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REVIEW ARTICLE

Biology and language: a consideration of alternatives[1]

DANIEL L. EVERETT

Department of Linguistics, University of Manchester

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Stephen R. Anderson & David W. Lightfoot, *The language organ: linguistics as cognitive physiology*. Cambridge: Cambridge University Press, 2002. Pp. xix + 263.

T. Givón, *Bio-linguistics: the Santa Barbara lectures*. Amsterdam: John Benjamins Publishing Company, 2002. Pp. xviii + 383.

1. Introduction

1.1 *Purpose*

This article reviews an important set of issues concerning biology and grammar, focusing on two recent books on this topic: *The language organ*, by Stephen Anderson & David Lightfoot (*TLO*), and *Bio-linguistics*, by T. Givón (*BL*). These books represent opposite extremes of the language evolution/acquisition debate and it is therefore instructive to compare and contrast them in a single review article. Beyond the books themselves, of course, looms the debate on the origins of human language, the very ontology of the physical vs. the non-physical, and the conception of our role as linguists in the greater endeavour of human learning. Therefore, my larger purpose here is to consider some of the issues that each side raises that the other side ignores and to suggest an alternative way of proceeding to understand language's emergence, based on recent work by various researchers that does not fit neatly into either of these two extremes.

Let's be clear about a couple of things before considering the books under review here in detail. First, knowledge about the neurophysiology and the evolution of language has never been demonstrated to be either necessary

[1] I would like to thank Ted Briscoe, an anonymous *JL* reviewer, Andrew Spencer and Paul Postal for comments on this article, though none of them are to be blamed for any errors that have remained or any misinterpretations I may have made of their remarks.

or sufficient for the understanding of grammatical structures or their use cross-linguistically. And it is not clear in either of these books that standard linguistic fieldwork or standard linguistic theorizing depends in any direct way on biology, whether synchronic (e.g. the Human Genome or anatomy) or diachronic (evolutionary studies). Second, none of the three authors of the two books under review has any serious credentials in neurophysiology. Third, these books contain no biological discussions of any depth. Not even joint studies with biologists, for example, are crucially referenced in either of the books under review here.[2]

So why do these three eminent linguists write on what can only be for now a largely speculative relationship between biology and language? Is it simply 'microscope envy'? Is it from a desire to show other scientists that linguistics is important? Consider what Anderson & Lightfoot have to say. According to them, the greatest success of post-World-War-Two linguistics

> results fairly directly from the adoption of an explicitly **biological** perspective on the subject: instead of seeing language as an external phenomenon, as a collection of sounds, words, texts, etc. that exists apart from any particular individual, contemporary linguistics increasingly concerns itself with the internal organization and ontogeny of a special kind of knowledge. (*TLO*: ix; emphasis in original, DLE)

It is this 'biological stance' that gives modern linguistics much of its appeal to Anderson & Lightfoot. Therefore, their book explores the consequences of this stance for theories of language.[3] It is more than a little disappointing, therefore, that *TLO* fails to present any evidence for the success of this BIOLOGICAL perspective.

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[2] I was pleased to read an anecdote on page xv of Anderson & Lightfoot recounted to illustrate the relative complexity of modern theoretical linguistics. They say that 'one of us, at a rather boring meeting, was sitting next to Dr. Rita Colwell … who would later become director of the National Science Foundation. Dr. Colwell idly picked up a book on morphosyntactic theory … "Well!" she eventually remarked … "You really do have to be a rocket scientist to be a linguist"'. I like this anecdote because one of the authors had earlier told me about this meeting and because the book in question was my own *Why there are no clitics* (Everett 1996). But though I am fond of that book and though I think it makes a contribution to the theory of clitics, it is not microbiology-hard, at least not to the 'mathematically challenged', e.g. myself. You just need to know some generative theory to read it.

In fact, both books are superficial with regard to evolution and/or biology and language. To see this, just compare either one of them with any number of biological studies or mixed studies of this relationship. One recent collection of papers is *The transition to language* (Wray 2002), though various others are listed in the references and mentioned in the text.

[3] Postal (2004: 296ff.) addresses this general issue in fairly acerbic (but entertaining) language, in his chapter on the most irresponsible passage ever written by a professional linguist (the author of the passage in question being Chomsky). He argues, convincingly to my mind, that claims to the effect that generative grammar studies biology are extremely wide of the mark.

Givón states, alternatively, (xv) that

> while some proponents of this radical uniqueness of *homo sapiens* may concede a genetic basis to the extraordinary – and thus presumably innate – linguistic capacities of our species, they continue to view its unprecedented emergence as governed by unique principles that transcend the mundane mechanics of evolutionary biology … A somewhat different perspective concedes that the human mind may be the product of adaptive evolution, culminating in a genetically-configured real organ, the brain … language and culture merely *fall out* of, or *emerge* from, the biologically-evolved mind-brain, requiring no further specific adaptations. (*BL*: xv; emphasis in original, DLE)

Givón agrees with Anderson & Lightfoot that a biological stance is useful, but his view of what that stance leads to is radically different. Givón takes a 'uniformitarian' view of human biology and language's place within it, whereas Anderson & Lightfoot take the view that language-related biology is special and discontinuous with the rest of cognition, what Creation-science researchers, among others, have called CATASTROPHISM.[4]

The strategies, tactics, and conclusions of these two books are extremely different, underscoring deep divisions in the field. Givón's book, for example, refers to work in the Chomskyan tradition, but fails to understand it and is tinged with heavy sarcasm and haughtiness when discussing this alternative. Anderson & Lightfoot avoid this tone, but perhaps reveal an even deeper lack of appreciation for the alternative tradition by failing to refer to it.

Their styles are also quite different. *TLO* is highly focused, carefully written (for the most part), and meticulous in all aspects of its scholarship – within a very narrow context. *BL* is fluent, wide-ranging, and in many ways much more exciting to read. But it is also sloppy in its handling of many facts and alternative analyses. Nevertheless, *BL* seems to me to have a healthier view of the issues, refusing to limit itself to the narrow range of linguistic phenomena addressed by *TLO*. I shall endeavour to be clear on these points as we proceed.

1.2 *Organization*

In what follows, I first summarize and discuss each book individually. Then I take up some of the general issues and suggest an alternative way of studying

[4] See, for example, Eldridge (1999: 35ff.) and Schwartz (1999: 3f.) on catastrophism's appeal to both some geologists and a few other scientists who take the Biblical record as a serious scientific proposal. In the 'Creation–science' perspective the relevant catastrophes implicated in human development are miracles. And from the vagueness of the claims in Generative Grammar as to how language might have emerged, something like evolutionary miracles seem to be implied. Bowler (2003: 112–117, 129–134) summarizes both concepts. Though these terms are not the usual ones used in linguistics, I use them here because of their standard and historical interpretations in geological and biological studies.

the problem, one that pays serious attention to the role of culture in shaping human cognition and language.

2. Summary and discussion of *The language organ*

2.1 *Summary*

Chapter 1, 'Studying the human language faculty'. The purpose of this chapter is to show that previous (i.e. non-Chomskyan) theories have focused on contingent aspects of language as an extensional set of utterances (an E-language) and behaviours and to contrast this with the Chomskyan study of language as an intensional set of properties in an individual's mind (the I-language).

Chapter 2, 'Language as a mental organ'. This chapter proposes that children bring to language-learning a priori knowledge that derives from a language organ. The case for this claim is built on the standard 'poverty of the stimulus' argument, namely that there is not information in the child's extracorporeal environment to construct its I-language, although all children do in fact construct an I-language. This 'paradox' is resolved if there is a language organ, i.e. if language is innate.

Chapters 3–8 ('Syntax', 'Sound patterns in language', 'Describing linguistic knowledge', 'Phonetics and the *I*-linguistics of speech', 'Morphology', and 'Language change') build a case for I-language, a language organ, and poverty of the stimulus by examining research results from syntax, phonology, phonetics, morphology, and historical linguistics. Each of these chapters is designed to support the conclusion that the knowledge children have of the various components of their (I-)language is richer than the environment, i.e. that there must be a language organ.

Chapter 9, '"Growing" a language'. This chapter discusses how this 'organ' matures and develops and how the language organ hypothesis can be tested.

Chapter 10, 'The organic basis of language'. This chapter examines the species-specificity of the language organ, arguing that only *Homo sapiens* has one.

2.2 *Discussion*

If Anderson & Lightfoot are correct, Chomsky deserves the Nobel Prize in Medicine, for single-handedly re-opening the apparently moribund science of anatomy and discovering an organ of the human body which has been overlooked in the history of studies of the human body. Of course, the language organ is unusual in that it cannot actually be seen. What could they mean, you ask, by an invisible organ?

Surprisingly, the nature of this 'language organ' never emerges clearly in *TLO*. Consider the following, partial list, from their book, of what the language organ is or might be.

First, THERE MAY BE MULTIPLE LANGUAGE ORGANS: 'The understanding of what linguistics is all about, however, with the focus on language organs, is actually a relevant one' (3). The plural here may just be a mistake, or the authors may be referring to the subcomponents of linguistic knowledge – syntax, morphology, phonology or even an open-ended number of organs. From what the authors say subsequently, either interpretation seems possible.

Second, THE LANGUAGE ORGAN IS KNOWLEDGE: 'This knowledge [i.e. what a speaker knows when they know the grammar of their language, DLE] is what we are calling a person's language organ' (16). This proposal strikes me as non-Chomskyan since it fails to distinguish the initial state (S_i) of the organism's language faculty from the final state (following parameter setting, etc.), something Chomsky is generally careful to do, with S_i being UG (Universal Grammar). Moreover, if knowledge is, as many philosophers would contend 'warranted belief' or some such, then this passage seems to reduce all our beliefs to organs.

Third, THE LANGUAGE ORGAN IS TISSUE:

> But what, after all, is an 'appropriate sensory organ'? Precisely some bit of tissue which is specialized, as a result of the animal's biological organization, to be sensitive to certain environmental events. But that is exactly what we claim for the language organ: it is a biologically determined aspect of certain tissue (primarily in the brain), rendering it uniquely sensitive to linguistic events in the environment. (221)

Notice that this definition specifically avoids saying that the language organ is found in a specific spot. Rather, it is a system of dedicated tissues, neurons, etc.

But what follows if this organ has no specific location in the brain and is merely the DEDICATION of a certain portion of brain tissue (from an area that might allow other 'dedications' as well, since we know that various parts of the brain are multifunctional)? By this definition is Broca's Area an organ? Or is my knowledge of recipes a 'gourmet organ'? Consider, for example, the issue raised by Pulvermüller (2002: 273), who states that

> recent studies of syntactic phenomena have great difficulty in proving that the physiological phenomena that are reported to co-occur with linguistic properties of sentences are strictly related to these linguistic properties per se … there would not be a high correlation between a bold brain response and a well-defined linguistic process, but there would be a brain response cooccurring with a variety of linguistic and psychological phenomena.

Or, as Pulvermüller (2002: 274) goes on to discuss, it is not even clear what we should look for in many studies – we get 'readings' but connecting these to specific linguistic properties, constructions, etc. is impossible without specific proposals on linguistics in brain structures, something currently lacking. Examples of this are found, for instance, in the studies in Marantz, Miyashita & O'Neil (2000), which includes various interpretations of brain-activation in linguistic tasks. Yet in none of these studies does any proposal emerge to justify dedicated brain tissue or a language organ. And this collection largely contains studies sympathetic to the existence of such tissue or such an organ.[5]

Thus, on the one hand the language organ is claimed to be what we know about language, as constrained by our biology. On the other hand it is purported to be some 'specialized tissue', tissue dedicated to a restricted range of tasks through interaction with the environment as well as by genetic endowment.

The apparent identification of knowledge with tissue might indicate that Anderson & Lightfoot adopt something like Chomsky's rejection of the 'harassing' ontological distinction between mind and body. Recently, for example, Chomsky has offered some rather stark judgments about the mind–body distinction:

> Ontological questions are generally beside the point, hardly more than a form of harassment. (Chomsky 2000, cited in Lycan 2003: 11)

> [P]erhaps, principles now unknown enter into the function of the human or animal minds, in which case the notion of 'physical body' must be extended. (Chomsky 1980: 5f.)

> The supposed concepts 'physical' or 'material' have no clear sense … There seems to be no coherent doctrine of materialism and metaphysical naturalism, no issue of eliminativism, no mind-body problem. (Chomsky 1994: 195f., cited in Poland 2003: 31)

Now, if we accept these proposals, then calling knowledge an organ and conflating this knowledge with tissue might be legitimate at some abstract level. But the cognitive price is very high – Chomsky's ontological speculations and his 'unknown principles' are hard to take seriously, as even Lycan (2003: 15) observes in an otherwise admiring article. Moreover, there are serious empirical reasons to doubt some of the core proposals of *TLO*.

Let us examine in particular Anderson & Lightfoot's arguments for the 'Poverty of the Stimulus' (PoS; for a much more detailed and different criticism of PoS arguments see Pullum & Scholtz 2002). The authors adopt Chomsky's strategy for trying to convince non-linguists of the innatist

[5] Part of the tissue vs. knowledge ambiguity regarding what the authors mean by the language organ may be due to their failure to think of language as 'software' and tissue as the 'hardware' it is running on. I won't press this though.

hypothesis, namely a marshalling of facts that 'could not have been learned' because they are underdetermined by the data. Since arguments in *TLO* are so similar to Chomsky's own, let's consider an old standby paradigm of Chomsky's:

(1) (a) The man is in the room.
    (b) Is the man in the room?

Based on the simple pair in (1), the child might be tempted to try a hypothesis along the lines of (2) (of course, although Chomskyan theorists tend to consider only movement-based analyses, there are various other possibilities, too numerous to list here, that do not involve movement at all).

(2) *Yes-no question formation hypothesis*
    Move the first verb of a statement to the beginning of the sentence to form a yes-no question.

But now consider (3).

(3) (a) The man who is tall is in the room.
    (b) *Is the man who __ tall is in the room?
    (c) Is the man who is tall __ in the room?

Example (3b) shows that the potential yes-no question formation rule hypothesized in (2) won't work. More significantly, however, examples like (3b) are apparently never produced by children in learning their language. Children seem to 'know' that only the main clause verb, (3c), not an embedded verb, (3b), is relevant to yes-no question formation. Simple subtraction of the knowledge available in the sentences of (1) and (3) from the knowledge demonstrated by the child (in never producing examples like (3b)) would appear to be sufficient to give us the knowledge that child brings, a priori, to the task, i.e. what Chomsky calls its UNIVERSAL GRAMMAR (UG).

But this oft-adduced type of argument on behalf of the 'poverty of the stimulus' (or what Chomsky these days refers to as 'Plato's Problem'), like most of the syntactic examples in *TLO*, is artificial. Such arguments almost seem to be predicated on the assumption that children learn their language from its written form, rather than its spoken form. This is so because Chomsky, and just about every single other author arguing for the 'poverty of the stimulus', ignore a huge portion of the child's 'stimulus', namely intonation, stress, information structure, and discourse priming, which are causally implicated in the child's parsing and learning decisions. Consider again the examples in (1) and (3) above with just a bit more information added (dramatically simplified, where ' marks default primary stress):

(4) (a) The man is 'here. (High pitch and stress indicate new information.)
    (b) ~~The man who is tall~~ is in the 'room~~.~~

So the child is in fact asking its yes-no question about the predicate with NEW information in the unmarked case. Any shift of the locus of new information (e.g. putting it in the relative clause, as in *The man who is TALL is in the room*) is marked by an accompanying change in intonation. Thus, the child can always attend to intonation to pick out the concept of 'main predicate' (via the unmarked intonation pattern) with no need of any knowledge of subordinate vs. main structure at all, regardless of the sentence intonation. This possibility is not considered in the generative literature at all, so far as I can tell. Whatever the role of intonation in language acquisition, it makes little sense to talk about 'poverty of the stimulus' or learning in terms of syntactic structures alone until intonation's role is better understood. Thus, the failure to consider a role for intonation and information structure in language acquisition is a serious flaw of the entire enterprise.

And this by no means exhausts criticism one might make of such arguments. Steedman (2002) offers very suggestive discussion of some of these issues. Though this does not answer the full range of questions regarding its acquisition of interrogatives, it does show that the child's 'stimulus' is not limited to word order and tree structure but also includes information on new vs. old information, recoverable from (hence learnable from) the discourse in which the sentence is embedded, and from the intonation. Advances in both information structure theory (see, for example, Lambrecht 1994) and intonation theory (e.g. Ladd 1996), dating back at least to the work of Dwight Bolinger (e.g. Bolinger 1986, 1989), provide many interesting tools and ideas which severely undermine the 'poverty of the stimulus' argument. Moreover, they suggest that the more interesting problem related to language acquisition is not, in spite of Chomsky's urgings, Plato's Problem, but what we might label 'Aristotle's Problem', i.e. how language emerges from local cultures and conventions, manifested in discourse structure, information structure, prosody, and morphosyntax.[6] *TLO*'s refusal to consider the potential contribution of any but this last aspect of grammar in language acquisition would appear to lead it to faulty conclusions, i.e. that knowledge must be a priori and, therefore, a new type of organ (if – and this is a big 'if' – such knowledge is specifically linguistic), and the consequent denial or trivialization of the physical. (Efforts to point out this ontological problem, however, are characterized by Chomsky as 'harassment'.)

Calling language an organ or organs is therefore ultimately less than helpful. It attributes some validity to seemingly contentless suggestions of Chomsky to the effect that 'the world is whatever we discover it to be' (Chomsky 1988: 144), used in what appear to be efforts to defend the concept

[6] Aristotle differed from Plato in his conception of language by, among other things, placing importance on the concept of convention, rather than merely 'ultimate reality'. Some discussion of this difference can be found in Seuren (1998: 11ff.).

of mind by the curious strategy of denying the mind–brain distinction. Moreover, because *TLO* does not fairly consider the stimulus the child is exposed to, artificially restricting it to morphosyntax, its arguments for innateness fail to convince. The only language organ it convinces me of is the organ on its cover, namely the brain. Moreover, *TLO* fails theoretically and philosophically because it fails empirically – no one can talk of UG or a language organ without considering the interaction of information structuring, prosody (especially intonation), and the role of culture in constraining grammatical forms (see Everett (submitted) for very detailed (and controversial) discussion).

3. Summary and discussion of *Bio-linguistics*

3.1 *Summary*

Bio-linguistics is titled after a term that, so far as I can tell, was first introduced into linguistics by Chomsky years ago (see Chomsky (2001: 41) for a history of the term). Givón's basic concern is one I share and it clearly applies to *TLO*:

> And while some proponents of this radical uniqueness of *homo sapiens* may concede a genetic basis to the extraordinary – and thus presumably innate – linguistic capacities of our species, they continue to view its unprecedented emergence as governed by unique principles that transcend the mundane mechanics of evolutionary biology. (xv)

Givón is on solid ground here, one worked by various others, Lieberman (1984) and Deacon (1997), for example.[7] So consider remarks made by Chomsky in his plenary session address to the Linguistic Society of America (Boston, 9 January 2004). Chomsky asserted that Alfred Russel Wallace (1823–1913) – whom he referred to as the co-founder of the theory of evolution by natural selection – denied that natural selection alone could account for the mental life of *Homo sapiens*, e.g. imagination, creativity, language, etc. But this is disingenuous of Chomsky for two reasons. First, Wallace was not the co-founder of evolutionary theory by natural selection. Darwin's work pre-dated Wallace's by many years and was backed up with copious research far outstripping Wallace's own work (for one history of evolutionary theory, see Bowler 2003). Wallace did independently come up with the idea of natural selection, but subsequent generations and his own generation were right to judge him as secondary (to Darwin), though clearly

[7] Interestingly, in Pinker & Bloom (1992), an effort to provide an evolutionary basis for the development of grammar principles along Chomskyan lines, the authors are forced to offer FUNCTIONAL motivations for each principle of UG. But they fail to say how these piecemeal responses to functional pressure could have developed into a new 'language organ' in anything like the available evolutionary time, or in the standard sense of UG.

important. Secondly, Wallace was a theist and explicitly expressed a priori theological commitments to the idea that God created man, so he was unwilling to accept that nature alone could be responsible for man. It was Darwin who allowed 'the chips to fall where they may' and based his views on his research, free from theological bias. Thus, there is no serious support in the original proposals on evolution for any discontinuity between the human psyche and the body of the type that Chomsky would have us believe. Givón is correct to assert that generative theory is unnecessarily and markedly out of alignment with standard views of natural selection, despite what Chomsky claims for Wallace.

Another excellent point of Givón's made at the outset is that

> [w]hen language is viewed as a biological phenomenon, then the study of diversity – both within the individual speaker or speech community and across languages – becomes enormously relevant. (xvi)

For its failure to pay serious attention to diversity, Givón labels generative linguistics as 'disdainful of the burdens of empirical science' (xvii). It is occasionally tempting to concede this point to Givón (though there is too much high-quality and empirically rigorous work in generative grammar, not to mention many unempirical functional works, to apply it globally. On the other hand, from a very different perspective and research agenda, Postal (2004) has provided powerful independent support for this severe judgment).

In the remainder of this section, I first summarize Givón's chapters, returning at the end to a general discussion and comparison with *TLO*.

Chapter 1, 'Language as a biological adaptation'. In this chapter, Givón traces what he takes to be the historical roots of functionalism in biology, the role of language in communication, and change and variation.

Chapter 2, 'The bounds of generativity and the adaptive basis of variation'. This chapter explores crucial aspects of variation and fuzziness of categories that a system that only provides discrete analyses, e.g. Generative Grammar (GG), simply cannot handle. Here Givón argues that, inter alia, 'a Platonic-Generative approach to grammatical categories cannot accommodate the subject GR [Grammatical Relations, DLE] properties' (53) of languages in which such properties are scattered across various categories, rather than focused in a single category, as, he claims, GG would have us believe. There is little new in Givón's discussion here, though, certainly nothing to link subject properties with a 'Platonic-Generative approach'.

Chapter 3, 'The demise of competence'. Here he argues that 'Chomsky's methodological *deus ex machina*, "competence", is fundamentally a pre-empirical philosophical prejudice, the preference for edited written text as a source of data rather than a cogently argued empirical stance' (78). Givón here argues that GG has severely handicapped itself by often failing to apply rigorous scientific methodology in its study of language and data-gathering,

e.g. careful field work (with spoken data), looking at example sentences in their larger discourse contexts, statistical analysis of findings, etc. (This is a point made earlier by Cowart (1997), among others, with somewhat different conclusions reached.)

Chapter 4, 'Human language as an evolutionary product'. Here Givón characterizes the two extreme poles of debate as 'extreme emergentist' vs. 'extreme innatist' and argues against both poles.

Chapter 5, 'An evolutionary account of language processing rates'. This chapter, apart from its rather grandiose title, really just 'reports the results of two experiments designed to test the effect of presentation speed of visual information on the verbal episodic memory of events and their participants' (163). The purpose of the experiments was to show that the 'major features of our language processing apparatus are an evolutionary extension of the primate visual information-processing system' (163). The remainder of the chapter discusses the two experiments in question. These are interesting experiments and quite suggestive. The argumentation for connecting linguistic processing to visual processing is not always fully convincing, but is nevertheless thought-provoking and challenging.

Chapter 6, 'The diachronic foundations of language universals'. This chapter argues that 'grammatical typology … [is] meaningless as a purely synchronic enterprise. This is so because the sum-total of the various structural types that can code a particular target functional domain in syntax is nothing but the sum total of the various diachronic pathways of grammaticalization' (217). Thus the key to linguistic change and linguistic universals is the functional pressure exerted on language form by communicative needs.

Chapter 7, 'The neuro-cognitive interpretation of "context"'. This chapter begins with a pithy, typically Givónian quote: 'Like many ardent empiricists before us, we tend to confuse what is methodologically available with what is theoretically relevant' (223). Here Givón argues (223) that 'context is a mental construct, assembled for the occasion and thus in principle dependent on judgements of *framing*, *perspective*, and *relevance*' (223).

The basic thesis of this chapter is that a constructed mental context is a vital component in understanding the constraints on language form and language content imposed by communication. In this chapter, Givón does an excellent job of fleshing out the concept of context as he sees it, its role in communication and subsequent implications for language form. The core of the chapter is section 7.5, 'The automatization of context: the use of grammar to manipulate the hearer's perspective'. Perhaps the greatest surprise and disappointment of the chapter, however, is Givón's failure to consider the relevant body of work on context in Relevance Theory, as developed by Sperber & Wilson (1986), as well as new work on Dynamic Syntax,

developed by Kempson, Meyer-Viol & Gabbay (2001), Marten (2002), and others. Also relevant here is work on the concept of contextual 'coercion' by Pustejovsky (1995). This failure to interact with major lines of research directly relevant to his central theses is a serious shortcoming of Givón's work here.

Chapter 8, 'The grammar of the narrator's perspective in function'. In this chapter Givón draws on lessons he learned in reading and writing fiction in order to understand the role of the narrator's perspective in constraining modality and evidentiality. He shows how this perspective can be an effective tool in the analysis of reference, mood, and truth conditions, among other aspects of discourse. It is not clear that this chapter really fits well with the overall biolinguistic emphasis of the book, but it is useful in understanding more about discourse as the product of intentional agents.

Chapter 9, 'Society of intimates'. This chapter title refers to smaller egalitarian and 'seemingly anarchic' societies wherein most speakers know most other members of the society. Such societies are claimed to be the evolutionary precursors of our own larger societies 'of strangers'. Unfortunately, this chapter appears to make the mistake of assuming that smaller societies today are more like those of 'primitive man' than they are like 'modern man'. Givón is somewhat successful in showing that communication is affected by societal organization and relative complexity (hardly a novel conclusion, however), but I think that the chapter is unsuccessful in showing any connection with syntactic analysis per se or with biolinguistics more generally. This and the previous chapter don't seem to fit well in the book as a whole.

Chapter 10, 'On the ontology of academic negativity'. The concluding chapter of this book, which is itself so very negative and sarcastic in many places, is dedicated to understanding the prevalence of negativity in academic discourse. The chapter begins with a superficial, disjointed, and cursory account of Popper's notion of falsificationism, ideas on scientific development proposed by Imre Lakatos, and the Peircean concept of abduction. Based on this seemingly random assortment of epistemological concepts, Givón concludes that academia is an inherently negative environment.

But while I agree completely that academia can be very negative, there is really no need to appeal to philosophy of science to account for this. Competition for resources, e.g. fame, gratification, promotion, salary increments, etc. seems sufficient to account for any academic nastiness, especially when coupled with basic traits of our species, e.g. insecurity, self-preservation, etc. Linguists' hostility would seem to derive from the same factors leading to hostility in any field of human endeavour, whether sports, politics, or used-car sales. For example, the violence and bitterness of the Tonya Harding–Nancy Kerrigan rivalry in world-class figure-skating hardly

needs explaining in terms of falsificationism. And if one linguist is nasty to another in print, the same factors are likely to be responsible.

3.2 *Discussion*

TLO is primarily intended for an educated lay audience, whereas *BL* seems intended for cognitive scientists. Nonetheless, it is possible and worthwhile to compare these as major exemplars of the formalist vs. functionalist philosophies of language within and among *Homo sapiens*. Both books are ambitious attempts to establish the superiority of their particular perspective and to undermine the positions of their major alternatives.

But to establish the relative inferiority of alternatives, one must understand those alternatives and make this understanding explicit and clear in one's discussion. Both *TLO* and *BL* fail to do this, the former by omission, the latter by misunderstandings.

Givón's misunderstandings of formal linguistics in *BL* are legion – too many to address here, though I do take up some of the more egregious ones directly. As a rule of thumb for the reader, though, if Givón summarizes a specific formal analysis or parameter proposed by formal linguistics, his summary is wrong.

Givón asserts that 'the various Generative schools of linguistics have taken relational categories such as "subject" and "direct object" for granted' (43). He then goes on to argue that a priori GRs (grammatical relations) do not pick out a well-defined class of objects in real languages, intra- or cross-linguistically. If Givón were correct in his characterization of GG on this score, the emergence of Relational Grammar (RG) in the 1970s would be puzzling. RG argued that Chomsky's theory was misguided precisely because it failed to take such relational categories 'for granted', i.e. as primitives of the theory. Chomsky has consistently maintained, in fact, that significant generalizations on the syntactic behavior of nominals are based on the structural position of each nominal and not on concepts like 'subject' and 'object' – and the success of recognizing GRs as primitives (leading to a strong following for RG) was one of the reasons that such notions were eventually incorporated in Lexical-Functional Grammar (LFG). Therefore, although Givón's arguments in this chapter might be taken as evidence against GRs, classic RG, and some versions of LFG, if they were correct, would serve to BUTTRESS Chomsky's statements since the 1960s on this matter.

The arguments of chapter 3 rest on Givón's fundamental misunderstanding of the so-called 'nonconfigurationality parameter'. Nonconfigurationality, never a universally accepted parameter of GG, was originally proposed by the late Kenneth Hale to account for the differences in word-order constraints between some Australian languages, e.g. Warlpiri, and English, though the concept was subsequently extended to many other languages. The basic idea is in fact very simple, boiling down (ironically, given the

vehemence of Givón's opposition) to *functional* constraints. These are that (i) hearers have to be able to determine in a sentence who did what to whom; (ii) if a language marks argument functions largely by word order (e.g. English), then it will need less morphology; (iii) if a language marks pragmatic-semantic roles by a rich morphology it will have less need to maintain rigid word order; (iv) the last point is a core motivation for the existence of 'nonconfigurational languages'. Givón ignores the fact that many generative linguists reject nonconfigurationality and erroneously presents it as somehow representative of the entire GG enterprise. Worse, he also misunderstands it. For example, he characterizes nonconfigurational languages as those where the '"real" subjects and objects are coded on the verb itself' (72). In fact, this is NOT the nonconfigurationality parameter at all, but the distinct (though related) hypothesis of Jelinek (1984), known as the 'pronominal argument' hypothesis. Nonconfigurationality is the hypothesis that in a given language there is no hierarchical syntactic structure, either globally or for some crucial phrases, e.g. the VP. Jelinek's pronominal argument hypothesis is one way to approach this problem, but it is not the only way. For example, Hale (1980) proposed W* or nonconfigurational languages BEFORE Jelinek's important 1984 paper, which was, again, a later effort to provide one line of explanation for Hale's findings.

Moreover, to counterexemplify nonconfigurationality, Givón shows that there are configurational languages with pro-drop (certain verbal arguments can be omitted from OVERT expression, as in Portuguese, Spanish, Chinese, etc.). However, this is not a counterexample to nonconfigurationality. Givón has gotten the implication backwards. Nonconfigurationality, as developed by Hale (1980, 1983) and many others, entails the implication in (5) but not that in (6).

(5) If a grammar is nonconfigurational→it has pro-drop.

(6) If a grammar has pro-drop→it is nonconfigurational.

Note that (5), but not (6), follows from the functional principles in (i)–(iv) above. Moreover, (5) is asserted in the original literature, but not (6). Therefore, not one of the pro-drop examples Givón discusses is a counterexample to any version of nonconfigurationality.

Givón proceeds in his attempt to show that GG is empirically sloppy, by arguing that if natural speech is examined, even English is nonconfigurational. But here his arguments are seriously flawed. He examines (77) a selection of naturally occurring English text (fragments of conversation) to make his point. But all his examples are deviant in the sense that they involve false starts, self-corrections, one interlocutor cutting off another, etc. In fact, he shows as much by providing an edited example following the 'natural' examples. But the question he begs is the source of the editor's opinions. Obviously, since the editing involved would be more or less replicated by any English speaker, there is something about the grammar of English guiding it.

But for the Warlpiri and other examples of nonconfigurationality in the GG literature, the claim (implicit for the most part) is that these would NOT be edited, or at least that the relevant portions of them (their nonconfigurationality) would not be edited, i.e. that they are well-formed just as they are. By failing to recognize this crucial difference, his examples prove neither that English is nonconfigurational nor that the parameter is nonexistent. This is one of the weakest sections of a book that has many weak sections.

4. Conclusion

Both *TLO* and *BL* are informative and represent research programs involving large numbers of scholars. Ultimately, however, both books fail to convince. *TLO* fails because of artificiality of coverage and overblown claims (e.g. the title). *BL* fails because it is chockablock with errors of fact and reasoning. Still, my overall sympathies lie with *BL*. Givón's arguments for a more careful methodology in linguistics, e.g. that found in standard social science research, are well-taken. In the remainder of this section, I want to consider two vital issues that both books ignore and offer suggestions as to how each book might have 'learned' from the other.

First, a crucial component of language form and language-learning omitted from discussion by both books under review is the role of culture in shaping grammars. Growing evidence for this role leads many linguists to disagree strongly with the following assertion on the relative unimportance of culture to grammar:

> In fact, virtually all linguists today would agree that there is no hope of correlating a language's gross grammatical properties with sociocultural facts about its speakers. (Newmeyer 2002: 361)

Although this claim is implicit in much linguistics research, it strikes me as little more than the result of researchers failing to find what they fail to look for. For example, in Everett (2004), I provide an example of the effects of Pirahã culture on the Pirahã language's segmental phonological constraints and representations, which surely must count as a 'gross grammatical property' of the language. Moreover, various studies in Enfield (2002) and in Gentner & Goldin-Meadow (2003) show crucial (bidirectional) interactions between culture and language. And Tomasello (2003) shows how certain aspects of language learning are dependent on aspects of culture-influenced cognition. Thus, Newmeyer's claim cannot be taken as a report on serious research findings (even if, as one referee suggests, his view is indeed held by most linguists).

A second problem with both books is their failure to interact seriously with the growing literature on linguistic evolution. As a start, one interested in such matters should consult Aitchison (1996), Hurford, Studdert-Kennedy & Knight (1998), Knight, Studdert-Kennedy & Hurford (2000),

Briscoe (2002), Wray (2002), Kirby & Christiansen (2003), and others in the references below.

A third serious shortcoming of both books is their failure to acknowledge one of the most important developments in modern linguistic theory – the recognition of and research on the synchronic, diachronic, and acquisitional roles of linguistic constructions in grammars. Recent studies such as Goldberg (1995), Culicover (1999), Croft (2001), Tomasello (2003), and many others have shown the crucial role that constructions play in our understanding of language and cognition. For example, the theory of constructions allows the notion of the Saussurean sign to be applied to syntax. Language learners need to learn such signs (e.g. words) individually. And the function and form of such signs will reflect cognitive, communicative, and cultural constraints whether the signs are words or phrases.

Moreover, it turns out that the way in which individual signs are learned can be extended to the learning of what are considered by some to be the purview of Universal Grammar. For example, consider the following:

> Children build up the abstractness of their item-based constructions using their general skills of intention-reading and pattern-finding. They understand the communicative functions of utterances that embody various syntactic constructions, as well as some of their constituents, by reading the intentions of the speaker. They then find patterns across item-based constructions by schematizing and making analogies. (Tomasello 2003: 141)

Culicover (1999) provides a sustained argument that the learning mechanisms that all theories must allow for idiosyncratic constructions ('syntactic nuts') are able to extend to cover regular grammatical behaviour. The implication of this is that since idiosyncratic constructions must be learned, not simply 'grown', as in a Chomskyan model, then the rest of grammar may also be learned. This places a burden on accounts such as that of *TLO* which require that one aspect of grammar (the 'periphery' – idiosyncratic structures or constructions) must be learned, while the other portion (the 'core' – non-idiosyncratic grammar) must be hard-wired into the brain or human genome. This burden is to show that two cognitive relations of grammar to the brain are necessary, rather than one. And if Culicover (1999) and the other works cited above are on the right track at all, then the question which arises is whether Universal Grammar is any longer needed in a theory that recognizes constructions and construction-based learning. If it is not needed, then this will deeply affect our view of language biology. This would count as an argument against the central theses of *TLO*, but would also be relevant to *BL*. Thus, it is a shame that both books ignore constructions.

Finally, let me make two suggestions as to how each book might have profited by considering the other. First, *TLO*, as we have seen, ignores

serious consideration of the standard view of natural selection, as well as the role of discourse, intonation, and information structure on language-learning. This is a crucial failure since including such facts could (and for some researchers clearly does) show that the stimulus is not so impoverished after all. *BL* offers a survey of facts that suggest this, I believe, if read carefully, and *TLO* would be a better book if it had at least attempted to come to grips with some of *BL*'s data and reasoning. On the other hand, *TLO* is directed to a specific audience with a specific introductory purpose, so including this additional information could have been too distracting. Thus, though one laments the omissions in this regard, they are perhaps understandable, relative to the purposes the authors had in mind. *BL*, on the other hand, ignores many detailed and intricate aspects of grammar and phonology, and indeed offers little hope that it would be up to the task of handling many of the cases discussed in *TLO*. There is nowhere in *BL* any analysis as careful as the many analyses offered in *TLO*. If *BL* had been as careful as *TLO* in its argumentation, it would have made its case much more effectively. As it is, it glosses over facts with superficial analyses that fail to get at the empirical richness discovered and displayed in *TLO*. I therefore see no evidence for a new field of biolinguistics nor for language organs in either of these books. Indeed, I think we are too far from the requisite knowledge of brain physiology and its connections with grammar to give any content to the concepts.

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Author's address: School of Languages, Linguistics and Cultures, The University of Manchester, Oxford Road, Manchester M13 9PL, U.K.
E-mail: dan.everett@manchester.ac.uk