To understand how the horse evolved, it helps to know what a horse is. To misunderstand the evolution of language and social competence, it helps not to know what they are.

Arbitrariness no argument against adaptation

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The minor disagreements I have with Pinker & Bloom's (P&B's) admirable target article are trivial and beneath mention; but I do believe I can supplement – even strengthen – their case with respect to two points. The first concerns the fact that human grammar is arbitrary. This seems to be the main reason why Piattelli-Palmarini (1989, pp. 22ff) thinks that language is not an adaptation. P&B make good and agreeable points about trade-offs (sect. 3.4.1), but I would stress still further that arbitrariness is no argument against adaptation. Indeed, it is probably true that *all* adaptations are more or less arbitrary. The characters classically called "homologies" are a good case in point; arbitrariness was practically part of their definition (Cain 1964; Coleman 1976; Ridley 1986). The genetic code is a universal homology; it is probably a "frozen accident" (Crick 1968). Any triplet of bases could, in principle, encode any amino acid; the particular code that happens to be used is an arbitrary choice from all the possibilities. But that does not mean it has not evolved by natural selection from a wobbly and simplified precursor. There would have been strong selection on the coding system both to replicate and be transcribed more accurately and to conform to the majority type in the population (once there was sex); it is implausible that it evolved without selection. Moreover, it is not only the code itself that is arbitrary. All that the organism needs is a means of replication. Given that constraint, it could be made of any molecule, of any shape, and with any "word" length and word structure in the code. Whatever the mechanism, natural selection would be the most likely force to establish it in the population: Something as complex as the molecular genetic machinery does not just crop up by chance.

The same point can be made about any homology. In many cases, there is no such advantage, once the character has evolved, to keeping it constant, as there is for the genetic code (and human language). Gould (1989b, p. 213), for example, in his recent book about the animals of the Burgess Shale, remarks that nearly all mammals have seven neck vertebrae. The number could be eight, six, or something very different and the neck would still work. That does not mean that necks did not evolve by natural selection, or that they are nonadaptive.

By extension, the same can be said for most other characters. Consider the dance language of the honey bee. It is again arbitrary. There is no inexorable law forcing bees to symbolize distance and direction by tail waggles. The bees could waggle their front legs, or their back legs, or perhaps, use some mixture of alary serenades and pheromonal stinks. The possibilities are endless. But again, that does not mean the dance did not evolve by natural selection.

In general, for some adaptations there will be many alternatives, for others few (or only one). The claim that language is not an adaptation because it is arbitrary amounts to restricting the term 'adaptation' to characters with a single optimal form. This would be a new usage indeed, new enough to rule out almost every classic example both of natural selection and of adaptation.

Thus, there are variant forms of the 'vertebrate' eye itself. P&B (sect. 3.4.2) point to arthropodan compound eyes, but the octopus eye makes the point more powerfully still. The compound eye is a very different structure, but the octopus eye differs from the vertebrates in several merely "arbitrary" details, such as their direct, rather than inverted, retina. This does make a difference too, of course; but it does not compromise the argument, because equally good eyes can be made on either design. The choice between them was an arbitrary historical accident, like that in the evolution of the genetic code. Imagine picking on one eye-type and reapplying Piattelli-Palmarini's argument (1989, p. 24). He says, "Adaptation cannot even begin to explain why the natural languages that we can acquire and use possess these central features (i.e., the sorts of arbitrary grammatical properties discussed in the target article] and not very different ones," concluding that language is an exaptation, not an adaptation. Now, it is true that we need historical knowledge to explain what sort of retina has evolved, but exaptation is no better able to account for the arbitrary elements in the design than is adaptation.

Even the evolution of melanism in the peppered moth had arbitrary properties. I am sure there are many variant black pigments, and the one that cropped up by random mutation is just one possibility among many. For that matter, camouflage is only one arbitrary choice among many kinds of defence against predators (Edmunds 1974). Does that mean that natural selection was not operating? Or that camouflage is not adaptive?

My second point concerns the nature of modern adaptationism. The question of why a character originally evolved is an interesting one, but it may be worth stressing that most modern work on adaptation is not directly concerned with it. Such methods as optimization (Maynard Smith 1978) and game theory (Maynard Smith 1982) consider only how natural selection maintains a character in a population. They are concerned with whether mutant forms of the character will spread. Earlier work on adaptation was also concerned with this question. The adaptationist's question has the scientific merit of accessibility. In some cases, it is easy to test whether natural selection favors a variant of a character.

I realize that the dispute between Pinker & Bloom, and Piattelli-Palmarini, Gould, and Chomsky concerns the origin, not the maintenance, of language by natural selection. But the debate has been partly inspired by a confusion about the term "adaptation." Piattelli-Palmarini suggests that the concept of adaptation has recently become less important in evolutionary biology. The change in emphasis that he detects, however, is merely verbal, not conceptual. Gould's attempt to confine "adaptation" to characters that are performing the same function as when they first evolved is a piece of verbal imperialism. Adaptation has traditionally had a much broader meaning. When game theorists ask how natural selection is maintaining a character, they think of themselves as studying adaptation. If we keep "adaptation" for designful organs, and do not limit it to designful organs that retain their first function, then neither adaptation, nor adaptationism, will suffer any reduced importance in modern evolutionary biology.

***Commentary by Elliott Sober appears on page 764.** –*Ed.*

The evolution of the language faculty: A paradox and its solution

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Piattelli-Palmarini (1989) is right in reminding us that the proper account of the evolution of human linguistic abilities need not be adaptationist. Still, at this stage, only an adaptationist stance seems to allow detailed and interesting speculation, as Pinker & Bloom (P&B) argue and illustrate. Many of their arguments consist in showing how aspects of language are advantageous. Arguments showing the advantageous (or nonadvantageous) character of properties of language, however, don't automatically carry over to properties of the language faculty itself, and, of course, it is the language faculty, rather than languages or grammars, that should be explained in terms of biological adaptation and selection. P&B tend to extrapolate tacitly and uncritically from language to language faculty, with some questionable results.

Why, for instance, do P&B concede that the diversity of human languages presents "a serious challenge" to adaptationist views? The alleged challenge comes from the view that the diversity of languages is nonadvantageous. Even if true, this would not make it particularly plausible, let alone entail that the language faculty has a corresponding nonadvantageous feature. To make available a language of some complexity, partial reliance on a learning mechanism seems more parsimonious than full innateness (as P&B themselves point out). Any degree of language learnability, as opposed to full innateness, entails language variability. Linguistic diversity, then, may be a nonadvantageous property of language and yet it may not only be compatible with, but even follow from the "good design" of the underlying faculty. Rather than meeting the "serious challenge" of linguistic diversity with detailed arguments, P&B might have exposed it as a plain fallacy.

Although extrapolation from language to language faculty causes P&B to overestimate the problem caused by language diversity, it causes them, more seriously, to underestimate the difficulty involved in describing as an adaptation a mutation that is advantageous, or so it seems, only in a population in which it is widely shared. P&B, following Geschwind (1980), misrepresent the problem. Let me quote Geschwind himself:

> . . . any mutation allowing humans to produce a signal verbally can be advantageous only if there is a mechanism for understanding that signal in other humans. Here one runs into a problem; the new appearance of a system for producing language would be ineffective, since other humans would not understand it. Conversely, the new evolution of a system for understanding language would not be effective, since there would be no other humans to produce it. (pp. 312–13)

A mutation resulting in an enrichment of the language faculty, however, would not cause an individual to speak or understand a richer language. It would merely make him capable of learning such a richer language provided it were spoken around him. The only languages spoken in our better-endowed individual's environment would actually be of the poorer kind, and hence he would end up speaking and understanding the same language as everybody else.

The true problem is not, therefore, one of useless speaking or comprehending abilities, it is one of useless learning abilities. It is a boot-strapping problem, which, in its general form, may also concern other biological bases of social interaction. Most domain-specific cognitive abilities (e.g., color categorization, face recognition) have a specific domain of information available in the environment well before the ability develops; they can be seen as adaptations to that aspect of the environment. This cannot be true of domain-specific cognitive abilities whose specific domain of information is initially empty and gets filled only by the behavior of individuals who already have and use the ability in question. Maintaining that such abilities have been selected (in the sense of "selected for") is paradoxical. Seeing such abilities as emerging in steps scatters the paradox but does not solve it.

If we think of the language faculty in this light, there are two ways to evade the paradox: Either we deny, with Piattelli-Palmarini and others, that the language faculty has been selected, or we deny that the domain of information relevant to the language faculty was empty before the emergence of the faculty itself. The choice is the same if, instead of thinking of the faculty as a whole, we think of its genetically distinct features. Imagine, for instance, a mutation that would make languages containing pronominals learnable, when existing languages did not contain any: We must either deny that this mutation would be an adaptation to the existing environment, or assert that, contrary to appearances, information necessary for this new ability to be of use would already be available.

P&B themselves tacitly depart from their adaptationist stance when they suggest that a grammatical mutation could be beneficial thanks to its being shared among genetically related individuals: This presupposes a prior, presumably nonadaptationist, explanation of the sharing itself. Such a departure from adaptationism may be appropriate, but it is not necessary. I would like to suggest how to make sense of the adaptationist alternative according to which the language faculty (or its components) did not need the presence of languages (or their relevant component features) in the environment in order to be advantageous. This will involve forsaking the traditional view of linguistic communication, a view that P&B do not question.

P&B take for granted and restate the two tenets of the traditional view: First, that organisms can communicate only what they can encode, and second, that languages are systems for encoding, and thereby communicating, propositional structures. In *Relevance: Communication and cognition* (Sperber & Wilson 1986; 1987), Deirdre Wilson and I have argued against both claims.

Regarding the claim that communication requires a code, we contrast the standard "code model" of communication with an "inferential model." Inferential communication is achieved by a communicator displaying evidence of his intention to inform the audience of something, and by the audience inferring the communicator's intention from the evidence displayed. We argue that inferential communication is possible, even in the absence of a code, among organisms that have a sufficiently developed ability to attribute mental states to others. It proceeds in the following manner: The communicator behaves in a manifestly intentional way so as to bring to the mind of the audience a concept or a conceptual structure, for instance, by means of mimicry or pointing. Guided by considerations of relevance in a way we describe in detail in Sperber & Wilson (1986), the audience starts from this conceptual structure to construct a plausible representation of the informative intention of the communicator. Codeless communication is thereby possible.

As for the claim that language encodes propositional structures, it is becoming universally rejected in pragmatics, for compelling empirical reasons. What sentences encode are incomplete conceptual structures that have to be contextually and inferentially disambiguated and enriched in various ways. P&B describe such inferential heuristic processes as involved in the odd case of trying to understand an ungrammatical string, or an utterance in an imperfectly known dialect. But in fact, inferential processes are and have to be used all the time, in the comprehension of every single utterance.

Rejecting the traditional view, we develop a relevance theoretic approach to human linguistic communication, seen as combining coded and inferential communication in the following way: Linguistic utterances are decoded into incomplete logical forms that serve as input to an inferential process of recognition of the speaker's intention, just as do conceptual structures activated by mimicry, pointing or other noncoded communicative behaviors. The difference is that linguistic decoding provides in an automatic and modular manner, much subtler and richer evidence of the communicator's intention than does the perception of noncoded communicative behavior. In other terms, linguistic utterances involve less mental effort, and allow much richer effects, and therefore greater relevance, than noncoded evidence. Still, on this view, decoding is an auxiliary subprocess in the overall inferential process of comprehension.

According to the traditional view, espoused by P&B, the function of the linguistic input system is to recover the semantic structure that was encoded by the communicator. Any departure from such strict decoding, including the addition of extra structure, should be seen as a departure from true comprehension. According to relevance theory, the function of the linguistic input system is to construct automatically a conceptual structure that must have been intended, but not necessarily encoded, by the communicator, thus providing, with minimal effort, a rich initial structure to be fleshed out by inferential processes.

Imagine an utterance consisting of the single word: "Water!", in a situation where it is manifest that the speaker wants to be given some water. As a result of decoding, the hearer might just access the single concept of water, and proceed to infer from that what the speaker wants. Or decoding might result in a sentential structure of the form NP-(V-NP), with "water" occupying the last NP position, and the other positions being left to be filled inferentially. In this second case, the decoding has prepared the way for the inference. If decoding requires less effort than inference, then this would make the whole procedure more efficient. Some might want to argue that single word utterances in present human languages are encodings of full sentential structures, so that attributing a sentential analysis to "water" is decoding in the strict sense. Maybe.

But now imagine a stage in linguistic evolution where the code available consists in simple sound-concept pairs, without higher structures at all. "Water" in such a primitive language encodes the concept of water and nothing else. This is enough – more than enough, actually – to allow inferential communication to take place. Now there appears a mutant who instead of proceeding inferentially from the evidence provided by the fact that the speaker has drawn his attention to the concept of water, is so endowed that he automatically inserts the concept into a sentential structure with otherwise empty slots, and proceeds inferentially from there. This "decoding," which in fact goes beyond what has been encoded, would improve the efficiency of his comprehension and his chances of reproduction. Such a possibility exists in principle for any mutation leading to an enrichment of the language faculty.

But what about the development of richer languages, given that, as I pointed out, the ability to learn is not an ability to produce? Here now is our second generation of mutants: They attribute to other people, sometimes rightly, most of the time wrongly, a mentality similar to their own; they attribute to linguistic signals properties not realized in the surface structure. This lack of surface realization may lead to ambiguities: In the above example, "water" could be in the first rather than the second NP position. There may be cases where our mutants want to eliminate the ambiguity. They haven't yet any linguistic means to do so. They may, nevertheless, use unencoded aspects of behavior, such as variable emphasis or evocative sounds as means of signaling the intended interpretation. On nonmutants, such helpful behavior will be harmlessly lost. Other mutants will be able to use it inferentially, and to resort to similar productions themselves. Their children, the third generation, as it were, will mistake these pieces of nonlinguistic communicative behavior for linguistic signals, and thus the language will be put in phase with the already enhanced language faculty.

One may, in the same vein, think of the very first mutation specifically leading to the language faculty as an ability to process with greater automaticity and reduced context-sensitivity – better: with incipient modularity – certain communicative behaviors of a strictly noncoded type. The mutant would then learn, or rather project, rudiments of a language where in fact there were none. The beneficial effect is one of more efficient comprehension. Another effect, harmless rather than beneficial at this stage, is a relative stereotype in the production of some communicative behaviors. Among the offspring of the mutant, the semimodular processing of these stereotyped communicative behaviors becomes decoding proper; these behaviors become a shared language. *Ecce homo loquens.*

If Pinker & Bloom want to maintain that the function of language is to permit communication by encoding propositional structures, then they still have to solve the paradox involved in describing the language faculty as an adaptation to an environment that is a product – and an indirect one at that – of that very faculty. If they are willing to update their view of the role of language in linguistic communication, and to grant that a language faculty and languages are of adaptive value only for a species already deeply involved in inferential communication (Sperber & Wilson 1986, p. 176), then they can fill a major gap in their adaptationist account.

This view of language

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The authors are to be honored for a paper that goes a long way toward countering the intemperate anti-Darwinism that has become the mode in some cognitive science circles over the past decade. They show, for the first time and in some detail, how Maynard Smith's (1969) concept of "adaptive complexity", elaborated by Dawkins (1983; 1986) for the mammalian eye, bat sonar, and other complex biological systems can be extended to language. By so doing, they have taken an important step toward reconciling modern linguistic theory with Darwinian natural selection. I have three points on which I want to comment.

My first point concerns the physical basis of language that has made possible its evolution by natural selection. As is common in discussions of the nature of language, Pinker & Bloom (P&B) lay heavy emphasis on syntax, relatively little on phonology. Yet phonology is logically prior to syntax (without phonology, no lexicon; without a lexicon, no syntax) and perhaps evolved earlier in our hominid ancestors, as it still develops earlier in the child. The hierarchical relation between syntax and phonology springs from a crucial principle of language design that distinguishes language from all other known systems of animal communication, enabling its speakers to finesse the physical limits of their signaling machinery and, ultimately, in the words of Von Humboldt that Chomsky made famous, to "make infinite use of finite media" (Von Humboldt 1836/1972, p. 70). This is the principle by which a limited set of discrete elements (gestures, phonemes, morphemes) is repeatedly sampled, combined, and permuted to yield larger elements (phonemes, morphemes, phrases) having properties quite different, in structure and functional scope, from those of their constitutive elements.

The principle, so familiar as to seem obvious, is quite rare in the natural world. Abler (1989), terming it "the particulate principle of self-diversifying systems," has shown that it is shared by two other systems: chemical interaction, for which the particulate units are atoms, and biological inheritance, for which the particulate units are genes. Abler contrasts particulate systems with blending systems, such as geology or the weather, in which the result of combining structures is an average, so that properties of the original components are lost, and no new level of discrete structure emerges. If words were formed by blending portions of the acoustic spectrum, or if sentences were formed by blending words, we would rapidly exhaust the communicative potential of the medium. By contrast, the particulate principle affords a vast range of typological variation – effectively unbounded sets of potential phonetic segments, lexical items, and lexical combinations – that is then subject to competing psychophysical, memorial, and motoric selection pressures toward ease of production and ease of comprehension (Lindblom 1983; 1984; in press). Each language is thus one of an uncountable set of solutions to the problem of selecting from the available variants a finite set that will afford "a kind of imped-