

REVIEWS

Evans, Vyvyan. 2014. *The language myth: Why language is not an instinct*. Cambridge University Press. xi, 304 pp. (ISBN 978-1-107-61975-3 paperback; 978-1-107-04396-1 hardback)

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This book takes on the question of whether a child's language acquisition depends on an innate informationally-encapsulated language module in the brain, essentially hard-wired grammar, or depends on general cognitive mechanisms. The former view was presented to a lay audience by Stephen Pinker in his 1994 book, *The Language Instinct*, and other publications. E takes on the task of presenting the latter view to a lay audience, primarily by presenting empirical arguments for why the former view is wrong. The title is of course a reaction to Pinker's, though I would have preferred *The Language Instinct Myth* or *The Myth of the Language Instinct*, as the myth is not language itself, but that language is an instinct, and there is already Harris 1981 with the same title.

E admits that most linguists now understand that the hypothesis of an innate informationally-encapsulated language module lacks empirical support and is not empirically falsifiable (see for example Comrie 1981, Ch. 1; Tomasello 1995; Itkonen 1996; Samson 1997[2005] — see also Samson 2001; Eddington 2008), and is therefore untenable from the point of view of empirical science. For this reason it may seem as if E is beating a dead horse in writing this book. However, he argues that many commonly used linguistics textbooks still perpetuate the myth of the innate language module, and there is of course the lingering influence from Pinker's work, with its many unsupported pronouncements about the "language instinct", "Mentalese", and "Universal Grammar", and its unfounded criticisms of linguistic relativity.

Aside from challenging what E calls the "language-as-instinct" myth, E also has a good discussion of the distortion of Benjamin Whorf's view of linguistic relativity, which is also repeated in textbook after textbook (e.g. Holmes 2008, Wardaugh 2002). E also debunks another pernicious myth, the myth that colour term research shows universals of colour naming. That is, this book goes beyond what has already been discussed in other critiques and is for a lay audience. This is then an important book, and one that is much needed due to the pernicious nature of all of these myths.

Although it deals with complex issues, this book will be easily understood by lay readers. Linguists will also benefit from reading this well-researched interdisciplinary book. The book, or chapters from it, can be used as supplementary material in linguistics classes, as it presents clear discussions of interesting topics such as the commonalities and differences between human and animal communication systems, linguistic relativity, the colour term controversy, the questions of innateness and modularity and what they would entail, and of course the functioning of the brain and how it is related to communicative behaviour.

What is interesting is the strong emotional reaction the publication of E's book has evoked from some generative syntacticians (e.g. Adger 2015, Dunbar et al. 2015, Hornstein 2014, 2015a,b). Christina Behme and E have responded together (*in press*) and individually in articles and in the blogosphere (see E's blog 2015a,b,c, and Behme's comments on Hornstein's blogs). We saw this same sort of reaction to Daniel Everett's challenges to the Chomskyan view based on his work on Pirahã (e.g. 2005). It seems likely, as E discusses in his blog (Evans 2015b,c), that this is the reaction of a paradigm that has reached a crisis in the sense talked about by Kuhn (1970). We are already seeing a move away from pseudoscience to empirical science in linguistics,¹ and it may be, as suggested in a *New Scientist* book review (Anderson 2014), the book will be a turning point "that will open people's minds to liberating new ways of thinking about language".

The book is structured around discrediting the "language-as-instinct" myth: after an introduction to the controversy in Ch. 1, subparts of the overall myth are taken one by one in each of Chapters 2–7 and shown to be empirically untenable. An alternative view, of "language-as-use" (usage-based approach) is presented along the way and expounded more fully in Chapter 8.

Chapter 1, "Language and mind rethought" (pp. 1–26), defines the controversy as the question of are we born with language, an innate Universal Grammar (UG), or does it "emerge from use, based on more general mental skills and abilities"? (p. 1). E argues that it isn't innate: a child has to learn the language by constructing it out of the input available in its environment, taking advantage of "general properties and abilities of the human mind — specifically our species-specific cultural intelligence" which reflects "human pro-social inclinations for intersubjective communication" (p. 3). The origin of the myth in the writings of Noam Chomsky

1. All of the sciences developed one by one out of philosophy by applying empirical methods. The physical sciences were the easiest, and so first we had physics and other more tractable sciences, and then later the so-called special (complex) sciences, such as biology, economics, and other social sciences. First the Neogrammarians and later Sapir, Bloomfield, Whorf and others in the early 20th century were trying to make linguistics into a science as well, but then Chomsky pulled it back into philosophy in the latter part of the 20th century.

and the specific aspects of the overall myth to be discussed in the following chapters are then briefly discussed. The last section of the chapter (p. 22ff) makes the important point that contrary to Chomsky's view that the human language module (UG) is due to a single mutation in a single individual 60,000 years ago (e.g. 2004, 2007, 2012), empirical evidence shows that the evolution of human communicative abilities and language was gradual, with co-evolution and accumulation of a number of different neuro-anatomical changes to create the possibility for modern communication and use of language. This is particularly important given Chomsky's rejection of natural selection (see Chomsky 2012 and criticisms of the views expressed there in Behme 2014 and Lieberman 2015, as well as Jakendoff's assessment of Chomsky's view as "virtually a retreat to mysticism" (2002: 234)). Another important point (p. 49) in this regard is that all body parts involved in speech evolved for other purposes (first pointed out by Sapir [1921: 8–9]).²

As important and useful as E's book is, I would have preferred an even more radical departure from the usual ideas about language and linguistics the lay public (and even many linguists) are exposed to. There is a sort of Whorfian effect in that most non-generative linguists were trained in the period when the Chomskyan approach was in ascendancy, and this often manifests itself in the habits of the way of thinking held over from that time, for example, using transformationalist terminology like "passivization", "relativization", "topicalization", etc. and in assuming categories such as noun, verb, subject, object, without argument, and in often using non-empirical methodologies such as making up one's own data, using sentences translated from English as data, and using speaker judgments of "ungrammatical" sentences. My own progress in understanding language has been in direct proportion with the degree to which I have been able to overcome my initial training in linguistics (which in the 1970's on the US east coast was 100% Chomskyan). In reading E's book it appeared to me E was still trapped in some non-empirical Chomskyan ways of thinking.

In Chapter 1, E states that "linguistics is the scientific study of language" (p. 3), and that linguistics started with Saussure in 1916, contrasting this with "the extreme form of rationalism which assumed language is an instinct unrelated to any form of non-human communication" (p. 4). There are a couple of things I'd like to say about these statements.  First, I think the view that linguistics is the study of language, as opposed to the study of communication, has been harmful to linguistics, as it has blinded us to the process of communication and all that is

2. E says "The only component which is specialized for sound production is the larynx", but while the larynx can produce sound, it is generally assumed that its main functions are to protect the lungs and aid in swallowing and to help stabilize the trunk during heavy lifting (see Seikel, King, & Drumright 2010: 223–225).

involved in that (e.g. ignoring the cognitive basis of communication — what is actually going on in meaning creation — and ignoring non-verbal communication other than sign languages), and has made linguistics ivory tower and irrelevant to all but theory building, making it difficult for linguistics PhD's to find jobs outside academia.³ Linguistics didn't actually start with Saussure, but that was when linguists began divorcing language structure from its use, and so lost the connection between communicative behaviour, of which language use is only one part, and the linguistic forms used in communication.⁴ Chomsky is the most extreme in this regard, denying the relevance of communication to language structure. So Chomsky is no counterpoint to the Structuralists, he is in fact more Structuralist than many of its later proponents, such as Charles Hockett (1968, 1977), not just in divorcing structure from use, but also in the non-empirical assumption that there is a rigid, closed system of language.

Second, the rationalist-empiricist divide is not related to Structuralism, as there are rationalist Structuralists, like Chomsky and the post-modernists, and empiricist Structuralists, like Bloomfield, who was influenced greatly by logical positivism.⁵ The quote above sets the tone throughout the book as being pro empiricism and anti rationalism. In this way it is reminiscent of William James' lectures on Pragmatism (1907), which (also in eight chapters/lectures) tried to make the case for pragmatist empiricism, critiquing the rationalist perspective current in his day. Aside from the feeling of *plus ça change, plus c'est la même chose*, I mention this because while James criticized the rationalist view as less useful for his purposes, he did not say it wasn't valid. It depends on one's goals. The two sides have been opposed since the time of the early Greek philosophers and on up through Locke vs. Descartes and Leibniz, and there will be no end to it. Before Chomsky,

3. There was even a serious suggestion on the Funknet listserv list some time ago that because of this we should stop training linguistics PhD's. Of course the solution is to train them in something useful, taking a communication-based or interaction-based approach. They would then be able to get jobs in any field which involves communication, such as marketing and advertising, much as psychology graduates do now.

4. The one linguist among the Structuralists who did not divorce syntax from the rest of language and its use and the communicative (interactive) situation is M. A. K. Halliday, and his work (e.g. 1994) has been found to be very useful in language learning and translation. Because of its usefulness, his framework, which is theoretically very sophisticated, has been denigrated by some self-designated "theoretical linguists" as "applied linguistics".

5. The behaviourism that Chomsky famously criticized (1959) was actually due to logical positivism, and not something inherent in Structuralism. Bloomfield [1933: vii] mentions not being committed to any psychological theory, and it was Skinner who was influenced by Bloomfield's empiricism, not the other way around (see Matos 2007 for discussion).

in the early and mid 20th century there were also debates between the mechanists (empiricists) and mentalists (rationalists) (see Gray 1980 for discussion).

With Chomsky the rationalist approach was dominant for a time, and now the pendulum is swinging back to empiricism. However, we shouldn't throw the rationalist baby out with the bath water; there is a place for rationalist philosophy and abstraction, and rationalist methodology does not entail a disdain for evidence; we just need to point out the problem of saying you are doing rationalist philosophy, as Chomsky does (e.g. Chomsky 1966),⁶ but at the same time making what purport to be empirical statements (see Eddington 2008 for discussion). So the problem is the irrational aspects of Chomskyan theory, which have reshaped linguistics into a bizarre ideological battle rather than an evidence-based discipline.

As Cartwright (1983 and elsewhere) has argued, abstract models can be useful (with different models of the same phenomenon being possible for different uses), but the laws that apply within the model only apply to the objects within the model, not to objects in the real world. She says (1983:17) "the objects of the model have only 'the form or appearance of things' and, in a very strong sense, not their 'substance or proper qualities'". Part of what is wrong with formalist linguistics is that rather than taking the abstraction as a way of understanding the phenomenon, they take the abstraction as the phenomenon, the problem to be solved. This is because of the nature of explanation in Chomskyan linguistics: the model is to be maximally constrained to produce all and only the possible forms in all languages. This constrained model is then taken as explanation, as it is said to reflect UG. Without the assumption of UG, though, there is no explanation in this approach, and it just becomes circular (i.e. observe a phenomenon, write a rule summarizing it, then say the rule explains the phenomenon; cf. Haspelmath 2002, 2004, 2010; Dryer 2006).

E understands that language is behaviour and not a thing, but he sees it as separate and different from other forms of human behaviour, as "it is blind to demographics, socioeconomics, and ethnic differences" (p.6). This statement clearly ignores the work in sociolinguistics and recent work in typology (e.g. Trudgill 2011, De Busser & LaPolla 2015), and I think this is another view that has limited the usefulness of linguistics as a discipline. Why should we treat communicative behaviour as so different from other behaviour? In fact communicative behaviour, which includes but is not limited to language, involves the same cognitive and other abilities as other types of behaviour, and many of the key factors involved in communicative behaviour, such as conventionalisation and habitualisation, are also involved in other types of behaviour. Once we recognize this, we can see how

6. Lakoff 1989:954 points out that within the Chomskyan camp in the 1960's it was an insult to be call an empiricist.

understanding communicative behaviour helps us to understand other forms of behaviour, and vice versa. Teaching this view I get comments from students like “Linguistics is everywhere!”, because they can see the same principles at work in many types of human behaviour and interaction.

E argues that language “tells us about things *and* it allows us to interact, in various ways, with other members of our species” (p. 29, emphasis in original) and that “both functions allow us to communicate with one another”, but this seems to separate interaction and communication, and seems to make communication dependent on language. If we are arguing for a more empirical view, we need to take interaction as the fundamental phenomenon, and show how language emerges out of that interaction (see for example Couper-Kuhlen & Selting 2001; Thompson & Hopper 2001; Auer 2005; Thompson & Couper-Kuhlen 2005; Hopper 2011, 2012).

Chapter 2 (“Is human language unrelated to animal communication systems”) debunks the myth “Language is the preserve of humans and humans alone”. It talks about animal communication with a view to showing that the question of animal and human communication isn’t a “humans, yes / animals, no” sort of question: the animal systems represent precursors and possible evolutionary stages on the way to full human communication and language use. E goes through each of Hockett’s (1960) design features of language and argues that they are neither unique to human language nor are they found in all human languages.

This chapter is mainly focused on language itself and how animal communication systems are or aren’t like language, but again, if we take the cognitive abilities used in communication as the basis for communication, not language per se, then we see even more clearly that there really isn’t a sharp difference between animals and humans in terms of how communication is effected, but only in the complexity of the inferences that are possible and in the complexity of the ostensive acts used to carry out communication.⁷

Human communication isn’t achieved by coding-decoding (cf. Love 2004), but by the abductive inference of a communicator’s intention in performing an act that the addressee can infer is done with the intention of the addressee inferring the intent behind it (inference of motivations for doing things is done all the time, but it isn’t communication unless the communicator intended for the

7. Just as our failure to look at language in the communicative/interactive context has held back our progress in understanding language, unimodal analysis of animal behaviour in labs has held back our progress in understanding animal communication. As Slocombe, Waller, & Liebal (2011:923) say, “It is clear that both human language and primate communication are intrinsically multimodal, and in searching for unimodal precursors to language scientists are overlooking the complexity that is inherent in this multimodal system (Partan & Marler 1999). Abandoning the traditional distinctions between gesture, facial expression and vocalization could therefore have a large and positive impact on the study of language evolution.”

addressee to infer the intention for the action; see LaPolla 2015 for discussion).⁸ This involves general cognitive abilities that are used for survival (the inferring of the reason for things generally and the inferring of the motivations of conspecifics). The inferences involve knowledge of the situation (context), of the individuals involved, and of their possible motivations in performing some action. As E discusses, chimpanzees can take into account what conspecifics know and don't know and can "read the intentions of others," which "allows them to infer the decision-making strategies of other chimps" (p. 43). They can also use calls flexibly, even to deceive conspecifics. The basic mechanisms for communication are there, and real communication happens, and includes social cognition/interactional intelligence (see e.g. Seyfarth & Cheney 2008, 2015; Engch et al. 2006; Whiten 2013), so human communication is special mainly in the nature of the ostensive acts used in communication. One point that could have been made here is that the development of language presupposes the existence of communication, as it emerges out of communication.

Ch. 3 ("Are there language universals?") debunks the myth "A Universal Grammar underpins all human languages". Here E cites Pinker at length on the types of things that Pinker says are universal, and shows that the items mentioned simply are not found in all languages. In the years since Pinker's book was published, there seems to have been a retreat within the Chomskyan camp from having a very explicit Universal Grammar to now saying that only recursion is universal (Hauser, Chomsky & Fitch 2002) or the single process of "Merge" ('taking two things and putting them together or taking one thing and taking a piece of it and sticking it at the edge'; Chomsky 2012: 16) is what distinguishes human language abilities.⁹ Yet "Merge", as defined, is just compositionality, and so it is hard to understand how this could be unique to humans. Simplifying UG to such an extent also undercuts the whole "poverty of the stimulus" argument, which is the justification for positing UG in the first place.

E then goes on to discuss how languages change, to show, from a usage-based approach, how the diversity of language structures has developed, the point being

8. E talks about variability and lack of conventionalization of signs in the Al Sayyid sign language, and says this is possible due to the iconic nature of the signs, but variability is also true of speech, and we can use forms that aren't conventionalized, as communication is achieved by inferring the speaker's intention in doing an ostensive act, not by decoding signs.

9. Originally UG was seen as a single fixed pattern, but the discovery of so-called "non-configurational" languages such as Warlpiri forced the move to seeing UG as involving parameters such as "configurational" vs. "non-configurational", basically treating the problem as the solution to avoid having to deal with it, but even that did not resolve the problems that our greater understanding of diversity has brought up, and hence the retreat from specifying too many actual features in UG. See also Evans 2015a.

that they are “diverging based on cultural and usage pressures, rather than being constrained by some over-arching universal principle or principles binding them together” (p. 94).

E published a summary article in the science magazine *Aeon* (Evans 2014), and there was a quick response from a group of four generativists (Dunbar et al. 2015), saying that E is wrong:

“Evans’ piece mixes up the idea of typological similarities and differences between languages and proposals about the structure of the human capacity for language

...

Language scientists differ on what pre-wiring for language they believe is built into our genetic code, but, to be clear, no one believes we are all pre-wired to speak the same language. More importantly, linguists do not claim that the patterns we can easily observe in languages are consistent. It has never been part of generative grammar to claim that all languages divide expressions into nouns, verbs, and adjectives, ...”

This is a bit disingenuous, as E never stated that people are pre-wired to speak the same language, or that observed patterns have to be consistent with each other (only that they have to be consistent with claims made by generativist theory), and the universality of nouns, verbs and adjectives was clearly assumed by generativist theory (e.g. in phrase structure trees), as argued by Pinker (1994: 238) as well (see Baker 2003 and Chung 2012 for more recent statements). Dunbar et al. then go on to say that there is a difference between what the generativists are talking about as universals, which they call grammatical universals, and typological universals, the kinds of patterns we can find in real languages. They say that the latter has no relevance to the former, so E’s discussion of typological patterns that go against supposed grammatical universals of generative grammar is irrelevant (see also Evans & Levinson 2009 and comments on that article). These grammatical universals are not defined, but the example of certain types of dependencies is given. Dunbar et al. make statements about which dependencies are universal, and say these are hard-wired into the brain. They claim it is impossible to falsify these universal laws using typological data:

“We often start by observing surface patterns in a particular language, but we then propose a hypothesis about what abstract structures and rules (like dependency formation) underlie the patterns. The next step is to rigorously demonstrate that the hypothesis is a good or a reasonable one. And, if the proposal violates a grammatical universal, then you have an argument. Only when all of the above steps have been taken can one show that a language violates a grammatical universal.”

This is consistent with Newmeyer’s view (1998: 191) that “typology is indeed irrelevant to grammatical theory”. This then is what I mentioned above, citing

Cartwright: the laws govern the theoretical constructs, and not real world objects, and so cannot be falsified by real world patterns. This is certainly not satisfying for those of us who do not subscribe to that particular abstract model of structures and rules.

Ch. 4 (“Is language innate?”) debunks the myth “Knowledge of language is present at birth. It is encoded in the microcircuitry of the human brain”. E starts off with a presentation of the rationalist arguments for assuming an innate language module, essentially the “poverty of the stimulus” argument: language is too difficult to learn from the input the child gets, and there is no negative evidence to help the child learn what is not acceptable (caregivers don’t correct the child), so language structure must be innate. Evidence claimed for this is the fact that children don’t make all the logical errors they might be expected to if they were learning from scratch with only a simple inferential learning ability and no negative evidence. E points out that this is not evidence of an innate grammar; it is only evidence that children don’t produce patterns they don’t hear adults use. He suggests that “it may in fact be the case that children take absence of evidence as evidence of absence” (p.105), and so only produce the patterns they hear. E also argues that the poverty of the stimulus argument seriously underestimates the learning abilities of children. He has a long discussion of evidence about neural structure and also child language acquisition, and argues that children can learn language because they have the general cognitive ability to recognize patterns and relate similar objects and events and to create analogies, and because they have cultural intelligence, which allows for intention-reading.

E’s discussion of intention-reading seems to imply it is only used in communication, but in fact it is a necessary survival skill in social primate groups. What he means here is to read the intention to communicate, which is a special case of the general ability to read intentions: it is when the person intends for the addressee to read his or her intentions in doing something (including saying something). E follows this with a brief description of the usage-based approach advocated by Langacker (e.g. 2000, 2002) and Bybee (e.g. 2006, 2010), in which the mental representation of form-meaning pairs is acquired through use, and the more frequent the use, the more entrenched the pattern will be, and the child will generalize across similar patterns to create mental schema. Corpus studies show what patterns do and do not occur in normal discourse, and the argument is that children pick up on these recurring patterns, using patterns that they hear, and not using patterns they don’t hear.

Ch. 5 (“Is language a distinct module in the mind”) debunks the myth “Language forms a distinct faculty or module of mind”. Again E starts off with a fair presentation of the history of the idea and the reasons why some people believe in the modular nature of not only language, but of the mind generally. The

module is a neural structure that is specialised for one function and one type of information, and is informationally encapsulated (i.e. cannot interact with other modules in its functioning), and so separate from other cognitive systems. It is said the module clicks on at a certain point in development when the child has acquired a certain amount of words and needs to start making more complex structures, with a great leap in grammatical ability when the module kicks in. Key evidence for modularity would be if certain genes were found to be responsible for language and nothing else, and injuries to a particular area of the brain affected language and nothing else or other functions but not language, and also if children acquiring language actually experience the great leap forward in grammatical ability when the module kicks in. E then presents evidence from neurology and child language acquisition to show that we do not find prenatal specialization, we do not find evidence of genes related only to language ability, we do not find injuries that affect only language or are limited to a single module when language is affected, and we find a consistent correlation between lexical development and grammatical development in the early stages of first language acquisition, not a qualitative leap in grammatical ability as the supposed module kicks in.

The idea of the modularity of the mind is due to a large extent to the harmful metaphor of the mind as a computer. Much like in the history of medicine, where conceptions of how the heart works due to Galen of Pergamon held back progress in our understanding of how circulation works and it was only with the application of the metaphor of the pump by William Harvey in the 17th century that we were able to make progress in understanding the heart, we need a new way of understanding the brain. E mentions work by Gibbs & Van Orden (2010) arguing for seeing the brain as a complex adaptive system. This is one promising direction. See also Onnis & Spivey 2012, which argues against the computational view and presents an alternative visualization for languages as trajectories in a multidimensional space, and Anderson, in press, which argues for “a set of neuro-developmental processes — including both Hebbian plasticity and neural reuse — that efficiently serve the adaptivity of the organism by marshalling the same limited pool of resources in different ways as tasks demand” (p. 1).

Ch. 6 (“Is there a universal Mentalese?”) debunks the myth “Meaning in natural languages ... derives, ultimately, from a universal language of thought, Mentalese”. Aside from what is called Universal Grammar, there is supposedly a universal language of thought, and it is this language of thought that allows us to learn the words and structures of natural languages. This view also is based on analogy with computers, Mentalese being the equivalent of machine language and natural languages being the equivalent of programming languages. This view is to explain the fact that babies who have not yet learned language, and animals who don't have language, can still think. Problems arise, though, when Fodor and

others try to pin down exactly how this innate language works and where the symbols in the language get their meaning. E argues it doesn't make sense to say we are born with concepts such as 'doorknob', so our forming of such concepts must come from experience. He then introduces the "language-as-use" view of concepts as situated in real-world experience. Concepts are said to be action-oriented representations of our experiences.¹⁰ For example, our concept of 'doorknob' includes not only what doorknobs look and feel like, but also how we interact from them, as the conception arises from our physical interaction with doorknobs. Because of this our mental representations are said to reflect our embodied interaction, and abstract concepts are structured sets of embodied experience. Evidence of both points is presented from psycholinguistic studies that show embodiment effects: the same sensory and motor areas of the brain are activated in talking about something used in an activity or an activity itself, e.g. hammer and hammering, as when actually perceiving or engaging in the action. When using or understanding language, people exhibit brain and muscle activation patterns as if they were engaged in the action being talked about. This shows the embodiment of concepts. This contrasts with the hypothesised Mentalese, which is not based on embodied experience and so would not be expected to show such embodiment effects.

Ch. 7 ("Is thought independent of language?"), my favourite in the book, debunks the myth, "Thought is independent of and cannot be dramatically influenced by language". This myth contends that the idea of linguistic relativity, that there is a relationship between thought patterns and linguistic patterns, is wrong. As mentioned above, E shows that what is commonly talked about as the views of Benjamin Lee Whorf (or the invented "Sapir-Whorf Hypothesis") is a misrepresentation of what Whorf actually said, for example claiming that there is a "strong" version of the idea of linguistic relativity that is actually determinism (that language determines thought), when in fact Whorf argued *against* such a view (e.g. 1956:239, see also Lee 1996, Ch. 3). E points out that the "Sapir-Whorf Hypothesis", with its "weak" vs. "strong" versions, was a fabrication of two opponents of relativism, Eric Lenneberg and Roger Brown, in the 1950's, long after Sapir and Whorf were dead, in order to refute Whorf's ideas, but this is the view that is often presented in linguistics textbooks and criticised by opponents of

10. This is very much in line with Anderson, in press, where it is argued that "thinking, calculating and speaking are adaptive behaviors, and as such involve the whole organism acting in and with its environment. These capacities are not limited to, nor are even primarily a matter of computation over a set of mental symbol structures. Instead, thinking involves iterated interactions with elements of the environment. It leverages our highly-developed and early-evolving capacities for acting in and manipulating the physical and social environment. Socio-cultural cognitive achievements such as language and mathematics are extensions of — not radical departures from — these basic capacities" (p. 2).

relativism. E argues that the Chomskyans are afraid of relativism, as it undermines their whole enterprise, but that rather than properly criticising it using experimental data or theoretical arguments they have created straw men to ridicule.

E also presents experimental evidence that Whorf was in fact right, that our languages do influence the way we perceive and understand the world. This is only natural, as our language represents our habits of thought and behaviour, so it is a chicken and egg sort of issue, and is very easy to demonstrate, as Whorf did, with phonemic judgments (e.g. whether you hear [p] and [b] as one sound/phoneme, as it is for English speakers, or two, as they are for Tagalog speakers — your language habits affect your perception).

E has a long discussion of research on colour terms, as this was supposedly evidence that Whorf was wrong. E shows that the work on the colour terms was problematic, and there are many exceptions to the supposed universals, and there is evidence from the acquisition of colour terms by children that goes against the predictions of the theory that colour terms are due to semantic universals. E argues instead for the “language-as-use” view that “colour terms emerge from objects and surface properties, with the colour category emerging over time, once the colour terms become dissociated from their original source objects” (p.211).

My only comment on this section is that E refers to the “restructuring” of cognition or the “augmenting” of thought by language, and this is certainly true for the proper acquisition of a second or third language,¹¹ but in the case of first language acquisition, we can’t talk about restructuring unless there was a structure there to begin with, either innate or acquired from experience before language acquisition. Research by the late Melissa Bowerman and her colleagues has shown that language influences the development of the categories (e.g. Bowerman 2004, 2007; Bowerman & Choi 2003). They liken it to the development of phonemic categories: initially the child can make many distinctions, but will later come to distinguish only those patterns found in their language. This is similar to Whorf’s “isolates of experience”, the bits of experience the child attends to as meaningful in the process of acculturation.

Ch. 8 (“Language and mind regained”) presents the view of “language-as-use” in more detail. E discusses first the question of why human language is different from animal communication. He argues that it is due to a species-specific cultural intelligence and cooperative behaviour, and that this cultural intelligence led to

11. To become truly bilingual/bicultural, there needs to be not so much a restructuring but addition of another set of categories, and the ability to switch between them, so for example the cognitive category of possession is different in Chinese and English, and a bilingual needs to be able to switch between the two categorisations in their representations of possession when switching between the use of the two languages.

the development of interactional intelligence, which “paved the way for language” (p. 230). He then presents the constructionist view of our mental grammars, and also argues that there are many things that are common across languages because they are common reactions to common situations of human experience, but languages become more diverse because of geographic separation and historical change.

The discussion of cultural intelligence focuses on cooperation, which is seen as uniquely human. I think this last part is not correct, as there are plenty of studies to show cooperation in dolphins and other animals that isn't of the individualist type described by E (based on Michael Tomasello's work), but it is true humans have developed a greater ability for cooperation. Yet I don't think cooperation of the relevant type is actually what is at the core of Grice's principle of cooperation, as argued by E (p. 232). Grice's insight was that humans are rational, in the sense that when we do something we do it for a reason, and assume other humans are rational, and we can exploit this assumption in communication by doing something with the intent to have the other person infer our reason for doing it. The person then recognizes a communicative intention, and by inferring the reason for the action the person creates the meaning we intended to communicate. The inference involved here is abductive inference, which involves creating a context in our minds in which the person's action makes sense (inference to best explanation). That is not cooperation in the usual sense, but is how communication works. E argues that interactional intelligence is what allows for the development of language, and defines it as “a predisposition to engage in interaction with one another in communicative settings for specific ends” (p. 235). This is a bit broad, as it would seem to be applicable to any social primate and to dolphins, among others. He discusses how this interactional intelligence is independent of language but it allowed for language to develop. Yes, communicative interaction necessarily preceded the development of language, as language emerges out of the process of communication, but I think the intelligence involved in communication is not special, but part of our general cognitive abilities to understand the world. Understanding the motivations of other humans is only one application of these more general abilities, in particular abductive inference. This is the “powerful meaning-making mystery that we all share” (p. 236, quoting Levinson 2006: 45).

E cites Robin Dunbar on a possible reason for the development of language: to facilitate gossip, which is seen as taking the place of grooming in other primate communities. But if, as argued above, communication is based on abductive inference of intentions, which involves the creation of a context of interpretation in which the action makes sense, then language can be seen as a mechanism (along with body movements and other types of ostensive act) as constraining the addressee's construction of the context of interpretation, leading them to include

or not include certain assumptions in the context of interpretation, and thereby making it more likely the desired effect will be achieved.

E argues that the mental grammar is “a repository of constructions” (p. 247) which are form-meaning pairs of various sizes and levels of abstraction (no separation into lexicon and grammar). Children hear actual instantiations of the constructions and little by little generalise out a schema, a mental grammatical unit. A hierarchical network of these mental grammatical units forms the language, and we can combine these constructions into new combinations to facilitate communication.

The last section of the book, called “One final reflection ...” points out that it is necessary for Chomsky to try to divorce language from communication because if language arose from communication, it would necessarily involve at least two people, and this would conflict with his idea that it was due to a single genetic mutation in one individual 60,000 years ago.

In all I find this a very useful and well-written book and certainly recommend it to anyone wishing to learn about the controversies discussed in it.

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