

The Creative Aspect of Language Use and Nonbiological Nativism

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1. Introduction

The Cognitive Science era can be divided into two distinct periods with respect to the topic of innateness, at least from the viewpoint of the linguist. The first period, which began in the late 1950s and was characterized by the work of people like Chomsky and Fodor, argued for reviving a nativist position, in which a substantial amount of people's knowledge of language was innate rather than learned by association or induction or analogy. This constituted a break with the empiricist/behaviorist/structuralist tradition that had dominated the field before that.

The second, more recent period added to the first period's claim of innateness the explicit claim that the innate knowledge in question is to be understood entirely within an evolutionary biological framework. For example, the innate knowledge is taken to be coded in the genes and to have arisen as an evolutionary adaptation. Within linguistics anyway, this second period began rather sharply in 1990 with the publication of Pinker and Bloom (Pinker & Bloom, 1990). Before that work, discussions of the evolution of language were relatively rare and peripheral to the field, whereas they have now become commonplace. It is not that Chomsky, Fodor, and other "first generation" cognitive scientists ever denied that the innate knowledge of language was biological in this sense. But they were not very interested in this aspect and thought there was little to gain in practice by developing the theory in this way.

In fact, I believe the current state of things in linguistics shows that the first generation's reticence on these matters was warranted. The basic notion that many of the fundamental principles of (say) syntax are innate in humans is a powerful and useful idea, and there is occasion for the practicing linguist to make use of it on an on-going, month by month basis. Questions of whether such and such a syntactic phenomenon should be attributed to the innate endowment or not, and if so in what form, arise regularly and provoke interesting and profitable discussion. In contrast, the additional assumption that this innate knowledge is a genetically encoded evolved adaptation, to be understood with the tools of evolutionary psychology has not been powerful or productive, and ordinary linguists need not appeal to it on a regular basis. It has not led to any substantive new discoveries that I am aware of, nor has it given deeper explanations for previously known but mysterious details about the language faculty. Sometimes it might do some damage, by making researchers unduly conservative about positing a highly structured innate endowment, in my own personal view. So at best, it has been an inert hypothesis, allegedly contributing to the foundations of the field at a level which is invisible to most practicing linguists. At worst it has raised mysteries about how the fields connect that it does not solve.

Different people react to this perceived disconnect between (one kind of) linguistics and biology in different ways. Some look at biology and infer that Universal Grammar could not be as the Chomskian linguists say it is. Others concentrate on the attested linguistic data and ignore the biology as being too crude, brutish, and speculative

to have any practical bearing on their linguistic theories. Still others deny that there is any serious tension at this stage of our knowledge—which undeniably has many large gaps in it. They might hope that the interface between linguistics and biology will become a practical and meaningful one as work continues to progress in both generative linguistics and evolutionary psychology.

In this paper, I want to explore another possible reaction to this practical disconnection—namely, the idea that there could be some innate ideas and/or processes that are not strictly biological in nature. The usual argument in favor of the evolutionary psychology approach to innate structure in language is the “it’s the only game in town” argument. Pinker and Bloom (1990), for example, emphasize that adaptive evolution is the only scientific explanation for functional complexity. Similarly, in a more general context, Carruthers (Carruthers, 1992) notes that the attraction of evolutionary psychology is that it provides a way of naturalizing nativism. Perhaps so, but this sort of argument in a domain where explanatory success is limited often sounds to me like an argument from the poverty of imagination. There is, of course, no *logical* entailment from nativism to biological nativism. The historical proof of this is that nativism is older than biology as a theoretical framework. The original, 17th century brand of nativism espoused by Descartes, Leibniz, and others long predates the main results of modern biology with which nativism is now associated in what Fodor (Fodor, 2000) calls the “new synthesis”.¹ So the idea that there might be innate structure to the (human) mind that is not explained by current biology is a logical possibility, to be decided for or against by the weight of empirical evidence. This is the foundational question I propose to consider in this paper. More specifically, I will argue that there is no evidence in favor of the additional biological assumption and a little evidence against it when it comes to our understanding of at least one important aspect of the human capacity for language.

My discussion will unfold in the following stages. In section 2, I review Chomsky’s paradigm-defining claim that the human capacity for language involves not only of the well-studied components of a vocabulary and a grammar, but also what he calls ‘the Creative Aspect of Language Use’ (CALU). In section 3, I present what are, as far as I know, the first explicit arguments that this third component is (like syntax) substantially innate in humans. Section 4 then asks whether the kinds of evidence that have sometimes been used to argue that syntax is part of standard biology are replicated for the CALU, claiming that they are not. In particular, I look briefly at evidence from neurolinguistic studies of aphasia, at genetic syndromes that target language, and at comparisons with other primates. Section 5 argues that it should not be a surprise that the CALU has not been explained biologically, since it cannot even be characterized computationally; it is an intrinsically abductive capacity and the computational theory of mind does not account for true abductive processes. In this, I draw a link between Chomsky’s concerns about the CALU and Fodor’s concerns about abduction as showing the limits of the current cognitive science paradigm.

2. A Factoring of the Language Faculty

¹And indeed the nativist tradition is even much older than this, including Plato and many pre-modern Christian thinkers.

If one wants to inquire into which parts of a complex phenomenon X (language) are to be attributed to theory Y (say evolutionary biology) and which parts are not, it is helpful to have some idea of what the major parts of X are in the first place. What then might be the parts of the human language faculty?

In fact, Chomsky proposed a first pass answer to this question back at the start of the cognitive revolution, in the 1950's (Chomsky, 1957, 1959). All of the successes in modern generative linguistics since that time arguably depend on his answer, whether this is realized or not. Basically, Chomsky factored language into three components (each of which could, of course, have great internal complexity): the lexicon, the grammar (syntax in the broad sense²), and what he came to call the 'Creative Aspect of Language Use', or CALU (Chomsky, 1966). In dividing up language in this way, he was making a practical and methodological decision; he was trying to distinguish those questions that were open to meaningful inquiry given the current state of knowledge, and those that were not. His claim was that syntax could be investigated, but the CALU could not for the foreseeable future (Chomsky, 1959). (Later, by 1975, he began to entertain the idea that we will never understand the CALU component, because it falls outside the domain of what our cognition can handle, much as the notion of a prime number falls outside the domain of rat cognition (Chomsky, 1975).) The project, then, is to explain what sentences like "Colorless green ideas sleep furiously" and "Harmless little dogs bark quietly" have in common—the property *grammatical*—and not to try to explain why or in what circumstances a person would be more likely to say one rather than the other (Chomsky, 1957). To use an analogy from the construction industry, the lexicon is like the bricks, mortar, and other building materials, while the grammar is like the building codes, which specify ways in which these materials can be combined to make larger units, such as walls, roofs, rooms, and ultimately houses. These two facets of the language faculty we have a reasonable hope of uncovering and understanding, according to Chomsky. But the construction industry would not get very far with just raw materials and building codes. It also needs architects to decide where the walls should go in particular cases to achieve a desired effect, and contractors to actually assemble the raw materials into walls in ways consistent with but not uniquely determined by the strictures of the building codes. In the same way, the human capacity for language must consist of more than a lexicon and a grammar; it also contains the power to assemble the words in accordance with the grammar to make actual sentences. It is this capacity that Chomsky calls the CALU, identifying it for the purposes of distinguishing it from grammar and putting it aside, at least in the bulk of his technical work.

This distinction is so much part of the common ground, at least for generative linguists, that it is easy to miss how important it is. But the pre-Chomskian behaviorist tradition crucially did not factor language in this way. For them, the project was to predict (and control) what a person would say when presented with a particular stimulus, as a result of the person's history of conditioning (Skinner, 1957). By framing the project

² The broad sense of syntax that Chomsky used at that time included also the phonology and aspects of what would now be called semantics—all of language that is rule-based and compositional. In this paper, I'll use grammar and syntax more or less interchangeably. For the broad construal of syntax, this is accurate; for the narrower sense of syntax (how words are combined to make sentences) it is not accurate, but harmless. Narrow syntax is not identical to grammar, but it is an important and representative subpart of grammar.

in this way, they were attempting to explain the form and the content of a sentence all at the same time, and in the same terms. For example, a certain visual stimulus would trigger a particular set of words by association, those words would trigger additional words by association, and finally the collection of words so activated might trigger a simple syntactic frame into which all those words could be slotted, again by association. Thus, looking at a Rembrandt canvas might trigger the word 'Dutch', and the canvas and/or the word Dutch itself might trigger 'great' and 'painter'. Then these three content words together might trigger the frame 'The ___ were ___ ___s' resulting in the utterance 'The Dutch were great painters.' What is being said and the structural conditions on it come about by essentially the same theoretical devices. And that way of looking at the problems of language proved empty and sterile, as Chomsky demonstrated with force. So 'verbal behavior' remained a weak point for Behaviorism because it did not distinguish the CALU from grammar and lexicon. In contrast, language became a domain of great accomplishment and credibility for cognitive science because Chomsky did make this distinction.

Chomsky's fullest positive characterization of what the CALU is in Chomsky (Chomsky, 1966), where he discusses with approval Descartes' observations about human language expressed in Part V of *A Discourse on Method*. The Creative Aspect of Language Use, Chomsky says, is the human ability to use linguistic resources (vocabulary items and syntactic rules) in a way that has three properties simultaneously: it is (i) unbounded, (ii) stimulus-free, and (iii) appropriate to situations. Descartes was interested in this constellation of properties because he believed that it could not be explained in purely mechanical terms, within a theory of contact physics. Descartes observed that no animal had communicative behavior (or any other behavior) that had these properties, nor did any automaton, existing or imaginary. He wrote:

Of these the first [test] is that they [machines] could never use words or other signs arranged in such a manner as is competent to us in order to declare our thoughts to others: for we may easily conceive a machine to be so constructed that it emits vocables, and even that it emits some correspondent to the action upon it of external objects which cause a change in its organs; for example, if touched in a particular place it may demand what we wish to say to it; if in another it may cry out that it is hurt, and such like; but not that it should arrange them variously so as appositely to reply to what is said in its presence, as men of the lowest grade of intellect can do. ... For it is highly deserving of remark, that there are no men so dull and stupid, not even idiots, as to be incapable of joining together different words, and thereby constructing a declaration by which to make their thoughts understood.

Descartes (and Chomsky) observes that it is easy to imagine machines that utter a limited number of words or set phrases. These words or phrases could be uttered deterministically, whenever a certain stimulus is experienced, or they could be uttered randomly, with no connection to the environment. What is special about human language behavior is that we "arrange [words] variously" (i.e. in an unbounded way), not in a reflex-like way determined by stimulus, and yet also not randomly but rather "so as appositely to reply to what is said in [our] presence" and "construct a declaration by which to make [our] thoughts understood" (i.e. in ways that are appropriate). That our

language use is unbounded is not enough to make it creative: it would not be creative to repeat back unchanged an infinite variety of sentences that we hear in our presence. That our language use is stimulus-free is not enough to make it creative: it is not creative to speak words randomly. And it is not enough that it be appropriate: it would not be creative to produce the three utterances “Danger: snake”, “Danger: eagle” and “Danger: leopard” in the correct circumstances.³ But behavior that is simultaneously unbounded, not determined by stimuli, and (not random but) appropriate is special. That is what Descartes took as being sufficient evidence that a creature has a mind, and what Chomsky said must be put aside if one was to make progress on understanding other aspects of language in generative, computational terms.

How does Chomsky’s tactical decision to factor the problems of language into vocabulary, syntax, and the CALU look 50 years later? The answer is that it still looks good. Chomsky was right that grammar/syntax could be ripe for investigation in terms of then-new formal notions like recursive rules, and linguists have full bookshelves to prove it. This has led to enormous new discoveries about the structure of English and hundreds of other languages. In contrast, there has been essentially no progress on the CALU, and the bulk of linguists have followed his advice not to pursue it—even to the point of nearly forgetting that it is there. And yet no reasons have come to light to deny that the CALU exists. We know a lot more about the building codes for sentences than we did—and a bit more about the building materials themselves—but not significantly more about the architects and contractors. Yet we must still assume that the architects and contractors exist, simply because there are actual sentences around us which do more or less comply with our building codes but which our building codes cannot account for the existence of or aspects of the nature of. We can say why the object follows the verb in a particular sentence, just as we can say why the wall forms a right angle with the floor. But our grammar cannot say why that particular object was used with that verb on that occasion, just as our building code cannot say why that wall is exactly there in that particular house.

If we accept this division of the language faculty into (at least) the vocabulary, the grammar, and the CALU, we can then go on to ask for which of these is there evidence that it is innate. Then we can ask for which of these is there evidence that it is biological in the usual sense. And finally we can ask whether the answers to these two answers match.

Before proceeding to that, however, let me pause to comment on why it is easy for cognitive science, evolutionary psychology, and related theories to overstate their results. It could be that the field is based on certain decisions about how to delimit the domain of inquiry that have become so much part of our habits that we forget about them.

³ See Cheney and Seyfarth (Cheney & Seyfarth, 1990) ch. 4, 5 for detailed discussion of vocal communication among vervet monkeys. They show that the alarm calls of the vervets are not only appropriate but may also be stimulus free. At least they are not reflex-like, because the calls are not automatically triggered by the seeing of a certain predator; they can be suppressed if an animal is alone or in the presence of rivals rather than kin/allies. They perhaps can also be falsified in order to distract or scare off a rival band of monkeys. But even if they are used in these ‘creative’ ways, there is no doubt that the vervet communication system is strictly bounded, consisting of less than 10 distinct vocalizations and no system for combining them compositionally. This is another illustration of the point that all three CALU characteristics are needed to qualify as true Cartesian creativity. (I thank Steve Lawrence for pointing out this reference to me.)

This is especially tempting when our theories aspire to completeness along one dimension. For example, it might not be so far from true that if you give me any English sentence I can parse its phrase structure completely, identify its nonlocal dependencies, and so on. Suppose then that I had a complete grammar for English. I could in a sense legitimately brag that my cognitive science can give me a complete theory of English. But that does not mean that I have made the slightest progress on the question that Skinner thought was important: how to predict (and control) what particular English speakers would say when presented with a stimulus. Would they say ‘Harmless little dogs bark softly’ or ‘Colorless green ideas sleep furiously’—sentences that are entirely the same as far as my so-called ‘complete’ theory is concerned. There could be another whole dimension to the phenomenon, which I am prone to ignore.⁴

3. Is the CALU Innate?

Let’s then work with this idea that the human language capacity consists of three possibly complex factors: vocabulary, grammar, and the CALU. Then we can ask which of these are innate. I accept the familiar Chomskian arguments that grammar is largely innate, apart from a limited number of parameters that are fixed by experience (see Baker 2001 for some review and discussion). I will not consider the vocabulary at all, although I note that its overall structure might very well be innate too, even though the particular sound-meaning pairs certainly are not. (See, for example, Baker (Baker, 2003) for arguments that the same three-way distinction between nouns, verbs, and adjectives is found in all languages, although a particular notion such as ‘tall’ might be expressed as a noun in one language (‘John has tall[ness]’) or a verb (‘John tall[s]’) or an adjective (‘John is tall’). I focus instead on the little-discussed CALU, and ask whether the same kinds of arguments that show that grammar is innate can be constructed also for it.

One kind of evidence that syntax is largely innate comes from the fact that it is universal in the human species. For example, it is thought that every language in the world distinguishes grammatical subjects from objects, and contains basic structures in which the object is more closely grouped with the verb than the subject is (Baker, 1988). Is the CALU similarly universal? The answer is, of course, yes. Every society uses its language in the free expression of thought. In every society people spontaneously make up new sentences in a way that is not controlled by their environment or any simple characterization of their internal states, but that is seen as appropriate and purposeful in the situation. We know of dolls and stuffed toys that give a fixed range of set responses in a mechanical or random fashion, but we don’t know any people groups that are like this. This applies to the long-isolated tribesman of places like Tasmania and the New Guinea highlands as much as to any other group. I know of no controversy on this point whatsoever.

⁴ When I say that the language faculty is factored into the lexicon, the grammar, and the CALU, I don’t necessarily mean that any of these are specific to language. They could be more general mental capacities that can be applied to language. For example, the lexicon is probably at least deeply connected to a more general conceptual system which can also be deployed in other ways (say, in nonverbal reasoning, below the level of consciousness). Similarly, the CALU may be nothing more than the human ‘free will’ (whatever that is) being exercised in the domain of language. I intend to leave this entirely open. In any case, language is interesting because it reveals our free will most fully and richly, as understood by Descartes, as well as Turing (Turing, 1950) and others.

A second kind of evidence that syntax is largely innate comes from the fact that it emerges so early, before children have learned many other things that seem simple to us. It has been shown that the very first two-word utterances of children, which appear before the age 2, already show evidence of syntactic structure. English children say ‘want cookie’ whereas Japanese children say ‘cookie want’, showing the particular kind of fixed word order that the full language they are learning specifies (Bloom, 1970; Brown, 1973; Slobin, 1985). Even more spectacularly, French children consistently put finite verbs before negation and nonfinite verbs after negation—even though they don’t yet use nonfinite verbs in the correct, adult-like way (Deprez & Pierce, 1993). This argument also applies to the CALU: children’s early utterances are also stimulus-free and purposeful. They are not, of course, unbounded at the two word stage, by definition. But there is much reason to think that the two-word utterances are abbreviations of larger structures that the child has in mind but cannot utter yet (Bloom, 1970). There is a fair amount of variety even in two word utterances, as children make different choices about which of a range of semantic relationships to express on a given occasion. And, more significantly, there is no ‘three word stage’. After a few months, language use explodes in an unbounded fashion so that it is no longer possible to enumerate the structural combinations the child uses. Children’s utterances also may not be appropriate in the adult’s sense; they are often tactless and abrupt, for example. But that is not the sense of appropriateness is at issue in the CALU; rather, the point is that the children’s utterances are not random strings of words that arise by free association. And in any case, it is clear that CALU behavior is fully in place long before children start kindergarten.

By far the most important argument for innateness is the poverty of stimulus argument, which has several flavors. Since this is the most important consideration, and since it could be controversial, I will linger over it a bit. Then I will mention two variants of this argument that have been used in the case of syntax, which seem also apply to the CALU.

The basic idea of poverty of stimulus arguments is that there is richness and structure in the cognitive state arrived at by the child that is not present in the child’s environment—or at least not in the data available to the child. (See, for example, Crain and Pietroski (Crain & Pietroski, 2001) for a recent review and discussion.) Typically, this arises when the data is ambiguous in a certain way: either grammar A or grammar B could create the observed sentences, and children never hear a crucial example that one grammar can account for and the other cannot, but yet one can show that the children all end up with grammar B. The conclusion is that they must have had some kind of innate bias toward grammar B rather than grammar A. As a linguist actively involved in fieldwork, I can attest that this sort of situation *arises all the time*. I constantly face the fact that even if I have been working hard on a language for years, some structure comes up such that nothing I know tells me whether it is possible or not. I need to resolve the matter by coaxing a native speaker into judging some carefully engineered sentence, but children somehow resolve the ambiguity without that opportunity.

There are several ways one might try applying poverty of stimulus reasoning to the CALU. Suppose that the CALU were not innate. That would mean that the child somehow picks it up from its environment. And there are various things to learn. The simplest might be: ‘My parents are not automata.’ They are using their vocabulary and grammar to make sentences in a way that is neither stimulus-bound nor random, but

rather in a way that is appropriate and purposeful. The second thing to learn, which may come from that is ‘I should not be an automata; I too may make sentences that are unbounded and stimulus free, yet appropriate to the situation and my goals.’ The third and most important thing to learn would be *how* not to be an automaton—how to develop the capacity to use the language in this way.

The last and most important version of this question I think we cannot even frame at this point. We have no theory, computational or otherwise, of how knowledge of vocabulary and grammar can be used to make an infinite variety of sentences in a way that is neither determined nor random. (I discuss this at more length in section 5.) And since we have no precise way to specify the knowledge that this capacity depends on or the process that it involves, we cannot estimate the amount of information that is involved. If we had free-speech programs, we might be able to count the bits in the simplest such programs. Then we could compare that to the information that is realistically accessible to children in their environment, and see whether it is commensurate. But we have no such programs, so we have no such measure. Since we don’t know what the CALU is with any precision, we cannot know what would be required to learn it. Therefore we also cannot know whether the information that is needed is accessible in the environment of the child.

I therefore retreat to what might be the simplest in this cluster of ideas: the question of whether my parents are automata or not. Surely mature people know that they are not, and it has great significance in how they live—in particular how they talk to others. We do not talk in a free, unbounded way to things that we believe are automata, such as dolls and voice-menu systems on the phone. So part of the CALU is knowing who to use it with—namely those that have the CALU capacity themselves. Now either the notion that my parents are CALU users, not automata, is innate, or it is learned. Suppose it is learned. How could it be? What kind of experiences would the child have with other people that would convince him that they are not automata?⁵ Are those kind of experiences available to the child? If not, and if children do regularly acquire this belief, then we have a poverty of stimulus argument that applies to at least this important part of the CALU. And I assume that if poverty of stimulus arguments apply to this the simplest component of the CALU, then a fortiori they probably apply to the other, more complex and mysterious aspects of the CALU as well.

We can get some hints that poverty of the stimulus might apply from the fact that (unlike children?) some intellectuals have thought that people are automata in the relevant sense. Skinner and the behaviorists, for example, thought that people’s verbal behavior was not stimulus-free. Descartes used the CALU as his test for other minds, but many of his contemporaries were not particularly impressed with this test, and considered it a weak point in the Cartesian framework. Turing, who did so much to

⁵ This question is similar to and may be related to the question of how a child acquires a “Theory of Mind”, as studied by Alan Leslie (Leslie, 1994) and others. But it is not identical to it. A child can decide that another creature has beliefs he knows to be false without explicitly using language, for example. One can also very well imagine another creature has beliefs and intentions while still not showing Cartesian creativity in language use. Cheney and Seyfarth (1990) tentatively believe this about vervet monkeys, for example. So the CALU question and the theory of mind issue are partly independent. (On the other hand, it seems very unlikely that a creature could show Cartesian creativity in language use but not have beliefs and intentions that are manifested by their language use. That is more or less the point of Descartes’ test for other minds and Turing’s test for intelligence in machines. So the two issues are not unrelated.)

formalize the notion of computation, thought that it would be possible for a computational device to use language in a way that would be indistinguishable from how people use it by the year 2000 (Turing, 1950). This suggests that, if it is true that people use language in a way that cannot be characterized as either deterministic or random, the evidence for this is subtle and not easily observable.

We can get a more concrete handle on this by recalling the disagreement between Chomsky and the behaviorists. The behaviorists believed that people's utterances (as well as their other behavior) was determined by the stimuli that impinged on them plus some (very impoverished) sense of their internal state; Chomsky did not believe this. How did Chomsky argue against the behaviorists, and is the evidence he used observable to a child? Chomsky (Chomsky, 1959) started from an example of Skinner's in which someone looks at a painting and Skinner says "one could very well say 'Dutch'". Chomsky agrees, but adds that one could just as well say any variety of other things as well, including 'clashes with the wallpaper', 'I thought you liked abstract work', 'Never saw it before', 'Tilted', 'Hanging too low', 'beautiful', 'hideous', 'remember our camping trip last summer'... or whatever else might come to our minds when looking at a picture. So we can have an unbounded number of responses to the same stimuli, many of which could count as appropriate to the situation (and hence not random). Suppose that we agree that Chomsky's argument is correct and compelling. The question is, is the crucial fact it hinges on observable? Is it the sort of thing one can see in another, and hence conclude that he is not an automaton? I think the answer is 'No', or at least 'Not easily'. Suppose that Chomsky's child had a chance to observe Chomsky in front of Skinner's painting. He cannot observe the many things that Chomsky knows he might have said; he can only observe the one thing that Chomsky did in fact say on this particular occasion. That observation is perfectly consistent with the view that Chomsky's response is determined by the stimulus; maybe he always says just that when he is confronted with such a picture in such a situation. Since his child has never seen Chomsky in *exactly* that situation before, there is no evidence against the automaton theory. Perhaps the child could compare Chomsky's responses to similar situations over time, but that would not be easy; one would have to decide what situations counted as similar in the relevant ways and keep track of a potentially unbounded amount of data to resolve the question in this way. Chomsky's argument is compelling when we put ourselves in that position: we have an inner sense of freedom that says "Yes, I could in fact say any of those things." But we cannot observe someone else's sense of freedom in this way; we can only indirectly evaluate whether they say a suggestive subset of the things that we would say in exercising our freedom. Therefore, there is a poverty of stimulus argument here. If other people are stimulus-free in their language use and if we all come to know this, it is not on the basis of readily observable data. Thus, this belief is likely to be innate.

Now consider the other way that the CALU could fail to hold: verbal behavior could be stimulus-free, but not appropriate. In that case, it could be modeled not as a deterministic computation, but rather as one that has some kind of random component. Chomsky (Chomsky, 1957) argued against this kind of view, and again he used unobservable data to do so. He considered a family of views that assumed that the n th word one utters is some kind of probabilistic function of the previous $n-1$ words. To argue against this, he asked the reader to imagine the sequence 'I saw a fragile --.' He

suggests that one has never heard the word ‘whale’ following this sequence of words, nor has one ever heard the word ‘of’ there. So both words have probability 0.0 as continuations of the sentence. And yet we have very different reactions to the two sequences: ‘I saw a fragile whale...’ is grammatical, whereas ‘I saw a fragile of ...’ is not. Chomsky concludes that grammaticality is not a matter of statistics or statistical approximation. ‘I saw a fragile whale’ is grammatical but not used because it is (almost always) inappropriate, whereas ‘I saw a fragile of’ is ungrammatical. Contrasts like this show that language use is not a stochastic process, that we have knowledge of language that goes way beyond the conditional probabilities of one word appearing in the context of other words. But notice that once again the data this argument depends on is not readily observable. Imagine that a child is entertaining the hypothesis that his parent is a random word generator. He will not be able to resolve the question in the same way that Chomsky did, since by hypothesis he never observes the parent saying either ‘I saw a fragile whale’ or ‘I saw a fragile of’. The relevant fact is that the parent *could* (in some respects) say the former but not the latter—but what the parent could do but never does is not observable. Again we have a poverty of stimulus issue. If people are not random word generators, and if we come to know that this is so, then it seems that this cannot be because we reliably have direct access to unambiguous crucial evidence. Rather, the idea is probably innate.

I have, of course, only considered two extreme positions: that my parents’ verbal behavior is completely determined, or that is completely random. Lots of other, hybrid combinations are possible, which include both deterministic and random elements, but are still compatible with my parents being automata. For example, suppose that after hearing ‘I saw a fragile – ‘ it is determined that the next word will be a noun, but random what particular noun it will be. That would account for the ‘whale’ vs. ‘of’ intuition without positing a mysterious CALU faculty. And I suppose that must be what the fully committed computationalist who doesn’t believe in mysteries must believe. I’m not really trying to press that point here. But it does reemphasize the poverty of stimulus point. If language users do in general have the (possibly tacit) belief that other people use language in a way that is neither determined by their situation nor random, but rather accomplishes communicative goals related to the free expression of thought, then they did not arrive at the belief by raw observation. It is very likely to be an innate belief. And since this is related to the CALU more generally (telling us who we should employ the CALU with), the CALU complex as a whole is probably also innate.

In the domain of syntax, there are other sub-varieties of the poverty of stimulus argument that one might try to apply to the CALU as well. For example, it has been claimed that children learning syntax never make certain kinds of errors. More precisely, they make errors that show that they do not know the idiosyncratic details of the language they are learning (as you’d expect) but not errors that violate Universal Grammar. They make mistakes with the parameter settings, but not with the invariant principles. This suggests that they are testing hypotheses when it comes to the parameters, but they are taking the principles for granted (see, for example, work by Stephen Crain, Rosalind Thornton, etc.). The parallel question would be whether children ever make mistakes with the CALU. Do they go through a period where they seem to be interpreting those around them as automata, whose words are some combination of determined and random, rather than purposeful and self-expressing? Do

they go through a period in which they themselves act this way? If so, that would show that they are trying out the hypothesis that people have a vocabulary and a grammar, but no CALU. But surely they do not do this. If there is any stage in which children act and interpret others as automata, it must be before the language explosion in the third year of life—and before this there is not much evidence from language use at all, one way or the other.

A final variety of the poverty of stimulus argument that has been applied to syntax is to look at language development in abnormal, unusually impoverished environments. If the experience available in such an environment is clearly reduced, but there is little discernable impact on the knowledge attained, that is taken as evidence that the knowledge in question has a large innate component. One classic linguistic example is creolization, when, as a result of the brutalities of the slave trade children did not get an adequate sample of any one native language to acquire it properly. What seems to happen is that they create a new language from the available parts that has its own UG-obeying regularities, not attributable to the mishmash of pidginized material spoken around them (Bickerton, 1981). Another classic example is congenitally deaf children of hearing parents, who are isolated from spoken language by their lack of hearing and who are isolated from sign language by accident or design. In these situations, deaf children in interaction with their care-givers make up a sign language *de novo*, and these ‘home signs’ are claimed to have many of the characteristics of more ordinary human languages (S. Goldin-Meadow & Mylander, 1983). The quality of the facts in these areas is controversial, but all agree that how they turn out has bearing on issues of innateness.

The question now is whether similar arguments could be constructed that would bear on the innateness of the CALU component of language. The answer seems to be yes. Probably no child has been exposed to a rich vocabulary and a full-fledged grammar who has not also been exposed to language use that is unbounded, stimulus free, and appropriate; the CALU was presumably modeled in the pidgin utterances on the sugar plantations, even if grammar was not, for example. But the case of deaf children not exposed to standard sign languages is relevant, since they have essentially no independent model of language use at all. Nevertheless, the home signs they develop seem not to be used the not mechanistically, but for the free expression of thought. Indeed, it is presumably in part the deep human need to interact in this way that pushes them to create their own new vocabulary and grammar in the first place. Such cases were apparently known already to Descartes, who writes:

In place of which men born deaf and dumb, and thus not less, but rather more than the brutes, destitute of the organs with others use in speaking, are in the habit of spontaneously inventing certain signs by which they discover their thoughts to those who, being usually in their company, have leisure to learn their language. (p. 45)

Modern work on home sign systems by Susan Goldin-Meadow and her colleagues bears out Descartes’ observation. There seems to be no doubt that children use their gestures in ways that are appropriate in the Descartes/Chomsky sense of not random and expressive of their thought. Home signs are perhaps more bound to the immediate situation of utterance than conventional languages are, because they tend to lack (a rich set of nouns) and rely on pointing to refer to things. But this does not mean that they are not stimulus-

free; it is not at all the case that children utter the same sign sentences mechanically when they are put in the same situations. The biggest concern might be whether the home sign systems are unbounded. Home sign sentences tend to be short, with a mean length of utterance of only 1.2 or 1.3 signs per utterance (S. Goldin-Meadow & Mylander, 1983), compared to MLUs of close to 3 for speaking children of comparable ages. But it is hardly surprising that there is some developmental delay, given the extreme impoverishment of the input and the limited time window for developing the home sign (I assume that sooner or later all these children end up getting exposed to some conventional language or other). Goldin-Meadow and Mylander do point out that the maximal length of utterance (as opposed to the mean) for most of her subjects was between five and nine signs, which is not significantly less than that of children with conventional input. They have also argued in detail that every child's home sign is recursive, allowing one proposition to be embedded inside another (Susan Goldin-Meadow, 1982, 1987). Indeed these embedding structures appear in home signs at about the same age (2.3 years) that they appear in the productions of hearing children. So this language use does count as unbounded. So even these children, with very limited outside input, develop the CALU capacity more or less on schedule. Moreover, Goldin-Meadow and Mylander compared her children's use of home sign with that of their mothers. She discovered that even though mothers made many individual signs, they were significantly less likely to combine those signs into structured sentences consisting of more than one sign than the children were. Approximately 15% of mothers' utterances consisted of more than one sign, compared to 30% of children's utterances, and recursion appears earlier in the child's signing than in the mother's in almost every instance. Thus, there is no evidence that the children are picking up CALU capacity from their mothers; rather it seems to be emerging spontaneously from within them, as one would expect if the CALU is innate.

Putting this together with the other arguments in this section, I conclude that one can probably construct a case for the CALU being to a large extent innate in humans that is as strong or stronger than the familiar case for syntax being to a large extent innate.

4. Is the CALU “narrowly biological”?

The next question is whether there is evidence that the CALU is biological. Of course, the answer depends on how one understands the terms. If biology is simply the study of life, then given that the CALU is a property of some things that are alive (humans, anyway) it is tautological that the CALU is a biological phenomenon. But in discussing this question I am interested in biology in a narrower sense: does the CALU fit comfortably in the framework of contemporary biology, such that it shows similar properties to things known to be biological and some of its properties are elucidated by the basic theories in biology. More specifically, is there evidence that the CALU is embodied neurologically, that the relevant neural structures are coded for in genome, and that these genes arose through evolutionary mechanisms. If the answers to these questions are yes, then the “new synthesis” paradigm of evolutionary psychology may be adequate for all instances of innateness. But I will claim that the answer is no, and hence that there may be a distinct category of nonbiological innateness. Again it will be useful in some cases to compare the CALU with syntax, which has been more studied.

I begin with neurology, which is the first and most concrete of these levels, and probably the best understood. It is common-place to assert, at least in popular presentations, that by now we know that everything one can imagine the mind doing is directly dependent on the brain. For example, Steve Pinker writes (Pinker, 2002) p. 41:

One can say that the information-processing activity of the brain *causes* the mind, or one can say that it *is* the mind, but in either case the evidence is overwhelming that every aspect of our mental lives depends entirely on physiological events in the tissues of the brain.

This is a very strong claim, stated with immodest words with universal force (“overwhelming”, “every aspect”, “depends entirely”). It has been tested with respect to the grammar and the vocabulary in many works. We can then consider the CALU in this light, seeing if there is overwhelming evidence is that this particular aspect of our mental lives depends entirely on physiological events in the tissues of the brain.

First, let us clarify what we are looking for. There is no doubt that the CALU capacity is dependent on the brain in the trivial sense that a person without a functioning brain will not be able to manifest that ability. That by itself need not be any more significant than the fact that a person without a tongue and with paralyzed arms may also not be able to manifest the ability, for obvious reasons. The more interesting and less obvious issue is whether there are particular neural circuits that serve this particular function, such that having those circuits intact is both necessary and sufficient for having the CALU capacity. Such circuits have been found for many other functions in perception, motor control, and language (to name a few). The question I’ll consider then is whether there is evidence for the CALU of this sort.

The oldest and perhaps the best line of research here is the study of aphasia—the effect of damage to the brain on language. This is fairly well-trod ground, with a history that goes back more than 140 years to Paul Broca’s work in the 1860’s (Caplan, 1987; Goodglass & Kaplan, 1972; Kertesz, 1979). The topic also receives new attention and funding after each major war. Clinicians have developed a relatively stable typology of 7-10 aphasic syndromes over this long history. This classification has its origins in a paper by Lichtheim in (1885), which set forth a proposal for a complete enumeration of all aphasic syndromes. Geschwind (1964) revived Lichtheim’s typology, and Benson and Geschwind (1971), in a major textbook of neurology, adopt Lichtheim’s classification, adding only three additional syndromes (which are largely conjunctions of the original ones). These authors also show that all of the important classifications of aphasia since Lichtheim’s differ from his almost exclusively in nomenclature, not in substantive descriptions of syndromes or in how those syndromes relate to areas of the brain. It still forms the basis of the most popular clinical classification of aphasias in North America (Caplan, 1987) p. 55). Controversies exist, but most of them focus on whether the 7-10 classical syndromes are discrete or whether they can shade into each other in a continuous fashion, and whether finer-grained differences in the symptoms can be revealed by closer, more linguistically informed scrutiny. But there is remarkably little disagreement on the general lay of the land, on what is—and is not—affected by brain damage. The question, then, is whether any of these classical syndromes affects the

CALU in a differential way, so we are tempted to say that the CALU circuit has been knocked out while others have been spared.

At first, the answer seems to be yes. The hallmark of CALU is language use that is unbounded, stimulus-free, and appropriate. Wernicke's aphasia (the second kind to be discovered) seems to be characterized by language production that lacks the 'appropriateness' feature. Here is a sample:

His wife saw the wonting to wofin to a house with the umblelor. Then he left the wonding then he too to the womin and to the umbella up stairs. His wife carry it upstairs. Then the house did not go faster thern and tell go in the without within pain where it is whire in the herce in stock. ((Goodglass & Kaplan, 1972) 59)

This speech is clearly unbounded and stimulus free, but it is not what we would consider appropriate. The speech of this kind of patient is characterized as fluent and has normal intonation and free use of grammatical elements, but it is rambling, disorganized, needlessly complex, and full of malapropisms, phonological errors, and nonwords. The impression that we are witnessing a random string of words (and word-like elements) can be pretty strong. This could be a population that really does say 'Colorless green ideas sleep furiously' or any other grab bag of words that occurs to them.

But Wernicke's aphasia cannot be just a disruption of the CALU capacity. This is seen from the fact that Wernicke's patients also have language disruptions that have nothing to do with the CALU. In particular, they have serious problems understanding words presented in isolation. Two prominent clinicians write about this syndrome that "The impairment of auditory comprehension is evident even at the one-word level. The patient may repeat the examiner's words uncomprehendingly, or with paraphrastic distortions. At severe levels, auditory comprehension may be zero..." (Goodglass & Kaplan, 1972). This deficit is thus not a problem with putting words together; it is a problem with the words themselves. Wernicke's aphasia must be a disruption of the vocabulary component of language and not (just) a disruption of the CALU component. Indeed, the classical understanding of Wernicke's aphasia is that the sound-meaning association involved in words is disrupted—a view that fits well with the fact that Wernicke's aphasia is caused by lesions in the "association cortex". Now given that we know that the vocabulary is affected in Wernicke's aphasia, considerations of parsimony lead us to ask whether this deficit is enough to explain the characteristic speech production of these patients, or whether we must assume that the CALU is affected too. In fact, the vocabulary deficit is sufficient. One can well imagine that Wernicke's aphasics have reasonable sentences in mind at some level, but they often activate the wrong pronunciations for the meanings that they intend. That by itself would be perfectly sufficient to create the effect of random-seeming strings of words. And in fact, a vague plotline can be discerned underneath Wernicke aphasic speech once one factors out the malapropisms. Here is how a Wernicke's aphasic describes a picture of two boys stealing from a cookie jar while their mother is busy washing dishes in an overflowing sink:

Well this is ... mother is away here working her work out o' here to get her better, but when she's looking, the two boys looking in the other part. One their small tile into

her time here. She's working another time because she's getting, too. So the two boys work together an one is sneakin' around here, making his ... work an' his further funnas his time he had. He an' the other fellow were running around the work here, while mother another time she was doing that without everything wrong here. It isn't right, because she's making a time here of work time here, letting mother getting all wet here about something. The kids aren't right here because they don't just say one here and one here—that's all right, although the fellow here is breakin' between the two of them, they're comin' around too. (Goodglass & Kaplan, 1972)

Although it wanders and is full of wrong words, there is a vague sense of what is going on here.⁶ So this type of aphasia shows us clearly that aspects of the vocabulary component are dependent on brain tissue, but not that the CALU is. Similar remarks hold for the other so-called 'fluent aphasias', especially the rather rare Transcortical Sensory Aphasia. These patients too produce random, purposeless sounding speech, including sentences like 'He looks like he's up live work ... He looks like he's doing jack of inarys ... He lying wheatly.' But their deficit shows up clearly even when they are giving one-word names to items ((Goodglass & Kaplan, 1972) p. 73).

For a cross: 'Brazilian clothesbag'
For a thumb: 'Argentine rifle'
For a metal ashtray: 'beer-can thing'
For a necktie: 'toenail ... rusty nail'

There is obviously a severe problem with the vocabulary here, and that is sufficient to explain all the other problems. (The major difference between Transcortical sensory Aphasia and Wernicke's aphasia is that TSA patients can repeat sentences that they hear much better than Wernicke's patients can. This has no bearing on CALU issues, because CALU is not necessarily involved in the capacity to repeat a sentence word for word; that is not a 'stimulus free' behavior.⁷)

A very different-seeming possible loss of the CALU is found in Broca's aphasics. Here the problem is not with the appropriateness, but rather with the boundedness of the linguistic output. In more severe cases, patients speak only one word at a time. Here is a sample conversation (Goodglass & Kaplan, 1972):

Interviewer: What did you do before you went to Vietnam? Patient: Forces
Interviewer: You were in the army? Patient: Special forces.
Interviewer: What did you do? Patient: Boom!
Interviewer: I don't understand. Patient: 'splosions.

⁶ It has been suggested that the wandering nature of Wernicke's aphasia speech is a side effect of the patient not understanding his own speech, because of his severe problems with lexical access. Therefore, he cannot effectively monitor his own speech, and does not get any feedback about when he has successfully communicated an idea. This makes him prone to repetition and wandering on a theme.

⁷ Two other syndromes in the fluent aphasia family are anomia and conduction aphasia. Anomia is another problem with the lexicon; it is hard to access content words, making the aphasic's speech full of vague words like 'thing', 'place', deictic words, and circumlocutions. Conduction aphasics have special difficulty in repeating sentences, but their free speech and comprehension is not so bad. In both cases, it is clear that the CALU is intact.

(More questions) Patient: me .. one guy
 Interviewer: Were you alone when you were injured? Patient: Recon... scout
 Interviewer: What happened; why are you here? Patient: Speech
 Interviewer: What happened? Patient: Mortar

Here is a Broca's aphasic description of the cookie theft picture:

Cookie jar ... fall over ... chair ... water ... empty ... ov ... ov... (examiner: overflow?) Yeah. (Goodglass & Kaplan, 1972) p. 54

One might well think of this as a deficit of the ability to put words together into sentences. But again, this is not the only problem that a typical Broca's aphasic has. They also have severe articulation problems, the prosody of their speech is affected, and their speech is slow and effortful, even when they are saying only one word. There are also syntactic problems (agrammatism), where inflections are lost and only the most primitive constructions are used. "While he [the Broca's aphasic] may try to form complete sentences, he has usually lost the ability to evoke syntactic patterns, and even a sentence repetition task may prove impossible from the grammatical point of view." ((Goodglass & Kaplan, 1972) p. 55) So the Broca's aphasic certainly has problems with articulation and grammar that do not directly concern the CALU, because they affect even one word utterances and repeated sentences. Are these deficits enough to explain their behavior without the CALU itself being affected? Apparently yes: if saying words is so effortful and syntax is not automatic, it is plausible to think that Broca's patients have complete sentences in mind but these get reduced down to one or two word utterances because of the difficulty of producing the sentence. This is consistent with the fact that their ability to interpret new sentences is relatively intact—an ability that presumably also draws on the CALU. As a result, their one word responses are appropriate if not unbounded. Moreover, they do not seem to be stimulus bound: note that the patient responds to the very vague question 'What happened?' with the statistically unlikely but appropriate in context answer 'mortar'. Goodglass and Kaplan also observe that the patient may try to form a sentence—even though they fail for more peripheral reasons. So here we see that grammar can be affected by brain damage (as well as articulation), but again there is no clear evidence that the CALU is affected.

What would a true CALU aphasia be like? Patients with this aphasia would have good object naming and word recognition, showing that the lexicon is intact. The speech would be fluent and free from grammatical errors, when (say) they are repeating a sentence or reciting a known piece like a song or the Lord's Prayer. That would suggest that the grammar is intact. But the speaker would fail to put together words spontaneously into phrases, and/or he would put them together in a seemingly random, purposeless fashion. All these symptoms exist—but this particular combination of the symptoms into a syndrome does not seem to be attested. So perhaps it is not true that brain damage can disrupt any mental function one can imagine; it does not disrupt the CALU itself.⁸

⁸ Transcortical Motor Aphasia (also called Dynamic Aphasia (Maruszewski, 1975) pp. 111-15) has the combination of symptoms that is most like the missing profile. It is grouped with Broca's aphasia as a nonfluent form of aphasia. However, there may not be such obvious problems with articulation, and

This conclusion mirrors a classical view in neurolinguistics. Lichtheim 1885 proposed a model of the language faculty that featured three distinct “centers”: motor (production), auditory (perception), and conceptual (Caplan, 1987). These centers were connected to each other by neural pathways, and the motor and auditory centers were connected to the outside world in the obvious way. Lichtheim explained the range of known aphasias by proposing that any center or pathway could be disrupted by brain injury—with the striking exception of the “concept center”. And, as already mentioned, the classification of aphasias that has emerged from this view has stood the test of time, and is still the basis of most clinical diagnosis more than 100 years later. The anomaly that his system had one crucial ingredient that was not prone to disruption by injury is treated as a conceptual flaw by subsequent neurologists (such as Caplan)—but they have not discovered the missing syndrome or proposed a way of rearranging the attested syndromes so that the gap does not appear. Lichtheim’s “concept center” is that aspect of the language faculty that is the last step in comprehension, the first step in production, and (in contrast to perception and articulation) is not involved in simple repetition. Thus it is plausibly the same faculty as what I have been calling the CALU. So the result of 140+ years of neurological research seems to be that there is no real evidence that the CALU depends on dedicated circuitry in the brain tissue.⁹

Now let us turn from neurology to genetics: what evidence is there that the CALU is genetically encoded? Is there a CALU gene, or set of CALU genes, somewhere in the human genome? If so, then one might expect to find developmental disorders that affect the CALU in a differential way—disorders that might ultimately be traced to genetic abnormalities. Are there such disorders? The classification of Specific Language Impairments (SLI) does not have as rich and stable a history as the classification of aphasias has, but it has been the subject of intensive research in the last 20 years or so. Standard classifications come from Bishop (Bishop, 2004) and Rapin and Allen (Rapin & Allen, 1983). Bishop (2004) tentatively identifies four types of SLI: typical SLI, severe receptive language disorder, developmental verbal dyspraxia, and pragmatic language impairment. The first three are rather clearly irrelevant to the CALU: typical SLI affects the grammar component (uniquely or along with other features of language); severe

repetition of sentences is quite good, free from grammatical errors. For some reason, this aphasia is not discussed in as much depth in my sources. Goodglass and Kaplan (Goodglass & Kaplan, 1972) have only one short paragraph, and neither they nor Kertesz (Kertesz, 1979) give an illustrative case study. The main symptom that these patients have, apparently, is a failure to initiate speech at all. Maruszewski describes it thus: “These patients lack an active attitude and do not initiate speech; they generally complain of “emptiness in the head” and inability to phrase in words the information they want to express.” He cites Luria’s view that “This was thought to be a kind of disorder of verbal thinking involving the loss of the ability to programme a text spontaneously in the mind.” This would seem to be a disruption of the CALU, if anything is. However, one probable indication that the CALU is intact but not easy to express is that these patients’ ability to understand novel sentences is apparently quite good, and the capacity to interpret novel sentences presumably draws on the CALU too. Also, these patients tend not to initiate even one word utterances, and such utterances would not require the CALU; this suggests that the problem is one of activating speech. Furthermore, Kertesz (1979) mentions that most patients with Transcortical Motor Aphasia are capable of bursts of (often agrammatic) speech at times, and Maruszewski (p. 113) mentions examples where the patients form very good sentences when they have a certain kinds of visual props to help them focus. This should not be possible if the CALU circuit were truly gone.

⁹Lichtheim’s own view was that the concept center was spread diffusely through out the brain, which means that it is not affected by localized lesions. That is, of course, another legitimate possibility.

receptive language disorder is a problem with auditory processing; developmental verbal dyspraxia is a problem with articulation or perhaps with more abstract phonological representation. Children with syndromes of the first and third types are apparently capable of speech that is unbounded, stimulus-free and appropriate—it is just grammatically flawed and/or phonologically deviant. The only type of SLI that might be relevant, then, is Pragmatic Language Impairment (Rapin and Allen's Semantic-Pragmatic Disorder). This is described as follows: "The child with early language delay goes on to make rapid progress in mastering phonology and grammar and starts to speak in long and complex sentences, but uses utterances inappropriately. Such children may offer tangential answers to questions, lack coherence in conversation or narrative speech, and appear overliteral in their comprehension." (Bishop, 2004: 321). Rapin and Allen's (1983: 174) description is as follows:

In general, the utterances they produce are syntactically well-formed, phonologically intact, and, on the surface, "good language". On closer examination, however, one discovers that the language is often not really communicative. That is, there is a severe impairment in the ability to encode meaning relevant to the conversational situation, and a striking inability to engage in communicative discourse. Comprehension of the connected discourse of the conversational partner also appears to be impaired, although short phrases and individual words are comprehended.

This might be seen as a CALU deficit: their speech is unbounded, stimulus free, but not appropriate. However, it seems like something different is being meant by 'appropriate' here. In characterizing the CALU, Chomsky and Descartes use 'appropriate' in opposition to 'random': it is the characteristic of speech that responds to a situation in a way that is neither deterministic nor probabilistic. And children with Pragmatic Language Impairment are capable of speech that is appropriate in this sense. What Bishop and Rapin & Allen seem to be getting at in their descriptions is something more along the lines of speech that is on its own wavelength. It might be purposeful, but the purposes do not mesh properly with those of their conversational partners.¹⁰ Rapin and Allen's example is instructive in this respect; they report (p. 175):

For example, the question "where do you go to school?" was answered by one of our children with "Tommy goes to my school because I see him in the hall everyday, but we have different teachers, and I like arithmetic but Tommy likes reading."

The child's response is inappropriate in that he clearly did not answer the question. But it is perfectly coherent and meaningful when taken in its own terms. He may be missing something about social sensitivity, but he is not missing his CALU in Chomsky's sense. Overall, then, while there may (or may not) be evidence in the literature on developmental disorders for a 'grammar gene' defects in which produce Typical SLI, there is no corresponding evidence for a 'CALU gene'.

Finally, consider the prospects for explaining the origin of the CALU in humans in terms of evolution. I suppose that if we do not know how the CALU is embodied in the neural hardware, nor how it is specified in the genetic code, the chances of

¹⁰ Indeed, both sources conjecture that this syndrome is related to autism.

constructing a detailed evolutionary account are slim to none. (Of course it is easy to tell stories about why having the capacity to freely express thought in a way that is appropriate to but not determined by situations is advantageous to survival and reproduction. The evolutionary paradigm can be applied to the CALU at that level. But I take discussions that exist only at that level of generality to be of limited interest—indeed, to be almost tautological.) But this gives me my chance to comment on a notorious issue of whether apes are capable of language when they are raised in the right environment. In order to make progress on this endlessly contentious question, one must have a clearly agreed-on notion of what is essential to language. The vagueness and polysemy of the word ‘language’ has apparently caused much misunderstanding and hurt feelings in this regard. But rather than getting embroiled in this in a wholesale way, I propose to stay focused on the factoring of the language capacity into (at least) vocabulary, grammar, and the CALU. Can apes acquire a vocabulary? Apparently yes; apes raised by humans have been shown to master a number of arbitrary signs in the 100s. Can apes acquire a grammar? Maybe. This has been taken to be the crucial question in much of the literature. Savage-Rumbaugh (Savage-Rumbaugh, 1994) and her colleagues have argued that the bonobo chimpanzee Kanzi can understand grammatically complex sentences in English, and shows three simple syntactic regularities in his own productions—including systematic ordering of verb before object, as in English. (Their tests do not approach the level of sophistication of those that clinicians use to probe the capacities of agrammatic aphasics, however.) But for my purposes here, the crucial question is whether apes can acquire the CALU capacity. Here the answer seems to be an unequivocal no. Even Kanzi, supposedly the most proficient of the apes, had a mean utterance length of just over one. Savage-Raugh (Savage-Rumbaugh, 1994) reports that only about 10% of his utterances consisted of more than one sign, and it was very rare for him to use more than two or three signs in one utterance. This language behavior falls short of the CALU on the unboundedness test. Kanzi compares unfavorably in this respect even with the home-sign-using children of Goldin-Meadow and Mylander, who have a mean utterance length of 1.25, use multiple sign sentences 30% of the time, and have a maximum sentence length of 5-9 signs.

Savage-Raugh (Savage-Rumbaugh, 1994) goes to some pains to explain that this boundedness of output is not Kanzi’s fault. His vocal tract is not well-configured to speak sentences longer than that. His hands do not have fine enough control to sign sentences longer than that, having been toughened by walking on them. His best method of communication is pointing to signs printed on a keyboard. But in this modality has an inherent limitation: once his vocabulary gets large enough to say an interesting range of things, it takes too long to find the symbols he wants within the unwieldy matrix of symbols. So there is a combination of factors that together have the effect that it is not fair to expect an ape to produce unbounded speech in real time, according to Savage-Raugh. Maybe so, but this sounds rather like a conspiracy. A simpler and more unified explanation consistent with the facts is that the CALU is lacking in the ape. Overall then, Descartes was correct that there is nothing like the CALU attested in the animal kingdom apart from human kind (see also Cheney and Seyfarth (Cheney & Seyfarth, 1990) on the scope of primate communication in the wild). Thus, there is little explanatory advantage to gain by saying that the CALU developed evolutionarily, by the gradual improvement

in or change in the function of a preexisting capacity through the mechanism of natural selection.¹¹

So overall we have seen that, like grammar, there is good reason to believe that the CALU is innate, given its universality in humans and poverty of stimulus considerations of various kinds. However, there is no good evidence from aphasia that it is neurologically embodied, no good evidence from developmental disorders that it is genetically encoded, and no good comparative evidence that it evolved from something that we have in common with closely related primates. The CALU seems to contrast in this respect with grammar, at least some aspects of which do seem to be affected in well established neurological syndromes, in a particular developmental disorder, and which has been exhibited (it is claimed) by at least one ape. So there is reason to think that there is a type of innateness that is not narrowly biological.

5. Computation, abduction, and the CALU

Having compared normal human language capacities to those of damaged humans, children, and apes, we can also compare them with the language capacities of man-made computational devices. This is relevant because cognitive theories are computational theories, more or less by definition. And we have a pretty good idea of how the brain can do computations. So therefore, it seems plausible that anything that can be specified computationally could fit into a narrowly biological paradigm, at least in principle. We know that computers can do a lot given enough computational resources (time, memory, CPU), and we know that the brain has lots of resources, given the huge complexity of its neurons and their various interconnections. So therefore it might seem that there is nothing mental that could not be understood within the biological paradigm. In contrast, if we learn that the CALU cannot be modeled computationally, for good reasons, that it is perhaps not so surprising that it also cannot be explained by the usual tools of biology.

Our confidence that the computational theory of mind can be a complete theory of mind could be another instance of the blindness that comes from understanding everything along one dimension making one forget that there is another dimension to consider. Fodor (Fodor, 2000) makes this case in some detail, arguing that computational psychology has seriously overstated its successes. In particular, he presses the point that the computational paradigm has no account for the phenomenon of abductive reasoning—i.e., inference to the best overall explanation, when there is no way of

¹¹ Ape language researchers often chafe at what they see as a double standard. Why am I so ready to attribute CALU to the 1.75 year old child and the Broca's aphasic and so slow to attribute it to the ape, when their linguistic outputs are similar, at least inasmuch as they consist mostly of one word utterances and never more than two or three words. I can understand why they feel that way. But considerations of parsimony cut different ways in the different cases. The Broca's aphasic clearly had the CALU before his stroke, so the question is whether he still has it. Parsimony leads one to favor a yes answer if other known difficulties are enough to explain the behavior observed. The young child clearly will have the CALU in another six months, so the question is does she already have it. Parsimony can lead one to favor a yes answer, if other known developmental changes are enough to explain the change in behavior. But the ape never manifests the CALU. So here parsimony leads one to favor the no answer, since there is no evidence to the contrary. After all, the CALU is a very special capacity that only a tiny percentage of things in the world have; surely a heavy burden of proof falls on someone who claims that a new kind of thing has this capacity.

knowing in advance what facts will be relevant. He points out that the transformations that a computer can do on an input—what we call “information processing”—must only depend on the syntactic properties of that input, on how it is put together. It cannot depend on the semantic properties of the input, such as what the various symbols refer to. This is not some accidental limitation stemming from how computer scientists happen to write programs; rather, it is embedded in Turing’s definition of what computation is. Thus, computers are wonderful at reasoning deductively (when well-programmed); they can tell what conclusions follow because of the form of the premises better than humans can. But they cannot reason abductively, telling us what conclusions follow because of the content of the premises. So whereas the theory of computation can give us a wonderful account of one type of rationality in terms of nonrational, physical processes, it cannot give us an account of another type, almost by definition. So Fodor identifies the question of how abduction is possible as the great mystery that hovers over cognitive science.

One obvious way to respond to Fodor’s argument is to deny that the human mind really does work abductively. Perhaps we only have the illusion that we reason abductively, whereas the truth of the matter is we are reasoning in a deterministic and modular way. Fodor’s primary example of abductive reasoning is scientific theory construction, which he takes to be a somewhat purified and amplified version of how our minds normally work. But his critics can respond that the striking thing about scientific theory construction is that human beings are not, by nature, very good at it. Only the most intelligent people do it, and even they make many mistakes that need to be corrected by communities of peers over historical time. Therefore, the development of science is dubious as an illustration of how human cognition normally works (see, for example Sperber (Sperber, to appear) note 7).

Here the factoring of the language capacity into vocabulary, grammar, and the CALU is relevant again. Grammar (syntax) can be handled by classical computational architectures; that is no surprise, since computation runs on syntax. But the CALU capacity is an abductive process par excellence. I think there is every reason to believe that non-modular, non-syntactic inference to the best explanation is rampant in our every day, ordinary ability to construct and interpret new sentences that are appropriate. That is how we compensate for the vagueness, ambiguity, and indeterminacy that natural language is full of, usually without even knowing it. I will walk through a simple example that concerns the interpretation of pronouns in discourse. Consider the following sentences:

- (1) John wasn’t feeling well, so he went to his doctor yesterday.
- (2) He examined him for an hour, but could find nothing seriously wrong.

Probably you understood the ‘he’ in sentence (2) as referring to the doctor mentioned in (1), and ‘him’ as referring to John. On what does this interpretation depend? Certainly it does not depend just on the syntax of (1) and (2). (3) has the same syntax as (2), but now ‘he’ is naturally interpreted as John and ‘him’ as the doctor.

- (3) He questioned him for an hour about what his symptoms could mean (but wasn’t satisfied by the answers).

So at least the interpretation depends on the lexical semantics of the verb in the second sentence, as well as the syntax of the two verbs. Now the meaning of the first verb turns out to be relevant too. So consider:

- (4) John wasn't feeling well, so his doctor saw him on short notice.
- (5) He examined him for an hour, but could find nothing seriously wrong.

In (5) (which is identical to (2)), 'he' refers to the subject of (4) (the doctor) and 'him' refers to the object of (4) (John). Syntactically speaking, this is the opposite of (2), where 'he' refers to the object of (1) and 'him' refers to the subject. At this point, one might think that the lexical semantics of the verbs is the key to the story, but other examples show that syntax plays a role too. For example, (2) cannot have the so-called reflexive interpretations in which the doctor examines the doctor for an hour or in which John examines John. However, (7), which has a different form of the object pronoun, may and must have one of these interpretations:

- (6) John wasn't feeling well, so he went to his doctor yesterday.
- (7) He examined himself for an hour....

Another minor change in the syntax in (9) and (10), and now the pronouns 'he' and 'his' can (but need not) refer to the same person, either John or the doctor. (9) is biased toward the referent being John; (10) toward the referent being the doctor.

- (8) John wasn't feeling well, so he went to his doctor yesterday.
- (9) He examined his infected toenail for an hour (while sitting in the waiting room).
- (10) He examined his charts for an hour (before entering the exam room).

Finally, consider (11) and (12). These are identical to (1) and (2), except that the feminine pronoun 'her' is used instead of the masculine pronoun 'him'.

- (11) John wasn't feeling well, so he went to his doctor yesterday.
- (12) He examined her for an hour, but could find nothing seriously wrong.

Now the competent speaker is inclined to interpret the subject pronoun 'he' as referring to John, not to the doctor as in (2). The reason is as follows: 'Her' must refer to someone that the speaker thinks I know about. All I know about is John and his doctor. John certainly refers to a male (confirmed by the use of 'he' to refer to John internal to (11)). Doctors can be female just as well as male, so 'her' must refer to the doctor. Therefore, 'he' cannot refer to the doctor in this case (even though doctors usually do the examining). John is the only other one I know about, so 'he' must refer to John. This is a complex chain of reasoning, based on both syntactic elements and real world knowledge—e.g. the fact that doctors can be women but people named John never are.

I think it is clear that there is abductive reasoning (inference to the best overall explanation) going on to make sense of these perfectly ordinary examples. Clearly, the conclusions do not just rest on the syntax. One could imagine enriching the syntax in

various ways. For example, one might model the difference between (2) and (3) by saying that the verb ‘examine’ is automatically replaced by one kind of syntactic structure in the course of processing, and the verb ‘question’ is replaced by another kind. Then there would be hope of running the pronoun interpretation off of the enriched syntax in a deterministic way. (Linguists have experimented with such “lexical decomposition” theories in the past, but they are now popular only in a limited domain—and certainly they are not used to explain the interpretation of pronouns.) So some factors that don’t look modular and syntactic on the face of it could be turned into factors that are syntactic. That would be tempting if the properties of the verb were the only nonsyntactic matter that pronoun interpretation depended on. But it definitely is not: the interpretation also depends on the meaning of the verb in the first sentence, on the kinds of adverbial modifiers that are included—and even on our knowledge of our culture’s naming practices and which professions are practiced by women. Indeed, it seems that almost anything we know can influence how we interpret pronouns. All of this is from the point of view of the language perceiver, but the same considerations apply to the language producer, who constructs sentences like (1)-(12) in ways that depend on what he (tacitly) thinks the hearer will be manage to understand abductively. (Of course, this won’t always work perfectly; there will be occasional miscommunications. But they are relatively rare, and don’t detract from the overall nature of what is going on.) So there is good reason to think that abduction is alive and well in the human mind, and one of its names is the CALU.¹²

Now I find a very interesting convergence here. Look at the state of the art in language processing by computers. There is no problem giving computers a vocabulary. Giving them grammatical knowledge is harder, but not impossible; systems exist that can parse naturally occurring sentences to a reasonable degree of accuracy, and we have fallible but not (quite) useless grammar checkers on our computers. But computers are still spectacular failures at spontaneously producing coherent speech and at interpreting such speech outside of very limited domains. Turing’s (Turing, 1950) famous test is yet to be passed by a machine, well after the 50 years he predicted. Indeed, the CALU part of language is clearly the locus of the most striking failure. Thus Shieber (Shieber, 1994) observes in connection with recent attempts to run something like the Turing Test in connection with the Loebner prize, that the programs that people are willing to rate as most ‘human-like’ to date succeed almost entirely because they find ways to avoid the expectation that conversation should be appropriate in the CALU sense.¹³ The semi-successful programs choose to pose as a psychoanalyst, or as a paranoid schizophrenic, or

¹² Sperber (Sperber, to appear) also holds that (something like) abductive reasoning does happen in the human mind. Nor does he deny Fodor’s argument that abductive reasoning cannot be explained computationally. However, he holds out the hope that the brain does abduction through noncomputational processes, suggesting that the many different subprocessors in the brain compete for energy resources depending on how active they are, how many inferences they are generating, and so on. Sperber gives a sense of how this could be by developing an analogy with muscle fatigue. But even if this process is not computational in the sense of information processing defined over an explicit representational data structure, I don’t see how it avoids Fodor’s argument. Certainly finding the maximum number from a set, or all those numbers that are greater than a certain threshold is (or can be modeled by) a computational process—indeed, a trivial one. I can see how an architecture of the kind Sperber suggests could as dumb or dumber than a classical computational system, but I do not see how it could be smarter, permitting true abductive inference.

¹³ I thank David Williamson for pointing this out to me.

as a whimsical conversationalist, or as a seven-year old child—all people for which we are willing to suspend to some degree our normal expectations concerning coherent and rational discourse. The state of the art in AI is very far away from systems that can manifest the CALU, maybe hopelessly far away (see also xxx). This should not be a surprise by now. The programs fail at least in part because the CALU is an intrinsically abductive capacity, and because the abductive cannot be reduced to the computational. So far it seems that Descartes' intuition was more accurate than Turing's: that producing speech that is unbounded, not conditioned by stimulus, but appropriate to situations does go beyond the bounds of mechanical explanation as we know it. So the CALU is what we don't know how to specify or implement in computational terms, and it is also the part of the human language capacity that seems innate but not narrowly biological based on neurolinguistic, developmental, and comparative studies.

Conclusion

In this paper, I have sought to remind people that the CALU is an important aspect of the human language faculty. I have gone on to argue that the CALU capacity is innate in humans; indeed analogs of all the standard arguments that syntax is innate can be found to show that the CALU is also innate. However, the arguments that show that syntax is embodied in specific neural circuits, that it is genetically encoded, and that it evolved from similar capacities that were shared with other primates do not go through. The claim that the innate structure of the human mind is (entirely) biological in this sense thus has little or no evidence in its favor, and adds nothing to our substantive understanding of the CALU. I conclude that there is reason to entertain a type of nativism that subscribes to the existence of innateness but that does not try to cash out that innateness as biology in the manner characteristic of evolutionary psychology.

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