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Language evolution: A brief guide for linguists

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Abstract

For the benefit of linguists new to the field of language evolution, the author sets out the issues that need to be distinguished in any research on it. He offers a guided tour of contemporary approaches, including the work of linguists (Bickerton, Carstairs-McCarthy, Chomsky, Hurford, Jackendoff, Pinker, Wray), animal behaviour experts (Dunbar, Hauser, Premack, Savage-Rumbaugh), neurophysiologists (Arbib, Calvin), psychologists (Corballis, Donald), archaeologists (Davidson), and computer modellers (Batali, Kirby, Steels). He criticises the expectation that recent discoveries such as 'mirror neurons' and the FOXP2 gene will provide easy answers. He emphasises the extremely interdisciplinary nature of this field, and also the importance of involvement in it by linguists, after more than a century of neglect.

Keywords: Language evolution; Protolanguage; Holophrastic language; Social intelligence; Mirror neurons; FOXP2 gene

While most linguists were looking the other way, so many books, articles and collections on the evolution of language have been appearing that one can hardly keep pace with them. Accordingly, many linguists have a lot of catching-up to do, and their task is not made any easier by the fact that knowledge of linguistics alone won't get them far. This is an interdisciplinary game, played by biologists, neurologists, anthropologists, archaeologists, computer scientists, philosophers, and more—as well as, or maybe I should say a good deal more than, by linguists. Therefore as a player with 20 years' experience I feel it incumbent on me to provide, for any linguists who might feel like joining the game, a rough map of this unfamiliar terrain, and to give them the kind of information guide-books usually give on the value of the various goods and services available there. People can, of course, study whatever they choose, but there is perhaps no other field of human inquiry which has been so vitiated by a failure to get priorities straight.

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1. What is meant by "language evolution"?

First and perhaps most important is that "the evolution of language" is far too vast and complex (and vague) a concept for anyone to say anything sensible about it. In order to get a handle on it, it is vital first to isolate it, and then to clearly delineate what are the main issues. On the isolation issue, here is a classic How-Not-To, courtesy of *Science*, no less:

> Language evolution has not stopped, of course; in fact, it may be progressing more rapidly than ever before. (Culotta and Hanson, 2004:1315)

Of course it has stopped, because the biological evolution of humans (saving the odd minor development like the spread of lactose tolerance or proneness to sickle-cell anemia) has, to all intents and purposes, stopped also. What is happening (and has been happening for perhaps as many as a hundred thousand years) is cultural change (sometimes misleadingly described as "cultural evolution"); within the envelope of the language faculty, languages are recycling the limited alternatives that this biological envelope makes available. It should always be a warning signal when writers engage in the kind of sleight-of-hand that persistently switches between "language" and "languages"; Culotta and Hanson do this in the sentence immediately following the cited one. But *language* evolution and changes in *languag**ES*** operate on different time-scales, involve different factors, and follow different courses to different ends (or rather, to the end of a complete language faculty in the first case and to no particular end in the second). To muddle them merely confuses an already sufficiently confused field.

1.1. Major issues

To get a grip on what's left, one has to focus on one or other of the two most central issues in language evolution:

(1) How did symbolic units (words or manual signs) evolve?

(2) How did syntax evolve?

Symbolic units and syntax are the only real novelties in human communication, and are therefore the most salient (as well as the most difficult) of the things any adequate theory of language evolution must account for. There is no reason to believe that the emergence of the two was either simultaneous or due to similar causes, and some good reasons for supposing the contrary. Chomsky (1980) has made a clear distinction between the conceptual and the computational aspects of language. The conceptual element, especially if we take it to include conceptual structure as well as the lexical instantiation of the latter, must be phylogenetically far older than any computational mechanism. Moreover, there is no reason to suppose that symbolic units could not have evolved in the absence of any linking mechanism; like 5% of an eye, a one-word-per-utterance language is better than nothing, and it is obvious from early-stage pidgins and foreigner talk that symbolic units can be used, and can even be used to make valid propositions, without anything one would want to call syntax, Simple logic dictates that symbolic units must exist before any way to concatenate these can come into existence (even if some pre-existing mechanism can be co-opted, the necessary neural modification involved in the co-option process will take time). Again, as with the language-evolution/change-in-languages question, symbolism and syntax involve different selection pressures – indeed, symbolism probably exerted a selective

pressure for syntax – and probably very different mechanisms, so that to confuse the two leads again only to further confusion. The reader should beware of any work on language evolution that does not bear, preferably directly, on one or other (or both) of these issues.

Secondary, but still of considerable importance, are the following:

(3) What was the initial selective pressure for the first steps toward language?

(4) Was the subsequent development of language gradual or abrupt?

(5) How did the development of phonology relate to (1) and (2)?

Phonologists may resent my downgrading of their trade vis-a-vis semantics and syntax, but a moment's thought justifies it. Unless one adopts the ''Singing Ape'' hypothesis (Darwin, 1871; Jespersen, 1922), there had to be things to say (words to start with, if not yet sentences) before there was any need for a means of saying them. But the ''Singing Ape'' hypothesis involves assuming that language began with meaningless sounds and that meaningful units were subsequently factored out of them. Several of the (to my mind, insurmountable) problems that this assumption raises are dealt with in subsequent sections. Moreover, as soon as we start thinking in evolutionary terms, we have to start thinking why any development would have been selected for, and what would have selected for it. It is easy to see how increasing numbers of words and/or increasing ability to combine these words into longer and more complex sentences would have forcefully selected for improvements in the vocal organs and increasing complexity in the categorization of sounds. It is hard to see how any developments in the sound system would have selected for more words or longer sentences, and equally hard to see what, in the absence of more things to say, would have selected for a more advanced phonology. Accordingly, phonology is most likely a secondary phenomenon whose evolution depended crucially on the evolution of symbols and syntax.

1.2. Lesser issues and non-issues

Among some of the other issues frequently debated, we find:

(6) Did language begin with speech or with signs?

If it could be shown that (6) had any substantive bearing on (1) or (2), or even on any of (3)–(5), this question might be worth the amount of literature devoted to it. Although claims for both uniquely-signed and uniquely-spoken origins have been made, support for either of them seems at best dubious, and I see no reason why one cannot remain agnostic on (6), pending more decisive evidence. My own preference, for what it's worth, is that language (or I should say protolanguage) began as a free-for-all, catch-as-catch-can mode that utilized sounds, signs, pantomime and any other available mechanism that would carry intention and meaning, and that it only gradually focused on the vocal mode, due to the latter's greater utility.

Then there is a non-issue that has consumed even more print:

(7) Did language evolve out of prior means of communication?

Bickerton (1990) develops several lines of argument showing conclusively that nothing resembling human language could have developed from prior animal call systems. To the best

of my knowledge, no advocate of what I originally called ''continuism'' (but which would be better described as ''genre continuism'': Bickerton, 2004) has ever answered or even seriously addressed these arguments. Yet many writers outside linguistics (e.g. Hauser, 1996, and as quoted in Holden, 2004) still insist that language developed directly from primate calls. Evidence for this proposal seems limited to the generally true statement that evolution does not generate novelties but merely tinkers with what is there, plus the assumption that if call systems are communicative and language is communicative, then ''what was there'' can only have been some sort of primate call system.

Since language evolution is an interdisciplinary field, it is at best incautious to frame hypotheses on the basis of evidence from a single discipline (although people do this all the time). It follows that for each of a large number of hypotheses in the field there exists evidence from some other discipline(s) that shows the hypothesis is untenable.

In the case of genre continuism, that evidence includes, but is by no means limited to, the following:

(8)
a. Calls are genetically based; words are culturally based.
b. Calls are always semantically equivalent to propositions (''Mate with me'', ''Stay off my territory'', ''Look out, there's a leopard''); words never are.[1]
c. Calls cannot predicate; any kind of language must predicate (even imperatives contain an implied subject).
d. Calls are indexical, not symbolic (in the physical absence of a referent they are either meaningless or misleading); words being true symbols can refer to things remote in space and time or even non-existent.

If genre continuists want to be taken seriously, they must at the very least explain exactly how a call system could have acquired the very different (indeed, diametrically opposed) properties listed in (8).

Another non-issue is (9):

(9) Did language begin in one place or several?

The answer is simple: it makes no difference. When languages are stripped of their superficial differences they show a remarkably narrow and fixed pattern. That pattern does indeed look like one that might result if all were descended from a single Ursprache. But a moment's thought will indicate that this does not necessarily follow. For while, if there were no biological envelope to limit the diversification of language, languages after tens of thousands of years could diverge in all sorts of weird and unpredictable ways, any such biological envelope would ensure that all languages would conform to a similar pattern, even if the ancestors of those languages developed in different places and at different times. Indeed, the emergence of creoles from radically mixed and degenerate input and their similarity wherever they do emerge (Bickerton, 1981) illustrate

[1] An anonymous referee objected that interjections form a counterexample to this claim: ''Help!'', ''Run!'', ''Here!'', etc. However, these are usually imperatives, with phonetically null but semantically real (and unambiguous!) subjects or even objects: ''(You) help (me)'', ''(You) run'', etc., and thus are really propositions of two or more words. If there is any ambiguity it is because the interjection itself is ambiguous, representing two semantically-distinct propositions: ''(You come) here'' or ''(I am) here''. Other types of interjection (''Ouch!'', ''Wow!'', etc.) are not even true words, since part of the definition of a word is that it can combine with other words.

the robustness of the biological template. A thought experiment also helps; suppose everyone over a year old was removed from the planet, and that wolves or other kindly animals reared enough of the survivors to establish a new community—would the language of that community resemble, or be totally different from, antecedent languages?

We can now turn to some of the more specific arguments centering on the issues raised above, taking them, for convenience sake, in a sequence roughly based on their probable order of emergence in human evolution. As linguists, it's all too easy for us to place major emphasis on the first word in the phrase ''language evolution'', so we should bear constantly in mind that language evolution occurred in real time (therefore at a specific period or periods of prehistory) and formed simply one part, albeit arguably the most significant part, in the overall evolution of our species.

2. Initial selection pressure

Over the years, many and various attempts have been made to determine what factor or factors caused language to emerge in one species and no other. Early attempts focused on co-operative hunting or tool-making, but since many animals hunt co-operatively, and since tool-making can be conducted or even taught (and indeed, among pre-literate peoples, generally is taught) simply through observation and imitation of models, few researchers support such proposals nowadays (Gibson and Ingold, 1993; Hurford et al., 1998). Currently, the favorite pressure is one that involves some form of social interaction, whether ''Machiavellian intelligence'' (attempts to outwit and dominate other group members, Humphrey, 1976), gossip as a substitute for grooming (Dunbar, 1996), or the development of some form of ''theory of mind'' (Povinelli, 1996; Worden, 1998): the ability to realize that the contents of another's mind need not be the same as one's own.

There is, however, a strong biological argument against any ''social intelligence'' approach. No other species has language, even in the most rudimentary form. If any adaptation is unique to a species, the selective pressure that drove it must also be unique to that species; otherwise the adaptation would have appeared elsewhere, at least in some rudimentary form. However, many primates (and certainly all the great apes, our nearest relatives) have an advanced social intelligence, employ Machiavellian strategies, and possess at least the rudiments of a theory of mind; moreover, experiments with apes (Premack, 1976; Savage-Rumbaugh, 1986) show they can be trained to learn and use systems that exploit the conceptual aspects of language, while lacking computational aspects. Thus all but one of the variants of ''social intelligence'' theories have no explanation for why humans, but no other species, acquired language.

That exception is Dunbar (1996), who claims a unique pressure for human ancestors: the growth of groups to a size too large to allow time for the grooming that forms a vital part of primate interactions. But other arguments render this implausible. There is no evidence that human groups grew larger than ape groups until quite recently, indeed what little evidence there is points in the opposite direction. Baboon groups are substantially larger than other ape groups, yet no grooming substitute has developed among them. No valid reason is given for why meaningless, soothing sounds would not have functioned as well (or better) as a grooming device than a more complex adaptation that required every utterance to have some sort of propositional meaning. The sole function claimed by Dunbar – gossip – could not have been exercised until language acquired a critical mass of at least several hundred words, for in its earliest stages its symbols were presumably enumerable in the single digits, and how many items of gossip could you convey with a single set of nine words? For language to enter the repertoire of human

behavior, it had to convey an adaptive advantage from its very earliest stages, or it would never have fixed.

So far, no plausible pressure has been shown to exist outside of the context of human niche construction (see Odling-Smee et al., 2003; Bickerton, 2002, in preparation). The scavenging niche occupied by hominids two to three million years ago virtually obligated a foraging pattern in which groups split into smaller units for search procedures but had to regroup to exploit substantial finds (such as the carcases of large thick-skinned megafauna, which hominids' possession of stone cutting tools would have enabled them to exploit in advance of other scavengers). This pattern required each sub-group to be able to inform the other sub-groups what it had found, in order to secure the best disposition of resources. In such a context, the smallest handful of single-unit utterances would have paid off in terms of survival. Subsequently, since the vocabulary was under cultural rather than biological control, there was nothing to limit any further expansion that might be required, except the fact that there was as yet no automatized mechanism for incorporating symbolic units into larger structures.

It is important, however, to emphasize that symbolic units are probably latent in the conceptual structure of a variety of species, hence that their emergence as a communicative device probably did not involve any significant neurological or genetic change in our remote ancestors. If those other species (the great apes, dolphins, African grey parrots, and even sealions) lack language, the reason is most probably that no compelling pressure selected for such a use of conceptual structure by members of those species. In other words, there was nothing a minimal protolanguage could have conferred on these species that would have enabled them to live longer or have more progeny.

Accordingly, we can safely claim that while syntax might require some substantial modification of the human brain, a protolanguage with symbolic units did not. There appears (contra Jackendoff, 2002) no reason for supposing that, once the first symbolic unit had been produced, there was any genetic limit on adding additional units. Symbolic units were culturally based from the start and can be added to any language without difficulty and without limit, while grammatical structures, being biologically based, cannot be added, changed or deleted.

3. Protolanguage

The notion that the earliest stages of language evolution involved a largely if not entirely structureless protolanguage was first clearly adumbrated in Bickerton (1990) and is now so widely accepted that the term seems to have passed into the general vocabulary of language evolutionists. However, both the part played by protolanguage in the evolution of language and the nature of the individual items it contained remain controversial.

For a number of writers, protolanguage was no more than the first in a series of stages that led to full modern language (Pinker, 1994; Jackendoff, 2002); for others, there was no intermediate stage between protolanguage and the earliest fully syntactical language (Bickerton, 1998; Davidson and Noble, 1993). This issue involves gradualness versus abruptness in language evolution, and we return to it below.

In this section I shall focus on alternatives to the model of a synthetic, pidgin-like protolanguage, differing from fully developed modern language in its vocabulary size, its lack of syntax and its lack of a modern phonology, but in no other significant respects, as originally set forth in Bickerton (1990). Slobin (2002) disputes evidence for protolanguage's lack of structure drawn from early child language on the grounds that children learning inflected languages (Turkish or Inuktitut, for example) acquire morphosyntactic structures in the earliest stages of

acquisition. This is true, but irrelevant. No inflected language preceded protolanguage, *ex hypothesi*, therefore there were no morphosyntactic structures to be learned; the telegraphic speech produced, before age two, by children acquiring uninflected language remains a highly probable approximation to pre-human protolanguage. Slobin also questions the evidence from trained ape productions, on the grounds that a trained ape ''is sampling from an existing language'' (2002:378). This is simply untrue. Most apes were provided with examples from a made-up ''language'' which can only be described as ''pidgin signed English''. Lexical items in this ''language'' may have been modeled in syntactically structured sequences, but no syntactic structure was explicitly taught, and the way the apes put the items together appears to have owed little to the models offered.

Hurford (2003) has proposed that in earlier forms of language, even up to modern human language, there were no proper names. He bases his argument on logical grounds: use of proper names implies certainty about the identity of their designee, and non-human creatures (presumably including hominids) cannot achieve such certainty, being easily fooled by imitations. But since (in life as opposed to logic) what words represent are concepts, not real-world entities, the fact that animals can be deceived about individual identities is wholly irrelevant; if a non-human has the concept of an individual, that concept can be named, and no real-world misapplication of the name affects it or its utility. In his response to commentary, Hurford states that ''Bickerton's assertion that 'they [animals] have a clear concept of a specific individual' is not backed by evidence'' (ibid, 309). This is simply false. A wealth of evidence from ethological studies of primates shows that apes have a very clear idea of the other individuals in their group and even the precise kinship relations of each person (if this were not the case, it would not be possible for A to avenge an action of B's by attacking C, who is B's nephew). We may therefore reasonably conclude that protolanguage had proper as well as common nouns.

Another limitation proposed for protolanguage is the absence of verbs. According to Carstairs-McCarthy (1999), the fact that language as we know it distinguishes between sentences and noun-phrases is surprising, and protolanguage could have consisted of nominalized phrases only. However, a verbless protolanguage seems intrinsically implausible. While most of the first words children learn are nouns, there are always a few verbs among them, and apes seem to learn verbs (with actions as referents) as easily as nouns (with entities as referents). Besides, as pointed out in (8c) above, predication is universal in language. Carstairs-McCarthy makes a valiant attempt to show that Monocategoric and Nominalized English (two imaginary languages consisting entirely of noun phrases) contain predicates, but since no non-arbitrary distinction between subject and predicate is available where everything is an NP, he is forced to define ''predicate'' as in ''predicate-argument structure''. Presumably, *gift* in *Mary's gift of an encyclopedia to John* would then be the predicate, while *Mary*, *John* and *encyclopedia* are arguments. But how then would you recognize the predicate? Only if you knew that *gift* was a nominalized verb while *encyclopedia* wasn't!

3.1. Was protolanguage holophrastic?

However, the most radical proposal with regard to the constituents of protolanguage is that these were holophrastic rather than synthetic. This proposal has been most thoroughly developed by Wray (1998, 2000), but is implicit in the work of many computer modelers (see below) and is explicitly endorsed by Arbib (2003, in press) for rather different reasons. Wray's proposals support genre continuism, since (as noted above in (8b)), animal calls are roughly equivalent to

holophrases, rather than words. Wray claims that protolanguage simply increased the number of such units to a point where they began to impose an excessive memory load, at which point the holophrases were decomposed on the basis of phonetic similarities. Here we can do no better than quote Wray's own example:

> So if, besides *tebima* meaning *give that to her*, *kumapi* meant *share this with her*, then it might be concluded that *ma* had the meaning *female person + beneficiary.* (2000:297)

This leaves out of account the possibility that, although the syllable *ma* might occur as Wray suggests, it would also occur in a number of holophrases lacking any reference to either females or beneficiaries. Not only is this extremely likely, but the only possible alternative is, if anything, even more damaging to Wray's case. For if *ma* occurred always and only where female beneficiaries were involved, the holophrastic protolanguage would be a hollow charade, a mere disguise for a medium already fully synthetic. But if *ma* also occurred where a female + beneficiary reading was impossible – contexts perhaps as numerous as, or more numerous than, those that can bear such a reading – why would the hearer assume that it referred to a female beneficiary in just those cases where such a reading was possible, and how would that hearer account for the other cases?

This and other arguments against the holophrastic model are presented in Bickerton (2003a) and Tallerman (this volume), the latter introducing more issues and treating the topic in considerably greater detail. Among the issues raised are other problems involved in segmenting holistic strings, such as that stemming from the hidden assumption that a holophrase has only one synthetic equivalent (i.e. *kumapi* has to be interpreted by all speakers as equivalent to *share that with her* rather than to expressions of similar meaning such as *let her take it* (minus the beneficiary reading) or *don't be so selfish* (minus both female and beneficiary readings), which would be equally valid in most if not all contexts of use; this too suggests that a holophrastic protolanguage can be no more than a synthetic protolanguage in disguise Another hidden but damaging assumption is that the protolanguage was framed in a sophisticated phonology—an improbable accompaniment to a medium that had barely escaped from the status of an animal call system. For if no sophisticated phonology existed, how could speakers judge whether any pair of phonetic tokens represented ''the same'' or ''different'' syllables? Lacking such a system, there would be no way of distinguishing phonemic contrasts from irrelevant low-level phonetic contrasts, therefore no way in which identity between syllables in different phonetic contexts could be reliably determined.

It can be concluded from the fate of this and the other proposals discussed in this section that there does not, as yet, exist any compelling reason for rejecting the original concept of a protolanguage as containing a categorially complete, if severely limited vocabulary of items roughly equivalent to modern words, but lacking a sophisticated phonology and any consistent structure.

3.2. The timing of protolanguage

The question of when protolanguage emerged merits a brief consideration, if only because an adequate theory must eventually be able to integrate language evolution into the overall development of the genus *Homo*. The wide range of estimates in the literature (from the australopithecine era to the emergence of our own species) suggests that there are inadequate constraints to determine dates at this point. Much depends, too, on the nature of the initial selective pressure. If this was scavenging (as suggested above) the likeliest time of onset would

be between two and three million years ago. It seems intrinsically plausible that a longish period elapsed between the emergence of symbolic units and the emergence of syntax, since complex connections not required by any previous brain operations had to be forged, while perhaps also a critical mass of ''recruitable neurons'' had to be achieved (Calvin and Bickerton, 2000). There the issue must rest until more evidence is available.

4. Syntax

Syntax, of course, presents the severest problem for evolutionists, its complexities being, it would seem, far in excess of the needs of the hunters and gatherers among whom it must have emerged (Premack, 1986). Moreover, although some researchers have written of ''phonological syntax'' or ''the syntax of birdsong'', these are mere metaphors; syntax is as unique in its structure as it is in its function, the linkage of symbolic units, and evolutionary biologists, trained in comparative methods, boggle at a population of one.

Moreover, the question – as one has to keep reminding non-linguists – is not just how did syntax come to be, but why it took the form that it did, rather than some other form. Virtually all work by non-linguists (and even some by linguists) simply ignores this question, but clearly we have neither understood syntax nor produced an adequate theory of language evolution until this last question is answered.

For some, the whole issue is ducked by supposing that syntax was somehow there already, or just grew automatically, or was invented by clever humans thanks to our big brains. One of the first to seriously tackle language evolution, Lieberman (1984, 1998) believes that once phonology had developed, the job was done—neural mechanisms in the motor control areas were simply co-opted to provide syntactic structure. This is a very common supposition, especially among those like Corballis (2002) who believe in the importance of brain lateralization and consequent right-handedness, and those like Stokoe (Armstrong et al., 1995) whose major interest is sign language. The idea is not impossible in itself, but no one has yet managed to flesh out an account of how it could have happened, or how it would have given rise to the distinguishing features of syntax rather than some other set of features. Until someone at least tries, there is really little to discuss here: the problem has been re-named, rather than solved.

A more substantive answer has been proposed by Carstairs-McCarthy (1999). According to this, the syllable served as a model for sentence structure, with the verb representing the nucleus, the subject the onset, and the object the coda. (Ingenious arguments are elaborated to fit SOV and VSO languages into this framework; constituents other than the S, V and O of simplex sentences are ignored.) However, no explanation is given for how (in terms of the necessary neurological changes) an acoustic element could have ''served as a model'' for a formal, abstract system of structure, or why this model rather than some other should have been selected for. Moreover, as Carstairs-McCarthy himself admits, the model makes no provision for either recursion or movement, and a syntax without these central properties is about as viable as *Hamlet* without the Prince of Denmark. The proposal essentially bases itself on the assumption, commoner among non-linguists than linguists, that word-order is central to syntax. Of course, as every syntactician should know, all of the important relationships in syntax are hierarchical and vertical rather than linear, and word-order simply falls out when you read the terminal nodes of a syntactic tree from left to right.

For a more complete explanation of syntax, one might look to those whose business it is, in particular the generativists, since these believe that syntax is biologically based, and must therefore have evolved. But until very recently, generativists have been as unwilling as non-

innatist linguists to tackle language evolution. In part, this appears to be due to Chomsky's never fully explained distrust of natural selection. This has led his followers to take up a position sometimes described as ''neo-neo-Darwinism'', which involves the invocation of a variety of possible non-selectionist mechanisms: Thompsonian laws of form, Fibonacci numbers, symmetry-breaking, self-organization and more (Jenkins, 2000). But, just as with the motor-area hypothesis, no scenario tangible enough for serious discussion has yet been put forward.

A further problem for generativists lies in their espousal of a modular basis for syntax, a natural reaction to the ''general-purpose-machine'' view of the brain held by behaviorists. Unfortunately, over the past two decades, evidence from brain-scanning devices has been accumulating, and while this evidence is hard to interpret (and the fact that few neurologists know any syntax doesn't help), it is indisputable that syntactic tasks simultaneously activate many different brain regions, most of which are also activated by some other task(s) (Indefrey et al., 2001; Dogil et al., 2002; Pulvermuller, 2002). This does NOT license returning to an undifferentiated, equipotential brain model, but it does suggest that the secrets of syntax may lie in the patterns of connection between brain areas, rather than (as in simplistic ''Broca's area'' accounts) any individual area or areas.

Into the middle of this confused and confusing situation there appeared in the journal *Science* a paper (Hauser et al., 2002) aimed at setting the scientific community straight with regard to language evolution. Its magisterial tone was surprising, considering how little work any of its authors had previously produced in the field, but no more surprising than the collaborators themselves: since Hauser was known as a strong continuist and Chomsky as a strong discontinuist, it was almost as if Ariel Sharon and Yasser Arafat had coauthored a position paper on the Middle East. In this paper, practically every aspect of the language faculty is treated as pre-existing the emergence of language, except for ''narrow syntax'' (whether this is the same as, or different from, the old ''core syntax'', we are nowhere told), which consists solely of recursion. Even recursion is supposed to derive from some prior computational mechanism employed by antecedent species for navigation, social cognition or some other purpose as yet undetermined, and then exapted for syntax; researchers are adjured to start searching for such mechanisms. The antidote to this paper is Pinker and Jackendoff (2005), which lists the many aspects of language (and syntax) that Hauser et al. ignore. Neither paper, of course, comes anywhere near answering the question posed in this section.

There are, however, some guidelines towards a solution that should serve us better than those so far discussed. A theory of the evolution of syntax has to account for both the inability of other species to acquire even the rudiments of it, and the apparent rapidity of its emergence suggested by the abrupt emergence of human creativity (if any alternative cause for this creativity is claimed, its role has to be clearly demonstrated, not merely stated). It seems likely, too, that there is some kind of connection between humans' extraordinary brain size and their possession of syntax – perhaps the two most salient of all the unique phenomena that the species exhibits (Calvin and Bickerton, 2000) – although this connection must be much subtler than previous naive claims that big brains made us clever and cleverness made us able to invent language (Donald, 1991). Calvin's suggestion that large numbers of neurons are necessary to maintain the degree of signal fidelity required by the production of complex sentences has yet to receive the attention it deserves.

Perhaps some light will be shed by the Dahlem Workshop on the Biological Foundations and Evolution of Syntax, to be held in 2006, where the issue will be debated by some 40 linguists, biologists, neurobiologists and computer scientists. If, by improved brain scanning methods, one could give a millisecond-by-millisecond description of the circuits activated when a novel

sentence is spoken and the order in which they are activated, it might then be possible to determine what it is that humans do that other species cannot—or at least one might begin to frame testable hypotheses, while perhaps some currently plausible suggestions for possible exaptation of functions in pre-human species might be ruled out.

4.1. The timing of syntactic language

The emergence of syntax is at least an equally debatable issue. Here, fortunately, a constraining factor applies to the end-point of the process. Since human languages, though diverse in their details, exhibit profound structural similarities in all regions of the world, shared elements (i.e. core syntax or Universal Grammar) must have been in place not later than the start of the human diaspora ~90,000 years ago. Note that this conclusion remains unaffected by the issue of whether or not there was a single original language. It also renders implausible the proposal by Klein and Edgar (2002) that a mutation in the human species occurring ~50,000 years ago could have created full human language—unless a ''second wave'' of the human diaspora flowed over and absorbed the first, so that the latter left no signature The motivation behind Klein's claim lies in the fact that human culture began to accelerate shortly after the date mentioned, as shown by a rapidly increasing number of artefact types including the first symbolic artefacts (statuettes, cave-paintings etc.). Like other mutation scenarios (c.f. that of Bickerton, 1990), Klein's proposal can also be ruled out on purely biological grounds: no single mutation could by itself have produced all the interlocking complexities of modern syntax.

However, the motivation for the claim must itself be partly right. The most stunning (as well as the least acknowledged, though the facts are nowhere in dispute) aspect of human evolution is its suddenness. All species ancestral to ours (plus our cousins, the Neanderthals) were conservative in the extreme, preserving their small and limited tool-sets virtually unchanged for hundreds of thousands of years. The emergence of our own species released a torrent of creativity that is still gathering speed. What caused this difference? Clearly, some startling increment in cognition. But what caused cognition to change so dramatically? The emergence of modern syntacticized language is the most plausible, indeed perhaps the only serious contender.

The obstacle that troubled Klein, as well as Noble and Davidson (1993) and others, lies in the fact that the explosion of human creativity seemed to occur only within the last 40,000 years. There are two answers to this. First, most of the artefactual novelties that appeared as a cluster 30–40,000 years ago had occurred as isolated phenomena in Africa over the previous 50–60,000 years (see McBrearty and Brooks, 2000 for a convenient summary of the evidence). To this extent, the ''Great Leap Forward'' is illusory, a merging of prior innovations probably cultural in its nature and motivation, possibly triggered by the start of intense competition with Neanderthals in southern Europe. Second, since the original population of our species was extremely small (a bottleneck of ~2000 individuals has been suggested), it must have taken a considerable time for a critical mass of skilled and creative individuals to accumulate. This suggests that the process of syntacticization was most probably completed at some point between 140,000 and 90,000 years ago (the former figure being currently the likeliest date for the speciation of *Homo sapiens sapiens*).

However, none of this touches the question of when syntax first appeared.

4.1.1. Gradualness versus abruptness

Pinker and Bloom (1990) was the first source to provide a coherent argument that the development of syntax must have been gradual. It viewed language as a complex organ, like

the eye, and assumed that, just as the eye developed by gradual stages from a simple light-sensitive cell, so language must have developed from a formless protolanguage by a series of minute increments. Curiously, their approach embodied a concept of syntax, already archaic by 1990, as a body consisting of multiple rules; speakers with n rules would simply be succeeded by speakers with $n + 1$ rules (though on where the first rule came from they remained silent).

This viewpoint elevates to the level of dogma Darwin's belief that evolution is a gradual process. So it is, usually, but a process known variously as pre-adaptation, exaptation or change of function can often produce an appearance of abruptness (the cooling panels of certain insects enlarged gradually until they suddenly became viable as organs of flight, for instance). Moreover, a particular trait (even a negative one) may piggyback on another that is directly selected for; proneness to sickle-cell anemia is preserved and spread because the allele that triggers it also confers a degree of immunity to malaria, and it has even been suggested that the remarkable persistence and stability of schizophrenia in all human populations (despite the fact that schizophrenics typically have few or no offspring) is due to some close genetic connection between schizophrenia and language (Crow, 1998).

A rule or principle of grammar can spread through a population only if those who have such a rule or principle produce more offspring than those who don't. No evidence for such a selective advantage with respect to any rule or principle has so far been adduced by Pinker and Bloom or any other researcher, and it would be hard even to imagine any.

If syntax developed gradually, it should be possible in principle to reconstruct its evolution in terms of a series of hypothetical grammars, each more complex than its predecessor. Still, 14 years after Pinker-Bloom, no one has yet made a serious effort to do this. The most solid attempt to fill in details of a gradualist scenario is Jackendoff (2002), envisaging nine stages of development. Unfortunately, several of these may be artefacts, as suggested in Bickerton (2003b). For instance, two stages are made out of the emergence of symbolic units: ''use of symbols in a non-situation-specific fashion'' and ''use of an open, unlimited class of symbols''. What reason is there to suppose that symbolic units did not constitute a (potentially) open, unlimited class from their very inception? In his response, Jackendoff (2003:698) suggests that symbols originally ''might be limited to a small innate or learned vocabulary, like primate calls''. ''Might'' is a strange peg to hang a whole linguistic stage on, and his alternatives are both bizarre. If a limited vocabulary was innate, it is no different from primate calls, which are also innate, so why would a species develop two parallel systems to do the same thing? If a limited vocabulary was learned, what would stop anyone from creating and learning more items?

A central, if never yet addressed, problem for Pinker-Bloom, Jackendoff and anyone else who takes a gradualist position on syntax lies in the relationship between language and cognition. If the achievement of full syntactic language was what sparked the creativity of our species noted in the previous section, and if syntax developed gradually, why did it not confer the least inkling of that creativity on species antecedent to our own? In his response, Jackendoff ''[did] not deny [Bickerton's] evidence or the interest of the issue. It's just that you can't do everything'' (2003:698). He seems not to realize that if you don't do that one thing, you surrender the whole gradualist case. If syntax confers enhanced cognition, and no enhanced cognition emerged prior to the appearance of our species, then syntax could not have developed gradually, period. There may be a way out of this impasse, but gradualists must realize that they can't rest on their Darwinian laurels here—if they are to maintain credibility, they must come up with *something*.

5. Computer simulations

We cannot unearth the evolution of language from the fossil or archaeological records, nor can we carry out laboratory experiments. But we can do computer simulations of it, and this has been a rapidly growing industry over the last several years. The main thrust of the movement is to show that once communication is intended, language can self-generate and self-organize until it arrives at a rule-governed system, with little (according to some) or no (according to others) assistance from any specific genetic endowment. Obviously this requires time, but evolutionary time is not in short supply.

The most convenient source for an overview of this approach is Briscoe (2002). In his introduction, Briscoe explicitly associates the approach with the claim by Hurford (1999) that language evolution and the evolution of languages can be handled by the same methods—not a very auspicious beginning. However, the main problem of research in this field so far concerns the unrealistic initial conditions assumed by the researchers, in particular that ''agents have direct access to each other's meanings'' (Steels, 2002:55). This is a serious flaw, as Steels points out, since we (and a fortiori our remote ancestors) had no such access to others' meanings when we first acquired language. But Steels's ''guessing game'' includes equally unrealistic initial conditions. For instance, two robots may be confronted with an array of a red square, a red triangle, a blue triangle and a green circle; one robot has to produce a word to describe one of these objects and the other robot has to guess which object the first robot means. The first robot ''may say *malewina* to mean [UPPER EXTREME-LEFT LOW-REDNESS]'' (to indicate *the red square* in ''their own language'', Steels, 2002:57). But here we are back again with an initial holophrastic language and all of its defects discussed above.

Steels is not alone in assuming decomposition of holophrases as a major engine of language development. Kirby (2000) begins with an artificially limited corpus of five proper nouns (which may be ''Agents'' or ''Objects'') and five action verbs, giving rise to a hundred possible three-word sentences. Simulated individuals are repeatedly asked to produce a string corresponding to one or other of these hundred possible meanings, but since they have neither words no grammar, they say nothing. From time to time, however, they produce at random ''some invented string of symbols'' (Kirby, 2000:308) which may consist of multiple units (e.g. *ecdeaabdda*) or single units (e.g. *d*). If you go on doing this for long enough, apparently, populations of simulated individuals will converge on the same string for the same meaning. But what is the likelihood that actual hominids randomly produced invented strings of symbols for indefinite periods of time? When nobody could figure out what they meant until a stable structure had developed?

There are several more variants on the computational approach, by Batali (1998), Hurford (2000) and others; somewhere among them one can find almost all the initial conditions that one could imagine, save for those that most likely obtained when language began in reality. Powerful and potentially interesting though this approach is, its failure to incorporate more realistic conditions (perhaps because these would be more difficult to simulate) sharply reduces any contribution it might make towards unraveling language evolution. So far, it is a classic case of looking for your car-keys where the street-lamps are.

6. Quick fixes

Given the chaotic state of the field, and the number of ever-proliferating theories, it is hardly surprising that researchers have tried to find some scientific Alexandrian sword that would quickly slice through the Gordian tangle of current confusion and provide a simple,

straightforward solution. Since newcomers to the field might find some of these proposed solutions appealing, I shall insert a couple of caveats here. However, since there will undoubtedly be more quick fixes in the future, the following litmus test should serve as a useful corrective to undue optimism: does the proposal bear directly on and elucidate either of issues (1) and (2) in section 1 above?

6.1. Mirror neurons

During the early 1980s, David Perrett discovered that there were neurons in the brains of macaque monkeys that would fire not only when the monkey made a grasping action but also when that monkey saw another monkey (or a human, for that matter) making a grasping action (Perrett et al., 1982, 1985). A similar discovery, which is almost always cited without any reference to Perrett's pioneering work, was made by Rizzolatti (Rizzolatti et al., 2001, etc.).

Perrett's discovery was largely ignored by the wider community, but Rizzolatti's identical discovery was quickly picked up and received rave reviews. In the field of language evolution they have been most strongly championed by Arbib (2003, in press). Arbib regards mirror neurons as crucial insofar as they enable imitation, and he regards imitation as essential for establishing parity—the condition under which the same symbol carries the same significance for both speaker and hearer. However, it is unclear, under circumstances in which someone uses *dog* to describe dogs, and I eventually realize that *dog* refers to a particular class of animal, what role imitation has played in this process. It might appear that I have simply learned an association the other speaker had already learned, and that my ability thereafter to imitate that speaker is a secondary phenomenon.

Mirror neurons may indeed play a part in ensuring that when someone produces the utterance /*dag*/, I too am able to produce the same phonetic utterance. But even here their role remains unclear. Are they innately programmed to recognize, say, a grasping gesture? Or do I first have to grasp something, or see someone else grasping, in order to program the neurons in question? The question is important, insofar as Arbib places the main emphasis on the role of neurons in enabling us to imitate the kinds of novelty (whether chance-generated or deliberately chosen) that are inescapable if language is to become the infinite system that it is. But by definition, any novelty is something that no-one but its creator has performed, so if the second alternative above is correct, no-one else would have a mirror neuron system for imitating it. Moreover, even if some neurons are innately programmed to mirror common actions, they could hardly be innately programmed to respond to action sequences that nobody had yet produced. Unless of course mirror neurons, like the immune system, come equipped with a set of basic features that can be recombined to imitate any conceivable target—but no-one has (yet) seriously claimed this.

For further arguments against mirror neurons playing any serious role in language evolution, see Bickerton (in press). For the present, suffice it to say that mirror neurons cannot, even in principle, shed any light on how symbols originated or how syntax originated—the two most basic questions in language evolution.

6.2. The FOXP2 gene

One promising route into the hidden history of language evolution is to examine developmental syndromes in which some aspect of the language faculty is impaired, and try to determine if any kind of genetic phenomenon underlies this syndrome. An obvious candidate for such research is Specific Language Impairment (SLI). SLI was observed in an English family

(known as the KE family) where it followed a distribution pattern suggesting a genetic origin, and indeed a team of researchers later linked it with a gene on Chromosome 7 known as FOXP2 (Lai et al., 2001) Like mirror neurons, FOXP2 quickly became a focus for media hype, and was widely described as ''a gene (or even *the* gene) for language''.

Clearly FOXP2 is connected with language in some way, but there are at least two unrelated sets of problems involved with it. The first properly belongs to SLI itself: this is probably a cover term for a variety of language disorders, including some that do not affect the KE family. Second, the precise function of the gene remains unclear. It is a transcription gene, which means that rather than having a direct effect on neural structure or behavior, it turns on a number of other genes whose precise identity remains unknown.

Part of the temptation for claiming FOXP2 as language-connected lies in the fact that it has undergone at least two changes during the period of human evolution, the second perhaps as recently as 1–200,000 years ago. Thus FOXP2 may indeed turn out to have something to do with human-ape differences, perhaps including language. But until we know exactly which other genes FOXP2 turns on or off, it is at best premature to claim any specific function, and simply unscientific to treat it as a major driving force in language.

7. Conclusion

In dealing with language evolution, linguists have at least one advantage over members of other disciplines. The unresolved controversies between various theories of syntax and phonology, not to mention the still virtually uncharted wastes of semantics, should have well prepared them for the striking lack of consensus and the incompatibility of different approaches that will confront them in the field of language evolution. Some of these controversies, indeed, will be quite familiar, having simply been transferred wholesale from our field to the larger one: for instance, the extent to which mental processes are modular or general-purpose, or whether language is primarily innate or primarily learned. About the only things people can agree on are:

(10)
a. Australopithecines probably had no language, but we had it by 50,000 years ago if not sooner.
b. There was probably some sort of protolanguage, though few can agree as to what it was like.
c. A selective pressure of some kind caused protolanguage to appear, and most people currently think it had something to do with social intelligence [though they are clearly wrong, DB].
d. There is probably some connection between the evolution of language and the evolution of cognition, even though few can agree on what that connection was.

Beyond that, pretty well every issue remains highly controversial. But the field has certain advantages. Whether we like it or not, it's going places, since it's a disgrace to the biological sciences that 150 years after Darwin we still can't explain how the most distinctive feature of our species arose. To enter it costs little: you can't do experiments, so no expensive equipment is required (although those with access to fancy new brain scanners have an edge over the rest of us, given that they know what to use them for). It's still a pencil-and-paper field, though with immeasurable amounts of reading and thinking involved. It is, accordingly, an ideal field for any ambitious young scholar itching to make his academic bones. But take care, it's a minefield out

there, strewn with explosive charges of little-known fact ready to blow up the fanciest new theory. The one thing I can guarantee is, it will broaden your horizons—and for us older folk, the mental gymnastics it involves should at least help us to keep Alzheimer's at bay.

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