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‘Sometimes nothin’ can be a real cool hand.’  
Cool Hand Luke

## Biology and language: response to Anderson & Lightfoot<sup>1</sup>

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### I. INTRODUCTION

I welcome the opportunity that Stephen Anderson and David Lightfoot have afforded me (Anderson & Lightfoot 2006) to express in more detail why I think that explaining human grammars and human linguistic capacities by means of a language organ is deeply flawed. Let’s remind ourselves what they are trying to do. They are trying to convince us that there is an organ of the body dedicated exclusively to language and that language is a unique capacity of human cognition. Now, I am not going to claim that there is no such organ nor any such unique capacity. What I argued in my original review article (Everett 2005a), and what Anderson and Lightfoot fail to respond to in their reply, is that their book offers not a single convincing argument for a language organ or for a language-specific capacity. In fact, no one, not Pinker, Chomsky, Jackendoff, nor any other writer in the literature, has even come close to establishing anything more than the modest point that a Universal Grammar or Language Organ is not utterly implausible. (See also Sampson 2005 for several strong arguments against Universal Grammar.) There could be a language organ. But there isn’t evidence for one yet. And I do not believe that my view on this issue is biased – it wouldn’t bother me one way or the other if there were a language organ. I am a descriptivist, happily spending my career trying to figure out the grammars of previously undescribed languages. I would be pleased to discover that there is a language organ. I would be just as pleased to discover that there isn’t one. As they say where I come from, ‘I ain’t got no dog in that fight’.

Let’s review the points on which Anderson & Lightfoot and I agree, before taking up the points on which we disagree. We agree that language and grammar as found in humans is species-specific (the tautological flavour of this statement is deliberate). Second, we agree that the human linguistic

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[1] I want to thank Geoff Pullum, Paul Postal, Barbara Scholz, Peter Ladefoged and Geoffrey Sampson for comments on a draft of this reply.

capacity must be instantiated physically. Third, we agree that humans have grammar, that grammar is hierarchically organized, and that many grammatical processes are structure-dependent. We disagree on just about everything else.

The first thing I want to make clear here is that, contrary to many statements in the Universal Grammar (UG)-inspired literature, the burden of proof lies with the proponents of a UG/Language Organ, not with their critics. The reason is simple: in studies of human cognition, there are many general principles that could be explored as alternative explanations for the sources of human language.

Although Anderson & Lightfoot present themselves as going about the normal business of science, simply proposing the best explanations that they, as hard-working scientists, are able to come up with, the solutions they propose ignore decades of vital work on language, the architecture of complex systems, neurophysiology, literature on human sentence processing, cultural effects on grammar, and other research domains that crucially bear on their claims.

Anderson & Lightfoot criticize my review article's treatment of their book for three supposed shortcomings:

- (i) my failure to address their specific linguistic arguments;
- (ii) my failure to consider carefully their speculations on the organic basis of the Language Organ;
- (iii) my view of biology is merely high-school level, failing to account for the more sophisticated view that they develop.

## 2. THE CONCEPT OF ORGAN

Let's consider points (ii) and (iii) first. The normal definition of an organ, if one looks at just about any dictionary at random, will go something like this: 'A differentiated part of an organism, such as an eye, wing, or leaf, that performs a specific function' (*American heritage dictionary of the English language*, 4th edn).<sup>2</sup>

To establish that a 'part' of an organism is 'differentiated', one would have to provide spatio-temporal coordinates for the part and a clear account of just how it is differentiated. Anderson & Lightfoot appear to believe that my insistence on spatio-temporal coordinates for the organ (that it not be invisible, that it have some roughly specifiable location) is 'high-school biology'. But without such a prerequisite, 'organ' becomes little more than

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[2] An alternative definition that some physiologists work with allows a more functional definition of ORGAN, i.e. a structure that contains more than one kind of tissue functioning for a common purpose. This is perhaps closer to what Anderson & Lightfoot have in mind. However, they have shown no evidence for even this attenuated notion of organ. And notice especially that a 'structure' is a spatio-temporal entity, not entirely functional.

the specification of the functions rather than the tissue causally implicated in the functions performed. In this case, ‘language organ’ would be simply a synonym for ‘grammar’.

True, Anderson & Lightfoot claim (p. ■) that there is likely to be a physiological basis for the language capacity in humans, but they never move beyond vague suggestions of ‘functional’ categorizations. In their response, they say that I dismiss too briefly their chapter ten, on the ‘organic basis’ of the language organ. In this chapter, they make three claims: (i) only humans have the capacity for language; (ii) language is a function of the brain; (iii) language is a particular faculty. More specifically, as they say in their response to my review, ‘our argument was that we could usefully think about the functions of the brain that clearly stem from its biology. And that, in turn, would entail conducting our linguistic descriptions and analyses in particular ways, for example, defining and tackling poverty-of-stimulus problems head-on’ (p. ■).

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But there is no new information in any of these claims, nor would most linguists disagree with any of them except the ‘poverty-of-stimulus problem’, which Anderson & Lightfoot fail to substantiate (I return to this below). The question is not whether the brain must underwrite the tongue (to adapt a lovely turn of phrase adapted from a remark years ago by Geoffrey Pullum) – we all know that it must. The question is whether there is evidence that parts of the brain are exclusively dedicated to language or whether instead language is possible due to the general computational power of the brain. The chapter on the organic basis of the language organ is compatible with either position and so does not help to resolve the question. And they never answer the criticisms of Pulvermüller (2002) that I mention in my review, namely, that no one really knows how to isolate parts of the brain and say that they have this or that function exclusively. Therefore, I didn’t give much attention to this chapter in my review. I cannot see how it bears on whether there is a language organ more specific than the brain.

They add, apparently as an attempt to strengthen their case, that they have published, by invitation, a piece (Anderson & Lightfoot 1999) explicitly requested by the editor of *The Annual Review of Physiology* and that none of the physiologist readers gave them any seriously negative feedback on their use of ‘organ’. But surely this is irrelevant (just as their undergraduate courses in animal behaviour are). Again, no one is disputing that there must be a physiological basis for human abilities. What is in dispute is whether there are highly specific mechanisms like language organs responsible for such capacities, or whether the higher across-the-board cognitive abilities of humans can also be responsible for their greater abilities at information-processing, when coupled with other, independently needed cognitive constraints (e.g. those proposed by Simon 1996, Hawkins 2004 and Everett 2005b – see below). Precisely because physiologists are not linguists, they no doubt were very interested in hearing about linguistic findings that suggest

that the brain has this or that property. Whether the readers of that journal took the term ‘language organ’ literally or as a metaphor doesn’t really matter. What does matter to me, however, is that the results of Chomskyan research are being presented to the non-linguist world as though these were findings accepted as convincing by linguistics as a whole, when in fact nothing could be further from the truth. There are many, many linguists around the world who believe that the ‘language organ’ and universal grammar hypotheses are baroque alternatives to empirical research, declaring an explanation reached by fiat – to wit, a Chomskyan analysis of this structure/phenomenon is too strange to be learned, too unconnected to meaning to be inferred, so, therefore, it must be innate. Assuming that UG seems no more useful to many linguists than assuming the id and superego to many psychologists or the Marxist view of history to many historians.

I believe that Anderson & Lightfoot further undermine their own case by many disclaimers about physical properties of language organs. They say, for example, that ‘species-specific cognitive organization clearly forms a part of biology regardless of whether that organization can be attributed to uniquely dedicated tissue’ (p. ■). Fine. I agree. But then why, again, use the term ‘organ’? It does no work for us in this sense. Of course what is species-specific about us is going to have a lot to do with our biology, but that doesn’t mean we can go calling different abilities ‘organs’, just because these abilities are ultimately underwritten by biology. Further, consider their remark that ‘[o]ften the limits to an organ are unclear’ (Anderson & Lightfoot 2002: 218). Yes, perhaps; but although where the finger ends and the hand begins may be somewhat arbitrary, fingers are not theoretical constructs. We can all agree where the bulk of a finger is! There is something physical we can agree on. In their chapter on organic unity, Anderson & Lightfoot ask of the language organ, ‘Where should we expect to find it?’ (p. 221). Then they explain that you cannot find the language organ.<sup>3</sup> They refer (on page 225 of the book) to a ‘functional anatomy’ of the language organ – why? Why is it not necessary to discuss other organs in these terms? Because other organs do not lack spatio-temporal coordinates. They then take a hand-waving approach to Pulvermüller’s objections, when they say (on page 225 of the book), that ‘actually ... there is little reason to expect detailed maps of language areas in the brain to emerge soon’. Right. Once again I agree. But they in effect admit here that the language organ is invisible, by any standard, unlike all other known organs, and nothing more than a theoretical construct. Calling a theoretical construct that lacks spatio-temporal coordinates an ‘organ’ strikes me as hyperbole.

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[3] However, in their reply to me they make the amazing statement that: ‘Similarly, the view that if linguistics were as we claim, syntactic trees should be visible in CAT scans, asserted in Everett (2001), seems to reflect more on the adequacy of current brain imaging techniques than it does on the nature of language’ (p. ■).  $vp[v[...]]_{NP[...]}$  in a CAT scan? Come on.

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## 3. SPECIFIC SYNTACTIC ARGUMENTS

Anderson & Lightfoot take me to task for not considering their linguistic arguments, noting that I focus instead on Chomsky's. The reason I chose Chomsky's example is simply because it is better and clearer than theirs. *A fortiori*, my criticisms also apply to what they say. But they ask for a deeper consideration of their cases, so let us take up the challenge.

So far as I can tell, Anderson & Lightfoot's treatments of auxiliary verbs, case, etc. simply show that grammars exist. For example, perhaps their strongest results related to auxiliaries are that modals are systematically different from main/content verbs (e.g. modals have different structural constraints on their placement) and that the changes affecting English modals all occurred simultaneously in the history of English (their proposals on English are in fact controversial, but I will accept them for the sake of discussion here). So, *ex hypothesi*, English modals are subject to grammatical constraints that operate diachronically and synchronically. But this surely says nothing about whether the grammar Anderson & Lightfoot propose must be formal, functional, innate, or learned. In fact, their analysis is suspect because they present a formal grammar in which meanings of auxiliaries, and generalizations across such meanings, play no role. Yet, many theories have argued that semantic-based generalizations provide more insights into grammatical processes than strictly form-based generalizations (e.g. Generative Semantics or Role and Reference Grammar). The methodological sleight of hand on which the generative enterprise rests is the omission of meaning and meaning-based analyses. But such an approach, once again, reverses the burden of proof. Anderson & Lightfoot claim that there is a language organ. But this only follows (and by no means automatically even then) if their formal account is the only one available. Communication-based functional accounts do not require a language organ that pre-specifies so much form; and yet Anderson & Lightfoot, as I pointed out in the original review article, do not offer any arguments against such accounts, nor do they even acknowledge the possibility of other accounts, something one must do to convince the lay reader (part of their intended audience) of solid linguistic arguments for a language organ. Therefore, their linguistic analyses, while possibly interesting qua linguistics, really have nothing to say about language organs, just about grammars. And I don't think the two are synonymous. Once again, therefore, I conclude that the authors have failed to make a case for a Language Organ.

## 4. ALTERNATIVE EXPLANATIONS AND PLATO'S PARADOX

What would Anderson & Lightfoot or anyone else have to do to establish the existence of a language organ? Most importantly, it seems to me, they would have to show how the properties of human language and grammar cannot



be accounted for by more general properties of the world and human brains. That is, they would need to convince us of the poverty of the stimulus or, as some call it, Plato's Problem or Plato's Paradox (Simon 1996: 211). But they never do this, except to show that, given their THEORY-INTERNAL assumptions, it seems unlikely that any other theory could work. At the same time, they fail, as all in this set of research programmes do, to consider some obvious information that would render the stimulus less poor. I mentioned in the review article (Everett 2005a: ■) that intonation could be a crucial part of the stimulus; but they (and perhaps all others in the generative tradition) ignore this. They say that the burden of proof is on me to show how intonation and other such facts could account for language acquisition. But this is certainly a strange place to heave the burden of proof. THEY are claiming that the stimulus is poor. I show a stimulus source that they ignore and they ask ME to prove its relevance. That is putting the burden of argumentation exactly backwards. If a theory is going to propose an invisible organ and a 'universal' grammar (that seems to me anything but universal), based on the poverty of the stimulus, yet simultaneously ignore intonational stimuli that are available to children from the womb, stimuli which other researchers have shown to be relevant for parsing and understanding sentences, then the burden of proof is on THAT THEORY, no place else, to show that intonation (or other stimuli) is irrelevant. I only needed to show in my review that the stimulus might not be as poor as they claim. I did this. Their only appropriate response is to show how intonation couldn't be causally implicated in grammar acquisition. In fact, there are various other relevant data that they fail to engage with that would largely obviate the need for a language organ, if correct. Let's consider a couple of these.

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Consider, first, alternative explanations for the tree-structures that we find in natural language (since tree node labels in more recent theory are specified lexically, rather than categorially, we can assume that tree-structure itself is what needs to be accounted for, lexical categories at least partially 'falling out' from rote learning and cultural context). In this regard, perhaps the most important article regularly neglected in the formal linguistics literature is the 'Architecture of complexity: hierarchic systems' by Nobel prize-winning economist (cognitive scientist, and computer scientist) Herbert Simon (1996: 183ff., originally published in 1962). Simon argues that hierarchic structures emerge in artificial systems as the most efficient way of dealing with complex environments, whether in economics, thinking, or symbol-manipulation systems such as language, among others. Simon (1996: 197) introduces the fascinating and extremely important notion of 'nearly decomposable systems', whereby he offers an account of why interactions between items dominated by the same 'node' in any complex system are more common than interactions across hierarchical nodes (the potential relevance of this concept for structure-dependent syntactic rules, including notions such as subadjacency, etc. is obvious). Language can be thought of

as a naturally-occurring nearly decomposable system in Simon's sense, 'in which the interactions among the subsystems are weak, but not negligible' (see top of page 198 in his book). But if this is so, then much of the core grammar proposed in Chomskyan linguistics, and that informs many if not all of the principles that Anderson & Lightfoot are so concerned with, could follow from general principles of nature, rather than specific principles of cognition or, *a fortiori*, language. Simon (1996: 187) makes it clear that physical and biological hierarchies are defined in spatial terms. Further, in the same section, he argues that the greater stability of hierarchical organization favours the evolution of such organization in natural systems, including symbolic systems, e.g. language.

Simon (1996: 207) goes on to argue the interesting point that 'if there are important systems in the world that are complex without being hierarchic, they may to a considerable extent escape our observation and understanding. Analysis of their behavior ... would be beyond our capacities of memory or computation'. This simply means that not only could language be hierarchic and nearly decomposable without appeal to a language organ, but that it would HAVE TO BE SO ORGANIZED.

Let's consider another alternative. An issue of language form which is crucial to any consideration of a language organ is the origin of more specific principles of linearization and ordering that underlie so many of the language universals that Anderson & Lightfoot, typologists and many other linguists are deeply interested in. Hawkins (2004) argues that efficiency and simplicity are vital organizing principles of grammar and that these account for much of linearization, thus appealing to principles of general human cognition, rather than to a specific language organ. He proposes the extremely interesting Performance-Grammar Correspondence Hypothesis (PGCH), which claims that 'grammars have conventionalized syntactic structures in proportion to their degree of preference in performance, as evidenced by patterns of selection in corpora and by ease of processing in psycholinguistic experiments' (Hawkins 2004: ■). Hawkins' research shows how performance can supplant grammar in explaining numerous grammatical universals. But then for every universal that can be explained by processing restrictions like efficiency and simplicity, there is one less argument for innate grammatical mechanisms or organs. Although Hawkins' work has been prominent for many years, Anderson & Lightfoot fail to consider it at all.

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Finally, Anderson & Lightfoot also fail to consider any role for culture in shaping grammars, except to comment briefly that culture is unlikely to play any role given the huge number of cross-linguistic universals and similarities (Anderson & Lightfoot 2002: ■). But notice that many if not all of those similarities might fall out from Simon's 'architecture of complexity' proposal, in conjunction with Hawkins' theory of complexity and efficiency. And there are many significant differences between languages, and many

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of these might be traced to culture. I argue in Everett (2005b) that culture can exercise an architectonic role in shaping grammars. It seems to me that one reason such effects are mentioned so rarely in the literature is because linguists don't look for them. And since long-term field research in a culturally vibrant community of speakers is relatively rare among linguists, especially those who advocate language organs, it is all too easy to dramatically underestimate the role that culture likely plays in shaping grammars. I take Everett (2005b) to be a severe counterexample to claims on the irrelevance of culture, as I also take it to be a counterexample to the claim that recursion is universal, as proposed in the recent work by Hauser, Chomsky & Fitch (2002) that Anderson & Lightfoot invite me to examine.

## 5. CONCLUSION

The concept of a language organ is too abstract (requiring an organ without spatio-temporal coordinates), is unnecessary, and is not causally implicated by any of the analyses in Anderson & Lightfoot's work. I conclude, therefore, just as I did in my review article, that Anderson & Lightfoot have failed to meet the burden of proof in establishing any explanatory need for language organs, as opposed to explanation in terms of more general work in cognition and human language that accounts for an even richer set of facts without this deity.

Moreover, Anderson & Lightfoot's theory is itself based on a theory of syntax that, so far as I can tell, lacks an object. UG/Language Organ-based linguistics claims to study those aspects of language that are innate and underdetermined by the stimuli in our natural environment. This means that this brand of linguistics cannot study (as part of UG) phenomena in which functional, communicative, non-linguistic, historical, cultural, or other non-formal considerations are causally implicated. But since we can never know what these considerations might be a priori, their exclusion by definition from the research programme leaves the research programme with no positively definable object of study.

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