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. 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 (among other things) is innate rather than learned by association or induction or analogy. This constituted a break with the empiricist/behaviorist/structuralist tradition that had dominated research before that.

The second, more recent period added to the basic claim of innateness the explicit claim that the innate knowledge in question is to be understood entirely within an evolutionary biological framework. The innate knowledge is taken to be coded in the genes and to have arisen as an evolutionary adaptation. Within linguistics, this second period began rather sharply with the publication of Pinker and Bloom (1990). Before that, discussions of the evolution of language had been relatively rare and peripheral to the field, whereas they have now become quite common. Chomsky, Fodor, other first generation cognitive scientists did not deny 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 by developing the theory in this way.

I believe that the current state of 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 the practicing linguist can make use of it on an on-going 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 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. At best, it has been an inert hypothesis, allegedly contributing to the foundations of the field at a level that is invisible to most linguistic practice. 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

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Grammar (linguistic terminology for innate knowledge of language) could not be as Chomskian linguists say it is. Others concentrate on the attested linguistic data and ignore the biology as being too crude 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, hoping that the interface between linguistics and biology will become more practical and meaningful as work progresses in both generative linguistics and evolutionary psychology.

In this chapter, I explore another possible reaction to this disconnection—namely, the idea that there could be some innate ideas and cognitive processes that are not strictly biological in nature. The usual argument in favor of the evolutionary psychology approach to innate structure in language is “it’s the only game in town.” Pinker and Bloom (1990) emphasize that adaptive evolution is the only scientific explanation for functional complexity. In a more general context, 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 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 an older theoretical framework than biology. 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 (2000) calls the “new synthesis.” 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 by the weight of empirical evidence. This is the foundational question I propose to consider here. More specifically, I 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 one important aspect of the human capacity for language.

My discussion unfolds as follows. In section 2, I review Chomsky’s paradigm-defining claim that the human capacity for language involves not only the well-studied components of vocabulary and grammar, but also what he calls “the Creative Aspect of Language Use” (CALU). Section 3 considers various conceptions of how this capacity relates to the overall structure of the mind. In section 4, I present explicit arguments that this component is innate in humans. Section 5 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 6 concludes with the observation that it is not surprising that the CALU has not been explained biologically, given that it is an abductive capacity and thus cannot be characterized computationally.

2. Factoring the Language Faculty

If one wants to inquire into which parts of a complex phenomenon X (e.g., language) are to be attributed to theory Y (say evolutionary biology) and which parts are not, it is

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1 Indeed the nativist tradition is even much older than this, including Plato and many pre-modern Christian thinkers.
helpful to have some prior notion of what the major parts of X are. What then are the parts of the human language faculty?

Chomsky proposed a first-pass answer to this question back in the 1950s at the start of the cognitive revolution (Chomsky, 1957, Chomsky, 1959). All of the successes in modern generative linguistics since that time arguably depend on his answer, whether this is realized or not. Chomsky factored language into three (possibly complex) components: the lexicon, the grammar (syntax in a broad sense), 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 grasp, 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 a person might say one rather than the other (Chomsky, 1957). To use an analogy from the construction industry, the lexicon is like the bricks and mortar of language, while the grammar is like the building codes and engineering principles, which specify ways in which these materials can be combined to make larger units such as walls, roofs, rooms, and buildings. These two facets of the language faculty we have a reasonable hope of 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 that are consistent with but not 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 primarily for the purpose of distinguishing it from grammar and putting it aside.

This distinction is so much part of the common ground 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 in this way, they were attempting to explain the content of a sentence at the same time as its form, using the same theoretical tools (e.g. association). That way of looking at the problems of language proved empty and sterile, as Chomsky demonstrated with force. “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 for cognitive science because Chomsky did make this distinction.

Chomsky’s fullest positive characterization of what the CALU is is in Chomsky (1966), where he presents with approval Descartes’ observations about human language expressed in Part V of A Discourse on Method. The CALU, 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 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.

For Descartes (and Chomsky), 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 to “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 also 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. But behavior that is simultaneously unbounded, not determined by stimuli, and (not random but) appropriate is special. That is what Descartes took to be 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. Indeed, Descartes’s intuition that this kind of linguistic behavior goes beyond the bounds of what can be achieved by a mechanical device has proven to be remarkably sound even hundreds of years later. Despite vast

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2 See Cheney and 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 inasmuch as vervets can certainly refrain from uttering them (e.g. when alone) and may be able to utter them falsely (e.g. to deceive other monkeys). But 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. Recall that all three CALU characteristics are needed to qualify as true Cartesian creativity.
changes in technology, his prediction that no mechanical device could produce an unbounded range of sentences not determined by stimuli but as appropriate responses to circumstances has turned out to be strikingly more accurate than Turing’s (1950) much more recent prediction that computers’ linguistic behavior would be indistinguishable from humans’ by 2000 (see section 6).³

Fifty years later, Chomsky’s decision to factor the language faculty into vocabulary, syntax, and the CALU still looks sound. Chomsky’s judgment that grammar/syntax was ripe for investigation in terms of then-new formal notions like recursive rules was correct, and has led to enormous new discoveries about English and hundreds of other languages. In contrast, there has been essentially no progress on the CALU, and most linguists have followed his advice not to pursue it. And yet there is no reason to deny that the CALU exists. We know a lot more about the building codes for sentences than we did, but not significantly more about the architects and contractors. Nevertheless, we must still assume that these architects and contractors exist, because there are actual sentences around us. These more or less comply with our building codes, but our building codes cannot account for their existence or aspects of their nature. 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 a given wall is exactly where it is in a particular house.

This distinction between the CALU and the grammar/vocabulary is not unique to Chomskian linguists. Essentially the same distinction is found in Levelt’s (1989) comprehensive psycholinguistic model of language production. Levelt identifies three different processors that are involved in language production: the conceptualizer, the formulator, and the articulator. Vocabulary and grammar are resources accessed by the formulator, whose job is to map “messages” onto “inner speech”. But before the formulator kicks in, the conceptualizer must take thoughts and intentions—which may not be expressed in language-like formats at all—and creates propositionally structured semantic representations called preverbal messages, which constitute the input to the formulator. Levelt’s conceptualizer has the signature properties of the CALU. Levelt ends his exposition of this component with a discussion of experiments by Ehrich in which subjects were asked to describe how furniture was arranged in a simple room. He observes the following (p. 157):

But all of these are tendencies, not iron laws. A speaker is free to choose one perspective rather than another. And indeed, the ways in which the same scene is described by different subjects are surprisingly variant. When one looks over Ehrich’s protocols, one is struck by the fact that no two descriptions of the same

³There is an interesting lesson to learn from current attempts at running approximations to the Turing Test in connection with the Loebner prize. Shieber (1994) observes that the programs that people rate as most human-like succeed almost entirely because they find ways to avoid the expectation that conversation should be appropriate in the CALU sense. The semi-successful programs pose as a psychoanalyst, or as a paranoid schizophrenic, or 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. So our best computers do not succeed in replicating the CALU capacity, but rather at sidestepping it. This underlines the fact that the CALU is not a computational capacity (see section 6).
furniture arrangement are identical. Each subject added a personal touch in terms of the objects, the relations, the qualities attended to, and the choice of perspective.

The subjects’ productions are clearly unbounded and stimulus free, so that even tightly controlled stimuli and a narrow task-definition do not guarantee a small set of results. Yet the variations do not strike one as being random; rather, they are interpreted as “adding a personal touch.” The productions are thus also appropriate. So the CALU shows up in Levelt’s research too, although under a different name.

3. CALU and the Overall Structure of the Mind

Where does this CALU fit into a larger picture of the human mind? There are three logical possibilities, and which seems most plausible will depend on one’s background beliefs about mental organization more generally. The first possibility is that the CALU is a distinct module of the mind, specific to language, a submodule of the distinctively human language faculty. This is essentially Levelt’s view, reviewed above. The second is that the CALU is a “central system” in a Fodorian picture of the mind, in which modules are limited to input-output systems (see Fodor 1983). On this picture, the central system(s) are responsible for all purposeful decisions about how to act; CALU behavior is simply what results when the central system acts via the language module(s). The third possibility is that the mind is massively modular, as in Tooby and Cosmides-style evolutionary psychology, but yet there is no CALU module. Rather, CALU phenomena result as emergent effects as the various modules of the mind—practical reason, theory of mind, cheater detection, etc.—interact in complex ways to determine what the person does. Which of these views one holds will affect the question of whether CALU phenomena support a nonbiological version of nativism.

It is hard to say something definitive about this issue at this point. While most cognitive scientists interested in modularity agree that Fodor’s (1983) characterization of modules is too restrictive, no weaker version of what a module is has become standard. Moreover, evolutionary psychology arguments that the mind must be massively modular are based on very general considerations that give little purchase on what the specific modular structure is.

Nevertheless, there are some decent reasons to maintain the first hypothesis, that CALU is a distinct module of the mind. The most basic is that constructing and interpreting novel sentences is a distinct functional domain and would seem to call for special abilities and techniques. The kinds of decisions that must be made in deciding exactly what to say and how to say it are in practice quite different from other kinds of decisions people have to make. Thus, from a general engineering perspective it is not clear that using the same architecture to create linguistic behavior as to create other kinds of behavior would be efficient or even feasible.

Second, most of the process of constructing novel sentences takes place quickly, automatically, and unconsciously, in a way that is typical of modular systems (Fodor, 1983). There are situations in which we are conscious of weighing alternatives and explicitly planning our speech—rehearsing for a job interview, for example. But these are a minority; more typically we start off with a very general gist of what we want to get across and blurt out particular sentences on the fly, so far as our conscious experience is
concerned. This is phenomenologically quite different from much of our central processing or practical reasoning, which is slow, effortful, and conscious.

Finally, it is striking that CALU-style behavior is strongly species specific, being manifest in almost every human and in no nonhuman animal. This makes sense if the CALU is a distinct module of the human mind, one of a few that set it apart from the minds of otherwise similar animals. In contrast, both the central cognition view and the interaction of modules view are committed to the idea that our creative and purposeful linguistic behavior is generated in the same way as other kinds of creative and purposeful behavior—such as navigation, foraging, or managing social relationships. But other species navigate, forage, and interact with kin as well as we do (or better). They must therefore have the central cognition or the interaction of modules that makes purposeful behavior possible in these domains. These views leave it mysterious, then, why they remain unable to speak in a CALU manner, even when they are given a rudimentary vocabulary and grammar (see discussion of the ape Kanzi in section 5). If we want to maintain the idea that the human mind is as little different from the minds of related species as possible, assuming that humans have a CALU module seems to be the best way. The alternatives require a more radical reorganization of the overall structure of the mind to account for the manifest difference in creative linguistic behavior. I therefore assume that CALU (alias the conceptualizer) is a module of the human mind, at least for the sake of argument.

4. Is the CALU Innate?

Accepting then idea that the human language capacity consists of at least three modules—the vocabulary, the grammar, and the CALU—we can go on to 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 focus instead on the little-discussed CALU, asking whether the same kinds of arguments can be constructed 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 clearly yes. In every society people spontaneously make up new sentences in a way that is not controlled by their environment or by any simple characterization of their internal states, but that is seen as appropriate and purposeful in the situation. We know of stuffed toys that give a fixed range of responses in a mechanical or random fashion, but we don’t know any people groups that are like this, not even long-isolated tribesman in places like Tasmania or the New Guinea highlands. I know of no controversy on this point.

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 age two, already show evidence of syntactic structure. A striking example is the fact that French children consistently (and correctly) put finite verbs before negation markers and nonfinite verbs after negation—even though they don’t yet use nonfinite
verbs in a correct, adult-like way (Deprez and Pierce, 1993). This argument also applies to the CALU: toddlers’ early utterances are already stimulus-free and purposeful. Their utterances are not, of course, unbounded at the two word stage, by definition. But there is reason to think that these two-word utterances are abbreviations of larger structures that the child has in mind but cannot fully articulate yet (Bloom, 1970). 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 are also appropriate in the sense that they are not random strings of words that arise by free association. Thus sophisticated CALU behavior is in place long before children start kindergarten.

By far the most important argument for innateness is the poverty of stimulus argument. Since this is a crucial consideration and could be controversial, I linger over it some, discussing three variants that have been used for syntax and that seem to apply to the CALU too.

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 (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. Nevertheless, one can show that the children consistently 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 regularly face new structures where nothing I know tells me whether they are possible or not, even though I have been working on the language for years. I can resolve the matter by coaxing a native speaker into judging some carefully engineered sentence, but children resolve the ambiguity without that opportunity.

There are several levels at which 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 induces it from its environment. And there are various things to induce. The simplest might be: My parents are not automata. Rather, they use their vocabulary and grammar to make sentences in a way that is neither stimulus-bound nor random, but is appropriate and purposeful. A second thing to induce is “I should not be an automaton; I too shall 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.

I do not think we can even frame the third version of this question at this point. We have no computational theory 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 (see section 6). Because we have no precise algorithmic 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. We thus cannot compare this information to the information that is accessible to children in their environment, and see
whether it is commensurate. In short, since we don’t know what the CALU is with any precision, we cannot know what would be required to learn it.

Let us retreat therefore to the simplest part of this cluster of ideas: the question of whether the people around me are automata or not. 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 the latter. What kinds of experiences could one have with other people that would convince one that they are not automata? Are those kinds of experiences available to the child? If not, then we have a poverty of stimulus argument that applies to this aspect 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 more complex and mysterious aspects of the CALU as well.

A hint that poverty of the stimulus might apply here comes from the fact that (unlike children?) plenty of intellectuals have managed to believe that people are automata in the relevant sense, from Descartes’s critics to modern-day behaviorists. In order to construct the argument, let us review how Chomsky argued against the behaviorists, to see if his crucial evidence is observable to a child. Chomsky (1959) took up 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. 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? 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 say; 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 exactly this 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

\[4\] 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 (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. Chenney 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.
be easy; one would have to decide what situations counted as similar 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 his 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 inner sense of freedom. At best, we can 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, that knowledge is not on the basis of readily observable data. Rather, it 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 a random component. 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 nth word one utters is a 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 0.0000 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. Notice that this argument also depends on data that 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 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, it is not because we reliably have direct access to the crucial evidence. Rather, the idea is probably innate.

I have only considered two extreme positions: that my parents’ verbal behavior is completely determined, and that it is completely random. Many hybrid combinations are possible, which include both deterministic and random elements. I suppose that some such hybrid view must be what the fully committed computationalist who doesn’t believe in mysteries must hold to. Even if they are right, this merely strengthens my poverty of stimulus point. If language users do in general have the 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 with it the whole CALU complex it is part of.

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, but not errors that violate the invariant principles of Universal Grammar (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, rather than as purposeful and self-expressing agents? Do they go through a period in which they themselves act as automata? If so, this would show that they are experimenting with the hypothesis that people have a vocabulary and a grammar, but no CALU. But I know of no evidence whatsoever that children ever go through such a stage.

A final variety of the poverty of stimulus argument is to look at language development in abnormal, unusually impoverished circumstances. 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 language to acquire it properly. What seems to happen is that they create a new language from the available parts that has its own Universal Grammar-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 users 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 (Goldin-Meadow and Mylander, 1983). The quality of the facts in these areas is controversial, but all agree that they bear on issues of innateness.

Can similar arguments be constructed that would bear on the innateness of the CALU component of language? The answer seems to be yes. The case of deaf children who are not exposed to standard sign languages is relevant to this point too. Although such children have essentially no independent model of language use, the home signs they develop are not used mechanistically, but rather for the free expression of thought. The relevance of such cases was pointed out already by Descartes (p. 45), and modern work on home sign systems by Susan Goldin-Meadow and her colleagues confirms that such children do manifest the CALU properties. There seems to be no doubt that children use their gestures in ways that are appropriate in the Cartesian sense. Home signs are more bound to the immediate situation of utterance than conventional languages are, because they tend to lack a rich set of nouns and rely heavily on pointing to refer to things. But this does not mean that the languages are stimulus-bound: children do not 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 (MLU) of only 1.2 or 1.3 signs (Goldin-Meadow and 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 empowerment of the input and the limited time window for developing the home sign (sooner or later all these children end up getting exposed to some conventional language or other). Moreover, Goldin-Meadow and Mylander point out that the maximal length of utterance for most of their subjects was between five and nine signs, which is not significantly less than that of children with conventional language input. They argue in detail that every child’s home sign is recursive, allowing one proposition to be embedded inside another (Goldin-Meadow,
1982, Goldin-Meadow, 1987), and these embedding structures appear in home signs at about the same age (2.3 years) that they appear in the productions of hearing children. Home sign language use thus does count as unbounded. Therefore, even these children, with very limited outside input, develop the CALU capacity approximately on schedule.

Goldin-Meadow and Mylander also compared their children’s use of home sign with that of their mothers. They discovered that even though the 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 the mothers’ utterances consisted of more than one sign, compared to 30% of the children’s utterances, and recursion appears earlier in the children’s signing than in the mothers’. Thus, there is no evidence that the children are picking up CALU from their mothers; rather it seems to be emerging spontaneously from within them.

I conclude that one can construct a case for the CALU being largely innate in humans that is as strong or stronger than the familiar case for syntax being innate.

5. Is the CALU biological?

The question now is whether there is evidence that the CALU is biological in nature. Of course the answer is yes in the broad sense that the CALU is a property only of things that are alive (i.e., humans). I am interested in the narrower question of whether the CALU fits comfortably in the intellectual framework of contemporary biology, so that it is elucidated by our basic biological theories. More specifically, is there evidence that the CALU is embodied neurologically, that the relevant neural structures are coded for in genome, and that the relevant 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 claim that the answer is no, and hence there may be a distinct category of nonbiological innateness. Again it is useful to compare the CALU with syntax, which has been more studied.

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

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 in bold words. Let us test it against the CALU, to see if there is overwhelming evidence is that this prominent aspect of our mental life depends entirely on physiological events in the tissues of the brain.

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 capacity. This by itself need not be any more significant than the fact that a person without a tongue (and with paralyzed arms) may not be able to manifest the ability. 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 functions in perception, motor control, and language. The interesting question is whether there is evidence for a CALU circuit of this sort.

The oldest and perhaps the best line of research on this is the study of aphasia—the effect of damage to the brain on language. This has a history that goes back more than 140 years to Paul Broca’s work in the 1860s (Caplan, 1987, Goodglass and Kaplan, 1972, Kertesz, 1979). Clinicians have developed a relatively stable typology of 7-10 aphasic syndromes over this long history. Their classification has its origins in a paper by Lichtheim published in 1885, which set forth a proposal for a complete enumeration of all aphasic syndromes. Geschwind revived Lichtheim’s typology in the 1960s, 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 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: 55). Controversies exist, of course, 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 glance, the answer seems to be yes. The hallmark of CALU is language use that is unbounded, stimulus-free, and appropriate. Wernicke’s aphasia seems to be characterized by language production that is unbounded and stimulus free, but 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 then and tell go in the without within pain where it is whire in the herce in stock. (Goodglass and Kaplan, 1972: 59)

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, because their CALU circuit has been destroyed.

But this is not the right interpretation of Wernicke’s aphasia. Wernicke’s patients clearly 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 paraphrasic distortions. At severe levels, auditory comprehension may be zero…” (Goodglass and 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, where sound-meaning pairs are represented in the “association cortex”, not (just) a disruption of the CALU component. Given 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 entirely 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, as in the sample above. 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.

Another possible loss of the CALU with very different symptoms is found in Broca’s aphasics. Here the problem is not with the appropriateness of the linguistic output, but rather with its unboundedness. In severe cases, patients speak only one word at a time. Here is a sample conversation (Goodglass and 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!
(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

One might well think of this as a loss of the CALU circuit, and hence as a loss of the ability to put words together into sentences. But again this is not the only problem that typical Broca’s aphasics have. 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 and Kaplan, 1972: 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. Again parsimony bids us ask whether these deficits are enough to explain the behavior without the CALU itself being affected. And again

5 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.
the answer is 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 difficulty in producing the sentence. This is also consistent with the fact that their ability to interpret new sentences is relatively intact—an ability that also draws on the CALU. This time we see that grammar can be affected by brain damage (as well as articulation), but there is still 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 abilities, showing that their lexicon is intact. Their speech would be fluent and free from grammatical errors when they are repeating a sentence or reciting a known text like a song or the Lord’s Prayer, suggesting that their grammar is intact. But the patient 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 symptoms does not seem to exist as an identifiable syndrome. So perhaps it is not true that brain damage can directly disrupt any mental function one can imagine; the CALU itself is not disrupted.6

This conclusion reflects a classical view in neurolinguistics. In 1885, Lichtheim 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 organs of speech and hearing 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. As already mentioned, the classification of aphasias that emerges from this view has stood the test of time, and is still the basis of clinical diagnosis more than 100 years later. The anomaly that his system had one crucial component that was not prone to disruption by injury is treated as a conceptual flaw by subsequent neurologists (such as Caplan)—but these neurologists have not discovered the missing syndrome or proposed a reconceptualization of 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

6 The not-so-often discussed Transcortical Motor Aphasia (also called Dynamic Aphasia (Maruszewski 1975: 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 repetition of sentences is quite good, free from grammatical errors. The main symptom that these patients have 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 writes (citing Luria) 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 sounds very much like a disruption of the CALU. However, one probable indication that the CALU is intact is that these patient’s ability to understand novel sentences is apparently quite good. 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 certain kinds of visual props to help them focus. This should not be possible if the CALU circuit were truly gone. Luria (1976) gives a comprehensive reappraisal of this kind of aphasia, arguing that patients’ ability to repeat complex sentences is more seriously impaired than classical neurologists recognized, and that this deficit shows up even when listing (and recalling) sequences of words not integrated into a sentence—a task that wouldn’t involve the CALU.
in production, and is not involved in simple repetition. Thus it is plausibly the same as the faculty I have been calling the CALU. The result of 140 years of neurological research, then, is that there is no evidence that the CALU depends on dedicated brain tissue.7

Next let us turn from neurology to genetics: what evidence is there that the CALU is genetically encoded? Is there a CALU gene—or a set of CALU genes—somewhere in the human DNA? If so, then one might expect to find developmental disorders that affect the CALU in a differential way, disorders that can 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 twenty years or so. Standard classifications come from Bishop (2004) and Rapin and 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 clearly irrelevant to the CALU: typical SLI affects the grammar component; severe 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 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 (1983: 174) say that despite these children having what seems on the surface to be “good language”, “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.” This sounds like a CALU deficit: their speech is unbounded, stimulus free, but not appropriate.

However, it seems that something different is being meant by “appropriate” in the descriptions of this form of SLI. 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 random. And children with Pragmatic Language Impairment are capable of speech that is appropriate in this sense. What Bishop and Rapin & Allen seem to be describing is more along the lines of speech that is on its own wavelength. It is purposeful, but the purposes do not mesh with those of their conversational partners.8 Rapin and Allen’s (1983: 175) example is instructive:

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7Lichtheim’s own view was that the concept center was spread diffusely throughout the brain, so it is not affected by localized lesions. That is, of course, a legitimate possibility, similar to the idea that the CALU is not a mental module but the result of many other modules in interaction, discussed (and rejected) in section 3.

8Indeed, both sources conjecture that this syndrome is related to autism.
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 did not answer the question. But it is perfectly coherent and meaningful when taken in its own terms. He may be lacking some social sensitivity, but he is not missing his CALU in Chomsky’s sense. Overall, then, there may be evidence in the literature on developmental disorders for a “grammar gene” whose defects produce Typical SLI, but there is little or no comparable evidence for a “CALU gene.”

Finally, let us consider the prospects for explaining the origin of the CALU in humans in terms of evolution. Since 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 constructing a detailed evolutionary account are slim to none. Nevertheless it is interesting to consider in this light the ever-contentious question of whether our ape cousins are capable of language when raised in the right environment. The answer, of course, depends greatly on what one means by “language”—which is a vague and polysemous word. The question can be sharpened somewhat by focusing on the idea that the human language capacity is factored 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 hundreds. Can apes acquire a grammar? Maybe. This has been taken to be the crucial question in much of the literature. 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 direct object.

But for my purposes here, the crucial question is whether apes can manifest the CALU capacity. Here the answer seems to be a clear no. Even Kanzi, the most proficient of the apes, had a mean utterance length of just over one. 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. His behavior thus falls short of the CALU on the unboundedness criterion. Kanzi compares unfavorably in this respect even with the home-sign-using children studied by 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-Rumbaugh (1994) goes to some pains to explain that the boundedness of Kanzi’s output is not his fault. His vocal tract is not well-configured to speak longer sentences. His hands do not have fine enough control to sign sentences longer than that, having been toughened by being walked on. His best method of communication is pointing to signs printed on a keyboard. But 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

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9 Of course it is easy to tell stories about why it is advantageous to survival and reproduction to have the capacity to freely express thought in a way that is appropriate to but not determined by situations. To that extent, the evolutionary paradigm can be applied to the CALU. But I take discussions that function only at that level of generality to be of limited interest—indeed, to be almost tautological.
combination of factors that together have the effect of making it unreasonable to expect an ape to produce unbounded speech in real time, according to Savage-Rumbaugh. This sounds rather like a conspiracy. A simpler and more unified explanation consistent with the facts is that the apes lack the CALU module present in the human mind. This was apparently correct that there is nothing like the CALU attested in the animal kingdom apart from human kind (see also Chenney and Seyfarth 1990 on the scope of primate communication in the wild). Thus, there is no clear explanatory advantage to be gained by saying that the CALU developed evolutionarily, by the gradual improvement or change in function of a preexisting capacity through natural selection.

6. Concluding Remarks: How Surprised Should We Be?

In this article, I have focused attention on one subcomponent of the human capacity for language, which Chomsky calls “the Creative Aspect of Language Use” and Levelt calls “the conceptualizer”. I have shown that there is good reason to believe that the CALU (like grammar) is innate, given its universality in humans and poverty of stimulus considerations of various kinds. At the same time, I have argued that there is no good evidence that the CALU is biological in nature. More specifically, there is no evidence from aphasia that it is neurologically embodied, no evidence from developmental disorders that it is genetically encoded, and no 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. I conclude that there is reason to think that there is a type of innateness that is not biological in nature.

This conclusion might seem astonishing to some, depending on their ontological beliefs. After all, we know that brains can perform computations, much as a computer can, and we know that brains are extremely complex. Therefore, it stands to reason that brains could in principle perform virtually any computation we can imagine, no matter how complex that computation might seem. So surely there must be room for the CALU

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10 This is the strongest empirical argument that CALU is a module of the human mind (see section 3). If CALU behavior were the result of central cognition or the interaction of modules in a massively modular mind, one would expect apes to have that capacity too. In particular, once a trained ape is endowed with a vocabulary and a grammar, one would expect CALU behavior to show up immediately, given that apes are perfectly capable of purposeful behavior in other domains.

11 Am I using a double standard when I do not attribute CALU to a trained ape, but do attribute it to a young child or a Broca’s aphasic, when the utterances of the latter are only marginally longer than those of the former? To some extent I am, but this is justified by the fact that parsimony considerations 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 whether she already has 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 clearly. So here parsimony leads one to favor the no answer. After all, the CALU is a very special capacity that only a tiny percentage of things in the world have; surely a substantial burden of proof falls on someone who claims that a new kind of thing has this capacity.
within the standard biological framework, even if we can’t yet work out all the details, one might think.

And yet one can grant all this and still not be astonished, if we remember what is really meant by computation. Turing defined very precisely what computation is, and for all its power, the notion has inherent limitations. Fodor (2000) reminds us of the significance of these limitations for current cognitive science. In particular, Fodor presses the point that the computational theory of mind has no account for the phenomenon of abductive reasoning—inferring to the best overall explanation, when there is no way of knowing in advance what facts are relevant. The transformations that a computer can do on an input—what we call “information processing”—must depend only on the syntactic properties of that input, on how it is put together. Computation cannot, by definition, depend on the semantic properties of the input, such as what the various symbols refer to. As a result, well-programmed computers are wonderful at reasoning deductively, telling us what conclusions follow because of the form of the premises. But they cannot reason abductively, telling us what conclusions follow because of the content of the premises. The theory of computation can thus give us a wonderful account of one type of rationality in terms of nonrational, physical processes, but it cannot give us an account of another type, almost by definition. Fodor thus identifies the question of how abduction is possible as a great mystery that hovers over cognitive science.

Fodor’s point is relevant here, because the CALU is a blatantly abductive part of the human mind—perhaps our most clearly abductive capacity of all.12 This follows almost immediately from the characterization of CALU as behavior that is unbounded, stimulus-free, and appropriate. A Turing machine cannot, by definition, have such behavior. Each step in the computations it performs are determined by the syntax of the input it receives. The whole notion of “appropriate” is an abductive one. We judge that what someone says to us is appropriate not at all on the basis of the syntactic structure of what is said, but entirely on the semantic properties of what is said. So constructing and interpreting novel sentences that are appropriate is an intrinsically abductive process. Therefore, it cannot be cast as a computational process, and we have no assurance that the brain as a biological organ can perform it simply from the acknowledged facts that

12I think that humans’ ability to construct and interpret coherent discourses is a much clearer example of abductive reasoning than Fodor’s favorite example, scientific theory construction. Fodor’s critics observe that the striking thing about scientific theory construction is that humans are not very good at it: only the most intelligent can do it, and even they make many mistakes that need to be corrected by communities of peers over historical time (see, for example Sperber (to appear: note 7). In contrast, nearly everyone constructs and interprets complex coherent discourses every day of their lives.

Sperber (to appear) agrees that (something like) a abductive reasoning happens 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 (cf. the module-interaction view of CALU mentioned in section 3). 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 itself (or can be modeled by) a computational process. I can see how an architecture of the kind Sperber suggests could as smart as a classical computational system, but I do not see how it could be smarter, permitting true abductive inference.
the brain is very complex and it can do computations. So inasmuch as the CALU is innate but abductive, we should not be surprised that it is a type of innateness that does not fit within the framework. We find here an important convergence between very general conceptual considerations about abduction vs. computation and empirical results from aphasia, dyslexia, and animal learning. I conclude that there is reason to entertain a type of nativism that affirms the existence of innate ideas and capacities but that does not try to cash those out as biology in the manner that is characteristic of evolutionary psychology.

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