weakest relation (and is not, in and of itself, causal) and that the evidence for supervenience, either in the direction of language to culture or in the opposite direction, was found to be rather fragile, makes the ontological dependence reading verification very unlikely.

Is that to say that we could find culture without language or language without culture? Beginning with the first possibility, it has been argued that there are non-human animal cultures, the best examples of which are the technological variations in tools and tool uses among chimpanzee groups (see de Waal 2001 for a discussion). This is not the place for an extended discussion of animal culture, but it should be noted that those "cultural" differences are extremely limited. This is a far cry from the notion of *culture* relevant to Everett's language-as-cultural-tool hypothesis. His view of culture is much richer as was seen in the discussion of the notion of *cultural niche* above (see 4.2). What about language without culture? There, no evidence is available. However, this does not necessarily mean that language is ontologically dependent on culture. As we have seen (see Section 3), Searle has shown very convincingly that a lot of culture (and therein the parts that most interest Everett, i.e., material culture, myths, and cultural norms) is created through language. It seems rather more reasonable to suppose that the systematic coincidence of language and higher culture is not due to ontological dependence in either direction, but rather to the fact that both can only occur in human groups, and that both depend on human nature. So, from all that has been said before it seems that none of the interpretations proposed in Section 2 for language as a cultural tool is really convincing. Could there be other, maybe less general, interpretations of the notion?

6. Of tools, cultural and otherwise

Let us begin by a definition of *tool* (borrowed from Wikipedia, http://en.wikipedia.org/wiki/Tool, january 5, 2012):

> A tool is a device that can be used to produce an item or achieve a task, but that is not consumed in the process. Informally the word is also used to describe a procedure or process with a specific purpose. Tools that are used in particular fields or activities may have different designations such as *Instrument*, *Ustensil*, *Implement*, *Machine*, or *Apparatus*.

In his book, Everett claims that languages are "tools to solve the twin problems of communication and social cohesion" (Everett 2012: 6). This seems very wide, indeed too wide for language to be a tool in the sense indicated by the Wikipedia definition, i.e., a device used for a specific purpose.

Whether language is or is not a tool, of course, depends on how widely one is ready to understand the word: it seems difficult to claim that language is a *machine*, or an *ustensil*, or even an *apparatus* (more specific terms), though it may be easier to accept that it is an *instrument* or an *implement* (less specific terms). Coming back to the definition itself, can language be said to be "a procedure or process with a specific purpose"? This would seem not to be true of language in general, but rather to be true of specific uses of particular linguistic constructions or items, as for instance in performative utterances. This suggests that it is not so much language as such, or even whole specific languages (e.g., French, English or Pirahã) as such, so much as their use in specific language games, that make some linguistic forms tools. This of course is due to the "specific purpose" specification in the Wikipedia definition. However, making do without this specification seems to rob the term *tool* of much of its meaning: It is not that we cannot think of a multi-purpose tool; it is that a specific example appropriate for comparison with the view that language as a whole is a tool is hard to find. Indeed, the most obvious case is that of the Swiss Army knife. *However, a Swiss Army knife is not so much a single multi-purpose tool as a collection of specific purpose tools.*[14] So, is language really and authentically a *single* multi-purpose tool, or is it, as is a Swiss Army knife, a *collection* of single purpose tools, diversely used in different language games?

As said above, language has different functions, or, rather, specific parts of specific languages have different functions in different language games. Let us begin by examining a few of the different uses language can be put too, in face to face interaction. The most obvious are, of course, the different types of illocutionary acts, as described, for instance, by Searle (1998: 148), who distinguishes between *assertive*, *directive*, *commissive*, *expressive*, and *declarations*. At the same time, it should be clear that each class of illocutionary acts gathers linguistic forms that have different specific purposes. This is especially obvious for the class of declarations, which encompasses a number of institutional acts, such as *declaring the meeting open*, *baptizing a child*, or *declaring war*. As well, as seen above (see Section 4.1), language can be used in the creation of cultural institutions (including language games).

There are, however, other uses to which language can be put, as Everett, quite rightly, insists: most obviously, language is not only a social tool, open to multiple uses in different language games, it also is a major cognitive tool for thought as well as a way of producing other cognitive tools. This is well illustrated by the use of language in counting (see Section 4.2).[15]

Therefore, languages are collections of single-purpose tools, rather than single multi-purpose tools, some of them clearly cultural, some clearly cognitive, all possibly influenced by culture to some extent.

7.   Conclusion

Thus it seems that Everett claims is rather inflated relative to what actually is the case. Language is not a cultural tool so much as a variety of tools, cultural, cognitive, social. What is more, the relations of language and culture are not quite as tight as Everett claims or as the Sapir-Whorf hypothesis has claimed. Does Everett's strongest claim regarding the notion that language, *as a cultural tool*, is an artifact, "an instrument created by hominids to satisfy their social need for meaning and community" (Everett 2012: xi) fare better? This "social" scenario for the existence of language entirely fails to account for linguistic generativity in view of the fact that nonhuman primate communication fulfills these functions without generativity.

I would like to end this paper by discussing one of Everett's claims regarding the non biological nature of language, which seems intended as a kind of *reductio ad absurdum* argument: That, if language is biological, one would expect to find "culture-gene mutations affecting specific languages of the world" (Everett 2012: 42) and these do not exist. In fact, recent findings (see Dediu and Ladd 2007; Nette 2007) suggest that such mutations exist. Dediu and Ladd established a strong correlation between (geographically dispersed) tone-languages and allele frequencies for two genes (*ASPM* and *Microcephalin*) in the populations speaking those languages as compared with speakers of non-tonal languages. The interpretation is that these specific alleles would facilitate the learning of tonal languages through better acoustic discrimination. To be quite candid, I think Everett's argument is rather poor, but it is not uninteresting that it may be wrong as well.

So, my conclusion would be that the jury is still out regarding the evolution of language, and that it may well be that the most convincing scenario is that proposed by Newmeyer (2003) to the effect that language is the product of a double evolution: A biological evolution of UG and a cultural evolution for those aspects of language that are not constrained by UG.

Notes

1. As von Fintel and Matthewson (2008) note, it is a twist on the notorious Eskimo story, where a (mythical) abundance of terms for snow is explained by Eskimo culture (and environment): Everett uses much the same kind of reasoning to explain the sparseness of the Pirahã language.

2. Understood rather widely as the overall *Weltanschauung* of a human group sharing the same language, that is understood cognitively.

3. It is one of the peculiarities of Everett's book that, while it pays lip service to Searle, both speech acts and the intimate link between language and cultural institutions are very sketchily described.

4. The notion has been developed by Renfrew (2007), who uses Searle's philosophical approach in an archeological perspective.

5. Borrowed from McLaughlin and Bennett 2011: 60.

6. Indeed, Everett (2005, 2008) claims that Pirahã does not have any color terms at all.

7. Again, Everett (2005, 2008) claims that Pirahã does not have any number terms at all

8. Just for the record, there are two lexical roots for snow in Eskimo, as shown by C.W. Schultz-Lorenz's (1927) Dictionary of the West Greenlandic Eskimo Language (quoted by Pullum 1989).

9. Generativity.

10. In other words, weak effability stands!

11. Indeed, according to Richerson and Boyd (2005), there is no systematic relation at all between language and culture.

12. The abstraction interpretation of the immediacy of experience principle, as we have seen, leads to absurd consequences, making it difficult to see how Pirahã has any substantive lexicon at all.

13. An example would be the ontological dependence of Caesar's murder on Caesar: the murder could not have existed if Caesar did not (Lowe 2010: 3).

14. Indeed, Cosmides has notoriously used the Swiss army knife metaphor for the multiple specialized modules that make up the human mind, according to the Evolutionary Psychology theoretical framework (at the conference "The Evolution of Social Behaviour Patterns in Primates and Man", London, April 4–6, 1995).

15. See Dascal (2004) for an extended discussion of language as a cognitive technology.

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Language is an instrument for thought. Really?

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This discussion article addresses the assumption formulated in Dan Everett's new book *Language: The Cultural Tool* that language is not only an instrument for communication, but also an instrument for thought. It argues that the latter assumption is far from obvious, and that, in any case, one cannot put communication and thought on a par in discussing the functionality of language.

1.   Language, culture, communication, and thought

In his newest book, *Language: The Cultural Tool*, Dan Everett (2012) (re)reveals himself as a convinced adherent of the view that language is crucially determined by, hence systematically reflects and crucially contributes to the transmission and shaping of, culture.[1] He herewith reconfirms his membership (since his conversion from his 'Chomskyan days' in the nineteen nineties) of the large and — after a long decline in the Chomsky dominated nineteen seventies and eighties — again growing group of scholars of language, in different disciplines, including linguistics, psychology and anthropology, who do not consider language an isolated and more or less 'arbitrary' cognitive (Chomsky 1972, 1975, 1986, and passim) or platonic (e.g., Katz 1981) object or entity (in linguistics commonly called the 'formalists'), but who see it as a coherently integrated part of human cognition and behavior (usually called the 'functionalists' — I will adopt these labels here).

Even in the latter group, the 'functionalists', not everyone always pays sufficient attention to the cultural dimension though. Yet, even if the notion of 'culture' is notoriously hard to define, as Everett (pp. 48ff) explicitly admits, it beyond any doubt requires a central position in the analysis of the human social world and of human perception and conceptualization of it (and of the world/reality at large).

And it is good that Everett reminds us, on the basis of convincing facts and revealing anecdotes, that language plays a central role in building, maintaining and transmitting it, and that language as such thus — inevitably — heavily reflects culture. Always having been a (probably quite radical) functionalist myself (see e.g., Nuyts 1992), I have no (substantial) quarrels with Everett's general arguments in this regards.

I do have some quarrels, however, with a more specific assumption Everett is making regarding the functionality of language. That language is a cultural tool is, of course, a very general statement; the question is what the more precise role of language in this is. Here Everett seems to make two assumptions, viz. that language is an instrument for communication, and that it is an instrument for thought (e.g., pp. 19, 50, and passim). How the former relates to the 'cultural toolship' of language is obvious: it is (among others) through linguistic communication that culture is transmitted, hence made. How the latter is part and parcel of the language and culture issue (at least, if I am correct in assuming that Everett does consider there to be a link between these two function characterizations) is less obvious to me, however. One might reason that if — as Everett assumes (see below) — language is constitutive for at least certain types of thought, then differences between languages in precisely these areas imply differences in how we conceive of the world, hence in how we act in the world. And this may then contribute to cultural diversity. But, even if the assumption about language as constitutive of certain aspects of thought would be correct (an assumption I will challenge in this discussion paper, though), is this then a way in which language can be called instrumental for culture? Not in any ordinary sense of the notion 'instrumental', I would think (see below on the definition of the notion of an 'instrument').

But the question how the assumption about language as an instrument for thought and the fact that language is a cultural tool relate is not the issue of my current reflections. My real issue is narrower, viz. it concerns the assumption as such that language is an instrument for thought, just like it is an instrument for communication.

The view that language is an — and in fact, is for mankind by far the most important — instrument for communication is of course one of the most basic insights underlying the entire functionalist movement in language research (see, e.g., Nuyts 1993), and again I have no quarrels with Everett's discussions regarding this matter. But that language is (also) an instrument for thought is far less common as an assumption in functionalist linguistics or in language psychology. Or in any case, it is rarely explicitly formulated there as an element of the functionality of language. But the view is probably somewhat more common in philosophy (see Dascal 1996 for an overview of historical and current positions in this regards; Dascal 2002, 2003:404ff, explicitly endorses the view himself). Paradoxically, it

is also sometimes formulated by formalists, including Chomsky (e.g., 1975; and see also Harman 1975), clearly in an attempt to 'downgrade' the communicative role of language: the reasoning is that if language has several functions simultaneously, then it cannot be determined by any of them in particular; or if language is primarily an instrument for thought, then it cannot be determined by its communicative use. (This claim by formalists is paradoxical, however, given their assumption about the 'autonomous' nature of grammar — if this idea were consistently pulled through, then language should not be an instrument for anything at all.)

I do have quarrels, then, with this specific assumption that language is an instrument for thought. I think it is far from obvious, and in any case far less obvious than the — beyond any reasonable doubt correct — assumption that language is an instrument for communication.

Let me stress that when I am talking about 'language' I am referring to 'human natural language', as 'implemented' in specific languages such as English, Dutch, or Pirahã. I am not talking about 'language' defined in a broad sense, as any kind of symbolic system. Or in other words, I am not addressing the question whether thought 'employs' or 'happens in' a propositional mode or not (although I will touch upon the issue in the course of my considerations). When we talk about language as *the* communicative instrument of mankind, we are talking about human natural language — and so we should do the same if we mention in the same breath that language is also an instrument for thought.

Let me also stress that I am not contesting the assumption — also occasionally formulated by Everett (e.g., passim in Chapter 11) — that language is a cognitive tool. Maybe Everett sometimes means this as an equivalent to the formulation in terms of language and thought. (Also many of Dascal's 2002 illustrations appear to concern cognition in a broader sense.) But 'thought' and 'cognition' are not equivalent — the former is a proper subpart of the latter. Saying that 'language is a cognitive tool' is making a statement of the same level of generality as saying that 'language is a cultural tool' (although the meaning of 'tool' may be somewhat different in both statements: human cognition is, but culture is not, an 'intentional actor' using language). And it is beyond doubt a correct one. In fact, even as an instrument for communication, language is a cognitive tool, for it is the human cognitive system which uses language as its major means to communicate with (i.e., transmit information to and acquire information from) the outer social world. (This is precisely the core assumption underlying the cognitive-functional perspective on language — see, e.g., Nuyts 1992, 2001a, 2004, forthcoming) What my quarrels are about, then, is the narrower assumption that language is an instrument for thought, in the sense of it serving as an instrument in the way we conceptualize — i.e., organize and process our knowledge about — the world (in the widest sense of the latter notion).

2.   What does it mean for language to be an instrument for thought vs. communication?

As indicated, when talking about the functions of language Everett repeatedly mentions its instrumentality for communication and for thought in one breath — and he then often even mentions thought first, e.g. on p. 19: "language is in the first instance a tool for thinking and communicating". This suggests that he considers both functional dimensions of language to be more or less equally important/central, and maybe even with thought having a slight precedence. Yet, upon further reading Everett's view in this regards actually turns out to be more nuanced. At several points he stresses that the primary function of language is communication, without mentioning thought (e.g., very often in parts one and two of the book, or again on p. 301). In fact, Everett spends many more pages/chapters discussing the communicative function, the thought function only becomes prominent in the last few chapters of the book. And on p. 117 he writes:

> Language is for thinking *and* language is for talking. But these functions are only narrowly separated from a practical standpoint. It seems likely that as early humans began to try to communicate with each other they gradually developed a tool for this. And an important side effect of this emerging language tool was that it was useful for organizing and ordering thoughts. The mental housecleaning and thought organization forced by the need to organize words and concepts into different orders, one at a time, for different purposes, would have improved communication and thought simultaneously. More ordered thoughts and communication would have been facilitated in a beneficent cycle of evolutionary improvement (p. 117).

Although the message contained in this quote is 'mixed' (see below), it suggests that Everett sees a difference, at least in how language became a tool for communication vs. for thought.

In fact, while Everett is very consistent all through about the absolutely central role of language for communication, one may seem to experience some fluctuations in how central he considers language for thought. At some points he states that it is absolutely central, e.g. when he writes on pp. 49–50:

> [I]t is immediately obvious that the most important thinking tool at our disposal, besides our brain, is not a calculator, a book, or a computer, but language. Thinking is possible without language. My dog does it. But non-linguistic thinking does not get us very far. Without language most concepts would be ineffable and unthinkable. No math. No technology. No poetry. Minimal transmission of thoughts from one mind to another. And without language it would be impossible to sequence our thoughts well, to review them in our minds, to engage in contemplation (pp. 49–50).

But at other points he seems to relativize the instrumentality of language for thought considerably, e.g., on p. 158:

> [W]e need to think in certain ways before we can have language. Language does not enable all thought. In fact, it is partially parasitic on thought. Even though language contributes to human thought, thinking nevertheless goes on in non-human heads also, just as it went on in pre-human hominid heads. In fact, if thought were not possible in some way without language, we could never have achieved language in the first place (p. 158).

He does so especially also in the first part of Chapter 10, where he argues, against a radical Whorfian position, that "thought is independent of language in important ways" (p. 259).

There are good reasons, indeed, to assume that language cannot be considered an instrument for communication and for thought in the same way, or in the same sense of the notion of instrument. Let us reflect a bit on the question what may be considered an 'instrument', as such.

The prototypical instrument is an entity which is more or less deliberately (in some sense of this word) 'invented' and 'created' by its user in view of the specific goal it is meant to serve, and which is thus 'shaped' as much as possible by/for the needs/requirements of this goal. (Instrumentality is never absolute, of course, it is always confined by the inherent properties of the 'materials' out of which the instrument is made, which put limits on how far the 'shaping' in view of the goal can go. That is why instruments can be relatively more or less efficient for their purpose.) And pursuing the goal then critically happens *in/by* applying the instrument. A knife is for cutting things, and the cutting of things happens *in* using the knife — the instrument is, in other words, critical for performing the activity for which it is meant. (There may of course be several different instruments for roughly the same purpose, but as a result of their different inherent features they will typically not do precisely the same things, and/or they will not do things at the same level of efficiency.)

Language is an instrument for communication in precisely this sense. As Everett also argues elaborately (and see, e.g., Tomasello 2008 for a very sophisticated and convincing perspective on this), there is every reason to believe it has been 'invented' and 'created' for this purpose (even if this has no doubt been a gradual process with many intermediary steps). And as is massively demonstrated in the functionalist literature, it is shaped and molded, in many ways, to fit the requirements of communication. And this goes for the entire linguistic system, in all its facets, from the levels of linguistic semantic and discourse representation to the level of the phonetic realization. And communication happens *in* using language — and (at least for the large majority of humans who do have language) there is

hardly any communication without it. (Even if other systems are used, such as gesture, this typically happens simultaneously with language use, whereby gesture adds extra information on top of the verbally communicated information — i.e., gesture supports the verbal message, and not vice versa.)

But is language in this same sense an 'instrument' for thought? Everett himself suggests, in the quote from p. 117 above, that language is not created for this purpose — and I entirely agree with him on this, for I would not know what kind of evidence there would be indicating that it has been (and see below). (Of course, language has been created to reflect thought — otherwise it could not be used to communicate thoughts/conceptual knowledge. But that is very different from saying that it has been created for the purpose of thought.) So at best, language is an instrument for thought in a weaker or less prototypical sense of the notion 'instrument'. In fact, occasionally one also uses things for specific purposes which have not been invented and created for the purpose as such, but which due to (from the perspective of the purpose at stake) accidental features are more or less useful to help achieve it (e.g. when a burglar picks up a stone to smash a window). It would be better to speak of an 'aid' rather than of an instrument in such cases. This is what Everett in the above quote from p. 117 assumes to be applicable for language and thought: although language was invented and designed for communication, it came in handy for the purpose of thought, too.

But does it? Everett states (still in the above quote from p. 117) that the functions for thought and for talking "are only narrowly separated from a practical standpoint" — but are they? Thinking and communicating would appear to be two entirely different types of activities, which thus also impose quite different kinds of requirements on their 'instruments'. (In this regards I think the formalists are basically correct when they reason — see above — that if language were to serve both a thought function and a communicative function, this would mean that language cannot be shaped for the purpose of any of them in particular. Their mistake lies with the 'if's.)

In fact, as suggested above, while one can easily see that/how all dimensions of the linguistic system, from the discourse organizational to the phonetic, do serve the purpose of communication, it is hard to see how this would also be true for the purpose of thought. Most clearly, features of phonological and phonetic organization and processing — while they are crucial to form transmittable messages, hence crucial to make communication possible — would hardly appear of any use for thinking.

And is the organization of words and concepts in language useful to help the organization of thoughts, as Everett suggests (in the same quote from p. 117)? This is far from obvious either. The organization of words in language — syntax, say — is a matter of linearizing bits of information (as rendered in the words) in view

of its transmission via a one-dimensional channel (articulation, auditory perception). And the linear organization is heavily determined by the communicative goal, i.e. the ordering of words is guided by elements to do with an efficient transmission of information to an interlocutor. This involves features such as (next to many others) taking into account what the interlocutor may be supposed to know already and what is unknown to him/her (information structure; this includes, e.g., the tendency to first offer information known to the hearer and to bring in information new to him/her later in the utterance, so as to allow him/her to anchor the latter into the former), or reducing processing efforts in order to warrant a smooth communication process (this includes, e.g., the principle that 'light' constituents tend to be ordered early and heavy constituents late in an utterance, in order to avoid too heavy burdening of working memory). In other words, syntax is to a large extent the effect of contextualizing the conceptual information to be transmitted in view of the properties of the local communicative situation (see below for a concrete example). It is hard to see how all such elements — linearization in view of a one-dimensional transmission channel, contextualization in terms of the local communicative context — would be of any use for thought. Quite on the contrary, key elements for conceptualization and thought would seem to be to achieve a maximal interlinking of information (i.e., a maximal 'networking', so to speak), and a decontextualization of and generalization over information (i.e. stripping it off conceptually irrelevant 'local' features), in order to make it useful/ valid across local contexts. This is precisely the opposite from what syntax does.

There can, in any case, be little doubt — and Everett (cf. also the above quotes) also accepts — that at least some thought does not happen in language (I will not repeat the numerous arguments to that effect here, for an overview see e.g. Nuyts 1990; and see also Pederson and Nuyts 1997). And this no doubt goes much further than what dogs do (see the quote from pp. 49–50 above), and I see no reasons to believe that "non-linguistic thinking does not get us very far" or that "[w]ithout language most concepts would be ineffable and unthinkable" (ibidem). For just one straight piece of evidence: mute people are excellent demonstrations of the contrary. In fact, Everett himself argues that color (pp. 256ff), and even kinship (pp. 243ff), are concepts also independently of language — but with these (and especially the latter) we are really not into 'marginal' or 'dog-level' elements of conceptualization and thought anymore (and see also below for further illustrations to this effect).

So, also in these regards, even if language does 'serve' thought, then it is certainly not the entire linguistic system which does, and then it no doubt does so much less intensely/massively than it serves communication. This again underscores the fact that language can at best be called an 'aid' for thought, and not an instrument for it. But the question remains whether it is even an aid.

3.   But does language really ever aid thought?

Even as an 'aid' (rather than an instrument), it must be the case that at least some thinking happens in language. Everett does believe this is the case, and he mentions mathematical thinking as a prime example.

> *Because math is different.* Because we need to learn numbers and counting together, neither is independent of the other. This is not, however, the case for the cognition of kinship and color. (p. 261). If a society wants math, it will need number words (p. 267).

Does it? Sure, math is different from color and kinship in the sense that it is a much more abstract domain (though it is not exclusively abstract: it depends on generalization over singular concrete entities). And sure, to do math one needs numbers — but I do not see how numbers are words. One needs number words to talk/communicate about numbers, but to count, as such, one only needs the numbers, and there seems nothing linguistic about these (they are 'propositional', but that is not the same — see above). Mathematicians can write up complete arithmetic formulas without using a single word (and they will need many words to explain them 'linguistically' to an audience, even of experts, and they will run a high risk that in doing so they will miss or mis/underrepresent certain aspects).

If Everett's assumption about why math is different — and is an actual example of a domain in which thought happens in language — has to do with the abstractness of the domain, then let me offer another example of an abstract domain, for which I have explicitly argued elsewhere (Nuyts 2001a, 2009) that, as part of thought, it cannot be 'linguistic', viz. the domain of epistemic modality. This domain is actually probably even more abstract than math, since — unlike in counting, which is ultimately based on singular concrete entities — there is no physical basis at all, in the sense that the epistemically modalized state of affairs is purely hypothetical.

The argument for why epistemically modalizing a state of affairs as part of thought cannot be something which happens in language is actually purely linguistic in nature. And it at once offers an illustration of why linguistic organization and thought organization are definitely not the same things (see above). (The discussion below is formulated from a language production perspective, but there is no 'theoretical significance' to this.)

The argument essentially starts from the observation that in many languages the semantic domain of epistemic modality is expressed by a considerable range of alternative expressive devices, from different parts of speech — i.e., by what I call a 'semantic paradigm'. In English this includes, among others, the alternative form types in (1) — and so in principle one can express the same epistemic judgment

over a state of affairs by means of forms from each of these categories, as shown in (2).

(1)   *Adverbs*:   maybe, probably, certainly, surely, …
      Adjectives: (it is) possible, probable, certain, (I am) sure, …
      Full verbs: think, believe, doubt, …
      Auxiliaries: may, might, could, will, …

(2)   a.   *Probably* John went to the bakery
      b.   It is *probable* that John went to the bakery
      c.   I *think* John went to the bakery
      d.   John *will* have gone to the bakery

Of course, the alternatives in (2) are not entirely equivalent, i.e. they are not synonyms in a literal sense. As I have tried to demonstrate in detail in Nuyts (2001a), on the basis of corpus analysis and by means of experiments, focusing on Dutch, German and English, these form types differ in terms of a number of usage properties. The most important functional dimensions differentiating between the alternatives in the languages just mentioned are shown in the flow chart in Figure 1.[2] (Only the adverbs and auxiliaries show no difference in terms of these dimensions — in fact *John may be home* and *maybe John is home* do come close to being synonyms. See Nuyts 2001a: 261ff on why these alternatives exist anyway in a language such as English.)

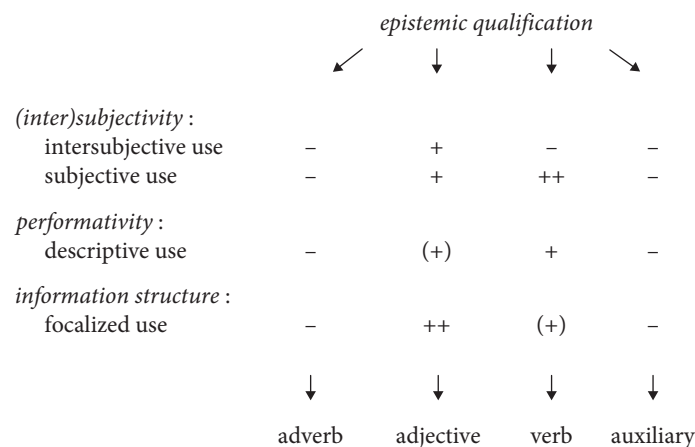| | *epistemic qualification* | | | |
	↙	↓	↓	↘
(inter)subjectivity :				
intersubjective use	–	+	–	–
subjective use	–	+	++	–
performativity :				
descriptive use	–	(+)	+	–
information structure :				
focalized use	–	++	(+)	–
	↓	↓	↓	↓
	adverb	adjective	verb	auxiliary

Figure 1. Functional properties of epistemic expressions

Here is a very brief characterization of the factors mentioned:

(a) The interactive status of the epistemic evaluation in terms of its subjectivity or intersubjectivity (cf. also Nuyts 2001b, 2012). This concerns the question whether

the evaluation is based on evidence only known or accessible to the speaker, which is therefore strictly personal, i.e., subjective; or, alternatively, whether the evidence for the evaluation is more generally known, such that the evaluation is shared by a wider group of people, possibly including the hearer, i.e., intersubjective. Consider (3).

(3)   a.   I *believe* he went to the bakery   [subjective]
      b.   It is quite *likely* that he went to the bakery   [intersubjective]
      c.   He *probably* went to the bakery   [neutral]

An epistemically used mental state predicate as in (3a) turns out in corpus data to be used quite systematically to convey a strictly personal, subjective epistemic evaluation. But an epistemic predicative adjective, at least when it is used in the pattern in (3b), turns out to systematically convey an intersubjective evaluation (in a grammatical context of the type *I am sure that*, however, it conveys subjectivity). And an adverb as in (3c) behaves in a completely neutral way, i.e., it conveys neither subjectivity nor objectivity.

(b) The question whether the speaker is committed to the epistemic evaluation, i.e. the matter of performativity versus descriptivity. In the performative use the speaker expresses his/her own evaluation of the state of affairs at the moment of speech, so it does involve speaker commitment. In the descriptive use, however, the speaker only renders someone else's, or his/her own but former evaluation of the state of affairs, so it does not involve speaker commitment to the evaluation at the time of speaking. Consider (4).

(4)   a.   I *think* Peter went to the bakery   [performative]
      b.   Peter *probably* went to the bakery   [performative]
      c.   John *thinks* Peter went to the bakery   [descriptive]

The mental state predicate in (4a) or the adverb in (4b) are used performatively, in these the speaker expresses an epistemic evaluation to which (s)he is fully committed. But the mental state predicate in (4c) is used descriptively, by means of it the speaker does not express an evaluation which (s)he holds him/herself, but renders someone else's opinion. Performative uses are the default for all expression types, but as shown in the figure, only some expression types allow descriptive uses.

(c) Information structure, specifically the possibility to focalize the epistemic evaluation (cf. also Nuyts and Vonk 1999). In (5), e.g., the predicative adjective is the major focus of speaker B's utterance.

(5)   A: Do you think he went to the bakery?
      B: Well, I don't only think so, I am even *sure* of it.

In general, epistemic evaluations only rarely get focalized, but if they do, then one can only use certain expression types, not others, as indicated in Figure 1.

So this is how the semantic paradigm of epistemic expressions in a language such as English works. Now, obviously, if abstract domains of thought were to rely on language in order for them to happen, the above observations would imply that speakers of English (so to speak) 'think about epistemic modality' in (at least) four different ways. This sounds quite incredible, however. As the flow chart in Figure 1 also indicates, there is just one process of 'assessing the likelihood of a state of affairs', but we can talk about the outcome of this thought process in a few different ways so as to adjust the information about it to the specific communicative context.[3] And these differentiating elements as such are completely irrelevant for the mental process of assessing the epistemic status of the state of affairs as such (this illustrates the above discussion regarding the contextualized vs. decontextualized nature of language vs. thought).

Here, then, is a little syllogism to 'prove' that we do not think about probabilities in terms of language.

– Premise 1: As indicated already, since the different alternative epistemic expression types all involve the same basic notion of epistemic modality, they must all relate, at the actual 'site' or 'locus' of the speaker's epistemic evaluation of a state of affairs in conceptualization or thought, to one and the same central concept. Claiming otherwise would mean giving up meaning as a coherent cognitive category. When producing an epistemic expression, then, a speaker 'works his way' from this one central concept to an expression form via procedures which are sensitive to the different functional factors listed in Figure 1.

– Premise 2: All major epistemic expression types are lexically/grammatically basic, and must therefore all be considered basic in terms of linguistic representation (in 'grammar') and processing — i.e., they cannot be productively derived from each other in the course of language production. For instance, imagine that one would assume that one of the alternative forms, e.g., the adverbs, is the 'basic form' representing the central concept of epistemic modality, and that all other expression types are derived from it. Producing an utterance with an epistemic auxiliary or mental state predicate would then obviously require very radical transformational operations, including lexical exchanges — nobody in present day linguistics and language psychology would consider such operations to be psychologically plausible hence theoretically acceptable.

– Consequence: If both these premises hold, then one cannot avoid the conclusion that the central level in conceptualization/thought at which the epistemic

qualification is conceived (see the first premise) cannot have/use the format of any of the linguistic alternatives in Figure 1. That is, this conceptual level must be more abstract than lexical semantic, let alone syntactic representation. Or, in other words, it must have a non-linguistic format.

The above argument concerns epistemic modality, but precisely the same point could have been made for most or all other 'qualificational' or 'tense-aspect-modality' categories in languages, since they, too, have semantic paradigms of alternative form types (not necessarily the same ones as in the epistemic paradigm though) for their expression, which are subject to comparable analyses in terms of them offering different contextualizations for the same conceptual notion. (This not only applies to other modal or modality related categories, such as deontic modality or inferential evidentiality, which are subject to basically the same differentiating factors as mentioned in Figure 1 — cf. Nuyts, forthcoming — but also to categories such as time and space, e.g., although here the functional dimensions differentiating between the alternative expressive devices will certainly differ in some ways, correlated with the different nature of these qualificational categories.)

All of this goes to show, then, not only that very important domains of conceptualization and thought — such as the entire range of qualificational dimensions operating over our knowledge of and assumptions about states of affairs in the world — do not 'happen' in language (and these are definitely matters which go far beyond what happens in the heads of our dogs and cats — see above). It also shows that even very abstract domains such as reasoning about the reality status of states of affairs do not happen in language. And all of this then makes one wonder whether there is any type/domain of thought at all which happens in language. Until a convincing case shows up, I, for one, am inclined to believe that this is not so, and that language is not even an 'aid' for thought. And, to repeat, the major reason for my doubt is that the essentially communicative nature of language bestows it with features which are crucial for facilitating the transmission of information, but which have no use for thought.

4.   Linguistic and non-linguistic thought as alternatives?

I should also stress, though, that nothing in the above denies, or is aimed to deny, that language influences conceptualization and thought in some ways. When a cognitive domain is in contact with another, it is bound to affect the latter. And the more intense the contacts, the stronger the effects. We think all the time, and we communicate a lot about those thoughts, and so thinking and language use very often go hand in hand (in the sense of them happening more or less

simultaneously and in interaction). And so one may expect strong influences of language on thought in this interaction. (The opposite influence is obviously there, since language is explicitly made to communicate about, hence to reflect, thought.) Whence 'neo-Whorfian' findings of the kind mentioned by Everett (pp. 264ff), such as Dan Slobin's (e.g., 1996) regarding the effects of the way our language codes (among others) manner of action on our conception of action. But 'having (strong) effects' is quite different from serving as an aid, let alone being an instrument. (Cf. also Talmy's 2000: 213ff broader distinction between verb-framed and satellite-framed languages and how he analyzes this difference in terms of how we conceptualize events — Talmy would certainly resist being called a 'Whorfian', and probably even a 'neo-Whorfian', though — cf., e.g., Talmy 2000: 409ff.)

But then again, Everett seems to interpret such findings as meaning that we can think about events either in the linguistic way or in a non-linguistic way — i.e., we have both options available. And the linguistic way is in Everett's view the more efficient, hence the 'default' way:

> [Wi]th effort and with time to think, we can display cognitive independence from language. French speakers can do as well as English speakers in translation, reading, and so on about manner of action, if they are given more time and asked to think about it. Although the language influences them, it does not by any means exert a tyrannical control over their thoughts" (p. 265).

But "[l]anguage affects our thinking in carrying out tasks quickly and in subtle ways" (p. 264), i.e., presumably, more quickly and subtly than non-linguistic thinking.

This looks like a yet weaker version of 'instrumentality' in the language-thought relationship: one in which language is sometimes/often yet not always/necessarily used to do the thinking in a specific domain. But again, I am not sure what actually supports Everett's analysis in these terms. The case of verb-framed vs. satellite-framed languages, at least, would not seem to be good evidence — quite on the contrary. As Everett also suggests in the above quote from p. 265 (and see also Talmy 2000: 287), the integrated way of conceiving about motion and its manner is probably the more 'efficient' one — so, in French speakers, who speak about motion in the satellite-framed way, why would thinking the linguistic way in terms of manner of motion be faster and subtler than thinking non-linguistically, hence, possibly, in the integrated way, about motion and its features?

But even more radically (and also applicable for speakers of English, or any language, no matter whether verb-framed or satellite-framed), why would one ever convert to language to do the thinking in this regards? It seems quite obvious that, in principle, thinking about three-dimensional space and movement through it is done much more effectively in non-linguistic and even in non-propositional

terms (e.g. in terms of something like Jackendoff's 1987 3D-structures). And intuitively, that is also exactly what I experience myself to be doing in 'real life' (and I am inclined not to consider myself an exceptional case). When I think about how to get to a nearby shop quickly in order to buy something to eat for my friends who have unexpectedly dropped in to spend me a little visit, why should I ever start framing/doing this in terms of (from the perspective of the required movements as such) strongly impoverished linguistic structures of the kind [motion verb + manner adjunct], or [motion+manner verb], rather than in terms of possibly quite sophisticated, even if schematic, 3D-motion representations (the details of which are simply not renderable linguistically)? (Quite in general, language is very 'clumsy' and 'rough' when it comes to rendering properties of many perceptual objects, from all perceptual domains — just try and express linguistically how garlic, jalapeños, or cilantro, taste. This is not a problem when language is only meant to serve as a communicative signal which triggers the relevant percepts/concepts in the interlocutor's mind. But it would be a gigantic problem if language were to be the instrument in which thought happens — just imagine what the salsa you would prepare for your visiting friends would/might taste like.)

Likewise, I do not believe that the findings by Steve Levinson (2003) and his team of cognitive anthropologists at the Max-Planck Institute for Psycholinguistics (see also Pederson et al. 1998) offer evidence for Everett's assumption (cf. p. 265) either. I share Everett's view that Levinson's conclusion that there are radically different ways of orienting oneself in space — essentially, using one's body vs. using dimensions of external geography (e.g., major landmarks in the landscape, or the poles) as an anchor for it — stands beyond any doubt, as it stands beyond any doubt to me that this is a difference in how people think (and not only talk) about this matter. (Although I would be careful to make any claims about whether the geographic way is per se more efficient than the anthropocentric one. Probably, a lot depends on the kind of 'space' one lives in: a terrain with very distinctive and prominent geographic features, such as mountains, a lot of water, etc., and/or with a high chance of clear skies with the sun visible most of the time vs. flat plains with hardly any landmarks standing out and with overcast skies 75% of the time. But this is immaterial for the present discussion.) But none of this shows that we revert to language to do the thinking here. All of this is perfectly compatible with the view that language just reflects how we conceptualize position in space, and that the latter is a matter of how we do these things in our 3D representation of the world around us.

Of course, the latter view crucially implies that our (non-linguistic) conceptualization is not universal, but variable. But that, too, is something which in my view stands beyond reasonable doubt (pace very common assumptions to the contrary in many areas of cognitive science). No doubt, some elementary basic

principles are universal in conceptualization — e.g., the fact as such that we orient ourselves in space, which is something we just cannot avoid doing, wherever we live and whatever the features of the environment there. But otherwise the (physical and cultural/social) world around people at different places on earth — e.g., the Pirahã hunter in the Amazon jungle vs. the Bengali rickshaw puller in the slums of Kolkata — is so fundamentally different, that this cannot but result in major differences in their conceptualization of that world, even regarding seemingly fundamental issues such as how they/we actually conceive of orientation in space.

5.   Conclusion

Nothing in the above discussion decisively proves that language — in the sense of 'human natural language' — is never used for the purpose of thinking — in the sense of conceptualizing and reasoning about the 'world'. But at least, I hope that my argumentation makes it sufficiently plausible why one might doubt that it ever is. And in any case, I hope the discussion demonstrates that, in terms of the functionality of language, one simply cannot put communication and thought on a par.

Notes

1. In this discussion article, whenever I refer to Everett's (2012) book I will only mention the page numbers.

2. In the figure, '−' means that the dimension is absent in the expression type, '(+)' means it is marginally present, '+' means it is present, '++' means that it is the major factor determining the speaker's use of this expression type.

3. Only the performativity vs. descriptivity issue is not a matter of adjusting to the local conditions. But that is immaterial for the present argument, see Nuyts (forthcoming) for discussion.

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Cognition, communication, and readiness for language

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This review article discusses some problems and needs for clarification that are connected with the use of the concepts culture, language, tool, and communication in Daniel Everett's recently published book, *Language: The Cultural Tool*. It also discusses whether the idea of biological readiness and preparedness for language (rather than grammar) can really be disposed of as a result of Everett's very convincing arguments against a specific genetic predisposition for the syntactic component of a grammar. Finally, it calls into question whether Everett really is true to his professed ideology of scientific ideographical pragmatism.

Key words: Cognition, communication, comparing languages and cultures, evolution, readiness for language, tool metaphor

1. Introduction

Perhaps the main claim of Dan Everett's multifaceted book is that we have no specific genetic predisposition for language or perhaps more precisely for the syntactic component of a grammar. There is no language organ given by genetics, in a uniform manner, to all members of the human species. This is not a new position. It was fairly common before the writings of Noam Chomsky and has all along remained a minority position among linguists and cognitive scientists. However, Everett pulls together many sources and types of evidence for this position in a way that is very convincing.

Since I have shared Everett's view on this issue for a number of years, I will not discuss his main claim, which I agree with. I will, however, consider some issues that are related and point to a need for specifications or modifications of the claim. The second main claim of the book is that language rather than being a genetically given organ of the mind is a cultural tool. It is the meaning and possible consequences of this claim that I will mainly be discussing in my review.

2. Language, culture, and tool

The title of Everett's book, *Language: The Cultural Tool*, presupposes that we are already acquainted with the fact (or claim) that languages are cultural tools. Are we? Perhaps we are not. Since all three concepts, language, culture and tool, are complex concepts, the claim might be true in some sense and less true in another. Here, definitions of the three concepts would have been helpful. Everett tells us how other researchers have defined "language" and "culture" but not really how he would define the notions himself. The notion of "tool" is left undefined. Starting with culture, I will discuss all three notions and point to some problems and needs for clarification. I will also discuss some other concepts that play an important role in Everett's analysis, namely "communication" and "cognition".

2.1 Culture

2.1.1 *View of Culture*

Everett quotes two definitions of culture provided by Edward Tylor and Clifford Geertz:

> Culture, or civilization, taken in its broad ethnographic sense, is that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society. Edward Tylor (quoted on pages 48 and 202).

> [A]n historically transmitted pattern of meanings, embodied symbols, a system of inherited conceptions expressed in symbolic forms by means of which men communicate, perpetrate and develop their knowledge about and attitudes toward life. Clifford Geertz (quoted on page 202).

Everett does not fully agree with either of these definitions, since they according to him put too little emphasis on values and they don't allow for dynamics and evolution. In several places, he provides his own view of culture but does not really give a definition; e.g., he writes (p. 48), "A working definition is that living culturally is sharing a similar set of ideas and values" (p. 169), "Culture is a set of values that originate in cognitive links produced by the interactive relationship between members of a society" (p. 169), or "Culture is about values and meaning" (p. 299).

A definition of culture is obviously important to the claim that language is a cultural tool. For example, if language is a cultural tool, does that mean it is not a natural tool? This depends on how you conceptualize the relation between culture and Nature. Both of the definitions of culture discussed by Everett are fairly narrow definitions of culture, at least if we look at the list of definitions of culture compiled by Alfred Kroeber and Claude Kluckhohn in 1952. Both of them, as well

as Everett's own remarks on culture, take their basis in culture as a collective mental phenomenon with some allowance for patterns of behavior and action.

Whatever definition is chosen, it would be good if it would make it relatively easy to relate to word structure and other structural phenomena in language, since this is part of what one expects if language is a cultural tool.

None of the descriptions (a definition is also a description) mentions the aspects of communal living, such as artifacts and other traces in Nature that are important in a broader definition of culture. In this broader sense, agriculture could be said to be the prime example of culture. Culture in this sense is always "cultivated nature" and if we apply this to language as a cultural phenomenon, then language is also "cultivated nature". If we use the tool metaphor, language will be a tool which like all other tools and artifacts has both natural and cultural aspects. Thus, it is both a natural and a cultural tool.

Convention is a concept which is closely related to culture. In fact, conventions are a presupposition both of cultures and of languages. It is therefore desirable to characterize them in some way. Everett does this in terms of "agreements" and "contracts" (p. 206). However, both "agreements" and "contracts" would lead to a much more limited notion of convention than is required. There are very few linguistic and cultural phenomena for which we really have agreements and contracts. To be adequate, Everett must therefore dilute the two notions and, after a long discussion of linguistic and cultural examples, he leaves the reader without really having defined or explained in what sense we are to take "convention" and in what sense we are to relate it to culture.

2.1.2   *Culture, language, and meaning*

Let us now consider some of the claims that Everett makes about the relation of language to culture. "There can be no culture without language" (p. 187). Is this really true? Can animals not have culture, i.e. non-nature given shared habits (bird dialects) without having language? Probably they cannot have culture without communication, but if we assume that communication always involves languages, this would lead to a very diluted notion of language.

On p. 240, in spite of the claim just cited, Everett provides a list of cultural "values" that children can acquire without language, i.e. "when to sit still, when to be quiet, how to express affection, what kind of clothes to wear, the length and intonations of verbal interactions, how to hold and manipulate your body, interpersonal etiquette, attitudes towards death, trustworthiness, barter and exchange, kinship systems, two-dimensional and three-dimensional objects. Since the claim that children can acquire all of these types of ability, which all seem culture dependent, without language, is a bit surprising, it is a bit disappointing that no evidence is given for the claim. If Everett were right about his claim that language is not required,

should it not lead to a modification of the claims concerning the power of language as a cultural tool and the claim that there can be no culture without language?

In fact, sometimes Everett also separates language from "the other side", when on p. 239 he writes that not all language processes are acquired culturally. This is also somewhat surprising, since we are then not really told which language processes he has in mind, nor how they are acquired.

Everett also claims (p. 169) that "culture brings meaning from the world". It is not clear what this statement means, but perhaps it means that there can be no meaning without culture. Does this have the consequence that if animals have no culture, they have no meaning? But don't the indexical and iconic signs (cf. Short 2007 and page 5 below), that probably are used by animals provide meaning? If they do, which seems likely, should we for this reason presume that animals also have culture or should we conclude that meaning can be produced in Nature without culture?

2.2 Language

2.2.1 *View of language*
Everett cites three definitions of language. According to the Merriam-Webster on-line dictionary (p. 31): language is "a systematic means of communicating ideas or feelings by the use of conventionalized signs, sounds, gestures or marks having understood meanings".

Henry Sweet (p. 31): Language is "the expression of ideas by means of speech –sounds combined into words, words are combined into sentences, this combination answering to that of "ideas into thoughts"".

Bloch and Trager (p. 31–32): "A language is a system of arbitrary vocal symbols by means of which a social groups cooperates.

Chomsky (p. 32): "A formal language is a (usually infinite) set of sequences of symbols (such sequences are "strings") constructed by applying production rules to another sequence of symbols, which initially contains just the start symbol."

Everett finds all three of the definitions unsatisfactory; Sweet's since it is too narrow and does not allow for sign language, Bloch and Trager's because it is too wide and would make baboon grunts a language, Chomsky's because it has no reference to meaning. He likes Merriam-Webster's definition best but, again, gives no definition himself. However, he provides a summarizing formula (p. 35): Language = cognition + culture + communication.

Although it is a little difficult to know what the "+" sign means in terms of relations between cognition, culture and communication, it is worth noting that communication involving cognition and culture can be achieved without symbols, using only icons and indexes, in Peirce's sense (cf. Atkin 2005 and Short 2007). So

1st proofs

here it seems that Everett has a wide notion of language. In other places, however, he gives other evidence of having a view of language that is more narrow and strongly influenced by written language (cf. Linell 2005; The written language bias in linguistics.).

On p. 119, Everett writes "Language contains nothing exactly like an index". However, the words *this*, *that*, *here* and *now* are all indexical in Perice's sense,. In linguistics they are mostly called "deictic", from the Greek δείξις *deixis* "display, demonstration", but often also "indexical" from the latin word indicō ("point out, show"), i.e., they signify by pointing or contiguity (closeness). To say that they have a "physical correlate" leads to a definition of "index" that is probably both too narrow and too wide. The claim that language has no indexes leads to (or possibly presupposes) a fairly narrow view of language, excluding "conventional indexes" and "conventional icons", sometimes also referred to as "symbolic indexes", "indexical symbols" and "symbolic icons" or "iconic symbols", depending on the degree of conventionality of the sign. In fact, on p. 122, Everett gives several examples of both types: long sentences contain complex information (conventional index) and speaking about Pirahã babies in high pitch (conventional icon).

Another example is when, on p. 178, he claims that all human meanings can be communicated by the use of only a small range of consonants, intermixed with three or more vowels. This reflects a non-communicative view of language, where the role of prosody and communicative body movements for emotions, stress, interaction and management is forgotten.

Further, Everett, throughout the book, thinks that words are more basic than morphemes and are the building blocks of language. "Words (rather than morphemes) are forms for concepts, phrases are forms for modified concepts…, sentences are forms for thoughts and groups of sentences, paragraphs or discourses are forms for expressions of multiple thoughts". He also writes: "Words are the starting point of the T-grammar because everything else in a grammar is built on words. Words are kept inside our mental dictionary" (pp. 141–142) and "A primary building block would be the ability of humans to invent words. Without words there is no language" (p. 157), as well as "Once we have meanings and concepts, most of these are turned into signs, principally words. Words are stored in every normal human's long-term memory" (p. 193).

This view leads to problem already with words like *books*. Is this one concept or two concepts? Is there not a difference conceptually between *book* and *books*? For example, that *books* but not *book* denotes either "a set of discrete book entities" or "an action, taking place at the same time as a designated reference time, usually the time of the speech situation".

Everett also has a fairly conventional, non-contextual view of word meaning, in spite of later realizing the importance of contextual background information

(p. 204). Words "have more functions than to merely express definitions" (p. 107). Except for in actual definitions, do they ever have this function?

He also claims that words in language without a writing system have literal meaning. But no attempt is made to clarify this highly obscure notion, which has its origin in the exegesis of sacred texts and not even in this context has a clear sense, not to speak of its status in multimodal linguistic communication, where there are no dictionaries which purport to codify "literal meaning". For example, on page 127, Everett equates denotational and "literal meaning". Does this mean that metaphors have no denotation? Words are also more different in function than perhaps is convenient if they are to be basic building blocks — "Every noun is a generalization about things" (p. 243). This is not true of proper names, which are also nouns. And the claim that "every verb is a generalization about events" is not true of verbs like the copula, auxiliary verbs and modal verbs or static verbs like 'exist' or 'belong' (p. 243).

As we have seen above, Everett in addition espouses the idea that we have a "mental lexicon" (p. 142), but nowhere discusses the relation of this "mental lexicon" to a "mental encyclopedia", a discussion, which if it had been carried out could have added more arguments to the idea that linguistic ability rather than being a separate organ of the mind, involves a particular way of organizing cognition.

2.2.2 *The role of context*

Another consequence of Everett's relatively narrow view of language is that, in spite of sometimes stressing the importance of context, he at other times, seems a little unaware of the influence of the general but sometimes radical role of context in relation to what is left out and explicitly marked and said in languages (pp. 196 ff). In spoken Swedish, 40% of all utterances have no verbs (see below and Allwood (2005)). The word *Che* in Chinese can mean *I eat* or *you eat* or *he eats* etc., depending on context. Context is made use of systematically in most languages but in different ways. Context often allows for more "elliptical" constructions than "armchair reflections" on grammaticality will allow.

So even in English (not only in Pirahã), the verb of saying can disappear (e.g., in stage instructions):

> Batman to Robin: Hello Robin.

The non-contextual perspective also leads to a neglect of the role of one-word utterances and gestures in conversation. As much as 24% of all utterances in dialog are one-word utterances and 40% lack verb (cf. Allwood 2005). A neglect of facts such as these, easily leads to an over emphasis of the role of verbal syntax. So ironically enough, even though Everett wants to downplay the necessity of recursion in

verbal syntax, he has still taken over a view of language and communication that is characterized by what Ragnar Rommetveit and Per Linell have called "the written language bias in linguistics" (cf. Linell 2005).

In line with this fairly traditional non-contextual view of language, on pages 50 and 55, Everett claims that ambiguity and vagueness are defects of language, yet in other places he claims the opposite (pp. 221–224). Here Everett claims that it is useful to be ambiguous and vague, where this earlier has been seen as a "defect of language". Perhaps this is evidence that his view of language is changing and is not static.

2.2.3   *Language, function, and form*

One kind of evidence for a functional, pragmatic view of language is encapsulated in the claim that function influences form. For example, Everett writes, on p. 6: "the function of language shapes its form". Although many examples of such influences can be found, many examples where there is no apparent influence from function can also be found, e.g. why does the interrogative mood (order of words, inflection, particles, intonation) or negation (inflection, particles, lexical items, word order) in different languages have so many different shapes? Presumably, the functions of asking questions and negating are fairly similar across languages. In other cases, there might have been a clear function originally, historically, but this function has become opaque over time, so that today synchronically there is not much evidence of the function any more. Everett, does not discuss counter examples of this sort, but instead seems to hold on to a fairly strong synchronic version of the thesis that function influences form, which, however, is not really examined critically.

Everett gives several examples of how function modifies form (pp. 25–27), but all of them concern duration (length), perhaps not the most convincing of formal properties. For example, he claims that longer expressions have more information (p. 106). This might be correct in many cases, but not always. The number π (≈ 3.14) is very short, but contains fairly complex information. The expression *yes, yes, yes, yes, yes* is five times as long as *yes*, but hardly contains five times more information.

2.3   Comparing languages and cultures

Everett holds that all cultures and languages are of equal ecological value: "But the fact that all languages are equal does not mean that they are all equally complex or equally versatile. What I mean here by 'equal' is the fit of each language to its cultural niche, as a result of standard human intelligence and evolution. Languages fit their cultural niches and take on the properties required of them in

their environments (p. 234)". They might be of different complexity, but they are all best for their particular ecological niche.

Since languages are not directly linked to biology, the differences in complexity that exist between languages are due to cultural rather than biological evolution. However, at the same time, drawing on Richerson and Boyd (2004), Everett claims that culture can influence biology (p. 19), "In fact there is evidence that culture can affect genes, thus enriching the process of natural selection", "the process of cultural evolution has played an active, leading role in the evolution of genes". If the latter is true, it remains entirely open (and possible) whether some of the differences between languages (for example, in complexity) could be linked to genetic differences between the speakers of the languages.

2.4 Tools

2.4.1   *View of tools*
Everett provides no definitions of 'tool' but we may note that the term *tool* belongs to a semantic field, where there are several other terms, like *instrument* and *means*, perhaps, in some circumstances, these terms would be more adequate than tool. How close to tools are really specific features of languages, such as nominal gender, case, definite articles, etc.? Some languages have them and others don't. If these features are serving specific cultural purposes in the languages and cultures that have them, what are these purposes, and are we allowed to conclude that these purposes don't exist in the cultures that don't have them?

2.4.2   *The tool metaphor and comparison of languages*
One of the main claims in Everett's book is that language is a "cultural tool". A problem with a strong reliance on a metaphor is that you do not know how seriously you are allowed to take the metaphor. For example, it is common to evaluate tools and claim that some are better and some are worse for a given purpose. It is also common to develop tools and to throw away the old tools without much regret.

Everett, on the other hand, does not really want to compare languages, since each language is supposed to be unique (pp. 318–319). Everett also thinks that all languages are of equal value, since each language is a tool to solve the "unique" ecological needs that exist in the culture it is associated with. So we presumably should not compare languages A, B, C with regard to, for example, their ability to facilitate talk about color, mathematics, complex kinship, or philosophy of mind, and as a result of our comparison come out with statements claiming that language A is much better than language B regarding talk about color, mathematics, philosophy of mind and kinship relations. No language is better than another if regarded in its unique ecological niche.

Language as a tool is, thus, very unlike other tools, where the normal procedure is to compare and evaluate and on the basis of the evaluation choose the best tool with no regrets about the tools that were left behind. In general, we would probably not want to make a special point of remembering that each saw, hammer or axe is unique to its particular historical situation (even if it is true that they were unique in some sense) and claim that they for this reason cannot be compared to other saws, hammers or axes. Neither would we probably want to say that the loss of a particular type of saw is a loss to humanity. Perhaps languages are not really tools or only tools in some respects. Perhaps other terms like *instruments* or *means* are just as adequate, or even more adequate, to capture the implicit means — ends relationship languages often serve in relation to the various activities in a culture.

Another problem with the claim that each language is unique and equal to all other languages from the point of view of its ecological niche can be brought out by the following analogy. Earlier in my life, my family used to arrange a ski race, usually as part of celebrating Easter eve in the Norwegian mountains. The problem was that really only one person was happy at the end of the race, namely the winner. So we devised a more tolerant and pragmatic solution. Each participant was competing in his/her own exact age group. In this way, all participants could be seen as winner in their own age groups and everyone was happy. A possible problem with the solution was that it had seriously diluted the meaning of the word "race" or "contest". A race or contest where everyone is guaranteed to win is hardly a race or contest in the normal sense of the word.

Similarly, a comparison where every language is declared to be best in its specific ecological niche and therefore equal to all other languages is hardly a comparison and the evaluation is not really the result of a comparison but of an a priori postulate that each language is unique and most well suited to its ecological niche.

Everett also shows his skepticism concerning evaluations based on a comparison of languages through another argument. On p. 286, he claims that languages are equal in expressive power since they can change and incorporate what they need (e.g., linguistic recursion, for example from another language):

> I want to emphasize that finding evidence that recursion or some other grammatical characteristic is missing or present in a given language tells us nothing about the overall complexity or worthiness of that language. Languages incorporate what they need to, according to the demands of their cultural niche. As theniche changes, the language can also change (p. 286).

In order not to make comparison difficult and perhaps a little pointless, this could possibly be better described as all languages are potentially equal, since they can change.

But perhaps we should take the tool metaphor more seriously and consider whether languages can be more or less similar to tools. Are some languages more tool-like than others? An affirmative answer to this question is actually fairly natural. The more invented a language is, the more tool like it seems to be. So it is very natural to think of so called computer languages (Fortran, Algol, Lisp, C, Java) as tools for programming computers. It also seems natural to think of the language of botanical classification or chemical formulae as a tool.

However, as we approach so called natural languages with languages like Esperanto, Volapük, Neo and Ido, some of the tool-likeness becomes weaker. And when we actually consider natural languages, such as Pirahã, Danish or English, the tool-likeness is still weaker. Even though parts of "natural languages" are sometimes explicitly invented by clever authors, translators or language academies like "Svenska Akademien" (the Swedish Academy"), they are in some sense clearly less tool-like than Fortran, Lisp, Neo or Esperanto. Possibly, the sense is that they are not invented to the same extent, but have often evolved organically and naturally in interaction between Nature and culture, sometimes with esthetic goals that are in no obvious way connected with the pragmatic usefulness that usually characterizes tools.

Let us now turn to a concept that has been presupposed in our discussion of all the three concepts discussed above; "culture", "language" and " tool", namely "communication".

3. Communication

3.1 View of communication

Everett accepts the "conduit metaphor" of communication and uses Shannon and Weaver's "transfer" based definitions (pp. 50, 56–58). He seems unaware of the criticism of this notion, to the effect that it makes the recipient too passive and leads to a neglect of the creative interpretation and interactive feedback contributions of an active co-communicator.

The Shannon and Weaver view of communication is a red thread throughout the book (pp. 118, 142, 161, 170, 215, 221, 280) and generally leads to a neglect of the view that face-to-face communication involves a "multimodal bidirectional flow of information" in favor of a telephone inspired perspective, where communicators are transmitters and receivers of symbols. Although Everett has indicated distance to the Chomskyan view of a formal language as a set of strings of symbols, this is still too close for comfort.

The Shannon and Weaver account of communication probably also has the effect of over emphasizing the role of the brain and down playing the role of the

rest of the body in communication. Co-activation and alignment between two or more communicators on low levels of awareness and intentionality are not really part of Everett's (or Shannon and Weaver's) view of communication. Rather, communication is seen as aware and intentional transmission of information from one brain to another (p. 58).

His acceptance of the passivity of the recipient implicit in the "transmission" model of communication also makes him think that the meaning of contributions to dialog are transmitted rather than co-activated and shared, thus adhering to a view of dialog and communication as a sequence of alternate auditory monologs, rather than as interaction, where meanings are co-constructed multimodally by the participants.

Possibly as a consequence of this, Everett underestimates the power of spoken language communication and assigns the possibility of editing and rhetoric to written language, while forgetting the advantages interaction and mutual co-construction give (p. 276).

Everett also underestimates the fact that, even today in western societies, people can live by telling stories (p. 278), e.g., TV sofa professionals and professional lecturers and that repetition does not only take place in spoken language, compare the role repetition often plays in newspaper articles (pp. 278–279).

In line with this, Everett claims that (p. 237) "Phatic communication carries little if any real information". What is "real information"? Is information about social bonding, emotions and attitudes not "real information"? Again, the impression is that Everett, in spite of professing the opposite, has a non-communicative view of language and as a result of this also has a more limited perspective, than is necessary and desirable, on language as a communicative tool.

Another problem is that Everett does not clarify what he sees as the semiotic basis for communication (index, icon or symbol). For instance, he claims that communities develop prior to communication. But is this really possible? It seems more likely that communities developed as a result of indexical and possibly iconic communication between higher primates.

A final remark is that the term "coherence" (p. 60) is perhaps not the only or even the best way of talking about functional-relevant sense in communication. As a matter of fact, it is a little unclear what "coherence" means outside of logic (where it means absence of contradiction) and it is not too difficult to imagine coherent discourse that does not make much sense. So, suppose that A, on a sunny day says to B "If it is sunny, the sun shines. Today it is sunny so the sun shines.". This utterance is coherent and even true, but does not really make much sense in the situation. The reason, among other things, is that the utterance is not really adjusted to factors like relevance to A's and B's motivation and the purpose of A's and B's joint activity, which probably will not be that they are engaged in a lesson on syllogistic reasoning.

4.   Language, speech, and multimodality

It is part of Everett's outlook on language that speech has no privileged status, so on p. 174, there are a number of claims concerning the equality of speech and sign language, i.e., that sign is as quick as speech, that speech and sign don't involve separate neuronal networks and probably also that gestural signs are symbols (words) and can be used for sharing information with the same degree of complexity as speech symbols (words). But no evidence is given. In fact, all of these claims are still somewhat contested and contestable empirical issues, where the final answers are not available yet.

In a similar vein, Everett assumes that speech and language are logically distinct (this is difficult to understand, since speech is a kind of language, like dachshound is a kind of dog). As evidence, he mentions, among other things, whistle language. This is not very convincing, since all whistle languages, as far as I know, build on properties of speech, either tones, as in Everett's examples, or vowel formants, as in the case of the whistle language on the island of Gomera. He then claims that speech and language have influenced each other. This is hardly surprising, since speech is a kind of language.

On p. 125, Everett writes "Language is flexible because it has no particular channel as part of its design." If this were true, then languages based on taste, smell, touch or sense of temperature should be as common as languages based on hearing and vision. But they are not. In fact, of the four senses mentioned, only touch has really been used at all (Braille). There seems to be a clear preference for hearing and vision. And, in fact, there is also a preference for hearing over vision, since for all hearing humans, spoken language is preferred over signed languages, even if some groups of hearing humans have gestural signed languages as an auxiliary language. For deaf people, signed languages are preferred over spoken language, even if, after the introduction of cochlear implants, also many deaf people seem to prefer spoken language.

The fact that gestural language or touch language or some other mode of communication can be used instead of spoken language does not mean that we have no genetic readiness for speech. It only means that our brain is plastic enough to allow us to develop compensatory mechanisms when the linguistic modality we are mainly ready for, i.e. speech and hearing, for some reason is not available.

It might also be true that speech is not the basic or most important mode for all types of communication; touch might be more important for expressions of love and aggression, facial gestures might be more important for expression of emotion and head movements for feedback and coordination etc.

Everett also thinks speaking is unimodal, acoustic. No mention is ever made of the multimodality found in the "McGurk" effect (the fact that our observation

of lip and jaw movements through multimodal integration seems to influence what speech sounds (phonemes) we actually hear, or of how gesture and speech in general are coordinated holistically to form integrated content, cf. McGurk and Mac Donald1976 and Wright and Wareham (2005).

In line with this, Everett claims that we do not mix modalities, "we do not mix hand gestures with consonants and vowels to produce words". Perhaps not, but we do mix auditory perception of consonants and vowels with visual perception of lip and jaw movements in order to hear what is being said (the McGurk effect) and we do mix deictic words like *that*, *this*, *here* and *there*, with pointing gestures to specify reference. We mix a friendly voice with friendly words and a friendly smile. Face-to-face communication is normally multimodal, making use of several modalities simultaneously.

5. Language, communication and cognition

Let us now turn to what Everett calls the "cognitive platform for language". Here Everett describes a number of cognitive features and abilities that he claims are used in language. One of the issues here (p. 163) is the extent to which the "components" of the "cognitive platform of language", i.e., intentionality, background, theory of mind, figure-ground, contingency judgments and consciousness, are mutually exclusive? Isn't figure-ground a special case of the workings of "intentionality"? Isn't "ground" a special case of "background"? Are not stored "contingency judgments" part of the "background"?

A clarification of "intentionality" is needed (ibid.). Do both words and mental acts have intentionality? Or do words have intentionality only by being associated with mental acts? Is Everett trying to make "intentionality", the same as "intensionality"? Does a word like "ouch" have intentionality? If so, what is it about or directed at? Is not consciousness involved in all the components? How do we know that the list is complete and that these are the most crucial components.

Since the components are not really put to use in a theory of communication or language production and language perception/understanding, it is hard to evaluate possible answers to the above questions. For example, is consciousness always involved in communication? Are not subconscious communicative processes possible, both on the production and on the recipient side, in communication? Most theories of communication would allow this, especially regarding indexical and iconic signs.

Linked to the above issues is the question of how unique human cognition is. On p. 166, it seems that Everett is saying that "consciousness", "background", "theory of mind", and "contingency judgments", but not "intentionality", are unique to

humans? They are claimed to be "building blocks" of language" which "came to exist after hominids came on the scene". However, most of these cognitive abilities seem possible to attribute to other animals than humans. For example, why is having a culture necessary for claiming that an animal has consciousness or intentionality or can relate to "background"? Why isn't ability to relate to memory enough? Are not many animals able to do this, when they are able to find their way home, e.g. magpies finding their way to their nest etc.? However, these statements run against other passages where animals are ascribed more cognitive ability.

Everett also claims that "animal communication" in the wild uses no signs (p. 118). Using the Peircean analysis of signs, we can see that this cannot be true for indexes, they use indexes all the time (e.g., ants and bees leaving trails to food). They probably use icons, as in deceptive behavior and it's unclear whether they have conventional behavior, as can be seen in the dialects of bird song that certain birds have. In the first two cases, the source of these signs is Nature rather than "a particular cultural history". Later (p. 121), however, Everett realizes that animals might have signs, but he does not actually tell the reader which alternative he in the end believes.

Another issue regarding cognition is that Everett takes it for granted that we think in concepts (pp. 142, 203). Can this really always be taken for granted? Is the flux of indexical, iconic and symbolic information stored in our memory and appearing in our thoughts really only conceptual?

When it comes to the relation between culture, language and thinking, Everett describes, somewhat inconclusively, in Chapter 10 — "Language, culture and thinking", how the Pirahã, can handle color and make kinship distinctions in relation to choice of marriage partner, without having support for this in their language. He also has an interesting discussion of thinking, recursion and expressive power. For example, he writes that (p. 262) English and Walpiri have different expressive power and he also writes that (p. 294) English and Pirahã are different in expressivity because of recursion. He then, however, notes that recursion in thinking and recursion in language might not have a one-to-one correspondence. The Pirahã might think recursively even if their language does not have syntactic recursion. All of this seems to show that the power of language over thinking is not so strong after all. But after a discussion of three positions with respect to linguistic relativity; "strong", "weak" and "very weak", Everett does not really tell us his own position. Is it "weak" or "very weak"?

6. Evolution

Everett cites convincing evidence to the effect that there is no specific language gene and that language is not modularized and localized in a specific location in

the brain. However, he recognizes that humans seem to have developed greater ability to process, produce and perceive the fine grained differences in sound that are needed for speech. We have also seen that it is difficult to point to any function outside of speech that this ability could have. If we combine this ability with the ability of human beings to associate fine-grained meanings and concepts with the sound differences in a symbolic (i.e. non-indexical and non-iconic) manner and notice that no other species probably has the same ability, it does not seem impossible that this ability is facilitated by genetic predispositions. Parrots can perceive and produce human linguistic sounds, but probably cannot associate these sounds with meanings and concepts. In this connection we note that Everett partly agrees when on pp. 240–241 he says that humans and dogs are different because of biology with respect to language categorization and linguistic support of categorization.

As already mentioned, Everett believes that cultural factors can influence human biological evolution (pp. 41 and 42; "Modern examples of culture changing genes are not hard to find".). If this is correct, if culture can change genes, why cannot language (the need for distinct production and perception of speech, the need for complex modality neutral cognitive information (meaning, concepts)), which after all is a cultural phenomenon, also do this? In fact, sometimes Everett comes close to at least partly claiming this, (p. 171) "our anatomy changed for vocal speech", but then a few lines later he writes "none of the part of the vocal apparatus evolved primarily for language". In order for the latter claim to be credible, we really have to know exactly what the non-communicative, non-linguistic functions were of all the changes of the vocal tract. Again on p. 176, he writes: "our ears and their inner workings have co-evolved with our sound-making system, it is not surprising that we have evolved to make and be sensitive to a relatively narrow set of sounds that are used in speech. Hearing and producing speech sounds are two vital evolutionary adaptations found universally in all healthy humans." One might say that this shows an ambivalent attitude.

The fact that our linguistic abilities are not neatly localized in the areas pointed out by brain researchers like Broca or Wernicke, but rather usually consist in interaction of several brain areas should not surprise us, if our linguistic ability, to a much greater extent than has been earlier assumed, consists in multimodal coordination of planning, production, perception, understanding and thinking. We might claim that it is for this complex coordination of cognitive abilities with a special reliance on speech organs supported by visible communicative body movements that we are genetically predisposed.

A view of this type could be compatible with a claim that we are genetically predisposed for a purpose driven complex coordination of abilities, which would be the basis for our problem solving ability and general intelligence (p. 96), which could then be seen as an "apriori ability", somewhat more complex than the "apriori

knowledge" Everett is arguing against. To some extent perhaps Everett overlooks this possibility because of the broader coverage of the English term "knowledge", which covers not only "true, justified belief" (know that), but also abilities, skills and capabilities (know how). The sketched view would also be compatible with the fact that genetic predispositions in general are easier to imagine in relation to abilities than to "true justified beliefs".

The suggested view would involve a genetic facilitation of linguistic communication and language supported thinking, but it would be a genetic facilitation of a different sort than that which has focused on a language as a verbal, syntax oriented modularized organ of the mind.

In fact, we could take the ambivalent attitude Everett manifests toward linguistic universals as support of the sketched view. As part of the argument against a genetic predisposition for language, he claims that there are no (or very few) universals of language, but in spite of this suggests several possible universal or near universal (p. 194) features (even if neither he nor anyone else has still investigated more than a small percentage of the world's 6 800 languages). Among the claims we find:

i.   All languages have nouns and verbs.
ii.   All languages tell stories.(Is it the language or the speakers who tell the stories?)
iii. All languages have hierarchical relationships.

Everett then claims that the occurrence of the universals follows from the make-up of our vocal apparatus, the basal ganglia and other biological components. Is not this a biological basis and readiness for speech that is not shared by other species?

As a matter of fact, some of the other examples that Everett discusses could also be used to support the view of language as cultivated Nature and suggestion for a genetic readiness sketched above by analyzing a little further some potential similarities (p. 257) between the phrases used for color in Pirahã, and words and phrases which are the etymological origin of color word in other languages (e.g., red = blood color or green = leaf color), in this way throwing light on how similarities between languages can occur as a result of similar interaction between culture and Nature.

Another issue concerning biological facilitation of language concerns how much learning is involved in acquiring a language. Everett argues against Chomsky's "poverty of stimulus" argument by noting that no one has ever shown that languages are incapable of being learned (p. 101), presumably because most humans, if not all, have learned a language, so Chomsky's claim is impossible to test. However, something is missing in this argument. Who is the agent of learning? Is it humans or some other species? If any other species than humans are

tested, presumably they will not be able to fully learn a human language, even if they are capable of learning some small part of it. It therefore seems strange not to recognize the unique human ability to learn languages as a species specific readiness for the complex cognitive-behavioral ability to learn and use a language.

7.   Instincts

By defining "instinct" as an ability that is manifested without a period of learning, it becomes fairly easy to show, for example, that there is no language instinct. In spite of the definition, some instincts, like the "learning instinct", (p. 185) seem to be affected by learning since we gradually learn how to learn.

Rather than assuming a "language instinct", Everett wants to show (p. 218) that "most of human language, its forms and function, result from the interactional or social instinct proposed by Aristotle, in conjunction with various features of the real world". This is a praiseworthy ambition, even if some work remains to be done. However, it is a little unclear why Everett thinks that an "interactional instinct" is better than a "social instinct" or, for that matter, a "communication instinct". It does not seem entirely obvious how one would choose between these alternatives (p. 187). At any rate, it seems reasonable to claim that there is an interactive or communication instinct and it also seems reasonable to claim that even if interaction or communication are instincts, this instinctual ability can be modified by learning, as can most other abilities for which there is an innate basis. Thus, perhaps the definition of instinct should be modified to something like " an ability that is at least partly (rather than fully) manifested without a period of learning".

This view of instincts can also be used to modify the discussion in Chapter 1 of whether language "just grows" or is learned. If language is cultivated Nature, as suggested above, perhaps it can both be learned and naturally develop. Compare the following abilities (some of which might be claimed to be instinctual), which all to varying degrees combine "natural development" with learning:

i.   learn to be breast fed (some babies have to be taught this)
ii.   learn to eat
iii. learn to crawl
iv.   learn to walk
v.   learn to whistle
vi.   learn to sing
vii. learn to talk
viii. learn to swim
ix.   learn to dance

Everywhere there is a genetically given basis, which to a greater or lesser extent develops in interaction with the environment (learning), i.e. more or less cultivation of naturally given abilities.

8. American perspective

Although the book, in general, has a completely acceptable level of scholarship, I will now turn to some remarks on the perspective and accuracy of some of the claims that are made.

As is perhaps natural (or rather cultural), Everett's book sometimes has a fairly American perspective. On page 32, Everett writes of Noam Chomsky; "no individual in history has had greater intellectual impact on the study of language than Chomsky". This is a little surprising since Everett seems to agree with very few, if any, of Chomsky's ideas. Only time will tell, but what about Panini (500 B.C.), Dionysios Thrax (200 B.C.), Ferdinand de Saussure, Otto Jespersen (beginning of the 20th century), Louis Hjelmslev and Ludwig Wittgenstein (mid 20th century). All of these individuals have stood the test of time much longer than Noam Chomsky.

Chomsky is also claimed to have pioneered the attempt to link a scientific theory of human language to the nature of the human mind (p. 66). This is not correct; there have been very many such attempts before Chomsky, all the way from Aristotle: Medieval speculative grammar, Port Royal grammar and several attempts in the 19th century. To say that these previous theories were not scientific is question begging. When it comes to basic parts of speech and syntactic functions, Western linguistics has not changed very much since the Middle Ages.

The claim that "It was in America, not in Europe, that the field of descriptive linguistics was born" (p. 235) is doubtful, given that historical linguistics is descriptive and that synchronic linguistic descriptions, including dictionaries and grammars had been produced by Europeans (often missionaries), since the 1500's.

There is also very scant reference to the historical origins of the hypothesis of "linguistic relativity" in Germany with scholars such as Herder, Humboldt and Boas. Instead, Sapir and Whorf are portrayed as the main heroes (p. 255).

9. Historical accuracy

Sometimes Everett's historical accuracy leaves a few question marks. On p. 117, he claims that Saussure was the first to discuss the "linguistic sign". In fact, words as signs and other signs were discussed both in the Greek (Aristotle, Stoics) and

Indian tradition (e.g., the Sphota theory, cf. Robins 1967:140), in the medieval European logical and grammatical tradition (e.g., terminist logic, Occam's sign theory), and by Charles Sanders Peirce, before Saussure. The ideas of Peirce on "semiotics" and Saussure on "semasiology" were probably without any direct connections to each other, but Peirce preceded Saussure, rather than vice versa (pp. pp. 117–119) and developed a more complex semiotic theory than Saussure, containing among other things the three basic sign types "index", "icon", and "symbol".

On p. 161, Everett claims that Husserl was the first to recognize that all our thoughts are directed at something. Is this correct? What about Brentano and the medieval theory of intentionality?

On p. 228, Everett claims that all languages in Europe, except Finnish, Basque and Hungarian, are Indo-European. However, the list is longer and includes the Sami languages, Estonian, Turkish, Ingermanian and other Finnish languages in Russia and claims are made about linguistic homogeneity in Europe 6000 years ago, for which no evidence is presented (p. 228).

On p. 235 in a footnote, there is hasty bracketing of non-American and non-European linguistics. Panini and other Sanskrit phonologists and grammarians were very advanced. The Arabic tradition of Sibawahi had a better grasp of spoken language than had the European or American tradition.

On p. 236, Everett overemphasizes the role of psychology for European linguistics. Philosophy was more important than psychology and Durkheim's sociology was very important for Saussure.

On p. 325, an incorrect account is given of the development of English as a case of isolated development from German, totally neglecting the influence of Danish and Norwegian Vikings as well as of the Normans, leading to a process of mixing and creolization between 1066 and 1350.

10. Concluding remarks

10.1 Agreements and disagreements — reconciliation?

Although agreeing with Everett's claim concerning the lack of support for the view that there is a specific genetic predisposition for the grammatical aspects of language as an organ of the mind and with his claim that language, in some sense, is a cultural tool, this article has argued for a modification of both the claim concerning a biological predisposition for language and of the sense in which language is a cultural tool.

Concerning a biological predisposition for language, I have argued that there does seem to be a species specific readiness for fine grained vocal sound

differentiation both in production and perception and for an association of complex information of different sorts (affective, epistemic, factual, abstract etc.) with the resulting fine differences in vocal sound, both segmental and supra segmental and that this association makes possible a holistic processing and purposeful use of a combination of indexical, iconic and symbolic information both in thinking and communication.

Secondly, I have argued that the claim that language is a cultural tool should be specified and modified in several ways:

i.   Shannon and Weaver's "telephone transmission" based analysis of communication should be replaced by a face-to-face based analysis, where communication involves a bidirectional, interactive multimodal flow of information.
ii. This should then be combined with a more communicative, interactive and multimodal notion of language, which would allow several new ways of exploring the instrumental functions of language.
iii. The analysis of culture should be made less "mentalistic" and generally seen as "cultivated Nature", incorporating artifacts and other types of traces in Nature, supporting patterns of thinking and behavior.
iv. There should be an analysis of "tools", possibly differentiating tools from other instruments and means; relating "language as a tool" to different types of purposes, goals and functions. Specifically, more analysis is needed of language as an instrument for the different types of social activity that are found in different societies and cultures.

If the suggested changes and modifications are combined, we will have an analysis of "language as a cultural tool" which will not exclude "language as a natural tool" and which will furthermore be compatible with language as a tool of communication and interaction in different social activities.

10.2 Linguistics and science

Finally, I would like to make some comments on the view of science and especially linguistics that Everett presents toward the end of the book. Given his interest in the description and documentation of all the languages and cultures on Earth, it is a little disappointing that Everett does not to greater extent reflect on how this might best be done. Instead he seems to presuppose that this should be done the way it has mostly been done historically, i.e. (p. 306) mainly by producing dictionaries and grammars and providing a writing system. New ideas such as e.g. using digital platforms for aided self — documentation or "crowd sourcing" (cf. Allwood 2006) are not considered.

On pp. 317–318, Everett presents what one is tempted to call an "apologetic philosophy of science" — a tolerance of lack of theorizing, where we are to accept exceptions to theoretical claims as of equal value to theoretical principles. This leads to an "ideographic" (rather than "nomothetic") view of science (pp. 318–319), where individual facts are seen as unique and of equal value to theory. This is then combined with a "pragmatic" ideology, where "truth" is replaced by "usefulness" as a goal of science and several incompatible scientific theories are allowed to coexist in a spirit of political liberalism and tolerance. Unfortunately, truth is not usefulness. There can be useful theories that are not true and there can be true descriptions and perhaps even theories that are not useful. An additional problem here is that there can be many types of usefulness. We can mostly ask: Useful for whom, for what purpose? We will usually get several different (perhaps not even compatible) replies.

Perhaps in the end a theory that is "truly useful" can only be a theory that is true. But if this is so, why not search for truth in the first place?

In fact, Everett, in spite of his ideologically pragmatic, liberal, relativistic philosophy of science, also affirms the importance of truth (p. 320), where he is calling for "accuracy of narrative — telling the best and most complete story about the facts". This sounds like a traditional truth seeking philosophy of science.

Acknowledgement

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Understanding others requires
shared concepts

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"It is a noble task to try to understand others, and to have them understand you
(…) but it is never an easy one", says Everett (p. 327). This paper argues that a ba-
sic prerequisite for understanding others (and also for having them understand
you) is to have some shared concepts on which this understanding can build. If
speakers of different languages didn't share some concepts to begin with then
cross-cultural understanding would not be possible even with the best of will on
all sides.

Everett stresses the great value of each language as a unique perspective on
the world and a "repository of the riches of highly specialized cultural experi-
ences ", and I fully agree with this. But to access those riches hidden in the thou-
sands of the world's languages we need to understand the meanings encoded in
each language (both in its words and its grammar). We could not understand
those meanings if we didn't have a stock of shared concepts (acknowledged even
by Whorf) with which we could build conceptual bridges between other peoples'
conceptual worlds and our own. Unfortunately, Everett seems unable to see
this point and in his eagerness to depict the Pirahã people as radically different
from the rest of the humankind he goes far beyond the linguistic evidence (as
presented in his own publications on the Pirahã language) — as one can clearly
see if this evidence is subjected to careful semantic analysis based on a coherent
methodology (see my commentary on Everett's "Cultural constrains on gram-
mar and cognition in Pirahã" in *Current Anthropology* 46:4, 2005).

For example, Everett claims that Pirahã has no word for "mother", no words
for "before' and "after", no words for "one", "two" and "all" and no words compa-
rable to 'think" and "want". These claims are based, I believe, on faulty semantic
analysis, and in particular, on a determination not to recognize polysemy under
any circumstances. As I see it, at many points this stance makes nonsense of
Everett's own data and distorts the conceptual world of the Pirahã. Since he does
not want to recognize the existence of any shared concepts, Everett is also not
prepared to address the question of a culture-neutral metalanguage in which
Pirahã and English conceptual categories could be compared. This often leads
him to imposing cultural categories of English (such as "evidence", "tolerance"
and "parent") on the conceptual world of the Pirahã. The result is a combination

of exoticism and Anglocentrism which doesn't do justice to Everett's long and intimate engagement with the Piraha people and their language. Sadly, it blinds him to what Franz Boas called "the psychic unity of mankind", reflected in the common semantic features of human languages and fully compatible with the cultural shaping of their lexicons and grammars.

Keywords: Cultural scripts, kinship, natural semantics metalanguage (NSM), numbers, polysemy, psychological concepts, semantic universals, universal human concepts

1. Introduction: What is possible in human languages?

Daniel Everett raises big and important questions about language, and, as people say in Australia, good on him. He wants to know, no less, "what it means to be human" (p. 261) and "what is possible in human languages" (p. 251). He doesn't think he has all the answers (yet), but he seems confident that he is going to find them and that he is on the right way: "I and my fellow researchers are still searching for the answers to some of the biggest questions about language" (p. 50). I wish him well, but as I will illustrate in this paper with specific examples, I believe that there are major problems with his approach, which, briefly speaking, relies on fieldwork divorced from conceptual analysis. For the moment, then, the most urgent question in linguistics and anthropology may be this: How can we best approach those big questions of "what it means to be human" and "what is possible in human languages".

Everett's key tenet, with which I thoroughly agree, is that languages are shaped by culture and that since cultures can be very different from one another so can languages. But can they differ from each other "without limit and in unpredictable ways", as Martin Joos (quoted by Everett) famously claimed half a century ago?

Everett notes that many linguists see Joos' statement as "naïve and unscientific", but, he states, "they are only partially right". This suggests that Everett partially agrees with Joos, although he distances himself from him at the same time. "It certainly does not seem that, although languages vary enormously, they vary 'without limit'", he says (p. 85). But if languages do not vary "without limit" then the question must be asked: what *are* those limits?

Unfortunately, on this crucial point Everett remains silent, emphasizing, throughout his book, the enormous diversity of languages and repeating that this diversity is far more important than any (unspecified) similarities between them.

"Long live diversity", says Everett (p. 302), and I whole-heartedly agree. But we cannot make sense of that diversity unless we hold on to some common measure. We cannot understand conceptual and cultural diversity of languages without

some stable reference point in language universals. "We cannot learn about the human condition by homogenizing it", Everett writes (p. 319). But neither can we learn about it by exoticizing far-away languages and cultures. The only way we *can* learn about it is by recognizing both the diversity and limits of that diversity — and by trying to specify, in detail, what these limits are.

Everett insists that "the most salient characteristic as we survey the languages of the world is not their similarity but their diversity" (p. 79). But diversity and similarity are two sides of the same coin: to "plumb the profound diversity of language", to use Everett's phrase, we need some plumb-line to plumb it with, and this plumb-line must be applicable to them all.

According to NSM research (cf., e.g., Goddard and Wierzbicka (eds) 2002; Goddard (ed.) 2008), such a plumb-line can be found in a set of universal human concepts which show up, in certain canonical combinations, in all languages. These elements include, for example, KNOW, WANT and SAY, DO and HAPPEN, SOMEONE and SOMETHING, and 60 or so others.

Everett speaks of "the absence of linguistic universals of any substance" (p. 88). In doing so, he simply ignores the findings of NSM research, to which my (2005) commentary on his 2005 paper, to which he replied, must have alerted him. This research has brought to light 64 universal semantic primes and a universal mini-grammar associated with them. Everett does not explain why he does not regard these 64 elements, lexicalized, as evidence suggests, in all languages, as linguistic universals of some substance (see Tables 1 and 2). More recently, NSM research has also brought to light a number of "semantic molecules", such as 'man', 'woman', 'child', 'mother' and 'father', which also appear to be present, as words or distinct meanings of words, in all languages (Goddard and Wierzbicka, In press).

As I will illustrate in some detail in Section 5, these empirically established primes and molecules, together with their associated minimal grammar, can serve as a "natural semantic metalanguage" (NSM) for portraying indigenous meanings from a universal, culture-independent perspective — without either exoticizing them or imposing on them an Anglocentric perspective.

I see a deep contradiction at the heart of Everett's linguistic philosophy. He insists that "languages are shaped by culture" (p. 298). If this applies to all languages, it applies also to English. But if English is impregnated with Anglo culture, then using English words as analytical tools for analyzing and interpreting other languages and cultures means imposing on them an Anglo perspective.

Nothing illustrates this blindness to the cultural underpinnings of English better than the important role the English cultural keywords *experience* and *evidence* play in Everett's portrayal of the Pirahã. As I have discussed in detail in my 2010 book *Experience, Evidence and Sense: The Hidden Cultural Legacy of English*, the meanings of the English words *evidence* and *experience* (and also *sense*) are unique

conceptual artefacts of modern English. Yet Everett uses them repeatedly to describe and interpret characteristic Pirahã ways of thinking. For example, he writes (p. 289):

> We begin with the 'immediacy of experience' principle that I have described in several publications. This principle is vital to the understanding of the interaction between culture and grammar in Pirahã, which require that evidence be given for assertions (…) In Pirahã culture everything that is said has to be 'warranted', that is, there has to be an evidential basis for it […].

From an English speaker's point of view, the meanings conveyed in Pirahã by what linguists call "evidentials" bring to mind the concept of 'evidence' (which is of course the source of that technical linguistic label). From a culture-independent perspective, however, such "evidentials" have little to do with what in modern English science, law, and many other areas is called "evidence". Roughly speaking, the "evidentials" answer the question "how do I know that it is like this"; "evidence", on the other hand, suggests that "I don't know and can't know but I have some reasons to think that it is like this". (Wierzbicka 1994a, 1996)

The need to signal limitations of our knowledge, and to carefully distinguish between 'thinking' and 'knowing' is a feature of modern Anglo thinking whose roots lie, above all, in John Locke's *Essay concerning Human Understanding* (1690). Societies like the Pirahã are not concerned with "evidence" for one's hypotheses, allegations and assertions. Thus, the refusal to combine a recognition of linguistic diversity with a recognition of semantic universals results, paradoxically, in combining it with an Anglocentric perspective on all languages and cultures. Portraying the Pirahã people as particularly concerned about backing up their assertions with "evidence" and always providing an adequate "evidential basis" for everything they say paints, in my view, a distorted picture of who they are and how they live — as does presenting them as people unable to generalize (no word for 'all', according to Everett, see Section 3) or to explain in their own words their own incest taboos (see Section 5).

There can be no doubt that the conceptual world of the Pirahã differs profoundly from the conceptual world of speakers of English. This means that most English words have no semantic equivalent in Pirahã, and vice versa. (In particular, the English word *evidence* has no counterpart in the Pirahã lexicon, and the concept of 'evidence', no counterpart in the Pirahã mental world). It doesn't mean, however, that there are no words at all which match in meaning in these two languages. Everett's own data provide ample evidence that there are such words. These words may be polysemous, but if one has at one's disposal a rigorous semantic methodology which allows one to sort out the different meanings of polysemous words then one can show precisely which meanings match and which don't.

1st proofs

In Section 2, I will show that such matching word-meanings include ONE and TWO, in Section 3, that they include ALL, and in Section 4, that they include WANT and THINK. All these elements are, according to NSM research, fundamental elements of human thought — which Everett has claimed are missing from the Pirahã lexicon. Thus, I will be defending in these three sections the view that these fundamental elements of 'the alphabet of human thoughts' (Leibniz, cf. Wierzbicka 2001, 2011), which have been found in languages as different as English, Korean, Ewe (West Africa), Yankunytjatjara (Australia), Mangepa-Mbula (Papua New Guinea), and many others, are not missing from Pirahã either.

In Section 5, I will address another fundamental question raised by Everett: can social cognition be independent of language? In particular, I will examine Everett's claim that the Pirahã have incest taboos without kinship terms to support them, that is, taboos which cannot be stated in Pirahã (although they can be stated in English) and which nonetheless the Pirahã people treat as binding social norms. I will show in this section how one can formulate these incest taboos in the natural semantic metalanguage in sentences cross-translatable into Pirahã. In formulating these taboos in this way, I will show how the NSM methodology allows us to make sense of the Pirahã kinship terms and kinship norms and to make them both intelligible to outsiders and consistent with Pirahã cultural practices and ways of speaking (as described by Everett himself).

2.   ONE and TWO: "There is one word for nose, not two"

On the basis of both theoretical considerations and empirical semantic investigations, NSM researchers have argued for a long time that both ONE and TWO are fundamental elements of human thought attested as words (or distinct meanings of words) in all languages (Goddard and Wierzbicka 1994, 2002; Goddard 2008).

However, during the last decade some scholars have raised doubts about the universality of ONE and TWO. In particular, one publication which attracted wide attention was Gordon's (2004) article in *Science*, provocatively entitled "Numerical Cognition without Words: Evidence from Amazonia". This was followed by further studies of Amazonian languages by Dixon (2004) and Everett (2005, 2008) and by psychologist Michael Frank and colleagues (2008).

Regrettably, in his new book Everett perpetuates the myth that Pirahã has no words for ONE and TWO — a myth that contradicts his own data. Here as elsewhere, the source of the confusion lies, evidently, in the polysemy of the relevant Pirahã words. Everett tells us that when he entered the Pirahã tribe in 1977, he was told "that they had a common and very simple number system, 'one', 'two', and 'many'" (p. 259). For a long time he accepted this account of Pirahã and was

not surprised that, for example, the word for 'one' (*hói*) had also another meaning: 'little', and that, as he puts it, "the *hói* that meant 'little' and the *hói* that meant 'one' must be homonyms. In a language like Pirahã, with only eight consonants, three vowels, and two tones, homonyms should be common" (p. 259–260).

Unfortunately, this eminently reasonable comment is followed by a declaration which, to me at least, makes little sense: "Eventually, though, the data led me to the inescapable conclusion that *hói always* meant 'small' or 'little', either of quantity (a small amount of manioc meal, for example) or of extent (a small man) and that there was only one *hói*, not two homonyms" (p. 260).

Everett does not explain why the conclusion that there was only one *hói* was inescapable, and the data that he presented in his publications lead, it seems to me, to the opposite conclusion. Here as elsewhere, Everett's insistence that Pirahã is radically different from all, or nearly all, other languages, appears to be ideological rather than data-based.

Gordon's (2004) article opens with the sentence "Most of the Pirahã tribe use a 'one-two-many' system of counting", expanded on the same page (p. 496) as follows: "The Pirahã counting system consists of the words *hói* (falling tone = "one") and *hoí* (rising tone = "two"). Larger quantities are designated as *baagi* or *aibai* (= "many")". On the face of it, then, Pirahã does have words for ONE and TWO, as we would have expected. However, having presented the words for ONE and TWO, Gordon goes on to point out that there are certain complications here: "the word for 'one' was sometimes used to denote just a small quantity such as two or three or sometimes more (…) Most of the time, in the enumeration task, *hói* referred to 'one', but not always" (ibid).

These apparent inconsistencies can be easily explained if we accept that the word *hói* has two distinct meanings in Pirahã: It can mean either ONE or LITTLE/ FEW (i.e., "small quantity"). Evidently, when *hói* is used with reference to two, three or more objects it is not used to mean ONE. The data presented by Gordon himself, and also by Everett (2005, 2008) and by Frank et al. (2008) make the conclusion that the word is in fact polysemous inescapable, to use Everett's word.

One clear indication of the existence of a word meaning ONE in Pirahã comes from Everett's own 2008 book, in which he describes how he was eliciting words for various body parts. As Everett (2008: 195) reports, the conversation went like this:

> "What is this?" I asked, pointing at my nose.
> "Xitaooi".
> "Xitaooi", I repeated, perfectly I thought.
> "Xaio, xitaopai", he said.
> Aargh, I thought. What is that -pai business doing at the end of the word?
> So, naively, I asked, "Why are there two words for nose?"
> "There is one word, xitaopai", came the exasperating answer.

"Just xitaopai?"
"Right, xitaooi", he said.

As Everett goes on to explain, his confusion was due to the fact that in Pirahã "-*pai*
at the end of body part word (…) means something like 'my own'. So *xitaooi* means
just 'nose' but *xitaopai* means 'my own nose'" (ibid.) What is relevant for our pres-
ent purpose is that evidently Pirahã does have words for ONE and TWO and that
when the informant insists that there is only one word for nose, not two, he doesn't
mean "roughly one": He means ONE.

Despite such clear evidence included in his own book, Everett denies that
Pirahã has a word for ONE. Evidently the reason for this is that, like Gordon, he
doesn't recognize polysemy.

We have already seen that the word *hói* can be used in Pirahã in two differ-
ent senses: ONE and LITTLE/FEW. In fact, Everett's data show that *hói* has also a
third meaning: SMALL, as in the phrase "a small child" or the sentence "the child
is small". As noted by Gordon, when *hói* is used in the sense of 'small', it has its
opposite in *sogii* 'big'. Can there be a confusion for the Pirahã speakers between
the meanings "one child" and "small child"? If the same phrase can mean, by itself,
either "one child" or "small child", then in principle there could of course in some
situations be a possibility of confusion. In most cases, however, the context no
doubt makes the intended meaning clear.

To suggest that for Pirahã speakers there is no conceptual distinction between
'one child' and 'small child', is in my view, to exoticize the Pirahã beyond belief.
Having performed the fusion of three distinct senses of *hói* Everet feels able to
declare, triumphantly: "The Pirahãs have no number words (…). This is the only
community in the world, so far as we know, that has been determined to lack any
numbers at all, not even the number 'one'" (2012: 260). It is not surprising that
having dispensed in this way with number 'one', Everett finds it equally easy to
dispense also with number 'two', to which I will now turn.

As we have seen, Gordon (2004) starts out by providing Pirahã words for ONE
or TWO: "*hói* (falling tone = 'one') and *hoí* (rising tone = 'two') (p. 496). Then he
goes on to qualify this statement:

> "…*hói* and *hoí*, the words for 'one' and 'two', were not always used to denote those
> quantities. Whereas the word for 'two' always denoted a larger quantity than the
> word for 'one' (when used in the same context), the word for 'one' was sometimes
> used to denote just a small quantity such as two or three or sometimes more"
> (p. 497).

But if the word *hoí* [TWO] can be used to refer not only to two objects but also to
three or sometimes more, what does this word mean? Gordon himself did not give
an explicit answer in relation to *hoí* [TWO], as he did in relation to *hói* [ONE], which

as we have seen he glossed as "roughly one". Everett (2005) was more explicit, suggesting for *hói* (ONE) "small size or amount" and for *hoí* (TWO) "somewhat larger size or amount" (and for the third quantitative word, *baagiso*, "many").

This, however, makes little sense in contexts where *hoí* is used to identify the quantity in absolute rather than relative terms. For example, when Everett (2008) asked his informant if there were two Pirahã words for nose, (*xitaopai* and *xitaooi*), he clearly meant TWO, and not "somewhat larger size or amount". Consider also the examples glossed by Everett as "there are two big red airplanes" (2005: 630) and "two heavy red barrels" (1986: 273). How could people talk about "two airplanes" or "two barrels" if they didn't have a word meaning 'two' and if the word *hoí* glossed by Everett himself as 'two' meant in fact "somewhat larger size or amount"? Even if he was mentally contrasting 'two' with 'one', 'two' is not "somewhat larger" than 'one'. It is twice as large.

At the same time, data presented by Frank et al. (2008: 820) show that in some contexts *hoí* mean something other than 'two', because when the number of objects of one kind that is shown to informants is gradually decreased, they are prepared to use *hoí* in relation to several objects (from 10 to 4).

Evidently, like *hói* [ONE], *hoí* [TWO] has two meanings, not one: The first meaning is 'two', and the second meaning is comparable to that of the English phrase *a few* (not *few*, but *a few*). For example, five or six objects could be regarded as "a few" (or "several"), but certainly not as "two".

In conclusion, the claim that Pirahã doesn't have words meaning 'one' and 'two' is unjustified. Like all other languages (investigated from this point of view), it has words meaning ONE (as in "one word") and TWO (as in "two words") — polysemous, to be sure, but not blurred.

3. ALL: Can the Pirahã people make generalizations?

In his 2005 article Everett asserted that Pirahã had no word for 'all'. In my commentary on his article, I challenged this assertion, pointing out that it was contradicted by his own data. For example, in his article, two Pirahã sentences, supplied with interlinear glosses, are translated by Everett himself with the word 'all':

> All the people went to swim/went swimming/are swimming/bathing, etc.

> The men all went to the field.

The reason why Everett was denying the presence of a word meaning 'all' was that the word glossed here as 'all' in other contexts could mean 'big' (as in the sentence about "two big red airplanes" cited earlier in this article).

Evidently the word *ʾogi* has two meanings (1. big, 2. all), one would have thought. But Everett was adamant that there was no polysemy here. His determination to deny that Pirahã had words meaning 'all' (or 'everyone') led to a particularly bizarre outcome in his examples, where a phrase clearly meaning 'everyone' is glossed as "people's bigness". When I pointed out in my commentary that a phrase like "people's bigness" made no sense and exoticized Pirahã thinking unnecessarily, Everett in his rejoinder strongly re-affirmed his claim that "the Pirahã lack the word 'all'" (p. 643).

In my 2005 commentary about Everett's claims about 'all', I wrote (p. 641):

> In claiming that Pirahã has no word for "all", Everett is joining the long tradition of "primitive-thought" scholars such as Hallpike (1979), who also claimed that, for example, Australian Aborigines had no word for "all" and, accordingly, were not capable of making generalizations. Everett insists that the Pirahã language is not in any way "primitive", but the fact of the matter is that without a word (or word-like element) meaning "all" speakers *could not* make generalizations. Accordingly, despite his protestations, Everett *is* presenting Pirahã as a "primitive" language.

Commenting on my challenge to Everett's account and on Everett's response to it, Goddard (2008: 10) wrote:

> Crucial to Everett's analysis is a refusal to recognize polysemy. According to him, because the word *ʾogi* means 'big' in some contexts, it means 'big' in all contexts. Though an NP like *hiaitiihi hi ʾogi* [lit. people 3p big] appears to express the same meaning as English 'all the people', for Everett its true meaning is something like "people's bigness". (…) When challenged by Wierzbicka (2005: 641) on the issue of polysemy, and with the counter-claim that the Pirahã expression *hi ʾogi* means ALL, Everett (2005) declined to say anything on the subject. (p. 624, Note 5).

The claim about the absence of a word meaning 'all' appears to be missing from Everett's 2012 book (although there is still a mention of missing quantifiers, p. 256). Perhaps this indicates that Everett is now more willing to accept that Pirahã does have a word meaning 'all', after all, and that phrases like "people's bigness" make no sense?

Everett concludes his discussion of numbers in Pirahã as follows: "…if we discover that a category of meaning previously believed to be universal, such as numbers, is not found in all languages, then this is itself an important result. It teaches us that, for whatever reason, the presence of this category does not define our species" (p. 270). By the same token, however, if we discover that the concepts 'one', 'two' and 'all' *can*, after all, be found — as words or distinct meanings of words — in all languages, this adds a small but significant component to what "defines our species". I would also concur with Donald Davidson (2001: 134–135) that part of what defines our species is the cognitive leap to "concepts expressed by the words 'some' and 'all'".

Cliff Goddard and I posited ALL as a conceptual universal in our 1994 book *Semantic and Lexical Universals* (it was first proposed by Goddard in 1986), and SOME was added to the list in my 1996 book *Semantics: Primes and Universals*. Davidson's recognition of both ALL and SOME as important ingredients of what defines human cognition was of course welcome. Everett's (2005: 643) assertion that "by the Pirahã evidence both Davidson and Wierzbicka are wrong" was not supported by evidence. In fact, his own data show that ALL at least is well attested in Pirahã.

4. WANT and THINK: Do the Pirahã talk about what they want and what they think?

Bearing in mind the pervasive presence of polysemy in all languages, let us ask about the Pirahãs' mental world. Everett tells us that they have words for every bit of flora and fauna around them (p. 287), but do they have words for what goes on in people's inner realm? In particular, do they have words for the concepts of WANT and THINK?

Everett is not absolutely clear on this point, because he seems more interested in settling scores with Chomskyan syntacticians about "recursion" than he is in the mental world of the Pirahã, but he seems to be implying that Pirahã has no words for 'thinking' and 'wanting'. To quote:

> There are many words in English that tend to be found in sentences with recursion. English words like 'think', 'believe', 'say' and 'want' all require subordinate clauses. Pirahã lacks all words of this kind (p. 287).

It is not quite clear whether Everett is saying here that Pirahã has no words meaning 'think' and 'want' or whether he only wants to say something about the syntax of these words in Pirahã. To keep the record clear, it is important to emphasize that in his published data (2005, 1986), we find numerous sentences about 'wanting', some of them with the verb *xog-* ('want') and some with the "desiderative suffix" *-sog*, which clearly also means 'want'. To illustrate from his 1986 book:

1. 'What does he want [xog-]?' (p. 239)
2. 'How much does Xabagi want [xog-]?' (p. 239)
3. 'Do you want [xog-] manioc meal?' (p. 252)
4. 'I want [-sog] to see the Pirahã, who are my brothers' (p. 212)
5. 'he wanted [-sog] to speak with the Pirahã' (p. 214)
6. 'He doesn't want [xog-] me to go' (p. 278).

Everett's primary focus on syntax, not on meaning (perhaps a remnant of his years as a Chomskyan) is evident in the comment (2005: 630) on the sentences glossed "The woman wants to see you": "want"-like embeddings are handled in Pirahã by a desiderative suffix on the verb, with no evidence of biclausality".

Thus, for Everett, what matters about 'want' is the syntactic question: can sentences with 'want' be, syntactically, 'biclausal', or not, and not what the Pirahã can say about 'wanting'. This throws an interesting light on Everett's "big question" of "what is possible in human languages?", quoted earlier. For me, what is 'big' here concerns *what* people can say in all languages, not *how* (with the help of what syntactic mechanisms) they can say it. Questions about "recursion" and "biclausality" (or otherwise) of sentences about wanting may be of interest to syntacticians, but not to anyone else. By contrast, the question of whether people in all societies can talk about what people want, is of broad human interest.

Evidently, Pirahã speakers, like speakers of other languages, can say that "someone wants something" (using the verb *xog-*) and also, that "someone wants to do something" (using the suffix *-sog*). Can they also say that "someone wants someone else to do something" or is this beyond the boundaries of what a Pirahã speaker can say? Example (6) above suggests that it is not: Whether biclausal or not, sentences about someone wanting someone else to do something are possible in Pirahã, as they appear to be in any human language.

I agree with Everett that it is not the case that whatever can be said in one language can be said in any other language. For example, when he says about the sentence: "Stop at the fifth house on the third road" that "there is no way to communicate that idea in Pirahã", I don't doubt that he is right. It is not the same, however, with the idea that "someone wants to do something". As Everett's examples clearly demonstrate, this idea *can* be readily expressed in Pirahã, as it can, evidence suggests, in any other language.

Recently, Nicholas Evans (2010: 59) expressed some doubts about the presence of a lexical exponent for the NSM prime WANT in the Australian language Kayardild. If he were right on this point and if the Kayardild people really couldn't talk about someone wanting something, this would indeed change our picture of what is possible in human discourse. Fortunately, in his comprehensive Kayardild grammar, Evans (1995: 259) himself provides examples of sentences such as *waydbala kurriju*, glossed as "some whitefellers wanted to see". Apparently, Evans' doubts about WANT in Kayardild are due to the polysemy of the "desiderative" suffix *-ju* and the "hortative" suffix *-jinja* (Evans 1995: 259–264, 263–264), just as Everett's doubts about the presence of quantifiers in Pirahã are due to the polysemy of the relevant Pirahã words.

In addition to the suffixes, it seems likely that the verb *janija*, glossed in Evans' (1995) Kayardild dictionary (which is part of his Grammar) as "search, seek", is

also polysemous and has a distinct meaning 'want', as is the case in the closely related language Lardil (Goddard 2001). (It is also possible that Evans has misunderstood what exactly NSM theory is claiming about the presence of WANT in all languages. For clarification of this claim, see, e.g., Goddard and Wierzbicka (eds) 2002, vol. 1, pp. 61–63; for a comprehensive discussion of WANT in Kayardild, see Harkins 1995). In my view, if it were really impossible in either Pirahã or Kayardild to talk about people wanting to do (or to see) something, or to want something to happen, this would be significant from the point of view of human universals. But the evidence suggests that this is not the case.

The same applies to 'thinking': It exoticizes and misrepresents the Pirahã to suggest that they have no concept of 'thinking' distinct from the concept of 'speaking'. Everett (p. 287) writes:

> Rather than saying 'John said that he was coming', the Pirahãs would say 'John spoke. He is coming'. That is ambiguous, making what is in English a single sentence in effect a small story. Or rather than say 'John thinks he is coming', they would handle this the same way, 'John spoke. I am coming'. Like other Amazonian groups, the Pirahãs tell us what they think that people are thinking or what they themselves are thinking by using the verb 'to speak', putting words in other people's mouths to convey their thoughts.

The use of the same word in two distinct senses, 'say' and 'think', is a common type of polysemy, and it is a mistake to interpret it as an absence of a lexical exponent for 'think'. Emily Knight (2008) has explored the question of the SAY–THINK polysemy in relation to the polysynthetic Australian language Bunuba, where the word *ma* can mean either 'say' or 'think'. As Goddard (2002: 29) reported:

> The existence of a real language-internal contrast between SAY and THINK is dramatized when the question is raised: How, in Bunuba, could one express a meaning such as 'I know what you said but what are you thinking?' When Knight explored this question with consultants, it emerged that the expression *ma + thangani* 'mouth-words' means unambiguously SAY and that *ma + gun.gulu* 'head' means unambiguously THINK.

Everett did not explore in a systematic way the use of the Pirahã word which he glosses as 'say' or 'speak', so he could not establish how the Pirahãs can distinguish (when they want to) between 'saying' and 'thinking'. But there is no reason to believe that Pirahã is somehow deficient in this respect, in comparison with Bunuba, or with any other language whose speakers can distinguish, when they want to, between speaking and thinking.

It is particularly noteworthy, in this connection, that according to Everett (p. 289), the Pirahã have a word (*sibiga*) meaning 'deduced or inferred', which indicates the grounds for believing something other than by observation or hearsay.

Clearly, *sibiga* is a lexically recognized form of thinking, distinguished by the Pirahã from what someone said.

Generally speaking, the suggestion that the Pirahã have words for every bit of flora and fauna in the jungle that they inhabit but don't have words for the basic psychological concepts 'want' and 'think', is reminiscent of the claims that Hallpike made in his *Foundations of Primitive Thought* (1979) about the alleged lack of words meaning 'think' and 'know' in various Australian and Papuan languages. As far as I can see, in both cases we are dealing with the results of superficial semantic analysis, oblivious to polysemy and lacking a rigorous semantic methodology. (cf. Wierzbicka 1994b, 1996; Goddard 1994).

5.   Kinship: Can the Pirahã state their own incest taboos?

It would be hard to disagree with Everett when he says: "Language is crucial for categorizing the world around us" (p. 241), and also when he applies this, in particular, to kinship. "Kinship always serves to identify and establish relationships that are valued culturally" (p. 243), he stresses, adding that "Kinship terms do more than merely categorize relationship (…), however. They also play a regulatory role in societies" (p. 244). Several different systems of kinship have been distinguished by anthropologists, including (inter alia), the 'Iroquois' system, the 'Eskimo' system and the 'Hawaiian' system. But in Everett's view, Pirahã is different from them all — and, predictably, it is the simplest.

> For decades, anthropologists believed that the Hawaiian kinship was the simplest system. There is a word for 'father', which refers to any male of *ego*'s father's generation; 'mother', any female of *ego*'s mother's generation; 'brother', referring to any male of *ego*'s generation; and 'sister', meaning any female of *ego*'s generation: (p. pp. 246–247 …). But it turns out that the Hawaiian system is not the least restrictive system. Pirahã kinship is even less so. The Pirahã system is similar to the Hawaiian system, except that it does not even distinguish gender for generation above *ego*. (p. 249).

Speaking as a linguist, not an old-school anthropologist, Everett could have noted that in the 'Hawaiian' system the words for 'mother' and 'father' are in fact polysemous: they have the primary senses of biological mother and father and various extended senses (cf., e.g., Scheffler 1978). Everett doesn't recognize polysemy in his discussion of kinship terms, however, and he doesn't try to state the meaning of any of the words he discusses. If he did try, he would find that it is impossible to assign a unitary meaning to a word glossed as "any female of ego's mother's generation" — a formula which in fact implicitly recognizes two meanings: 1. mother, 2. other females of mother's generation. If the phrase "*ego*'s mother" did not refer

only to the biological mother, then the formula would be circular. Everett, however, does not seem to notice this choice between polysemy and circularity and treats the words 'for mother' (and also those 'for father') as monosemous.

Here as elsewhere, this failure to recognize polysemy leads to what I see as a distorted and exoticized picture of Pirahã. Evidence suggests that all known languages have words meaning 'mother' and 'father'. These words may be polysemous, but their primary biological senses are distinct and identifiable on language-internal grounds. (Goddard and Wierzbicka, In press.) In Everett's account, however, Pirahã is different: It doesn't have words meaning 'mother' and 'father', although — he claims — it does have a word meaning 'parent': *baixi*. In fact, Everett (p. 249) appears to suggest, there are only four kin terms in Pirahã: *baixi* 'parent', *xahaigí* 'sibling' *hoísai* 'son' and *kai* 'daughter' (although the diagram provided by him includes a fifth one, for "sibling's child").

But although Everett glosses *baixi* as 'parent', in fact it is not clear that he really believes it himself. Rather, he appears to be suggesting that it means anyone in "the generation above *ego*". But without being anchored in the concepts of 'mother' and 'father' what could "generation above *ego*" possibly mean?

Whatever it means (in the researcher's mind), it is clearly not a phrase that would mean anything to a Pirahã speaker given that Pirahã has no words for either "generation" or "ego". The question then must be asked what *baixi* can mean for the Pirahã, and how a Pirahã person could explain its meaning using Pirahã words. Here, the only possible answer appears to be that Pirahã speakers could not define this word at all, because they couldn't say "mother or father". Thus, Everett appears to be implying that he can explain, in academic English, what the Pirahã word *baixi* means, but that Pirahã speakers couldn't possibly explain it in their own words, because they don't have the necessary words to do it with.

To me, this seemingly inescapable conclusion could be taken to imply the same European sense of intellectual superiority that Everett rightly denounces elsewhere in his book. The most disturbing aspect of Everett's story, however, has to do with the incest taboo. "Pirahã does have an incest taboo", he tells us, "no marrying of biological parent (…) is allowed", even though "the language lacks specific kinship terms to outlaw this" (p. 250).Everett himself appears to find it astounding that the Pirahã people have an incest taboo without having words meaning 'mother' and 'father', for he insists, again and again: "there are no terms for (…) 'biological parent' (mother or father) in Pirahã, yet still no member of the community would marry anyone that bore such a relationship to them, even in the absence of a linguistic term for that relationship" (p. 250).

But if there are no words meaning 'mother' and 'father', how do the Pirahã know that they must not marry their 'biological parents'? And how did Everett himself find out about this taboo, if the Pirahã people were not able to tell him

because they had no words to tell it with? If we discount telepathy, the answers to these mystifying questions appear to be quite simple, and they are quite consistent with Everett's data, though not with his interpretation of the data. I would suggest that there are two cultural scripts in Pirahã society ([A] and [B]) which can be stated in simple words cross-translatable into Pirahã as follows (cf. Wierzbicka, To appear):

> [A] people know that a woman cannot be a man's wife if she is this man's mother [*baixi*]

> [B] people know that a man cannot be a woman's husband if he is this woman's father [*baixi*]

It is not the case, then, that the Pirahã have an incest taboo even though "there are no terms (…) for biological parents (mother or father)" — a taboo somehow mysteriously transmitted through generations by pure thought, without words. The taboo against marrying one's own mother or father can be clearly formulated in words. As for the extensions of the words for biological mother and father to various classificatory relatives in the "generation above ego", the way the Pirahã deal with the occasional need for disambiguation is indicated by Everett himself, in the context of his discussion of 'siblings', biological and otherwise (2005:632): "The nominal suffix *gíi* 'real' or 'true' can be added to most nouns, including kinship terms: (…) *ʔahaigí* 'same generation', *ʔahaigí-gíi* 'biological sibling' (lit. 'real sibling')".

Evidently, the primary sense of 'mother' and 'father' can the distinguished from the extended uses of the term in the same way. It is essentially the same way which is used in disambiguating the words for biological mother and father (and some other close relatives) and their classificatory extensions in Australian languages (cf. Scheffler 1978). Thus, if need arises, scripts [A] and [B] formulated above can be made more precise as follows:

> [A′] people know that a woman cannot be a man's wife if she is his "true *baixi*"

> [B′] people know that a man cannot be a woman's husband if he is her "true *baixi*"

These are the rules that, I submit, people must be passing on (in some form) to their children and that at some point they must have also passed on to Everett. Do they have words to pass them on with? Yes, they do: For 'mother' they can use *baixi*, and for 'father', they can also use *baixi*. Evidently, the word is polysemous, but this polysemy does not prevent the scripts formulated above from being perfectly clear and unambiguous.

It is interesting to note that Everett didn't choose the word *parent* to state the Pirahã incest taboo, along the following lines:

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> [AA] people know that a woman cannot be a man's wife if she is his true parent

But to call a woman a man's 'parent', rather than, more specifically, his 'mother' sounds strange to many people, and 'true parent' even more so. In addition, *parent* implies an adult–child relationship, that is, a *parent* is, essentially, 'a *child*'s mother or father', so perhaps for this reason, too, it can sound incongruous in the context of a marriage taboo.] If these were the reasons why Everett did not propose a formula along the lines of [AA], then I can only agree with him on this point.

Nor did Everett propose a version like [AB], which would be syntactically simpler and therefore might at first seem more attractive:

> [AB] a person can't marry his or her parent

Here, the most likely reason is that Pirahã — like most languages of the world — doesn't have a verb like *marry*, which implies a symmetrical relationship between the two parties. The fact that, as Everett tells us, Pirahã men can 'rent' their wives and daughters to Portuguese river traders indicates that among the Pirahãs the relationship between the sexes is anything but symmetrical. Finally, and most importantly, to define the concept of 'parent', whether in Pirahã or in English, one would have to go via 'mother' and 'father', so there is really no escape from these two fundamental building blocks of all kinship systems.

In an earlier article, Everett (2005:632) reports that during one four-week period in 1995 he worked exclusively on trying to build a genealogy for an entire village. How did Everett go about collecting genealogies, if there are, as he claims, no words for 'mother' and 'father' in the language? The only plausible explanation seems to be that he did in fact use the Pirahã word *baixi* for both 'mother' and 'father' (disambiguating with the words meaning 'woman' or 'man'). As Everett emphasizes himself (in another context), Pirahã has only eight consonants and three vowels (and two tones), so there is a lot of homonymy (I would prefer to say, polysemy) in the language. Since the language does have distinct words (and not only word meanings) for 'woman' and 'man', the two senses of the word meaning 'mother' and 'father' can always be easily disambiguated. (It is worth mentioning that in his 1986 Pirahã grammar, Everett himself glossed the word *baixí* with 'mother' and 'father').

What applies to the Pirahã taboo on marrying one's biological mother or father, applies also, by and large, to the taboo on marrying one's (full) brother or sister.

How could the Pirahã people draw such a distinction between "full siblings" and "half siblings" if they didn't have words meaning 'mother' and 'father'? And how could they tell the fieldworker that there was a taboo on marrying a "full sibling"? Everett doesn't explain. According to my account of the Pirahã kinship terms, the answer is quite simple: There is another pair of cultural scripts (scripts

[C] and [D]) that the Pirahã can convey in some form to their children, and also to the fieldworker. These scripts might look like this:

> [C] people know that a woman cannot be a man's wife
>   if she is his *xahaigí-gíi* ('real sibling')

> [D] people know that a man cannot be a woman's husband
>   if he is her *xahaigí-gíi* ('real sibling')

I am using here Everett's gloss 'real sibling' for the sake of convenience, not because I believe that the word *xahaigí* really means 'sibling'. On the data before us, there is no need to posit polysemy for *xahaigí* along the lines of 'brother' and 'sister', and neither is there any need to describe its meaning with a technical English term like 'sibling'. Rather, the meaning of *xahaigí* (apart from classificatory uses) can be regarded as unitary and can be stated, like the meanings of comparable terms in other languages, in terms of the words meaning 'mother' and 'father':

> [E] *xahaigí*
> a man can say about a woman: "this is my (true) *xahaigí*" if it is like this:
>   this woman's (true) mother is this man's (true) mother
>   this woman's (true) father is this man's (true) father

> a woman can say about a man: "this is my (true) *xahaigí*" if it is like this:
>   this man's (true) mother is this woman's (true) mother
>   this man's (true) father is this woman's (true) father

> a child can say the same about another child if it is like this:
>   this child's (true) mother is this other child's (true) mother
>   this child's (true) father is this other child's (true) father

Since the Pirahã kin terms glossed by Everett as 'sibling' can in fact be defined through terms shared by Pirahã and English, the cultural scripts proscribing marriage between 'full siblings' can also be presented through shared concepts. Thus, the scripts stated above as [C] and [D] can also be presented as [C′] and [D′]:

> [C′] people know that a woman cannot be a man's wife if it is like this:
>   this woman's (true) mother is this man's (true) mother
>   this woman's (true) father is this man's (true) father

> [D′] people know that a man cannot be a woman's husband if it is like this:
>   this man's (true) mother is this woman's (true) mother
>   this man's (true) father is this woman's (true) father

Cultural scripts such as these are both intelligible through English and crosstranslatable into Pirahã. I believe they make superfluous both the hypothesis of telepathic transmission of incest taboos and the hypothesis that these taboos can only be formulated in English and not in Pirahã itself.

1st proofs

Reflecting on his own interpretation of the Pirahã kinship terms, Everett writes: "Field research on unstudied or little-studied languages and cultures … is the only way for us to expand our knowledge of what is possible in human languages" (p. 250–251). I agree that field research on unstudied or little-studied languages and cultures is indeed a great way to expand our knowledge of what is possible in human languages. The data emerging from fieldwork, however, need to be interpreted, and this requires a coherent interpretive framework. Without such a framework, reports from the field can also cloud the picture of what is possible in human languages, and given the air of authority often surrounding such reports, they can at times be as harmful as they can be, at other times, illuminating and enriching.

"The presence of an incest taboo in Pirahã in spite of an absence of kinship terms to support it" suggests, according to Everett, "that thought is independent of language in important ways" (p. 259). There is even more to be learned, he further suggests, from the fact that "the Pirahã practice incest avoidance without words to do so (…); this set of correlations between missing terms and cognitive abilities raises an even larger question, namely, what studying the Pirahã can tell us about what it means to be human" (p. 261).

Everett's generalizations about what it means to be human, derived from his dedicated and in many ways impressive fieldwork among the Pirahã raise nonetheless a question different from his own, namely, how much can we reliably learn about "what it means to be human" on the basis of fieldwork alone, unsupported by a rigorous analytical methodology and a coherent interpretive framework.

6. Conclusion: "What it means to be human"

Reading through the examples in all the chapters in *The Handbook of Amazonian Languages*, where Everett's grammar of Pirahã was published (in 1986), can be a moving experience. Again and again, one finds in these examples sentences referring to 'wanting' and to 'knowing', to 'seeing' and to 'hearing', to 'doing' and to 'saying', to 'now', 'before' and 'after', to 'two' and 'many', to 'if' and 'because', to 'living' and 'dying', and also to 'men', 'women' and 'children', and to 'water', 'fire' and 'ground'. All these basic words, which have been posited in NSM-based research as human universals, show up, again and again, in the *Handbook of Amazonian languages*, as they do in comparable reference works such as *Handbook of Australian Languages* or *Handbook of African Languages*. These simple words, recurring in the linguistic records of the field workers from different parts of the world seem to be telling the same story, and jointly suggesting a linguistic answer to Everett's question about "what it means to be human".

Everett (p. 7) emphatically rejects the argument that "languages around the world share fundamental similarities that … must result from a common core all humans are born with" (p. 29) and insists that the diversity of languages is much greater than their similarity. But there is no conflict between the view that, on the one hand, there is a conceptual common core in languages that all humans are born with, and on the other, there is an enormous diversity both in the lexicons and in grammars, which are shaped by different cultures. The differences come from different cultures. And where do the similarities come from?

Languages can differ dramatically across the world and still have a shared conceptual lexicon and a shared mini-grammar — not in the sense of Chomsky's "universal grammar" (of which Michael Tomasello, quoted by Everett (p. 192), says simply that "it is dead"), but in the sense of an innate lexico-grammatical core. This would explain their similarities, just as differences between cultures can explain their diversity.

"Languages are shaped by culture", says Everett (p. 298), but as if this was not enough, he also says that languages are "creations of human cultures" (p. 202). But there is a difference between being "shaped by culture" and being "created by culture".

As for languages being *shaped* by cultures both in their lexicons and in their grammars, I have pursued this idea in many publications myself, from my 1979 article "Ethnosyntax and the philosophy of grammar", to the 1997 book *Understanding Cultures through their Key Words*, the 2006 book *English: Meaning and Culture* and, *Experience, Evidence and Sense: The Hidden Cultural Legacy of English* (2010a). (Although Everett doesn't mention me by name, he must be aware of at least some of these publications, because at one point (p. 130) he summarizes one of the chapters of my book *English: Meaning and Culture*, referring to me, somewhat oddly, as "one researcher").

So I am very much in favour of the idea that culture shapes grammar (as well as lexicon), and have been advocating it myself for over 30 years. At the same time, however, I have been arguing that languages are not cultural artefacts created *ex nihilo*, but that they do have an innate basis, manifested as their shared lexical and grammatical core. Crucially, this shared core gives us a common measure for investigating the diversity of languages and a lingua franca for explaining meanings and ideas across languages and cultures.

"It is a noble task to try to understand others and to have them understand you, but it is never an easy one" says Everett (p. 327). As I have argued for decades, a basic prerequisite for understanding others, and for having them understand you, is to have at your disposal some shared concepts on which this understanding can build. To achieve genuine cross-cultural understanding, fieldwork — even heroic fieldwork — is not enough: we also need a shared conceptual lingua franca.

Table 1. Universal semantic primes (English exponents), grouped into related categories.

I, YOU, SOMEONE, SOMETHING~THING, PEOPLE, BODY	substantives
KIND, PART	relational substantives
THIS, THE SAME, OTHER~ELSE	determiners
ONE, TWO, MUCH~MANY, LITTLE~FEW, SOME, ALL	quantifiers
GOOD, BAD	evaluators
BIG, SMALL	descriptors
KNOW, THINK, WANT, FEEL, SEE, HEAR	mental predicates
SAY, WORDS, TRUE	speech
DO, HAPPEN, MOVE, TOUCH	actions, events, movement, contact
BE (SOMEWHERE), THERE IS, HAVE (SOMETHING)~BE (SOME- ONE'S), BE (SOMEONE/SOMETHING)	location, existence, possession, specification
LIVE, DIE	life and death
WHEN~TIME, NOW, BEFORE, AFTER, A LONG TIME, A SHORT TIME, FOR SOME TIME, MOMENT	time
WHERE~PLACE, HERE, ABOVE, BELOW, FAR, NEAR, SIDE, INSIDE	space
NOT, MAYBE, CAN, BECAUSE, IF	logical concepts
VERY, MORE~ANYMORE	intensifier, augmentor
LIKE~AS~WAY	similarity

Notes: — Primes exist as the meanings of lexical units (not at the level of lexemes) — Exponents of primes may be words, bound morphemes, or phrasemes — They can be formally complex — They can have combinatorial variants or "allolexes" (indicated with ~) — Each prime has well-specified syntactic (combinatorial) properties.

Table 2. Exponents of universal semantic primes in three additional languages

Japanese	Spanish	Russian
WATASHI *I*, ANATA *you*, DAREKA *someone*, NANIKA~MONO~KOTO *something/thing*, HITO~HITOBITO *people*, KARADA *body*	YO *I*, TU *you*, ALGUIEN *someone*, ALGO~COSA *something~thing*, GENTE *people*, CUERPO *body*	JA *I*, TY *you*, KTO-TO *someone*, ČTO-TO~VEŠČ' *something~thing*, LJUDI *people*, TELO *body*
SHURUI *kind*, BUBUN *part*	TIPO *kind*, PARTE *part*	ROD~VID *kind*, ČAST' *part*
KORE *this*, ONAJI *the same*, HOKA *other*	ESTO *this*, LO MISMO *the same*, OTRO *other*	ÈTOT *this*, TOT ŽE *the same*, DRUGOJ *other*
HITO-~ICHI- *one*, FUTA-~NI- *two*, TAKUSAN *many~much*, SUKO- SHI *little~few*, IKUTSUKA *some*, MINNA *all*	UNO *one*, DOS *two*, MUCHO *much~many*, POCO *little~few*, ALGUNOS *some*, TODO *all*	ODIN *one*, DVA *two*, MNO- GO *much~many*, MOLO *little~few*, NEKOTORYE *some*, VSE *all*

Table 2 (*continued*)

Japanese	Spanish	Russian
II *good*, WARUI *bad*	BUENO *good*, MALO *bad*	XOROŠIJ~XOROŠO *good*, PLOXOJ~PLOXO *bad*
OOKII *big*, CHIISAI *small*	GRANDE *big*, PEQUEÑO *small*	BOL'ŠOJ *big*, MALEN'KIJ *small*
OMOU *think*, SHIRU *know*, HOSHII~-TAI~NOZOMU *want*, KANJIRU *feel*, MIRU *see*, KIKU *hear*	PENSAR *think*, SABER *know*, QUERER *want*, SENTIR *feel*, VER *see*, OÍR *hear*	DUMAT' *think*, ZNAT' *know*, XOTET' *want*, ČUVSTVOVAT' *feel*, VIDET' *see*, SLYŠAT' *hear*
IU *say*, KOTOBA *words*, HONTOO *true*	DECIR *say*, PALABRAS *words*, VERDAD *true*	GOVORIT'~SKAZAT' *say*, SLOVA *words*, PRAVDA *true*
SURU *do*, OKORU~OKIRU *happen*, UGOKU *move*, FURERU *touch*	HACER *do*, PASAR *happen*, MOVERSE *move*, TOCAR *touch*	DELAT' *do*, PROISXODIT'~SLUČAT'SJA *happen*, DVIGAT'SJA *move*, KASAT'SJA *touching*
(DOKOKA) IRU~ARU *be (somewhere)*, IRU~ARU *there is*, MOTSU *have*, (DAREKA/NANIKA) DEARU *be (someone/something)*	ESTAR *be (somewhere)*, HAY *there is*, TENER *have*, SER *be (someone/something)*	BYT' (GDE-TO) *be (somewhere)*, BYT'~EST' *there is*, BYT' U *have*, BYT' (KEM-TO/ČEM-TO) *be (someone/something)*
IKIRU *live*, SHINU *die*	VIVIR *live*, MORIR *die*	ŽIT' *live*, UMERET' *die*
ITSU~TOKI *when~time*, IMA *now*, MAE *before*, ATO *after*, NAGAI AIDA *a long time*, MIJIKAI AIDA *a short time*, SHIBARAKU NO AIDA *for some time*, SUGUNI *moment*	CUÁNDO~TIEMPO *when~time*, AHORA *now*, ANTES *before*, DESPUÉS *after*, MUCHO TIEMPO *a long time*, POCO TIEMPO *a short time*, POR UN TIEMPO *for some time*, MOMENTO *moment*	KOGDA~VREMJA *when~time*, SEJČAS *now*, DO *before*, OSLE *after*, DOLGO *a long time*, KOROTKOE VREMJA, *a short time*, NEKOTOROE VREMJA *for some time*, MOMENT *moment*
DOKO~TOKORO *where~place*, KOKO *here*, UE *above*, SHITA *below*, CHIKAI *near*, TOOI *far*, MEN *side*, NAKA *inside*	DÓNDE~SITIO *where~place*, AQUÍ *here*, ARRIBA *above*, DEBAJO *below*, CERCA *near*, LEJOS *far*, LADO *side*, DENTRO *inside*	GDE~MESTO *where~place*, ZDES' *here*, NAD *above*, POD *below*, DALEKO *far*, BLIZKO *near*, STORONA *side*, VNUTRI *inside*
-NAI *not*, TABUN *maybe*, DEKIRU *can*, -KARA *because*, MOSHI (BA) *if*	NO *not*, TAL VEZ *maybe*, PODER *can*, PORQUE *because*, SI *if*	NE *not*, MOŽET BYT' *maybe*, MOČ' *can*, POTOMU ČTO *because*, ESLI *if*
SUGOKU *very*, MOTTO *more*	MUY *very*, MÁS *more*	OČEN' *very*, BOL'ŠE~EŠČE *more*
YOO~DOO~YOONI *like/how/as*	COMO *like*	KAK~TAK *like*

Notes — Primes exist as the meanings of lexical units (not at the level of lexemes) — Exponents of primes may be words, bound morphemes, or phrasemes — They can be formally, i.e., morphologically, complex — They can have combinatorial variants or allolexes (indicated with ~) — Each prime has well-specified syntactic (combinatorial) properties.

Acknowledgement

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Anna Wierzbicka, born and educated in Poland, is Professor of Linguistics at the Australian National University, and a Fellow of the Australian Academy of the Humanities, of the Academy of Social Sciences of Australia, of the Russian Academy of Sciences, and of the Polish Academy of Arts and Sciences. She has two Honorary Doctorates, one from Marie Curie-Sklodowska University, Poland (2004) and one from Warsaw University, Poland (2006). She is the winner of the Dobrushin Prize for 2010 (established in honour of the Russian mathematician Roland Lvovich Dobrushin) and of the Polish Science Foundation's 2010 prize for the humanities and social sciences. Together with her Australian colleague Cliff Goddard, Wierzbicka created the "Natural Semantic Metalanguage" (NSM), based on the empirically established intersection of all sampled natural languages (see Goddard & Wierzbicka, eds, 2002, *Meaning and Universal Grammar*). She is the author of numerous books, including *Semantics: Primes and Universals* (1996), *Understanding Cultures Through Their Keywords* (1997), *Emotions Across Languages and Cultures: Diversity and Universals* (1999), *What Did Jesus Mean? Explaining the Sermon on the Mount and the Parables in Simple and Universal Human Concepts* (2001), *English: Meaning and Culture* (2006), and *Experience, Evidence and Sense: The Hidden Cultural Legacy of English* (2010).

Not quite organizational

A response to Raymond W. Gibbs and Nathaniel Clark

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Gibbs and Clark's comments on *Language: The Cultural Tool*, "No need for instinct: coordinated communication as an emergent self organized process," is one of the most stimulating papers I have read in some time. They bring their perspective as psychologists to the problem closest my interests — the origin, use, and nature of language. In particular, they offer a fresh understanding on how language emerges from the need of humans to organize themselves (see also Everett, in progress).

This paper is exactly the approach to the origins and nature of human language that is needed if the science of language is to move beyond mysterious genetic postulates that have little predictive value and are all too often "just so" stories of how complex behaviors such as language emerge in our species. See

Every paragraph in their paper is a ringing blow against the need for a Universal Grammar of any type. It isn't that they have solved all the problems or that they haven't begged any questions. Rather, what I find so appealing in their account (just as I do with Robert Van Valin's Role and Reference Grammar (VanValin 2007)) is that it takes a domain, language, in which we have been told that things are far too complicated to be learned and reveals the inner workings to be surprisingly less complex than we may have thought previously, demonstrating how one might in fact go about learning the full range of causally implicated knowledge in the domain.

Simply the existence of a partial story of the nature of language that is orders of magnitude simpler than a more traditional theory, such as Chomskyan theory, is an argument that the more complex story is less likely.

Most of what Gibbs and Clark write complements Language. There is no fundamental disagreement between my idea that language is a tool shaped partially by cultural values for the purpose of building a community and their idea that many aspects of language are the result of self-organizational dynamics. In fact, I am

convinced that much of language is effected by self-organizational processes. As they observe, contingency judgments play a large role in this dynamic emergence of language. And contingency is one of the important cognitive platforms for language that I underscore in Language.

Thus I agree (almost) completely with statements such as the following (p. 1): "Both broad regularities and specific variations in linguistic structure and behavior can be accounted for by self-organizational processes that operate without explicit internal rules, blueprints, or mental representations." To the degree that cognitive scientists can deliver on a program like this, they will have removed the motivation for belief in a universal grammar, especially given that the models currently available make no predictions, are unsupported by studies of human anatomy and physiology, and are largely a matter of theory-internal architectures.

Gibbs and Clark also make a point near the beginning of their paper that I consider absolutely essential for consideration by modern experimental psychology when they say (p. 4ff) "One reason why experimental studies, employing large numbers of naïve participants, are popular in psychology and cognitive neuroscience is that they enable scientists to discern people's linguistic and cognitive abilities using 'indirect methods…' But even controlled experimental studies investigating the behaviors of many participants can be problematic." Their methodological points are some of the most important in this paper.

But Gibbs and Clark's self-organizational proposal does not seem to be quite enough to get from zero to language. If robots without innate knowledge can begin to come up with the rudiments of language, then why can't bonobos or other species? Their model seems to me to lack to fundamental ingredients — a start point and an end point. What triggers our species to begin the community-building that, according to Language, is crucial for the emergence of language? And what is the final state of the linguistic knowledge that speakers attain (or is their a final state) to one degree or another? What limits the states?

Gibbs and Clark themselves implicitly allow that self-organization is insufficient to achieve the final state, when they talk about metaphors, background knowledge, and other capabilities that Language refers to as "platforms." Self-organization obviously does not occur in a vacuum.

Consider their reference to Lakoff & Johnson's (2003) now classic work on metaphor. When they refer to "underlying conceptual metaphors" (p. 7) they at once allow for more than mere self-organization and they provide an important space for culture to enter the discussion. The reason is that metaphors are not innate. They are cultural constructions, perhaps deeply embedded in societies over time — creations of people's values.

I was nevertheless fascinated with the authors' discussion of their research on idioms and metaphors because, as they point out (p. 8) there are many similarities

between the problems they faced with California subjects and my own design problems underscored by comments from Pirahã subjects.

Another important point with which I am in complete agreement is made when they point out (p. 9) that average performance on a particular psycholinguistic task does not necessarily indicate the presence of an internal mental state. This leads them to criticize, I believe convincingly, Fodor's well-known revival of the discredited phrenology of the past, by relabeling it "modularity." One telling point that they make in this regard, similar to arguments made in Language, is that (p. 11) "Many studies within cognitive neuroscience demonstrate that damage to particular brain sites does not provide selective functional deficits, contrary to the predictions of classic modularity theory." What surprises me in this regard is that this is a well-known and well-documented criticism but seems to be completely ignored in the nativist and modularity literature, especially of the Fodorian, Evolutionary Psychology, and Universal Grammar varieties. In this same section the authors underscore a very important claim of Language, namely that "… people have much greater flexibility in their online use and understanding of language than would ever be predicted by the strict, serial model favored by modularity theorists."

Gibbs and Clark (p. 13) underscore yet another argument of Language when they say that modularity arguments often attribute abilities to specific modules "… with little consideration of broader principles that may drive different experimental effects across many studies." As I have pointed out in discussions with modularity theorists myself, when this mistake is made it misses generalizations, a fault Chomsky has urged his followers to avoid since the inception of his research program. It would be like analyzing the constituent displacement in the example below as N-movement rather than NP-movement:

> John saw the man.
> The man was seen ___ by John.

The classic Chomskyan analysis of such examples is that the words "the man" "move" from the object position of the first phrase to the subject position of the second phrase, leaving a gap. While it is true that the noun "man" is involved in this alternation, it would be a mistake to say that this is the movement of the noun only. It is clearly the movement of the entire NP, since the article "the" moves along with the noun "man." N-movement is not what is happening — that would miss a generalization. By the same token, claiming that this or that part of the brain is dedicated to a specific linguistic function, when we know in fact that the linguistic function is just one of that area's functions, is to miss a generalization. All neuroscience studies of language functions in the brain that I am aware of are like this, i.e. there is no purely linguistic-dedicated portion of the brain — all neurological

functions are at a larger granularity, affecting more skills (points made throughout the work of Philip Lieberman's laboratory at Brown University, for example). Thus, as they conclude on p. 13, "We lack empirical justification to motivate modular distinctions…" And crucially, "Drawing a causal link between average behavior and some psychological structure or mechanisms fails to explain the incredible variability in linguistic structure and behavior."

They further discuss a severe problem that has been overlooked by many nativist researchers, a problem also pointed out in recent work by Philip Lieberman, namely that "The most common method of locating neurons active during a specific task … rely on a subtractive method of comparing fMRI blood oxygen levels during the target task to activations in the same region of interest … during a baseline task. But … the brain is spontaneously active, with neural firings exhibiting 'complex dynamics and rich patterning of spontaneous network activity.'" The subtractive methodology so common in modularity/nativist neuroscience is too problematic to be relied on as crucial evidence.

On p. 17 they criticize my endorsement of a proposed interactional instinct, as part of their desire to eliminate instincts, because it is "teleological." But this does not follow. There is nothing teleological about the human need to interact. The desire to communicate, to interact with conspecifics, is fully compatible with the neurocognitive flexibility that both Language and Gibbs and Clark's work rightly see as so important. Human infants do seem to be motivated — unlike infants of any other species — to interact. There is plenty of room for the cultural principles of Language and the self-organizational principles of Gibbs and Clark to do exactly what they are claimed to do, in conjunction with such an instinct. So their objections seem to miss the mark with regard to the interactional instinct. That said, I have no a priori commitments to instincts. In fact, quite the opposite. I would be delighted if someone could explain how the construction of linguistic systems begin without Aristotle's social instinct or any other. Though I am not aware of any candidate at present, my point in raising the idea of an interactional instinct is that at least this would fit the definition of an instinct, whereas talk of things like a "language instinct," a "faith instinct," are wide of the mark.

One assertion that the authors make (p. 19) which I think is extremely important and absolutely sensible is "Any instance of speech communication fundamentally constitutes a different task for the participants given their idiosyncratic histories, dispositions, and situation resulting in a differently self-organized mind and body." This type of highly variable, individualistic account of communication is also fully compatible with the theory of Language and it is exciting to see the parallel research that has led these authors to such a similar conclusion.

They proceed (p. 21) to offer an interesting discussion of contingencies, one of the important platforms for language discussed in Language. Their own

discussion of contingency, like virtually every point in their article, complements and enriches the ideas of Language, adding much that Language, owing to my own limitations of space and research background, was not able to include. But there is nothing incompatible with this part of their article, leading to the conclusion on p. 23 that "characterizing the ways people use language in interpersonal communication must take into account all of these interacting forces…" and the interactional instinct proposed by Language. Again, the interactional instinct is not limiting in the ways that they suggest.

On page 27, where the authors discuss speaking patterns and posture as evidence of larger self-organizational principles at work in shaping communication, I am reminded of a phenomenon I observed (Everett 1988) among the Pirahãs when researching their stress system. I noticed that a couple of language teachers, one young boy in particular, waved their arms when repeating individual words whose stress pattern I was investigating. After some time I realized that they were "conducting," i.e. that their arm movements coincided with the stressed syllable of each word. Later Peter Ladefoged and I (Ladefoged, Ladefoged, and Everett (1997)) were able to train (in minutes) speakers of the Banawá language of the Amazon to simultaneously pronounce individual words, while tapping (with 100% accuracy) the stress patterns of their words. This type of experiment confirms the authors' points that language is not an isolated system and that we communicate with more than our tongues, each communicative act being a unique occurrence.

In their discussions of interpersonal interactions in the latter part of the article (p. 27ff) the authors show clearly how social understanding and building community (especially when they discuss coordination in eye-movements between speakers) work together with the construction of language. This is exciting indirect confirmation of the central hypothesis of Language, namely, that language is a tool for building community.

Later they claim that individual intentions (p. 31) are not responsible for the emergence of language which, they claim, is actually produced by "… dynamical couplings of interactants with each other and with their ecological context." But as philosophers and others who have worked on group intentions know, the work of people in groups is not incompatible with the possession and importance of individual intentionality. Here I think that the authors have perhaps overstated their case somewhat.

As we reach their conclusion (p. 33ff), while I find the bulk of this article convincing, useful, and exciting, I still do not believe that they have adequately come to grips with the failure of other species to have language, if it is strictly produced by the self-organizational forces they propose.

Breathability, learnability, and the illusion of design

A response to Haselager

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The case is often made that language is innate because all humans learn a language. In several recent publications I have argued that this doesn't follow. On the one hand, if language were innate, not all humans would be able to learn all languages. Second, this is logically equivalent to saying that air is innate because all humans breathe some. Language is rather a tool that must fit human biology (or human biology must adapt to language, if one adopts a more platonic view of language) or it would have no functionality. If air did not fit human biology (or vice-versa), there could be no human life as we know it. Arguments from functionality to design are never particularly strong. All functionality shows is that the tool and its user somehow have evolved to fit one another.

In his extremely stimulating and useful article, Pim Haselager argues for a view that has come to be known of Embodied Embedded Cognition. It is a view I find extremely appealing and convincing. It doesn't supplant the work of LCT, but it complements it nicely.

In reading his piece, it becomes clear that Haselager and I share many conclusions about the relevance of Chomskyan linguistics to a genuine science of language. Like Cowley, Haselager finds the systematicity of language to be more a result of the organism's environment than the organism's internal representational system. Or as he says "Systematicity need not be a feature of the representational system (e.g., in virtue of it having a constituent structure), but a feature of the world in which cognitive systems evolved." This makes sense to me. And it is compatible with the theory laid out in LCT.

Also important for understanding the importance of the social in language are the experiments Haselager reports on regarding joint attention and how this can lead to a standard set of symbols, a shared lexicon, and so on (where, by the way, "shared" does not mean "possessed externally to each individual member"). As he

says, echoing words of LCT, "in order to learn the meaning of a word in actual practice, one must be able to substantially reduce the number of possible hypotheses." It is part of the cultural role of language that we fix the meanings of words symbiotically, between the individual and the group.

Haselager's focus on the various types of joint attention (checking, following, and directing) seems absolutely essential to understanding how communication more generally and language more specifically works among humans (and how these come to differ from other species).

In his Section 5, Haselager raises the interesting issue of recipient design. We clearly design our utterances, moderating our performance via various types of feedback, from body posture, eyebrow position, hand movements, words, and so on of our interlocutors in discourse. This leads us in communication to model our hearers. Likely all of communicative human behavior follows similar concerns for the hearer. Musical performances. Speeches. And so on.

As was the case with Cowley and Gibbs & Clark, Haselager rightly points to the significance of society for the formation of language. But it is his notion of "embodied meaning" that I find most appealing, because as we embody the meanings that come from social interactions, this is the source of the "dark matter of cognition and culture" that I have referred to in LCT and elsewhere in this special volume. Embodied meaning is a useful concept in showing how the knowledge outside of us may come to play such an important role inside each of us, capturing well the idea that the brain is part of the body and moving us away from the Cartesian dualism that has plagued linguistics for several decades now.

Haselager's research and ideas take us closer to an understanding of language as an embodied cultural tool and I look forward to thinking more about how to use (or steal) his ideas in my future research.

Exocognitive Linguistics

A response to Cowley

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Like the paper by Gibbs and Clark, Stephen J. Cowley's principal criticism of Language is that it focuses narrowly on the organism rather than the enveloping society which shapes the organism. His paper, "Linguistic fire and human cognitive powers," argues that my work does not go far enough to getting us to a "people-centered" linguistics. While I am sympathetic with the majority of what Cowley writes this article, I nevertheless think that he goes too far in rejecting the importance of the learning and storage of linguistic information in the brain.

Cowley and I agree that linguistics should be people-centered, that communication is a function of the whole person in a specific environment, that implicit information is vital to understanding the explicit utterances found in human interactions, and that the symbiosis between the individual and the environment is important to help us understand the nature of human language as a cultural, communicative tool. We further agree that there is an acute overemphasis in modern language studies on knowledge rather than behavior and that the activity of "sense-making" is more properly the object of linguistic study than mere *abstracta* (e.g. derivations, lexicons, phonology, and so on).

But we disagree about many things, especially the role of the brain and the individual organism. The dichotomy between the organism and its environment is a false one. I believe that Cowley would agree with me on this. But to him it is false because the organism has few properties that cannot be better understood as part of the organism's ecology and society whereas for me the dichotomy is false because both are important to study, not because only one of them is. In particular, Cowley's view, which I am tempted to refer to as the acephalic view of language, goes too far in denying a role for the brain in language studies. This leads to a disagreement as well over the importance of the lexicon and storage of meanings and forms as signs in the brain. Ultimately, I believe that we agree about much more than we disagree. And I believe that some of the polar oppositions that Cowley suggests are in reality pseudo-oppositions

As Language points out, one of the fundamental components of human societies and meaning in understanding how language emerges from interactions is what I refer to as the "dark matter" of cognition and discourse — i.e. the things that we do not talk about much, if at all, but which are crucially implicit in what we do talk about. This dark matter is, in my opinion, the result of "talking like who you talk with" and "believing like who you believe with." In other words, in the course of our individual development in a particular society we come to share beliefs about the world that are so fundamental to who we are that we do not need to explain them or even talk about them. In Language, I give one striking example of this from the analysis of a Wari' text (the Origin of Corn). As we continually talk to the same people we learn what they know and they learn what we know and this shared knowledge is driven down into the tacit storage of our experiences. Different cultures and societies will have different dark matter arising from their own interactions, values, background knowledge, and so on, as discussed in Language.

Thus I could not agree more with Cowley when he says (p. 3) that the failure to look at the organism as part of a society "… led cognitive science to premature theorization of disembodied mental faculties." Cognitive Science has gone wrong by, ironically, a reification of the brain. He is further correct to express concern that modern cognitive studies have reified the brain. And I agree completely when he rejects the idea of human language as a "prototype of computational machine code."

My opinion begins to diverge from Cowley's however, on p. 4, where he criticizes the notion of "Shannon information". What I meant by the introduction of Shannon's ideas in Language is merely that Shannon lays out one component of the communication problem that language is a tool for solving. I should have been clearer and more inclusive in this part of the discussion, letting the reader know that I do not believe that Shannon modeled much at all of what I am interested and further spelling out the kinds of exocognitive considerations that Cowley rightly raises in his article. As we attempt to model the entire communicative task of human beings as members of cultures and societies, Shannon's flowchart of communication (LCT p. 57) is meant as nothing more than a link in the chain. I think that Cowley is simultaneously right and wrong about the significance of Shannon's work. He is right that there is much more to communication and language than what Shannon's chart or Shannon information indicate. But he is wrong to throw it all out for that reason.

Cowley states correctly (p. 5) that "Everett allows meaning to drive most of grammar." But he is incorrect to further state that this is incompatible with both information processing or individual intentionality. The idea that meaning is socio-cultural in its essence in no way conflicts with the idea that there is a significant role for language in the individual organism.

There is no way around words! We store signs in our brains. And we store them in society. The originate in society and in culture, but they migrate to brains. Each brain will have its own idiosyncratic record of vocabulary, partially corrupted or altered from the usage of that vocabulary by the sources from which the individual acquired it. The question Cowley raises "Why should each of us possess a mental lexicon?" (p. 6) is answered easily, i.e. because there is evidence that we have them, use them, and could not function without them.

To me the so-called "cognitive revolution" has been over-hyped. It hasn't delivered near so much as some claim for it. And by switching the focus from behavior, society, and culture to the "black box" of the brain, cognitive studies have deprived themselves of the principal explanans for their explananda. But this does not mean that understanding individual brains and storage capabilities is useless or that it lacks a significant payoff in understanding language. Thus I reject Cowley's evaluation (p. 6) that Language is "flawed by its cognitivism." Language might be flawed for any number of reasons, but this is not one of them.

I completely agree with what Cowley says at the outset of Section 3.1. that "human thinking uses *aggregates* of resources." Well of course it does. Who could deny this? And I further agree (p. 6) that "while situated and embodied, human cognition is *also* historical, distributed, and non-local." This not only makes sense but it is fully compatible with Language and in fact Language makes this clear in various ways, especially in its discussion of "dark matter."

On p. 7, Cowley makes a brilliant remark with which I could not agree more strongly, "Science itself becomes a paradigm of how cognitive outcomes draw on cultural tools." It is the drawing on cultural tools and social practices that creates the dark matter shared by scientists. And I applaud Cowley's citation of Mace's aphorism (1977: 43) that we should "Ask not what's in your head, but what your head's inside of." However, I only agree with the second part of this! We should ask *both* what is inside our heads and what they are themselves inside of. This is merely another false dichotomy.

Cowley's summary of some of some of my views about Banawá and Pirahã language-culture relations is stellar. I could not have done a better job. But then he claims that this view is incompatible with the construction of a "phonologically structured mental lexicon." But this is a non-sequitur. First, there is nothing incompatible between thinking of language as a cultural tool and arguing that individuals possess their own pieces of this tool, stored in their own brains. Second, there is no statement in Language that the mental lexicon is "phonologically structured," merely that it contains information about form and function.

On the other hand, Cowley is right that if language is a cultural tool it cannot be merely possessed by individuals but must in some sense be "out there," a type of exocognition, of the type that I talk about briefly in Language when I mention that

my personal lexicon of flora and fauna is a very small set of my culture's lexicon. But I do have one according to my own reflections about what words I know and generations of research. Cowley never provides any evidence against this latter view, i.e. that the individual has a mental lexicon.

When Cowley discusses (p. 11ff) phatic communication and information, he rightly criticizes me for saying incorrectly that phatic communication expresses no "real information." What I meant by that infelicitous choice of words was no "propositional information." Perhaps this confusion on my part reveals a bias towards a more Chomskyan view of cognitivism, in spite of my opposition to this view, based on the way that I was socialized in science. That is an interesting topic for subsequent self-analysis that I can fortunately forego here, however. Of course phatic communication conveys social information that is valuable.

But from p. 11 onwards, Cowley loses me in many of his statements, such as "speech is *not* based on conscious experience or word choice…" Without evidence or reference to experimental support or other argumentation, this is a gratuitous remark. Moreover, it is not the case that I advocate this as the basis of language. That is, while I agree with statements like (p. 12) "verbal patterns … [are] anchored by a network of practices," it is not at all clear to me why this would be incompatible with some knowledge of verbal patterns being stored in the heads of individual speakers. Where is the evidence, for example, that this could not be the case? I have certainly seen evidence and articles arguing that it is the case. Again, there is no contradiction in believing both in a people-centered linguistics, which I believe that both Cowley and I do, and simultaneously that individuals learn and store information about their languages.

On page 14, Cowley asserts that "if language is rooted in culturally concerted activity, there can be no pre-given linguistic 'object.'" What does "pre-given" mean? If it means that the individual precedes the social or the social precedes the individual, I agree. If it means that the individual cannot know things and possess knowledge of his language, then this makes little sense to me. Again, no evidence is given.

Recently at a conference on Language sponsored by the Department of Philosophy at Tel Aviv University, zoologist Ilan Golani and his colleagues presented a paper arguing that rats use recursive mental operations in their navigational behaviors. This is the kind of evidence that tells me that animals have brains that encode knowledge and abilities independently of society. Humans also do, even when that knowledge was acquired as members of societies and cultures. That simple fact, that there is no fundamental dichotomy between individual knowledge and socially constructed knowledge and tools, is the only weakness in Cowley's otherwise extremely useful discussion.

Response to Reboul

Between cognition, communication, and culture

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Reboul's article rehearses many of the criticisms that have been raised against my work over the years, adding to this list a few additional criticisms of her own. Her criticisms are by and large clear and to the point. This clarity is a boon to the reader and it facilitates rebuttal, making it that much easier to provide targeted responses.

I believe that Reboul is incorrect in her understanding of a number of issues, some about my book, some more general. Some of the errors in her reasoning are based — in my opinion — on a faulty understanding of basic linguistic concepts. In this regard I see four systematic confusions in her thought. First, she (like Wierzbicka) fails to distinguish vagueness from ambiguity. Multiple but clearly distinct meanings (falling under separate truth conditions) — ambiguity — is not the same as vagueness, the case of an expression having no clear meaning(s). Second, also like Wierzbicka, Reboul conflates semantics and syntax. She seems to think that the ability to say something implies the possession of a specific syntactic construction. But a semantic argument is never a syntactic argument. Third, Reboul suggests supervenience as the model of the culture-language relationship, even though Language is clear that the relationship intended is one of *symbiosis*. Her decision to model the symbiotic interaction between culture and language based on supervenience leads to confusion about what is at stake. She also seems surprised by some rather mundane facts about linguistics and anthropology, such as the idea that cultures and languages may change at variable rates. All of these issues are discussed below. Either language and culture could exist without the other, but at very different degrees of complexity from natural human use.

In my reply I will largely ignore Reboul's discussion of the Sapir-Whorf hypothesis, since this has no bearing on the thesis of *Language*. However, it should be said that her survey of work on Sapir-Whorf commits errors similar to her discussion of my proposal of language as a cultural tool. She tries to work in the idea of "supervenience" though it seems equally inappropriate here. Further, she fails to cite a great deal of work on this hypothesis, especially Slobin's (see, for example, Slobin 1996).

This reply is structured as follows. I will first address the basic methodological points of field research that Reboul misses. Next, I turn to consider the empirical points she raises about my work, beginning with the biggest elephant in the room, recursion. I then move on to discuss numbers, quantifiers, and some other objections she makes to my claims for interesting gaps in Pirahã language. In the third section I discuss the inappropriateness of supervenience as a tool for understanding the language-culture nexus. I then discuss culture change, language change, and definitions for important concepts that Language is built on.

Field research is a challenging enterprise and one about which I have written extensively (Sakel and Everett 2012; Everett 2004; Everett 2001). As I have said, it imposes and requires humility, a virtue to be recommended to all, of course, but one the fieldworker cannot avoid:

"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." (Everett 2004, p. 141)

In Sakel and Everett (2012) we lay out a methodology for conducting field research on either well-studied or little-studied languages in any part of the world. Most of the principles we engage with in the book are well-known to experienced field researchers. Here are two that I find germane to the discussion of Reboul.

The first principle is: Do not confuse what people can or do talk about with how they talk about it. Knowing, for example, that a speaker of Pirahã can say something equivalent in meaning to "The man who is tall is in the room", such as "The man is tall. You know him. He is in the room." does not mean that that the language has relative clauses. It just means that they can get across roughly the same idea that English does by means of relative clauses. Circumlocution does not predict structure.

The second is: semantics is not syntax. There is never in any language anywhere a one-to-one mapping between the meanings of a language and the forms in which those meanings are expressed. Saussure made this clear more than a century ago, as did Aristotle millennia ago, in showing the arbitrariness of the form-meaning relationship in signs. We'll return to these, but let me first say what I mean by calling language a "tool."

The definition of tool at *dictionary.com* serves my purposes just fine. A tool is "anything used as a means of accomplishing a task or purpose." Nothing

complicated then, since the claim of Language is that language is primarily a means for achieving community, as well as building and reinforcing culture. It has various other functions as well. In literature, for example, language builds community, provides pleasure, provokes thought, and so on, all according to the culture in which it arises. It carries out the tasks set for it by culture. Given this concept of tool, it should be clear that I have never intended that culture explain all aspects of linguistic form. To see why consider the lowly shovel, a tool for digging. This explains many things about the shovel. It accounts for the particular shape and angle of the blade. It also accounts for the variety of types of shovels, since there are a variety of digging tasks. There are snow shovels, spades, gardening shovels, roofing shovels, and so on.

The specific function of a shovel will thus explain some features of the material the shovel is made of. Some blades are made of flexible material, some are more rigid, etc. The function of the shovel guarantees other things as well, such as the fact that the handle won't be a single layer of cloth.

However, the fact that shovels are for digging won't tell us whether the blade is of steel or iron or hardened plastic, nor will it predict whether the handle is made of wood or steel. There are often cases where multiple forms can fulfill roughly the same function. Just so, claiming that language is a cultural tool does not tell us that language supervenes on culture. As I make clear in Language, in fact, culture is but one of the external forces that shapes language. Language is no more supervenient on culture than culture on language or language on the vocal apparatus. There is no contradiction in saying that different components of languages may have different sources.

Reboul is also correct to criticize me for not making it clear what I mean by cultural niche, though again the simple dictionary definition is the basis for what I intend by this term. "… the position or function of an organism in a community of plants and animals." Humans are animals that live in communities. So the cultural niche focuses on the relationships between the community that are based on values, knowledge, and other components of culture, as defined in Language. Language does not supervene on these values, any more than a rabbit supervenes on prairie brush. It adapts to it, interacts with it, shapes it and is shaped by it, as any evolved item interacts with its niche. Like an ecological niche, a cultural niche is a symbiotic creation.

Now I want to turn to consider some of my more controversial empirical claims that Reboul's objects to, beginning with recursion. The claim that Pirahã lacks recursion has provoked considerable controversy and Reboul makes it clear that she is very skeptical about my claims in this regard.

From the outset, however, it is important to be clear on an important fact, namely, that the thesis of LCT is unaffected whether or not I am right about

recursion in Pirahã grammar. If it turned out that Pirahã did have recursive syntax after all, this would not affect the validity of the arguments in LCT against a universal grammar or those in favor of the cultural shaping of grammar. Pirahã is but one example in the book, howbeit an important one. Nothing crucial for Language hinges on whether or not Pirahã syntax is recursive.

Moreover, in my debate with Nevins, Pesetsky, and Rodrigues (2009 (NPR); Everett 2009), referenced by Reboul, I have made it clear that even if they are right in every point of their analysis (which is almost entirely a restatement of the analysis I proposed originally in my 1983 PhD dissertation) that Pirahã has embedding, they would have demonstrated only a single level of embedding in Pirahã. Embedding is not recursion, however. So neither Everett 1983 nor NPR falsifies the claim that Pirahã lacks recursion.

However, it turns out that there is plenty of evidence that Pirahã lacks both recursion and embedding. To quote a recent chapter on Pirahã syntax by Jackendoff and Wittenberg (2011), based on their independent analysis of a much larger set of data than Nevins, et. al. looked at:

> "All the constructions we have discussed can be accommodated by a limited embedding *[embedding is not recursion, DLE]* phrase grammar with parts of speech and morphology. If Everett 2009 is right, and each clause is a separate sentence, then the language reduces to a flat phrase grammar." *[i.e. non-recursive, DLE]*

Jackendoff and Wittenberg even go on to argue in a subsequent chapter that the Indonesian language, Riau, also lacks recursion, in fact displaying a sentential syntax even simpler than Pirahã's. But why is it important whether Pirahã, Riau, or any other language lacks recursion? Because this undermines the view of innate grammar advocated in recent years by Chomsky and his colleagues.

In 2002, Marc Hauser, Noam Chomsky, and Tecumseh Fitch published a paper in Science entitled "The faculty of language: what is it, who has it, and how did it evolve?" in which they argued that the grammars for all human languages are recursive, such that "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." (Hauser et al., 2002, p. 1571).

The authors did not define recursion in this article. In fact, after I published my paper (Everett 2005) arguing that Pirahã falsified their claim, Chomsky and several of his followers responded that the Hauser, et.al. paper did not mean recursion in the general sense, but Merge, in the highly theory-internal sense of Chomsky's most recent theory. Interestingly, Chomsky (2012) has claimed recently that Merge can be limited by stipulation so as to produce sentences without embedding. Setting aside that such a stipulation would render the claims of recursion empirically vacuous, Merge doesn't match the discussion of the original Hauser,

et. al. article. For example, only unrestricted recursion, but not Merge, guarantees that there is "no arbitrary upper bound to sentences." Since I show that there is an upper bound to Pirahã sentences, Chomsky's recent changes to Merge seem specifically intended to avoid the implications of this finding — but at the cost of empirical relevancy for his theory.[1]

Recursion has been defined in many ways. One of the best surveys of recursion is Luuk and Luuk (2011). Others are found in the collection (van der Hulst (2010)) of articles on recursion that emerged from the first-ever interdisciplinary conference on recursion, which I organized at Illinois State University in April of 2007. The basic idea, clearly the one intended by Hauser, et. al. is: "Recursion is an operation that applies to its own output."

But linguists often claim recursive status for kinds of structures, rather than processes, such as "recursion is shown by one syntactic category embedded within another of the same type", e.g., an S within an S; an NP within an NP

- Conjunction: S — > S Conj S; NP — > NP Conj NP; VP — > VP Conj VP
  • [[John and Mary] and Bill]
- Clausal complements: S — > NP VP; VP — > V CP; CP — > Comp S
  • [John thinks that [Mary said that [the girl cried]]]
- Possessives: NP — > NP 's N; NP — > N
  • [[[[John]'s mother]'s brother]'s house]
- Possessives: NP — > NP of NP
  • [the house of [the brother of [the mother of [John]]]]

There is evidence in Pirahã against each of the different perspectives above. I discuss here a few of the predictions that such an analysis makes that can be independently tested in the grammar of Pirahã. These are given below and taken from Everett (2012).

Before citing these arguments, it is important to answer Reboul's concerns here. She claims that my arguments for the significance of certain gaps in Pirahã should be rejected by three lines of reasoning:

> "*the first, and most obvious one, is to contest the reality of the gaps, i.e., to contest the validity of Everett's linguistic analyses; the second one is through the distinction, made by von Fintel and Matthewson (2008) in their paper about semantic universals, between universality of syntactico-semantic forms, and universality of (weak) effability, based on the fact that despite syntactico-semantic variability, there is universality of translability through convergence on the expression of identical truth-conditional propositions; the third one is to contest that the immediacy of experience principle explains the specific gaps Everett claims to have found in Pirahã.*"

I answer these each in turn, beginning with the "reality" of the gaps. I then turn to the issue of translatability and compositionality, then on to the matter of whether immediacy of experience can explain the gaps.

The issue of whether the gaps I discuss are real or not can only approached by comparing the predictions of my analysis with others. There is in fact a great deal of evidence that I have adduced for my analysis that has not yet been responded to in a single of the many critical responses that have been published over the years (for more data see Piantadosi, Stearn, Everett, and Gibson 2012) . The facts that are predicted by the analysis that Pirahā lacks recursion — but by no other analysis — are (Everett 2012):

First, the lack of recursion correctly predicts that factive and epistemic verbs will be absent (though there is a — crosslinguistically common — use of the verb 'to see' for 'to know'). This prediction is made because if Pirahā lacks recursion, then there is no way to express factive verbs as independent verbs, since these would require a complement clause, requiring embedding and thus, ceteris paribus, a recursive rule in Pirahā syntax. Pirahā expresses such notions via verbal suffixes, consistent with the 'no recursion' hypothesis, not with complement clauses.

Second, Pirahā has no marker of subordination. This is also predicted by my hypothesis, because if Pirahā lacks recursion, there is no subordination to mark.

Third, Pirahā has no coordinating disjunctive particles (e.g. 'or'). The absence of explicit markers of disjunction is predicted by my hypothesis, since disjunction entails recursion.

*Fourth, Pirahā has no coordinating conjunctive particle (e.g. 'and'). There is only a more general particle, **píaii**, which may appear preverbal or sentence final and which means 'is thus/simultaneous' (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.*

Fifth, Pirahā has no syntactic complement clauses. If Pirahā has recursion, where is the unambiguous data? I have claimed that it lacks embedded clauses. Others claim that it has them, but they only show that quotatives could be embedding. No work has ever suggested that there are multiple levels of embedding, which certainly would be expected if Pirahā has recursion (modulo Chomsky's recent ancillary constraint on Merge, discussed earlier).

Sixth, Pirahā does not allow recursive possession. The point of Pirahā 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. My critics might be correct to suggest that German, like Pirahā, lacks prenominal possessor recursion. But this criticism is irrelevant, because German has postnominal possessor recursion. Pirahā has no possessor recursion. This is predicted by my analysis, but not by theirs.

Seventh, Pirahã prohibits multiple modification in the same phrase. As I have discussed above and 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.

Eighth, Pirahã semantics shows no scope from one clause into another, e.g. no "Neg-raising." Pirahã lacks examples such as '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. This is also predicted, correctly, to be impossible in Pirahã under my account, since it would entail recursion.

Ninth, Pirahã shows no long-distance dependencies except between independent sentences, i.e. discourse. The kinds of examples that are standardly adduced for long-distance dependencies include:

'Who do you think John believes ___ (that Bill saw___)?'
'Ann, I think he told me he tried to like ___'

On the issue of semantic truth conditions, I discuss those in my reply to Wierzbicka and below. Let me focus here on the issue of compositionality. Reboul cites von Fintel and Matthewson (2008):

> *"A language which lacked recursion would potentially not have need of compositionality. What would such a language look like? Its sentences would not necessarily consist merely of structure-less strings of lexical items; it is possible to construct non-recursive phrase-structure rules (…). However, in the absence of recursion, the number of possible sentences would be finite. This would mean that a child could potentially simply memorize lexical items and structures along with their corresponding meanings(…)"*

Yet as I have already discussed in Language, this does not follow at all. Sentential recursion is not the basis of semantic compositionality, except in certain theories such as, arguably Montague Grammar, Minimalism, and a couple of others. Consider the three sentences that I use as the title of chapter eleven of Language: "You drink. You drive. You go to jail." This perfectly grammatical set of English sentences allows for semantic compositionality, allowing for the natural, conditional sense of the utterances, i.e. "IF you drink AND you drive, THEN you will go to jail." The point of this example, ignored by Reboul, is to show that semantics and syntax are not in one-to-one correspondence and that compositionality of meaning is not dependent on recursion in the syntax. The objections of von Fintel and Matthewson have no force outside the confines of their particular theoretical assumptions.

This does raise a related question, though, one that several critics have raise regarding the possibility of a language without sentential recursion. Chomsky has long urged upon us the idea that language is "the infinite use of finite means." In other words, humans have infinite grammars/languages but only finite brains. Yet there are several serious problems with this idea. I discuss two here. The main one is that the assertion is based on a fundamental confusion of Chomsky's. Second, this claim ignores the role of discourse, seeming to find the nonfiniteness of language in sentence structure rather than discourse structure.

Paul Postal (2012) demonstrates that Chomsky's idea that language makes "infinite use of finite means" is incoherent:

"NC's response was simply duplicitous since the statement that 'there is a perfectly coherent sense to the notion of infinite use of finite means' was a nonsequitur. Recursive functions are set-theoretical objects and their existence thus implies nothing about brains having infinite outputs. What NC needed to show but obviously could not was not how an abstract system could recursively enumerate a nonfinite collection of abstracta, which had in no way been questioned. His burden was to demonstrate how every element enumerated by such a system could be biological, part of a specific aspect of the physical world. Instead, NC just refused to respond to the questioner's actual a propos and entirely distinct point.

The incoherent idea that NL is both biological and infinite cannot be reconciled via appeal to potential infinity, as in various NC statements…"

The point Postal makes in this paper, my wording, is that Chomsky attributes set-theoretic properties to human tissue. It is a fundamental category mistake, such as when Chomsky claims that this or that property of syntax is a property of the brain (if syntactic structures were a property of the brain, for example, we would expect to see syntactic trees in cat scans or more finely tuned instruments.).

Moreover, as I discuss in *Language* and in a paper I presented at SUNY Stony Brook several years ago, the real source of creativity in language is not sentential syntax but discourse. Pirahã is a non-finite language in the sense that they can form discourses and conversations of indeterminate length on any topic they choose (within cultural constraints). But it has a finite syntax if it lacks recursion. And, as I also made clear in the Stonybrook lecture, Chomsky's idea is incoherent (a la Postal).

The idea that the ability to say or understand certain things is dependent on specific syntactic or lexical structures is deeply misguided, but it is common enough among many researchers. For example, Dr. Jill DeVilliers (2003), states, 'Before the possession of the appropriate grammatical machinery and key vocabulary (such as the mental state verbs, believe, think, etc.), children may have a range of important understandings of both their own and other people's mental states, but the explicit understanding of the content of false beliefs is not possible.'

This is a very strange idea for two reasons. First, if there is anything linguistics has taught us over the past 150 years, at least, it is that semantics and syntax are independent in many important ways. Second, this idea is immediately falsified even in English, as I point out in *Language*.

For example, one can say "John has gray hair." The truth conditions for this sentence depend on whether John does or does not have gray hair. But if one says "Bill says that John has gray hair", the truth conditions for this utterance are based on whether Bill said this or not. The color of John's hair is now irrelevant. De Villiers claims that this is because of the recursive [sic] structure of the quotative and that without this structure children cannot learn others' points of view. But now consider "John has gray hair. Or so Bill says." The truth conditions seem equivalent here to the embedded structure earlier, yet there is neither embedding nor recursion in this example, immediately falsifying the claim that structure and semantics are linked in the way proposed.

The situation with regard to quantifiers, numbers, etc. — issues that exercise many researchers, including Reboul and Wierzbicka, is similar. The ability to talk about something is affected by but not completely dependent on linguistic structures.

For example, in my reply to Wierzbicka I address the problem raised once again by Reboul's reply, namely, that the fact that the Pirahãs have words that can be used in contexts similar to Indo-European quantification does not mean that Pirahã has quantifiers. Rather than repeat my arguments here, I refer the reader to my discussion in this same volume of Wierzbicka's objections. Like Wierzbicka, Reboul fails to recognize the distinction between vagueness and ambiguity. This applies to Pirahã numbers, quantifiers, and so on.

Take as an example Reboul's discussion of the Pirahã word **báaiso** which, as I pointed out in Everett (2005) superficially appears to overlap with the English word 'whole' or 'all.' She says,

"One Pirahã produces an utterance translatable as "The foreigner will likely buy the whole [báasio] anaconda skin". The foreigner, in fact, buys only a part of the anaconda skin. However, it is still possible to say truthfully, according to Everett, "Yes, he bought the whole [báasio] thing", which, of course, could not be truthfully said, in these circumstances, in English. It is not clear, however, that this is an argument against báasio being the universal quantifier, for it may be the case that, in the second sentence, the interpretation of báasio as part of could be a case of pragmatic loosening (see Carston 2002:27). The question is not whether báasio is always used in the same way as English whole, but whether it can be used in such a way. On Everett's own showing, it is used equivalently to English whole in the first sentence (otherwise, his interpretation of báasio is just faulty: it does not mean whole in either sentence). Thus, there is no reason to think that ʻaió hi báasio ʻoaob-áhá hi ʻogió ʻoaob-áha (the

sentence used to mean that the foreigner has bought a part of the anaconda skin in Everett's example) could not have the same truth-conditions as The foreigner has bought the whole thing in the standard English interpretation."

Reboul's twofold error in this passage is, on the one hand, to confuse 'translatable' with 'means' and, on the other hand, vagueness with ambiguity. This is again a result of failing to distinguish polysemy from vagueness. If you visited the Pirahãs and asked me to translate this utterance for you into Pirahã, this is how I would translate it. This would be the closest equivalent in Pirahã . But it would not mean exactly what it means in English because in Pirahã there is *never* any way to produce an utterance with exactly the truth conditions of 'the *whole* skin.' How do we know this? By the very tests I provide in Everett (2005), namely, that if a Pirahã asked for **báaiso** and you took a slice for yourself in front of him, he would never object that he hadn't received the 'whole thing,' And so on for all tests I have devised. The English and Pirahã expressions are not intertranslatable. Period.

Proceeding further, Reboul objects to my use of the 'immediacy of experience principle' as an explanation for the Pirahã facts:

> *"... if Everett's hypothesis is true, one would expect it to be impossible to say anything like 'The foreigner will likely buy the whole [báasio] anaconda skin', no matter what linguistic form is used. Yet, as Everett himself acknowledges, this is not the case... Everett answered that objection in the case of colors, claiming that*
>
> *[A] property name that generalizes over immediate expressions is an abstraction, a variable. (...) Color terms are abstractions, the descriptions of colors are not. Abstractions violate the proposed principle of immediacy; phrasal descriptions do not (Everett 2005: 642).*
>
> *This seems to shift the immediacy of experience principle from what one can talk about (which would be vulnerable to the weak effability objection), to a prohibition of abstraction principle. But this triggers a strong objection: It is not only color terms that are abstracted from instances, but any common names, from cat or dog to anaconda and fish (indeed, given the variety of species of fish, fish is rather more abstract than cat or dog, corresponding to a concept above the species level)."*

Again Reboul's objection is based on a confusion. In this case she confuses generics ('dog(s),' 'bird(s),' etc) with quantifiers. It is well-known that generic terms are not quantifiers (see the discussion in my article on Wierzbicka). I have addressed the presence of generic terms in Pirahã elsewhere, but let me repeat the argument here. All languages have to be able to distinguish things from events, in one way or another. Without this discrimination language would not work as a tool for any culture. Therefore, culture cannot override this. And there is evidence, in the distinction between generics and quantifiers in the literature more generally, that generics are more basic. They provide a different, less precise, but more essential type of abstraction than quantifiers. So we predict that cultures could differ as to

whether or not they prohibit quantifiers but not in the presence of nouns, along with their attendant generic properties.

Reboul seems to have missed the discussion of the Immediacy of Experience Principle in Language, criticizing its first formulation in Everett (2005). The most recent statement of this is found in Everett (2012):

"The cultural constraint on Pirahã grammar, which I have termed the "immediacy of experience principle" (IEP), is based on the deceptively simple fact that the Pirahãs require evidence (as Missourians supposedly do). The IEP requires, among other things, that all Pirahã sentences be "warranted" by evidence and that this warrant be represented on the verb. There are three relevant suffixes which mark this, known more technically as "evidentials". These mark "hearsay" (someone told you about you're saying, you didn't see it yourself); "deduction" (you see the evidence, but did not see the act, as in "John left + deductive suffix", meaning something like, "John must have left, because his canoe is gone", or "… because I can see his footprints leading off into the jungle"); and "direct observation" (as in "John left. I saw him leave" — where, unlike the English language, in Pirahã the "I saw him leave," part of the sentence would be suffixes).

Nothing can be uttered unless it is in principle warranted by one of these suffixes. The interesting consequence for Pirahã grammar (and theories of human language) is that this culturally-based requirement for evidence makes recursion in the grammar impossible. Recursion is ruled out because any grammatical category (noun, verb, sentence, and so) specified in a verb's meaning (its lexical frame) must be "authorized" by the evidential suffix and only categories so authorized may appear. But a phrase buried within another phrase carries units that are not part of the meaning of the verb in which they are embedded and so they are not authorized by that verb's evidential marker.

For example, the verb "give" requires three nouns (or "arguments"): the giver, the thing given, and the goal of the giving. John (the giver) gave the book (the thing given) to Bill (the goal). It is not strictly grammatical in English to say only "John gave" or "John gave the book." Outside of literature, you have to give all three arguments each time. So there are three arguments required of the English "give".

Pirahã's cultural requirement on evidence allows only three arguments. To say in Pirahã something like: "John's sister's best friend gave Bill's father-in-law's buddy a book" would leave "sister's" and "father-in-law's" unwarranted — these are not found in the verb's required three arguments (giver, given, goal). In Language: The Cultural Tool, I explain in more detail how this cultural requirement for evidence rules out recursion in Pirahã. Although other languages also have evidentials, it is the high priority the Pirahãs attribute to the immediacy of experience principle which underlies Pirahãs unusually strong evidentiality-recursion connection.

So not only does Pirahã represent a severe counterexample to the idea that re-cursion is the principle genetic facilitator of human languages, it also shows that grammar in its most fundamental forms cannot be merely the unfolding of a built-in genetic program, but can be shaped profoundly by the values of the culture of which it is part. This, along with the lack of an FLN and external factors (discussed briefly be-low) affecting linguistic forms render the idea of a universal, innate grammar largely, if not totally, superfluous."[2]

In other words, the effect of culture on grammar is mediated through scope condi-tions on evidential suffixes. This answers Reboul's (and others') objections to the IEP as a constraint on Pirahã syntax.

Turning to another matter, Reboul (p. 3) claims that my model is based on the idea that language supervenes on culture. This supervenience idea apparently fol-lows from her idea (p. 2) that both "Everett's hypothesis" (that culture shapes many aspects of language) and the Sapir-Whorf hypothesis are top-down processes. But they are not. In fact, it is not at all clear what evidence Reboul bases this assertion on. Nevertheless, because of this perspective, Reboul proceeds to relate cultural and linguistic properties by saying that one is supervenient on another (p. 4ff), even though in Language, I go to great lengths to describe the relationship be-tween culture and language as symbiotic. Supervenience is at odds with such a characterization. In symbiosis each member of the relationship shapes the other and it is impossible to say that either supervenes on the other. There is no uni-di-rectionality. It is possible for a culture to have properties (such as valuing a college education) that have zero to do with grammar and vice versa (such as verbs using ablaut for derivational morphology). There is no claim ever in Language that lan-guage is fully determined by culture or vice-versa (as some of the other articles in this issue point out). In Sakel & Everett (2012,) we argue that the field researcher should only attempt to test linkage between independently verifiable features of culture and language, so as to avoid circularity.

Table One. Cognition, Grammar, Culture Connections

Constraint Relationship			Representative Theory
1. *cognition*	→	*grammar*	Chomsky's Universal Grammar
2. *grammar*	→	*cognition*	Linguistic Relativity (Whorf)
3. *cognition*	→	*culture*	Brent Berlin and Paul Kay's work on color terms
4. *grammar*	→	*culture*	Greg Urban's work on discourse-centered culture
5. *culture*	→	*cognition*	Long term effects on thinking of cultural restrictions on certain behaviors
6. *culture*	→	*grammar*	Ethnogrammar; individual forms structured by culture (This is the focus of *Language*.)

On the larger set of relationships linking culture, language, and cognition, in Everett (2008) I provide the following chart and discussion of the relationship between them. What is crucial to observe here is that the columns are not mutually exclusive. Each one is somewhat right. That is because the nature of the relationship between all is symbiosis, not supervenience:

As I pointed out in 2008, we all know that any attempt to understand how culture, cognition, and grammar interact and affect one another must avoid simplistic solutions to the understanding of what shapes the 'human experience'.

The first row in the table above expresses the case in which cognition, which I loosely mean as either the cerebral or mental structures necessary for thought or thought itself, exercises control over grammar. Noam Chomsky has focused exclusively on the effects of cognition, in this sense, on grammar for several decades, proposing the idea of a Universal Grammar as his idea of how cognition limits human grammar.

Row two symbolizes the Sapir-Whorf research tradition, which looks at the grammar-cognition interface from the perspective of how grammar, that is, the way our languages are structured, might affect the way we think.

For the third row, the names that come to mind are Brent Berlin and Paul Kay. Their work purports to show that all cultures' classification of colors follows restrictions imposed by the human brain's physical constraints for recognizing tints, hues, and relative brilliance of colors. This cerebral-cognitive limitation imposes constraints on the classification of colors in all cultures.

Row four represents the perspective of anthropological linguists like Greg Urban of the University of Pennsylvania. Urban's work makes the case that language can affect culture in interesting and subtle ways. One of the examples he discusses that I found particularly interesting concerned the effect of the grammatical constructions passive (such as 'John was seen by Bill') vs. active ('Bill saw John') on the concept of the hero in different societies.

Row five represents the research that investigates how culture can affect cognition. The Pirahã case is a good example. Pirahã's lack of counting is a result of cultural constraints, as we discussed earlier. But this cultural by-product has cognitive effects — Pirahã adults — unlike Pirahã children — often find it very difficult to learn to count after a lifetime spent in a numberless environment.

Finally, the last row in the table represents the research that others, including me, are doing on local and global effects of cultural values on sentence formation, word structure, and sound structure. This is controversial work too and also goes against much received knowledge in linguistics. It is what the immediacy of experience principle, for example, is trying to get at.

Finally, I want to say something regarding Reboul's skepticism of the idea that language and culture can change at different rates.

Let's begin with English. Why do English speakers refer to bovines in the pasture as "cows" but refer to them as "beef" when eating them? One could never say, "I ate cow" but "I ate beef." The answer is simple. The French-speaking Normans ate what the Anglo-Saxon peasants raised. So the French word "beef" was what was used for eating and the Anglo-Saxon word "cow" was used for producing. This linguistic fact is the result of a cultural state of affairs. And the linguistic state of affairs persists even though the cultural state has changed — English speakers are no longer dominated by French speakers.

In the Xingu park of Brazil, there are many speakers of languages from distinct language families that have been brought together over the years by the Brazilian government into a single reservation, much as was done in the US state of Oklahoma in the 1800s and early 1900s. Although the languages remain distinct, the cultures of the Xingu Park peoples have become more and more alike over the years, to the extent that the cultures almost form a single "cultura xinguana," in spite of persistent linguistic differences. In this case, cultures are changing but the languages remain roughly the same, as in the English example above.

In the United States, American Indian languages have been lost at an appalling rate and quantity. Nevertheless, some languages, e.g. Navajo, are still spoken much as they once were, even though the speakers wear cowboy hats, are bilingual in English, and have moved a long ways towards assimilation within American culture.

There are many other examples one could give, but any standard introductory textbook in historical linguistics can provide many examples of linguistic change without cultural change.

The final issue I want to discuss in this reply to Reboul is the objection she raises to *Language*'s central thesis that language is cultural, not innate:

"I would like to end this paper by discussing one of Everett's claims regarding the non biological nature of language, which seems intended as a kind of reductio ad absurdum argument: That, if language is biological, one would expect to find "culture-gene mutations affecting specific languages of the world" (Everett 2012: 42) and these do not exist. In fact, recent findings (see Dediu and Ladd 2007; Nette 2007) suggest that such mutations exist. Dediu and Ladd established a strong correlation between (geographically dispersed) tone-languages and allele frequencies for two genes (ASPM and Microcephalin) in the populations speaking those languages as compared with speakers of non-tonal languages. The interpretation is that these specific alleles would facilitate the learning of tonal languages through better acoustic discrimination. To be quite candid, I think Everett's argument is rather poor, but it is not uninteresting that it may be wrong as well."

Why does she think that the argument is poor? In fact it is an argument raised by Phil Lieberman in his forthcoming book, *The Unpredictable Species: What Makes Humans Unique*. The idea that language (I-language, grammar, etc) is carried by the genes definitely predicts that it is subject to mutations. This is not an argument — it is an *entailment* of nativist theory. And cultures, as Language points out, provide one source of selectional pressure. What counts against nativism of the Chomskyan variety is the clear failure of this prediction. Moreover, the "counter-example" to my claim that Reboul provides merely strengthens my case.

The claim that Reboul is supposed to be criticizing is the idea that one population could, through selection of some genetic features of language, be unable to learn the language of another population. The findings of Dediu and Ladd, if true, far from falsifying my claim support it. This is because they show that evolution can enhance perception by human populations of the phonological/phonetic forms that they commonly use. Their results apparently show that some populations speaking tonal languages become better at perceiving tones than others. But this has no bearing on my claim at all, because *all* languages use pitch in very sophisticated ways. Therefore, this enhancement would benefit all speakers of all populations and could not — even in principle — become the basis for one population losing the ability to learn the language of another. However, this raises a fascinating question. If this is indeed an example of a cultural (speaking a tone language) constraint affecting one's genes, then the absence of the opposite effect, the principle prediction of Chomskyan Universal Grammar — a genetic mutation that would render one population unable to learn the grammar of another — becomes even more mysterious.

Moreover, this genetic alteration is of perception, not of syntax, which are very different matters. In *Language* I make the point quite explicitly that aspects of the sound system of language *are* genetically based — in particular the shape of the vocal apparatus and the co-evolution of the vocal apparatus and aural perception. I distinguish carefully sound systems, especially the perceptual and production apparatuses, from syntax proper. Reboul once again rushes to judgment and fails to distinguish important, clear, but perhaps overly subtle points.

In conclusion, Reboul's objections to my specific analyses of Pirahã, as well as to the proposal that language is a cultural tool more generally are founded on mistaken assumptions and 'urban myths' of linguistics that have been propagated by Chomskyan linguistics since its founding.

Notes

1. Merge is a function that takes two objects (α and β) and merges them into an unordered set with a label. The label identifies the properties of the phrase. In Minimalism, no phrase structure can be formed without undergoing Merge. Since Merge is by definition a recursive operation, no language can exist without recursion. Q.E.D.

For example: Merge (α, β) → {α, {α, β}}

If α is a verb, e.g. 'eat' and β a noun, e.g. 'eggs', then this will produce a verb phrase (i.e. where alpha is the head of the phrase), 'eat eggs'. In Everett (2012) I discuss further problems with the "Merge as what Chomsky means by recursion" idea.

2. I assume that every language shows similar effects of culture on grammar. Pirahã just has some that are particularly easy to see. Another obvious example of the influence of culture on grammar is literacy. As societies adopt a written language, for cultural reasons, their grammars often change. Perhaps, more accurately, they begin to adopt a second grammar — the grammar of written versus spoken speech. Many studies show that written language and spoken language differ in numerous, often profound, ways, such as in the length of sentences, complexity of paragraphs, and so on. The new features of the written language are alterations in our relationship of the way we express our syntax, owing, ultimately, to the cultural decision to write the language.

Language can help us think. Really.

Reply to Jan Nuyts

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I am in substantial agreement with Nuyts in much of what he has to say. And as with the other authors in this volume, I have learned a great deal from Nuyts's criticisms and suggestions. So I will keep these remarks brief, focusing only on our main point of disagreement, namely, whether language can help us think.

Nuyt makes several points in his paper (all using language!) that have caused me to rethink the received wisdom among many linguists that language is a crucial tool for thought or the foundation for thought. However, though Nuyts's arguments are challenging, he has not yet convinced me that language could not nor does not play a role in thinking.

In this reply, I will argue once again argue for the claim of LCT that language is a tool for thought. However, I also attempt to clarify what I mean by this. And I underscore my agreement with Nuyts's point that we need to focus on languages in thought, i.e. natural human languages, and not simply "language-with-a-capital-"L".

To begin, let me say that I mean a couple of things when I say that language is a tool for thought. And there are several things I most surely do not mean. As I make clear in LCT, thought is older than language, not only because it exists in species like canines and felines without language, but because thought clearly preceded language in the evolution of the hominin line. One of my favorite images from the fossil record, discovered in 1976 by Mary Leakey at Laetoli in Tanzania, is from fossilized australopithecine footprints in volcanic ash. The footprints show a smaller and two larger pair of imprints, parents and child perhaps, walking side by side. They stop, turn, probably to look at something, then continue on. Though it is speculative, these footprints might be evidence of a nuclear family. What is not speculative is that the footprints are of a species that had thought and nothing like modern language, in spite of their evolutionary connection to modern humans.

In known human ontogeny we see babies develop relationships and beliefs prior to developing words for those beliefs. This is evidenced best perhaps in

the work of Heidi Keller and her colleagues at the University of Osnabrück in Germany (Keller 2007, among many others).

But somewhere along the way in human evolution language emerged. And language brings several components that are simultaneously useful for and influential on thought. Of course, sometimes language is given too much credit and Nuyts is right to point out the ways in which thought is independent of language. In fact other things not discussed by Nuyt that people have credited to language seem to me to be independent of linguistic ability. Things such as linearity of thought and hierarchy of concepts (as with hypernyms and hyponyms) do not seem to require language. And the principal manifestation of language in humans, speech, almost certainly has little or no role to play in thought, where the components of speech, phonetics and phonology, are disconnected from expression of thoughts so far as anyone knows.

On the other hand, there are clearly ways in which language is a tool for thought or, as Marcelo Dascal (2004) has stated, language is a "cognitive technology." The first way is in helping us to process the world around ourselves more quickly. For example, the psycholinguist Dan Slobin (1987; 1996), has argued in his famous work, "Thinking for speaking," that language affects the way we think. This is of course an extension of the ideas of Herder, Sapir, Whorf, Levinson, Lucy, and many others known as "linguistic relativism."

But linguistic relativism is not the only way in which we see language serving thought. In several articles, various researchers have made the case that the Pirahãs lack any concept of numbers or counting. Some (Gordon (2004), C. Everett & Madora (2011)) claim that this is due to linguistic relativity — the lack of number words impedes the ability to count. But my own view is that *both* Pirahã's lack of numbers and counting are due to cultural values, as I outline in LCT. Regardless of whether the ultimate explanation is culture or relativity, however, the point is that without certain linguistic tools, people cannot think effectively about particular tasks. So in a matching task in which we dropped eight objects into a tin can, each one making a loud sound when it hit bottom, no Pirahã successfully imitated our behavior with ten objects. Why is this? In Frank, Fedorenko, Everett, and Gibson (2007), we argued that this is because without numbers it is too difficult to remember all and only the members of groups of objects larger than three to successfully imitate the behavior of the researcher by precisely matching the number of objects dropped into a can, etc. by the researcher. In this case, possessing number words clearly helps the brain perform particular tasks.

Also, in LCT, I talk about the importance of language for expressing and thinking about intricate and recursive points of view. For example, consider the following set of utterances: "The moon is made of green cheese." "Bill believes the moon is made of green cheese." "Peter believes that Bill believes the moon is made

of green cheese." "Sam believes that Peter believes that Bill believes the moon is made of green cheese." With recursion, we can continue to embed one sentence into another as in these examples. Neither the production nor the interpretation of such examples is particularly difficult. But take another language, say, Pirahã, and things change dramatically. Lacking recursion, Pirahã would have to say something like the following English phrases: "The moon is made of green cheese." "The moon is made of green cheese. Bill believes this. Peter believes THAT. Sam believes THAT."

Maybe there is a way to get matching interpretations out of these sentences. But I do not think so. Moreover, if we continue to extend these examples then the recursive structures will "win out" in ease of production and interpretation over the paratactic, non-embedded strategy in the English examples below and in languages like Pirahã and Riau that lack recursion. In this sense, linguistic structures facilitate expression of certain ideas. Thought there is no experimental evidence I am aware of to support this particular set of observations, I would bet that experimental evidence would confirm a greater degree of difficulty in understanding the second set of sentences, even by native speakers of a language without recursion.

Thus Nuyts's thesis is not only at odds with the examples above and many others in LCT, it is also in direct opposition to a large body of Whorfian and Neo-Whorfian literature. This does not mean that he is wrong, of course. But it does mean that his work is not done and that much of the effort before him is uphill.

Linguistics, Truth, and Culture

A Response to Jens Allwood

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Allwood's major objections are that I have failed to adequately define my terms and that my discussion of utility vs. truth is not consistent with my own practice.

As Allwood points out, I do not completely agree with either the definitions of culture by Tylor or Geertz. But I do make clear in Language that I adopt Geertz's definition with the modification of adding values. I would change it as follows (my addition to Geertz's definition is given in italics, the word *values*):

"A[n] historically transmitted pattern of meanings, *values*, embodied symbols, a system of inherited conceptions expressed in symbolic forms by means of which men communicate, perpetuate and develop their knowledge about and attitudes toward life."

So what do I mean by values? Just the following (paraphrasing the definition offered in thefreedictionary.com: "A value is a standard, principle or quality considered worthwhile by a society." The ranking of values, crucial to my understanding of culture (incorporated into Geertz's definition as "patterns of values") is understood as cultural practice showing the relative priorities between values (not all values will be so related). This idea of ranking is clearly influenced by Optimality Theory's notion of ranking in grammar, itself borrowed from physics and applicable in most domains.

This done, let me say what I mean by distinguishing utility from truth. I loosely adopt Rorty's views. I believe that what I am trying to do is to justify my beliefs via arguments. I come up with ideas and hypotheses and I construct a series of arguments to justify these ideas. Whenever someone justifies their ideas via argumentation, they have participated in a social practice. It adds little or nothing to this exercise to then call the justified ideas "true." Truth does no work here. In fact it is counterproductive. As Rorty points out, if something were "true" it would never need to be revised for any future audience. But the history of human learning, socially or individually, is one of revising ideas that were once considered

settled and well-justified. Rorty's point, much like William James's, is simply that truth adds nothing to this. It is a vestige of religious belief. Nothing more.

So I do the best that I can do to justify my beliefs, ideas, and proposals following the social practices of a particular scientific discipline or society. Of course, I follow certain social practices in arguing for my ideas, such as the practices of linguistics or anthropology. Yet I am not a relativist. It certainly doesn't add anything to my motives to say that I am looking for truth rather than merely understanding. And the "understanding" I seek is just enough to use the ideas in ways that I find beneficial. Again, there just is no fact of the matter when it comes to truth.

And this is not circular. Because this view does not commit me to its own truth. If you disagree with me fine. That doesn't mean that I think you fail to perceive the truth of my views. It means, rather, that you were not convinced by me as I am not of you. There is no external standard other than society and our goals. Do you want a rocket to take people to the moon? Then if it gets to the moon (and back, hopefully) then your formulas were useful. Were they also true? If it makes you feel good to call them so, fine. But such metaphysics adds nothing to my scientific practices. Or to yours.

Allwood calls me to task in his comments for not talking about some things which he must have overlooked, since I did discuss these in the book. For example, he says that language "is both a natural and a cultural tool (p. 3)." I make the distinction between biological vs. cultural tools and I argue that the shape of the human vocal apparatus is a biological tool for a cultural purpose. I argue that the platforms upon which language rests are natural not cultural. So in this sense, Allwood's suggestions are completely harmonious with Language.

Moreover, on p. 3, Allwood criticizes me for not discussing the relationship between convention and culture. But there is a long discussion in Language on convention as part of culture, so there is no force to this objection I can see.

Allwood further objects that it is "a bit disappointing (p. 4)" that I do not offer evidence for the partial list of cultural values and practices I offer that children learn prior to learning language. There is a large research program on such matters, however, which I reference in the book. The best place to enter this literature is Keller (2007). All of my claims are supported by the research of Keller and her colleagues at the University of Osnabrück, among other places.

Allwood further objects that I have a bias towards written language over spoken language. This is very hard for me to grasp. I have spent most of my adult life studying agraphic languages and have never published anything based on written vs. spoken language that I can recall. Certainly there is nothing in Language that would privilege written over spoken language, so I cannot accept this criticism.

His discussion of Peirce is illuminating. I will say that his reading of Peirce strikes me as insightful and instructive. Peirce is clear that things other than

footprints are indices, such as proper names. Although I am not sure that Peirce's own definition including footprints and proper names is a consistent one, I nevertheless accept Allwood's criticism here. No argument in Language relies on the absence of indexes from human languages, however.

Elsewhere, Allwood disagrees with the idea that all human meanings can be communicated by language. This depends on one's definition of meaning. In this case I meant propositional meaning. Of course, as he writes, there are other kinds of meaning, such as emotions (depending on how far one wishes to stretch the definition of "meaning") which cannot be expressed by language. I agree.

In his Section 2.2.2. on the "Role of Context", Allwood again asserts that I have a narrow view of language, avoiding the role of context. This I also don't understand. Language is a book about how language is shaped by contexts, cultural and situational, among others. And I discuss a contextual idea that I consider to be absolutely essential to understanding language and culture, namely, the "dark cognitive and cultural matter of discourse," what is left unsaid because it is known contextually. He and I have no disagreements about context.

Allwood believes he has found a contradiction in my writing on ambiguity and vagueness where I state on p. 50 and 55 that they are defects of language and on pages 221–224 that they are useful. They are both. Ambiguity and vagueness can obscure communication either deliberately or nondeliberately. In the former case they are advantageous to the speaker and thus are useful. In the latter case they often render the speaker's message unclear in ways that are not desired. This is an example of how they can be defects in communication.

In his discussion of my claim that the "function of language shapes its form" Allwood criticizes me for not noticing that there are forms without functions. But his examples do not work. Word order is a vital function and is used for many purposes, regardless of its origin. That is, some aspects of form may originate for one purpose and then be coopted for another. But there is no component of language for which it can be shown that there is no function. He also misconstrues what is meant by the claim that greater information is manifested by greater length. From the fact that "Yes" means one thing and "Yes, yes, yes, yes" means something more, it does not follow on anyone's account that the latter must carry four times more information than the former. Information is not one to one but is based on contrast as well. However, if you said "I love you so so so much" and I responded "But I love you so so so so much", then arguably my response carries more information.

The most important error that Allwood makes in interpreting my ideas however comes when he says that "it remains entirely open (and possible) whether some of the differences between languages (for example, in complexity) could be linked to genetic differences between the speakers of a language." This absolutely does not follow from Language and in fact Language predicts the opposite. The

reason that this does not follow is simple — there are no genes for language so there is nothing that natural selection could favor in this regard, without affecting other components of cognition. It is possible that genetic defects of some sort could spread throughout a population rendering it cognitively impaired and that this could affect language. But if there are no genes and no biological basis for language, there is no way for natural selection to form languages that are more complex or less complex than others, etc. These would all be cultural artifacts and the speakers of any language will always be able to be able to learn any other language, at least as children. What Allwood mentions here, however, *is* a prediction of Chomsky's Universal Grammar, which claims, in spite of the absence of evidence, that there is a genetic basis for human grammars.

Allwood seems puzzled by what I mean by tool when he says "If these [gender, case, definite articles, etc., DLE] are serving specific cultural purposes in the languages and cultures that have them, what are these purposes, and are we allowed to conclude that these purposes do not exist in the cultures that don't have them?" Although this is a reasonable set of questions on the one hand, on the other hand it seems to insist that for a culture-based model of language to make sense, it must provide simple answers to all questions. In fact the opposite is the case in a serious theory.

If we discover that a language lacks definite articles there are several potential reasons why. First, the function of definiteness may be expressed elsewhere in the grammar. This is the case of Piraha, for example, where telic aspect usually carries with it (and this is not uncommon) an implied definite interpretation for the verbal object and, potentially subject, while atelic aspect implies an indefinite reading. At first blush, therefore, it may seem that culture has nothing to do with this. But as Enfield (2002) shows, there are in fact times when culture can force the morphological vs. the syntactic expression of a particular category. Is that the case in Piraha? I don't know, frankly. The theory that language is a cultural tool, as I have stated many, many times does not entail that all aspects of language are cultural. To establish a link between culture and grammar one would have to first establish independently the structure in question then link that to a particular value, also independently established, of the culture. Moreover, cultures may have exactly the same values but rank them differently. This is the case in Amazonia, for example, where many languages have something like an "immediacy of experience principle" along the lines I suggest for Piraha, but where Piraha (also discussed in Language) ranks this value higher, producing different grammatical results from the same principle in surrounding languages. There is nothing trivial about this.

In later pages, Allwood claims that if language is a tool it is very unlike other tools. This doesn't follow. A careful reading of Language reveals, I believe, that language is exactly like other tools in allowing its users to solve a problem that they

could not solve without it, i.e. communication and building communities. Since these are crucial to our survival, it is a tool we can never live without.

In a few places in his commentary, Allwood seems to confuse "ecology" with "culture." While my theory may ultimately be compatible with ecological/materialist based theories such as that of Marvin Harris, it is crucial not to confuse ecology and culture. I make no claims about the influence of ecology, surroundings, etc. on language. So the fact that the Tenharim (Tupi-Guarani) live in almost identical surroundings very close to the Pirahas does not mean that their values and the ranking of those values is even similar to the Pirahas. My view of culture is that it is, as Geertz urges, largely a mental phenomenon, a decision to make some things, concepts, behaviors, and so on, more or less important than other things. The environment will have a role to play in this, but it underdetermines culture and is not causally implicated in many cultural values.

Allwood suggests that computer languages are more tool-like than human languages. But if the thesis of Language is correct, i.e. that language and culture form one another symbiotically, computers do not have languages, they have language-like codes written for them with humans who do have languages. And the same goes for most invented languages like Esperanto. If it has native speakers with a distinct culture that is one thing. Otherwise it is a language-oid creation and not a language.

Allwood also claims, at the beginning of his Section 3.1. that my view of language is still close to Chomsky's purely formal notion of language because I appeal to Shannon's view of communication. This doesn't follow at all. Nothing in the Shannon view of communication is incompatible with information flowing between hearer to speaker simultaneously, nor with multiple channels of information flow, which Language discusses at several points. The Shannon diagram that I provide is simply a breakdown of one "phase" of the communication problem. I am, contra Allwood, in no way committed to the erroneous view that dialogs are not co-activated and shared. This does not follow from anything I have said. I give many examples of spoken language and make it clear throughout Language by example after example from field research that spoken language is the primary object of study for linguists, not the written language. However, Allwood is correct to call me to task for saying that phatic communication carries little "real information." I was wrong and he is correct to say that phatic communication definitely carries information. What I meant was that phatic communication carries little propositional information.

Communities never develop prior to communication. But they can develop prior to language. Allwood at times seems to think that when I talk about language I mean all communication and vice-versa. These are distinct. Language is a highly specialized form of communication. Also, Allwood puzzlingly criticizes

me for omitting reference to relevance of utterances in my account of language and culture. It is true that I do not discuss the importance of "relevance" (as, say, in the sense of Sperber and Wilson (1986)), but it is completely compatible with my account and of course I expect that among the many cultural constraints on language relevance will play a major role.

Allwood claims in the beginning of section four that "speech is a kind of language." I disagree. Speech is a *channel* for language. It is not language nor a type of language. It is a vehicle for language, as the various modalities in which language can be expressed show clearly.

In Section 5 he claims that many components of the platforms that Language discusses, intentionality, figure-ground, contingency, etc. are simply subparts of one another. Well, they are clearly related. But this doesn't mean that they should be reduced to one another. To take one example, Searle's (1983) theory of intentionality, it is extremely difficult to see how this could be reduced to a figure-ground distinction. Both ground and figure objects can be separate objects of intentionality while remaining figure and ground. Figure and ground play roles in discourse that intentionality does not. And so on. Contingency doesn't reduce to background knowledge, since it is largely encoded in the brain as statistical generalizations. Google search, for example, uses contingency statistics but not background cultural information.

Later on in his discussion, Allwood says that "Everett claims that 'animal communication' in the wild uses no signs … Everett realizes that animals might have signs, but he does not actually tell the reader which alternative in the end he believes." I think it should be clear from Language (and if it isn't this is surely my fault, not the reader's) that I am referring to the subset of signs Peirce referred to as symbols. Animals do not show those in the wild.

Near the end of his paper, at the beginning of section eight, Allwood quibbles with my view that Chomsky has had more impact on linguistics than anyone else. He mentions many worthy names. Several of them perhaps more insightful or just plain better linguists than Chomsky. But this does not alter my judgment. Pick up any citation index for the past 50 years and citations of Chomsky on language will greatly surpass citations of Jesperson, Panini, Plato, etc. "Impact" is a social term. It is not a question of intelligence or importance but of social effect. In that Chomsky is still unsurpassed (unfortunately!).

My conclusion is that Allwood raises many useful points and has helped correct errors of omission and commission in my thought at several points but that overall his criticisms result from the Whorfian effect of his own theory on his reading of Language.

Understanding others requires adaptive thinking

Response to Wierzbicka

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Introduction

Who knows what human nature is, outside, perhaps of a few of the world's greatest writers of literature? Who knows what the "right" theory of human language will turn out to be?

Language-using primates ought to realize that we are a recently evolved species whose cognitive limitations and variations are not themselves known, much less their explanation. More generally, we are unable to determine whether there is a solution for any particular problem in any domain that we set out to solve. We can only work at our concerns a little bit at a time, stepping back occasionally to look at the work that is emerging. In this sense, the task of the scientist is reminiscent of the sculptor's except that we often do not know in advance what the thing we are sculpting might actually look like. But we chip away until we think we have what we were looking for. It just turns out that others will always come along later and cut away more of the stone, considering what we have done partial at best. Some may plaster pieces back on, saying we cut away too much.

Prof. Anna Wierzbicka's article "Understanding others requires shared concepts" criticizes my analysis of Pirahã and it implications for her theory of Natural Semantics Metalanguage (NSM). By writing so explicitly and clearly about her disagreements with me, she has done the reader a favor because it becomes possible for me and for the reader to compare clearly what is at stake in the two accounts.

Wierzbicka's objections to my work have to do with her commitment to the same kind of universalist a priori knowledge that Chomsky appeals to for his notion of Universal Grammar. Whereas Chomsky develops a theory of universal structures, Wierzbicka's work is reminiscent of the Generative Semanticists in espousing a theory of universal meanings. In that sense, her work is diametrically opposed to the central thesis of Language, namely, the proposal that understanding

human language requires careful study of language in its cultural context, guided by central ideas, but abandoning non-empirical assumptions about what all languages must have in common. This leads her work into the same reasoning cul-de-sac typical of universalist theories. Because she assumes the very same universals that she wishes to prove, she constructs a circularity that her adherents will have a difficult time breaking out of.

One of the least empirical of her theoretical assumptions is the fuzzy idea that there is a "psychic unity of humankind." If by this she means that all humans share some of the same cognitive characteristics and bodies with roughly identical physiologies, I agree — who could not? In fact, this is the point of what I say in my chapters in Language about the "Platforms" for language and "How to build a language". All humans have to be built in a certain way for language to be possible and languages have to be built in certain ways for communication to be possible. But the goals and bases for language are grossly underspecified and thus the empirical task of the linguistic field researcher is formidable, as Sakel and Everett (2012) make clear.

Wierzbicka's theory and argumentation are equally problematic. First, she insists on a set of universal concepts without any empirical anchoring. Second, her paper is teeming with misunderstandings of semantic field research and the confusion of basic semantic notions, e.g. "polysemy" with "vagueness". With regard to the former, her failure to understand truth-conditional arguments for the establishment of quantifiers, numbers, and the like renders her theory and this article in particular less useful than they might otherwise be. Her confusion with regard to the latter seems to arise at least partially from a common mistake among linguists with minimal field experience, namely, the idea that a semantic analysis of a language may be based on translations of the vernacular rather than on the vernacular itself. Sakel and Everett (2012) discuss this type of methodological problem at length and offer practical suggestions on how to do the kind of semantic field research that Wierzbicka here seems to misunderstand.

I want to begin with Wierzbicka's central criticism of my work, as I understand her, found in the following (p. 2):

"The result is a combination of exoticism and Anglocentrism which doesn't do justice to Everett's long and intimate engagement with the Pirahã people and their language. Sadly, it blinds him to what Franz Boas called "the psychic unity of mankind", reflected in the common semantic features of human languages…"

It is worth taking a brief detour to consider this notion of the "psychic unity of mankind" because it shows itself in a number of theoretical approaches to human cognition, language, culture and so on. I think that it is not a particularly helpful notion, except at a level much more general than Wierzbicka supposes. And its pedigree is not quite as she describes it.

The "psychic unity of mankind," is an idea that did not originate with Franz Boas, as Wierzbicka claims, but rather with Adolf Bastian (1826–1905) the German polymath (ethnographer, psychologist, mythology, among others). Bastian's work was hugely influential, impacting Boas's ideas on ethnography, inspiring Jung's theory of "archetypes," and sparking Joseph Campbell's quest for pan-human myths.

Bastian proposed this "psychic unity" as the basis for what he expected to become a science of human culture and consciousness. There were two parts to Bastian's psychic unity claim. I agree with the first one, but reject the second one. Wierzbicka's quarrel with me clearly concerns the second part of Bastian's claims. First he argued that the mental acts of all humans result from physiological platforms common to Homo sapiens. This seems indisputable, modulo the kinds of genetic change influenced by culture that I refer to in Language. Humans are roughly identical physiologically. But as I develop the idea of platforms in more detail in Language, humans share more than their physiology. We all have a theory of mind, a well-developed intentionality, a desire/instinct to communicate and build communities, and so on. These are not concepts per se. They are emergent properties of human brains, shared to some degree with other species, but present in their most robust forms only and always among Homo sapiens.

But Bastian and Wierzbicka go farther (as do Chomsky (1995), Carey (2009), Hauser (2006), and many others). Bastian, the intellectual fountain for Wierzbicka's NSM, claimed that all humans carry in their brains a set of elementary ideas (Elementargedanken). Through these ideas he predicts that all humans will share basic building blocks of thought.

According to Bastian, there is a unity among all humans but this unity manifests contingent local variations, based on history, geography, etc. The underlying unity of mind he referred to as humankind's "elementary ideas". He referred to the local variants of the elementary ideas as "folk ideas" (Volkergedanken). He believed that by collecting ethnographic data through field research that we are able to understand and study valid laws of mental development as they reveal themselves in different regions of the world. Bastian — like so many theorists — believed that our object of study ultimately is not the individual per se, but rather the "folk ideas", the "collective mind", of a particular people.

The more one studies various peoples, Bastian thought, the more one sees that the historically conditioned "folk ideas" are of secondary importance compared with the universal "elementary ideas". Bastian believed that social groups manifest a social "soul" (Gesellschaftsseele) in which individual minds are nurtured.

Bastian further believed that "elementary ideas" should reconstructed from "folk ideas" — the social surface forms of peoples' ideas. Because one cannot observe the collective representations per se, Bastian felt that the ethnographic

project had to proceed through a series of five analytical steps (from Koepping, 1983):

1. Fieldwork: Empirical description of cross-cultural data (as opposed to arm-chair philosophy; Bastian himself spent much of his adult life among non-European peoples). With this Bastian and I are in complete agreement.
2. Study of the folk ideas of specific peoples and how they represent themselves
3. Study of the distribution of folk ideas (e.g., say, commonalities in stories of the jungle among Amazonian groups) in particular geographical regions. Bastian referred to commonalities in a region as "idea circles".
4. Deriving elementary ideas from the study of resemblances between folk ideas and patterns of folk ideas across regions.
5. Builing a larger theory by the study of elementary ideas to arrive at an understanding of the psychic unity of mankind. Ultimately, Bastian believe that this unity derives from an "underlying psychophysiological structure" common to our species.

Obviously, this research program is important. If successful, Bastian hoped that it would lead to cross-culturally grounded psychology.

What Bastian argued for was nothing less than what today we might call a psychobiologically grounded, cross-cultural social psychology. The key to developing this robust science of human consciousness was to collect as much ethnographic data as possible from all over the world. He believed, as I do in fact, that this was essential before folk cultures became too "tainted" by contact with other cultures. Through ethnographic research, he wrote, we can study the psychological laws of mental development as they reveal themselves in diverse geographical settings. This produces a view of how to study humans that has been extremely influential in the modern social sciences and humanities, including Wierzbicka's NSM. So Bastian claimed that sociocultural variation is due both to universal processes inherent human "psychophysiology" as well as to the influence of particular local environments. Both Wierzbicka and I would be in agreement on this methodological point.

In other words, the "psychic unity of mankind" is a very specific kind of thesis about the similarity across our conspecifics. Wierzbicka's NSM derives from Bastian's since, as one might reasonably understand her work, Bastian's Elementargedanken underly her Natural Semantic Metalanguage.

But Wierzbicka's and Bastian's ideas puzzle me. Why would *concepts* be anyone's first guess of what humans share in common? All humans certainly share roughly the same physiology (though there are culturally-induced genetic differences, as I discuss in Language). And we all share the same cognitive and physiological platforms for language (again discussed in Language). The union of the

cognitive and physiological platforms for language and cognition seems robust and quite sufficient to account for similar ways of approaching the world common throughout the species, whatever those turn out to be. It isn't clear that the ideas of Elementargedanken or NSM add anything to our quest to understand humans. This is not to say that they could not turn out to be true, ultimately. But they are a strange beginning point. And indeed by taking them as the beginning point they interfere with one's ability to understand the significance and "genius" of local data.

We know that all humans share certain physiological and cognitive platforms (as I point out in Language — including intentionality, theory of mind, the ability to distinguish foreground from background, contingency judgments, and so on). But concepts are different. Why should we believe that all humans have a word for mother? Of course we know that all humans *have* mothers, but why believe that they would all have a specific word for mother, beyond the utility of the word? Why believe that there is anything other than utility that might explain why so many human languages have words for numbers, colors, quantification, and so on? This is a delicate empirical issue after all. We only do a disservice to ourselves if we assume this from the outset.

Looking for universal concepts found in a universal metalanguage is fraught with conceptual difficulties (even setting aside its foundational implausibility), since the proposed universal concepts themselves are found mainly in the eye of the beholder or the postulates of specific theories. This effort is also problematic because the universality of these concepts is a hypothesis which can only be tested by looking for them in every human language. And if we then reject the counter-examples we find, such as Pirahã, on the grounds that the researcher's ideas violate the "psychic unity of mankind" then we fall into a most vicious circularity.

To my mind the case of Pirahã and other languages is simple. They falsify the Bastian psychic unity hypothesis — including the manifestation of this idea in the NSM of Wierzbicka and her followers. And this is not because Pirahã is so strange. Contra Wierzbicka's claims in her paper, although I have rightly claimed that Pirahã is the first language documented with certain traits and without others, I do not believe for a minute that it will turn out to be the only language to lack numbers or to possess an Immediacy of Experience Principle. Nothing in my account of Pirahã and certainly nothing in *Language: The Cultural Tool*, is predicated on the desire to "depict the Pirahã people as radically different from the rest of humankind." I have, to the contrary, argued for the past thirty years that Pirahã is like every other language on earth in sharing some characteristics with other languages and manifesting other features, especially those influenced by Pirahã culture, that are unique. In this technical sense all languages are unique — what Sapir (1921) referred to as the "genius" of each language — though of course there are

many theoretical generalizations possible at various levels about human languages. (In fact the Immediacy of Experience Principle, which I discuss in Language, is predicted to exist by Daniel Dor's theory of experience and language. Dor (2010) and Dor and Jablonka (2004)).

Wierzbicka's theory and her arguments against my analyses are, moreover, weakened by a lack of familiarity with field research methodology, confusion about elementary concepts of semantic theory, and the same type of Procrustean bed approach to data that is all too common among theories of language universals.

Let me lay my cards on the table. I do not believe that there are no language universals, though we aren't really sure about any at present. Too many are simply "ways of talking" in a particular theory and do not translate from one theory to another. Nor do I believe that no semantic or morphosyntactic universals will be discovered. There simply aren't any known at present, including the proposals of Wierzbicka's NSM.

Moving on to the specifics of Wierzbicka's arguments, there are three serious mistakes that Wierzbicka makes in her eagerness to offer alternatives to my analysis of Pirahã quantifiers, numerals and so on. First, she confuses vagueness with polysemy. Second, she bases her analysis on English translations (belying an even more serious error, namely, that she confuses what people can think with what they can say, a fundamental, howbeit common, category mistake among linguists). Finally, she confuses generics with universal quantification.

Let's look at her examples. She claims that there are clearly times when Pirahã **hói** means 'one.' But her only basis for saying this is that in the free translation I provide I use the English word 'one.' I have been careful in saying that I, as most other field linguists, provide free, non-literal translations of vernacular examples in order to facilitate easier reading. In fact, the words that Wierzbicka translates (with no basis for her translation other than the English translation, an unfortunately anglo-centric and methodologically naive move) as 'one', 'all', and so on are not polysemous but *vague*. What is the difference? To claim that a word is polysemous is to claim that it has more than one interpretation, but that each interpretation is clear. Thus the word **hói** — if it can mean "one" as Wierzbicka claims — in even one of its uses must meet truth conditional tests for "one" in at least those circumstances. But to claim that a vernacular meaning is vague — my claim for the words that I occasionally translate as 'all', 'one', etc — is to claim that there is no precise translation. Such words "smear" across a semantic space and do not ever correspond exactly to what NSM posits as quantifiers, numerals, etc.

How could we tell, for example, whether the English word "ball" is polysemous or vague? As a noun, it seems to be able to mean either "spherical object for throwing or kicking" or "formal party." If I say "The ball was tossed up in the air" I am not referring to a formal party. In this particular context, it must mean the

spherical object. That is, each meaning of a polysemous word is clear and precise once the context is understood.

On the other hand, if the meaning of a word is vague, its boundaries will not be precise. For example, if I say "This room is dark", what does that mean? Or if I say "That is a big animal", what does big mean here?

Now, Wierzbicka argues that the Pirahã word which I translate as "small amount" can mean "one." Why? Her only evidence is that there are times that its reference overlaps with the reference of the English word "one." But in these cases is the Pirahã word actually ambiguous between "one" and "small amount" or is it simply vague and underspecified, such that where English would say "one" and where English would say "small amount" Pirahã has but a single expression that is neither exactly one nor exactly the other? How could we tell? We can, again, only settle such questions by testing for truth conditions or by controlled experimentation. Mere overlap in translation settles nothing here and it is inappropriate to use this as evidence.

All of the experiments by Frank, Federenko, Everett, and Gibson (2008), and C. Everett and Madora (2011), show clearly that there is no circumstance in which the Pirahã word **hói** means exactly one. It is a starting amount in tasks in which speakers know that the quantity will increase. If "one" is the starting amount it will be described by **hói**. But if speakers count backwards, then all speakers use **hói** before reaching one. If every speaker of the language regularly uses the term **hói** to refer to quantities larger than one, then it makes no sense to say that it has the meaning "one." It is a vague term, as claimed in my 2005 paper, and context can narrow but never completely fix its reference.

The same applies to the word **xogió**, which I translated as "all" in my earlier work but as "bigness" or "a lot" in my later work. Is this word ambiguous between "all" and "a lot" or is it vague? Wierzbicka's NSM theory *requires* it to be ambiguous, because otherwise Pirahã has no word for "all" and the NSM is falsified. But this is not my problem. The only way to determine whether there is a word "all" in Pirahã is by truth conditions. I stated this in my reply to Wierzbicka's comments on my 2005 article and it is well-known in semantic theory and semantic field research (again, see Sakel and Everett (2012)). However, Wierzbicka either ignores this or has not come to grips with the significance of truth conditions in determining whether a language has quantifiers or not.

If I tell a speaker of English who happens to be a butcher that "I want to buy all of that pork shoulder" and the butcher replies "OK," then I expect a certain behavior, namely, that the entire piece will be sold to me. If the butcher tells me how much the entire shoulder will cost then cuts a slice off and gives me the rest, I will — quite rightly — feel and claim to have been cheated (unless we agree that this skin may be subtracted from the whole or does not "count" for some reason).

Frank, et. al. (2008) studied numerical cognition, examining their ability to count, to perform matching tasks, their ability to track quantities in other ways, and the semantic range of their number words. These experiments, separately corroborated by C. Everett and Madora (2011), demonstrate convincingly that there is no lexical item in Pirahã that has the truth-conditions of "one". Wierzbicka ignores this careful quantitative research, perhaps because both studies confirm the assertions of Everett (2005) that Pirahã has no words for number.

One phrase that is used to try to get at number in Pirahã (since there is, for obvious reasons, no expression that means literally "how many is this/are there") is **hi go gíiso xaoxaagá**. This means, literally, "he focus this be:present". A free translation would be "what is present here?" In the appropriate context, where quantity is in focus, the Pirahãs will use **hói** to refer to a relatively small amount and **hoí** to refer to a relatively larger amount. The word **báagiso**, which literally means "pile up" or "cause to touch" is used for an even larger amount, roughly corresponding to the English "much/many."

Now Wierzbicka ignores all of the experimental work just mentioned and instead focuses her attention on the translation I offer for these words in a different context, where numerical cognition is not in focus, a context in which my English translation was not intended to be literal. As anyone with translation experience knows, there are times when successful communication requires non-literal translation.

In this non-literal sense, several translations I have given of different Pirahã utterances over the years provide English or Portuguese expressions for which there is in fact no equivalent in Pirahã. There are two examples in particular worth mentioning in the context of Wierzbicka's criticisms. The first is one that she herself cites from my work. The other is similar, but not one that she cites.

As Wierzbicka correctly observes, I did say that I was at one time confused by the existence of two words for "nose", one indicating a non-reflexive form for "nose" and the other a reflexive form of the word. (The latter would be used in "I hit my nose" and the former in "He saw my nose."

I did in fact translate what my language teachers taught me as "two words for nose." But, again, this is a free translation, not the vernacular. A more literal translation would be "a bigger quantity" or "not merely that". The point is that there is *no* exact translation of number between Pirahã and English so one merely makes a decision for the purposes of the free translation as to what is the closest fit in a given context. Of course any field linguist knows that the free translation is not what the analysis is based on, especially in a specific context where I have just made it clear that Pirahã lacks words. This is a fundamental methodological flaw in Wierzbicka's approach, although perhaps I could have underscored more effectively the distinction between the vernacular and the free translation. The Pirahãs

show their ability, documented in all the studies cited above, to distinguish "one" vs. "two" without having words for these concepts. No Pirahã words *ever* meet the truth conditions for "one" or "two" but this does not mean (and why would it?) that the Pirahã cannot easily distinguish one object from two objects (even though the experiments also show that they cannot distinguish four objects from five easily, or six from seven, etc.).

The same reasoning applies to any case where I said that the Pirahãs told me that there are "two" words for something. This is simply an English translation — very loose — of the Pirahã idea of **hói**. It does underscore a point that I have made many, many times in my writing, however, namely, that people can think about ideas that they have no words for — exactly how science is often done, after all (e.g. when physicists discovered "quarks" and only subsequently named them using a word from James Joyce).

In her discussion of my claim that Pirahã lacks universal quantifiers, Wierzbicka appears to confuse generics with universal quantification (a vital distinction for semanticists, e.g. Carlson and Pelletier (1995) and Sarah-Jane Leslie (2008)). The Pirahãs clearly have generics. They can thus talk about classes of things, such as "dogs", "jaguars", "Brazil nuts", and so on. But as is well-known in the semantic literature, the functions and truth-conditions of generics are not co-terminus with those of quantifiers. As I have also pointed out in Language and elsewhere, generics seem crucial from a functional perspective. Quantifiers not so much.

There is one error that Wierzbicka catches in my writing, however. She says that I claim that Pirahã lacks the verb "want" but then give an example of a phrase **Ti xóogabagaí gáihí** "I want that." The Pirahãs definitely do have this word. What I meant in the context, but stated incorrectly, was that Pirahã has no embedding verb "to want". To say, for example, "I want to see this", the Pirahãs use a desiderative suffix rather than a biclausal construction, e.g.

> **Ti kobai-sog   -abagaí   gáihí**
> I   see   -desiderative -frustrated initiation that
> "I almost begin to want that."

Conclusion

Wierzbicka's NSM must be seriously reevaluated in light of the Pirahã data that I discuss in many works. Her arguments against my conclusions are based on fundamental confusions between vagueness vs. ambiguity and between what people can think about vs. what they can talk about. In this sense, it seems clear that her a priori commitment to the NSM and Bastian's ideas of "psychic unity" make it very difficult for her to appreciate the profound nature of linguistic and cultural

variation in the world, leading her to a Eurocentric view of the world that obscures important lessons from cultures her NSM did not anticipate, such as the Pirahãs. In this sense, the NSM faces exactly the same problems as any universalist theory, e.g. Chomsky's, which has no place for the "genius" of individual languages.

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